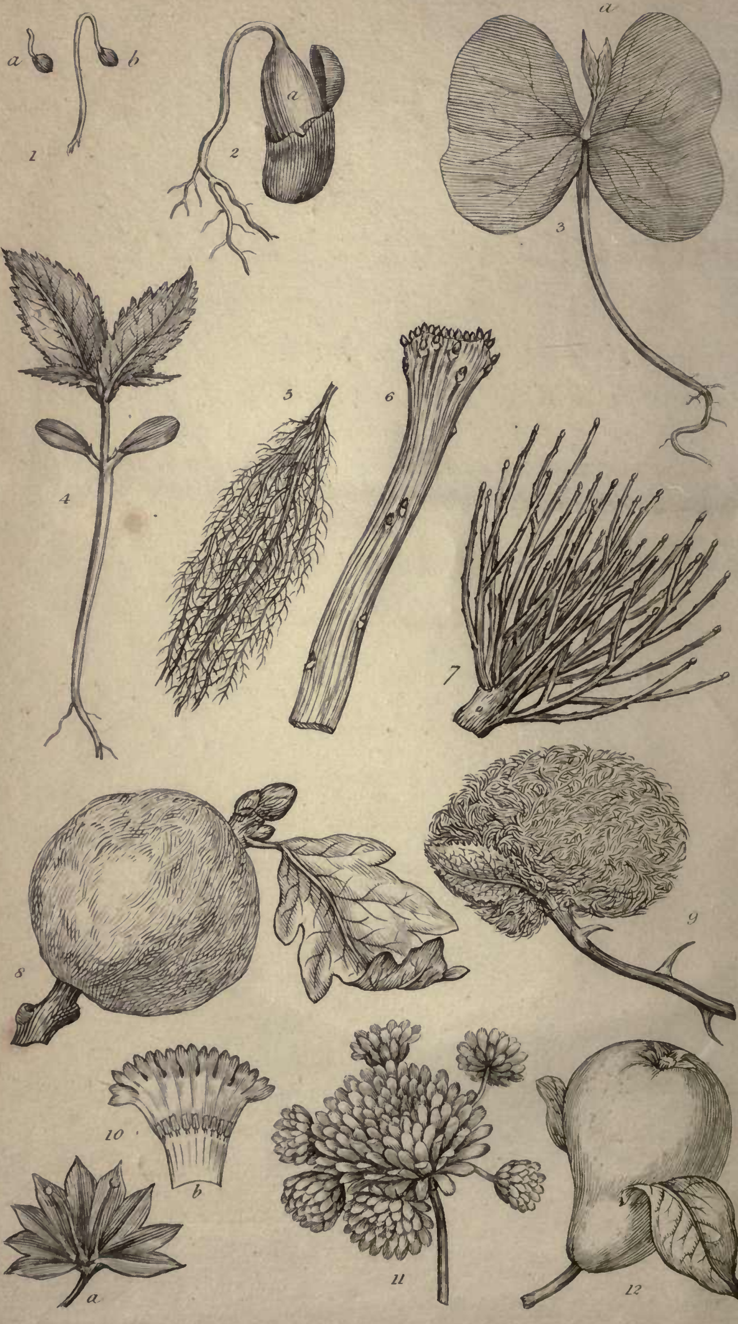
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A

SYSTEM

OF

PHYSIOLOGICAL BOTANY,

BY

THE REV. P. KEITH, F. L. S.

VICAR OF BETHERSDEN, KENT; AND PERPETUAL CURATE
OF MARR, YORKSHIRE.

Illustrated by Nine Engravings.

IN TWO VOLUMES.

Verè scire est per causas scire.
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CONTENTS.

A SYSTEM

OF

PHYSIOLOGICAL BOTANY.

BOOK IV.

OF THE PHENOMENA OF VEGETABLE LIFE.

FROM the analysis of the vegetable structure whether external or internal, and of the primary and constituent principles of which the vegetable is composed, or to which it may be reduced, as exhibited in the several books of the preceding volume, the transition to the subject of the functions of the vegetable organs is both natural and easy. It cannot however be said, that the subject is itself of easy investigation, embracing as it does the phenomena, both physical and chemical, of the whole of the process of vegetation, from the period of the first and incipient symptoms of the agency of the vital principle as displayed in the evolution of the seed, till that of its ultimate and complete extinction as denoted by the death of the plant.

Division of the subject.

The subject therefore necessarily involves the several following topics, which shall each constitute the ground of a separate chapter:—Germination; nutriment; digestion; growth and developement of parts; anomalies of vegetable developement; sexuality of vegetables; impregnation of the vegetable germe; changes consequent upon impregnation; propagation and dispersion of the species; causes limiting the dispersion of the species; evidence and character of vegetable vitality; casualties affecting or destroying the vitality of vegetables.

CHAPTER I.

GERMINATION OF THE SEED.

All plants spring from seed.

Germination is that act or operation of the vegetative principle by which the embryo is extricated from its envelopes, and converted into a plant. This is universally the first part of the process of vegetation. For it may be regarded as an indubitable fact, that all plants spring originally from seed; the doctrine of equivocal generation being now most completely exploded, and an additional proof adduced of the uniformity of the operations of nature. But seeds will not germinate at random, and in all circumstances whatever. They will germinate only under certain conditions, and till such conditions take place the vital principle lies dormant in the substance of the seed. But when a seed is placed in the soil, or in circumstances otherwise favourable to vegetation, the vital principle is immediately stimulated into action, producing a variety of combinations, and effecting a gradual change in the parts of the seed. The radicle is converted into a root; the plumelet into a trunk or stem with its leaves and branches; and a new plant is formed capable of extracting from the soil or atmosphere the food necessary to its growth and developement.

What then are the conditions necessary to germination? What are the changes produced during the process? And by what means are the changes effected? These inquiries shall form respectively the subject of the three following sections.

SECTION I.

Conditions necessary to Germination.

THE conditions necessary to germination relate either to the internal state of the seed itself, or to the circumstances in which it is placed, with regard to surrounding substances.

SUBSECTION I.

Maturity of the Seed.—The first condition necessary to germination is, that the seed must have reached maturity. Unripe seeds seldom germinate, because their parts are not yet prepared to form the chemical combinations on which germination depends. There are some seeds, however, whose germination is said to commence in the very seed-vessel, even before the fruit is ripe, and while it is yet attached to the parent plant. Such are those of the *Tangekolli* of Adanson, and *Agave vivipara* of East Florida,* as well as of the *Cyamus Nelumbo,* of Dr. Smith,† or sacred Bean of India; to which may be added the seeds of the common Garden Radish, whose germination I have often found to be completely effected in the pod, at least in the case of plants that had been allowed to remain after the usual period of gathering. Peas have been, also, known to germinate even when gathered and committed to the soil, in a green and soft state;‡ and I have known a Lemon seed to send out a radicle an inch and a half long, and a plumelet visible to the naked eye, before it was yet extricated from the fruit. But these are examples of rare occurrence; though it is sometimes necessary to sow or plant the seed almost as soon as it

Exception.

* Barton's Elements, p. 58. † Exotic Botany, No. 7.

‡ Seneb. Phys. Veg. vol. iii. p. 377.

is fully ripe, as in the case of the Coffee-bean; which will not germinate unless it is sown within five or six weeks after it has been gathered.

But most seeds if guarded from external injury will retain their germinating faculty for a period of many years. This has been proved by the experiment of sowing seeds that have been long so kept; as well as by the deep ploughing up of fields that have been long left without cultivation. A field that was thus ploughed up near Dunkeld, in Scotland, after a period of forty years' rest, yielded a considerable blade of Black Oats without sowing. It could have been only by the plough's bringing up to the surface, seeds that had been formerly too deeply lodged for germination.

SUBSECTION II.

Exclusion of Light.—The second condition is, that the seed sown must be defended from the action of the rays of light. This has no doubt been long known to be a necessary condition of germination, if we regard the practice of the harrowing or raking in of the grains or seeds sown by the farmer or gardener as being founded upon it. But it does not seem to have engaged the notice of men of science, or to have been proved by direct and intentional experiment till lately. The first direct experiments that were instituted on this subject, are those of Ingenhoutz,* who found that Experiments of Ingenhoutz and Senebier.

* Exper. sur. la Veg. vol. ii.

seeds germinate faster in the shade than in the sun, and hence concluded that light is prejudicial to germination. Senebier,* who afterwards repeated the experiments of Ingenhoutz with the same result, drew from them also the same conclusion.

But it remained to be determined whether the prejudicial effect was to be attributed merely to the light, or partly to the heat accompanying it. From the experiments of Ingenhoutz and Senebier, the injury appeared to be occasioned by the light only; because the comparative experiments in the shade and in the sun, were made at equal temperatures as indicated by the thermometer. With this conclusion, however, though apparently legitimate, M. Saussure professes to be dissatisfied, because the thermometer placed even under the recipient is, in his opinion, incapable of indicating the actual degree of the heat of the solar rays impinging on the surface of the seeds, which he believes to be carried to a very great height, though still escaping our instruments of observation. But this mode of reasoning is, to say the least of it, still more inconsequential than the former; because it is setting up a mere probability from which nothing can be inferred, in opposition to a direct fact, from which something surely should be inferred. It may indeed be true, that the degree of heat impinging on the surface of the seed is so great as to impede its germination; but as no direct proof can be adduced in support of the opinion, we must just rest satis-

Of Saussure.

* Mem. Phys. Chim. vol. iii. p. 341.

fied with the indications of our instruments, till such time as other instruments shall be invented capable of detecting their errors; and with the previous conclusion, till such time as some positive fact shall be opposed to the experiments from which it is deduced.

SUBSECTION III.

Action of Heat.—A third condition necessary to germination is the access of heat. No seed has ever been known to germinate at or below the freezing point. Hence seeds do not germinate in winter, even though lodged in their proper soil. But the vital principle is not necessarily destroyed in consequence of this exposure; for the seed will germinate still, on the return of spring, when the ground has been again thawed, and the temperature raised to the proper degree. But this degree varies considerably in different species of seeds, as is obvious from observing the times of their germination, whether in the same or in different climates. For if seeds which naturally sow themselves, germinate, in different climates, at the same period; or in the same climate, at different periods; the temperature necessary to their germination must of consequence be different. Now these cases are constantly occurring and presenting themselves to our notice; and have also been made the subject of particular observation. Adanson found that seeds which will germinate in the space of twelve

Different seeds require different temperatures.

hours in an ordinary degree of heat, may be made to germinate in the space of three hours by exposing them to a greater degree of heat; and that seeds transported from the climate of Paris to that of Senegal, have their periods of germination accelerated from one to three days.* Upon the same principle, seeds transported from a warmer to a colder climate, have their period of germination protracted till the temperature of the latter is raised to that of the former. This is well exemplified in the case of our green-house and hot-house plants, from which it is also obvious that the temperature must not be raised beyond a certain degree, otherwise the vital principle is totally destroyed.

SUBSECTION IV.

Access of Moisture.—A fourth condition necessary to germination is the access of moisture. Seeds will not germinate if they are kept perfectly dry. Water, therefore, or some liquid equivalent to it, is essential to germination. Hence rain is always acceptable to the farmer or gardener, immediately after he has sown his seeds; and if no rain falls, recourse must be had, if possible, to artificial watering. But the quantity of water applied is not a matter of indifference. There may be too little, or there may be too much. If there is too little, the seed dies for want of moisture; if there

From rain or artificial watering.

* Familles des Plantes, vol. i. p. 84.

is too much, it then rots. The case is not the same, however, with all seeds. Some can bear but little moisture, though others will germinate even when partially immersed; as was proved by an experiment of Du Hamel's, at least in the case of Peas, which he placed merely upon a piece of wet sponge, so as to immerse them by nearly the one half, and which germinated as if placed in the soil. But this was found to be the most they could bear; for when totally immersed in the water they rotted.* There are some seeds, however, that will germinate even when wholly submersed. The seeds of Aquatics must of necessity germinate under water; and Peas have been also known to do so under certain conditions.

Submersion.

SUBSECTION V.

Access of Atmospheric Air.—A fifth condition necessary to germination, is the access of atmospheric air. Seeds will not germinate if placed in a *vacuum*. Ray introduced some grains of Lettuce seed into the receiver of an air-pump, which he then exhausted. The seeds did not germinate. But they germinated upon the re-admission of the air, which is thus proved by consequence to be necessary to their germination.†

No germination in vacuo.

The experiments of Homberg do indeed seem to militate somewhat against this conclusion. They are recorded in the Memoirs of the French Aca-

* Phys. des Arb. liv. ii. chap. viii. † Phil. Trans. No. xiii.

demy, for the year 1669; and the inference deduced from them is, that seeds in general do not germinate if deprived of atmospheric air; but that Cress-seed, Lettuce-seed, and a few others will germinate even in the vacuum of an air pump. But the same experiments, when afterwards repeated by Boyle, Muschenbrock, and Boerhaave, with a much better apparatus, did not confirm the latter part of the result. On the contrary, they tended all to prove that no seed germinates in the vacuum of an air-pump; and that in the cases of germination mentioned by Homberg, the vacuum must have been very imperfect.

The same experiments were again repeated by Saussure the younger,* who says that the seeds of Peas gave indications of germination in vacuo in the course of four days, but never effected any developement of their parts beyond the first appearance of the radicle. But is this a sufficient proof that germination had been really begun? Perhaps it might have been nothing more than merely the effect of the water with which the Peas were moistened, distending their parts; and perhaps we should conclude upon the whole, that in a perfect *vacuum* no seed will germinate; but that in the most perfect *vacuum* hitherto formed by human art some seeds may germinate.

Elucidations from pneumatic chemistry.

Such were the discoveries of phytologists concerning the agency of atmospheric air in the pro-

* Saus. sur la Veg. chap. i. sect. i.

cess of germination, at a period when the study of mechanical pneumatics was but yet in its infancy. It was not yet foreseen that chemistry, lending its aid to the developement of the causes of the phenomena of vegetable life, was to elucidate by means of pneumatical discovery, the mysteries of germination. But this has proved to be the fact. The discovery of the several gases, and of their various chemical properties, has contributed more than all other circumstances put together, to explain and elucidate the phenomena of vegetation. The first experiments on this obscure but interesting subject are those of Scheele; who discovered soon after the introduction of pneumatic chemistry, that Beans did not germinate in any kind of gas indifferently; but that oxygene gas is necessary to the process. Achard afterwards proved that no seed will germinate in nitrogene gas, or carbonic acid gas, or hydrogene gas, except when mixed with a certain proportion of oxygene gas; and hence concluded that oxygene gas is necessary to the germination of all seeds, and the only constituent part of the atmospheric air which is absolutely necessary.

By Scheele, Achard, and others.

The experiments of M. Achard were afterwards repeated and confirmed by a number of other chemists, particularly Cruickshank and Saussure, who found that seeds will not only not germinate in nitrogene gas, but will die if put into it even after germination has been begun, at least if the

Who find that oxygene is the grand agent in germination.

radicle only is developed.* Senebier found also that seeds will not germinate in an artificial atmosphere that does not contain at least one eighth part of its bulk of oxygene; but that the most favourable proportion is when it contains one fourth part.† It has been ascertained, however, that seeds will germinate even in an atmosphere of pure oxygene, though not so readily as when presented in a state of mixture or combination with other gases. It cannot indeed be necessary that the oxygene consumed in germination should be presented to the seed in an uncombined state; as is obvious from the natural agency of the atmospheric air, as well as from direct experiment. Humboldt found that the process of germination is accelerated by means of previously steeping the seed in water impregnated with oxymuriatic acid.‡ Cress-seed treated in this manner germinated in the space of three hours, though its ordinary period of germination is not less than thirty-two hours. The experiment was afterwards repeated by Saussure, with a similar result; and may be regarded as perfectly correct. Thus it is known that this acid parts very readily with its oxygene. The cause, then, of the rapid germination of the Cress-seed is obvious; and the proof that the oxygene does not require to be

* Saussure sur la Veg.

† Mem. sur l'Influ. de l'Air, Nich. Journ. 1801.

‡ Journ. de Phys. xlvii. p. 63.

presented to the germinating seed in an uncombined state, incontrovertible.

In all cases of germination, however, the presence of oxygene is necessary. For even of those seeds that germinate in water, the germination takes place only in consequence of the oxygene which the water contains in an uncombined state. Saussure introduced into a recipient placed over mercury, a quantity of boiling water, into which, when it was cooled down to a proper temperature, he introduced also some grains of Peas, together with a few seeds of *Alisma Plantago*, and *Polygonum amphibium*. They exhibited no symptoms of germination when the quantity of water introduced was not more than seven or eight times the weight of the grains. But when the weight of the water was an hundred or two hundred times more than that of the grains, they then germinated; and the radicle was developed in proportion to the quantity of water employed. The solution of the phenomenon is as follows:—the boiling had not yet deprived the water of the whole of the oxygene it had originally contained in an uncombined state; and it required but to be presented in sufficient abundance to effect the germination of the seed.*

And always necessary.

But the period necessary to complete the process of germination is not the same in all seeds, even when all the necessary conditions have been furnished. Some species require a shorter, and

Period of germination.

* Saussure, sur la Veg. chap. i. sect. i.

others a longer period. The Grasses are among the number of those plants whose seeds are of the most rapid germination; then perhaps cruciform plants; then leguminous plants; then labiate plants; then umbelliferous plants; and in the last order rosaceous plants, whose seeds germinate the slowest. The following table indicates the periods of the germination of a considerable variety of seeds as observed by Adanson.*

	Days.		Days.
Wheat, Millet seed....	1	Orache	8
Spinage, Beans, Mustard	3	Purslain	9
Lettuce, Anise seed....	4	Cabbage	10
Melon, Cucumber, Cress-seed.	5	Hyssop	30
Radish, Beet-root	6	Parsley.	40 or 50
Barley..............	7	Almond, Chesnut, Peach	1 year
		Rose, Hawthorn, Filbert	2 yrs.

SECTION II.

Physical Phenomena.

When a seed is committed to the soil under the conditions that have been just specified, it begins, for the most part, soon after to inhale or imbibe air and moisture, and to expand and augment in volume. This is uniformly the first symptom of incipient germination, though not always an infallible symptom; because the seed may swell with

* Fam. des Plant. tom. i.

moisture merely by being soaked in water, though the vital principle should be totally extinct. But the first infallible symptom of germination is to be deduced from the prolongation of the radicle beyond the extent to which it would attain merely in consequence of soaking. In the latter case the augmentation of the radicle is limited by the extent and capacity of its envelopes, or by the quantity of moisture necessary to its saturation; or by causes inducing incipient putrefaction. But in the former case its augmentation is circumscribed by no such limits: for it not only assumes a swoln and distended appearance in consequence of the absorption of moisture; but acquires an additional and progressive increase in the actual assimilation of nutriment, bursting through its proper integuments, and directing its extremity downwards into the soil. (*Pl.* IX. *Fig.* 1 and 2.)

Evolution of the radicle,

The next step in the process of germination is the evolution of the cotyledon or cotyledons, unless the seed is altogether acotyledonous, or the cotyledons hypogean. (*Pl.* IX. *Fig.* 2.)

Of the cotyledon,

The next step, in the case of seeds furnished with cotyledons, is that of the extrication of the plumelet, or first real leaf, from within or from between the cotyledon or cotyledons, and its expansion in the open air. (*Pl.* IX. *Fig.* 3 and 4.)

Of the plumelet,

The last and concluding step is the developement of the rudiments of a stem, if the species is fur-

And stem.

nished with a stem, and the plant is complete. (*Pl.* IX. *Fig.* 3 and 4.)

The above general remarks are founded on the evidence of the following particular observations. In a season the most favourable to vegetation, Malpighi sowed some seeds of the Gourd. At the end of the first day the seeds were considerably swoln, and the envelopes so much moistened that a fluid oozed out of them when pressed with the finger. A hole was also discoverable in the envelopes at the summit of the seed, through which the moisture seemed to be conveyed to the cotyledons, that had already begun to assume the form of seminal leaves. At the end of the second day the interior membrane seemed to be somewhat torn, and the plantlet somewhat extended, exhibiting on a transverse section taken about the middle, longitudinal fibres and tracheæ, as well as utricles, bark, and pith. The radicle was also distinctly visible. At the end of the third day the exterior membrane had become brownish, and its utricles more distended; the radicle had burst its integuments; and the plumelet had begun to expand. At the end of the fourth day the plantlet had perceptibly augmented in size. The radicle was covered with protuberances from which the lateral branches were to issue; and the interior envelope was somewhat shrivelled, but still covering the seminal and other leaves, in which the nerves were now perceptible.

At the end of the sixth day the leaves of the plumelet had escaped from the seed, though still contained within the cotyledons, being soft but perceptibly covered with hairs. At the end of the ninth day the plantlet had wholly escaped from its integuments, though the plumelet was still enveloped in the seminal leaves, yellowish in its appearance, but gradually assuming a tinge of green. At length its extrication was effected, and the radicle converted into a root, and the rudiments of a stem developed; and on the twentieth day the plant was complete.*

In the course of the summer Ledermuller sowed some grains of Rye in a good soil. At the end of an hour the embryo was perceptibly swollen, and a protuberance distinguishable from which the radicle was to issue. At the end of the second hour the radicle was discernible. At the end of twenty-four hours the embryo had escaped from its integuments. On the second day the fibres of the root had augmented, but the leaves had not appeared. On the fourth day the first leaf (which means, as I should think, the cotyledonous sheath of Gærtner) began to appear above ground; its colour was red. On the fifth day it had grown to the length of an inch, and its colour was now green; and on the sixth day the second leaf appeared.

In both of the above cases the first visible effect was the swelling of the seed in consequence of the

* Anat. Plant. Pars altera.

absorption of moisture. But it was not yet precisely ascertained by what particular medium the moisture had entered; whether by the whole of the surface of the envelopes, or only by a particular point. The latter part of the alternative was soon rendered the most probable.

Mr. Gleichen, having steeped some peas in water for the space of twenty-four hours, observed that when they were pressed between the fingers the water issued from the scar. It seemed therefore likely that it had also entered by the scar, and in order to ascertain the fact he covered the scar of a few seeds with wax, and then put them in water. But the result was that they did not absorb so much moisture in several days, as they had absorbed without the varnish in so many hours. He observed also that peas with the scar varnished did not germinate. It followed therefore that water penetrates the seed chiefly by the scar. A slight degree of doubt, however, seems to have been attached to this conclusion in consequence of some experiments of Senebier, who in repeating those of Gleichen found that seeds did not refuse to germinate, even when the scar was luted. But as he acknowledges at the same time that he was not quite certain whether his lute was water-tight, it is to be presumed that Gleichen's experiments were correct.

The moisture then necessary to germination penetrates the seed chiefly by the scar; but partly also, no doubt, by the foramen, where it exists, and partly by the surface of the envelopes.

But how is the moisture, which is absorbed at the scar or otherwise, transmitted to the plantlet, the radicle of which exhibits the first certain symptoms of germination? Does it enter the plantlet immediately? or is it conveyed to it through the medium of some particular channel? It was early suspected that the moisture destined to give developement to the plantlet first passes through the medium of the cotyledons. This opinion was founded upon the apparent adaptation of the cotyledons for the purpose both of absorbing and transmitting moisture, in consequence of their soft and fleshy texture, and of the vessels dispersed throughout their substance, which, after uniting at last into one bundle, are incorporated into the very body of the plantlet, and are by Grew regarded as the seminal root.* They are sometimes visible even before germination has taken place; but particularly after it has made some progress. On the surface of the transverse section of the lobes of the Bean, after it has been well soaked in water, or after its germination has been begun, they appear in the form of small spots or specks; and on the surface of the longitudinal section, or even on the natural and inner surface of the lobes, their various ramifications may be traced, fewer as they approach their point of union with the radicle, and

* Anat. of Plants, book i. sect. 24.

branching out into minuter divisions as they recede from it.

It passes through the cotyledons.

But it remained to be proved by experiment that the above are the vessels through which moisture is conveyed to the plantlet. It was thought that the fact might be ascertained by means of moistening the germinating seed with a coloured fluid, which was accordingly done by Gleichen, Bonnet, and Senebier, who found as the result of their respective experiments, that the fluid had tinged the vessels of the lobes.* This was a presumptive evidence of their use, but was not quite decisive; for it was still possible that the fluid might have entered by the radicle, and then passed into the lobes. But when Bonnet moistened only part of the cotyledons with a coloured fluid, he found the plantlet tinged also. The fact seemed now satisfactorily ascertained; but other expedients were also adopted with a view to prove or confirm it. The cotyledons were cut off altogether, and the plantlet thus committed to the soil. This experiment had been made indeed by Malpighi at a much earlier period, though not with the same view.† But the result was the same in both cases. The plant perished under the experiment. It perished even when the cotyledons were cut off after germination had made some progress; or if it did not absolutely perish it remained stunted and dwarfish.

* Seneb. Phys. Veg. vol. iii. p. 363.

† Anat. Plant. Pars altera, p. 18.

But it had been also observed that seeds which have lost their cotyledons by means of the depredations of insects do not germinate; and that vegetation also ceases if the plant is too soon deprived of its cotyledons or seminal leaves, even after the radicle has become a perfect root.* It follows therefore that the nutriment necessary to the developement of the plantlet either originally exists in, or intermediately passes through, the cotyledons.

First to the radicle,

But if the nutriment destined to the support of the plantlet passes through the cotyledons, to what part of the plantlet is it first conveyed? This is to be ascertained by tracing the fibres dispersed throughout the lobes, to their point of union and junction with the plantlet, which according to the most accurate dissection is the upper extremity of the radicle.† The nutriment therefore destined to the support of the plantlet first enters the radicle, and is afterwards conducted to the plumelet. Eller, indeed, has maintained that there are vessels in the seed passing immediately from the cotyledons to the plumelet. But later anatomists have not been able to discover them. Even the patient and indefatigable Hedwig could find no traces of any such vessels. It is to be presumed therefore that they do not exist. But a still stronger ground of presumption is that, in the phenomena of the ger-

* Seneb. Phys. Veg. p. 373.

† Grew's Anat. of Plants, book. i. sect. 23.

mination of the seed, the radicle is always unfolded the first, and the plumelet only in a subsequent stage of the process.* It follows therefore that the plumelet derives its nourishment from the radicle, as the radicle from the cotyledons.

Which elongates by descent.

But this is by no means the most singular circumstance relative to the developement of the parts in question. The constant and unerring uniformity with which the radicle and plumelet respectively exert themselves to gain the position and situation best suited to the future developement of their parts, is a phenomenon exhibiting more that is calculated to excite the admiration of the phytologist.

Invincibly.

If a seed or nut of any kind is placed in the proper soil, with the apex of the radicle pointing downwards, the radicle as it elongates will descend in a perpendicular direction, and fix itself in the earth; and the plumelet issuing from the opposite extremity of the seed, will assume a vertical direction and ascend into the air. This is the natural order of the developement of the seminal germe; and from the relative situation of its respective parts, the mode of its developement does not seem to be at all surprising. But the circumstance exciting our surprise is that the radicle and plumelet will still continue to effect their developement in the same manner, however differently the seed may have been deposited in the soil: for if its posi-

* Grew's Anat. of Plants, book i. sect. 37.

tion shall accidentally happen to have been inverted, so as that the radicle shall be above and the plumelet beneath, the former will then bend itself down till it gets a hold of the soil; and the latter will, in like manner, bend itself upwards till it reaches the air. And no human art has ever been able to make them assume contrary directions, or to convert the one into the other, as the root and branch of the vegetating plant may be afterwards converted. Du Hamel, whom no phytologist has surpassed in the invention of expedients to unmask or to control the operations of the vegetative principle, instituted a variety of experiments with a view to effect this conversion, but failed in them all. He first placed an acorn between two wet sponges suspended from the ceiling of his room, so as that the radicle was uppermost and the plumelet undermost. The result however was that the radicle after bursting its integuments assumed a downward direction, and the plumelet in its turn an upward direction, till each had gained its natural position. He then filled a tube with earth, and planted also an acorn in it, in an inverted position. But the radicle and plumelet had no sooner escaped from their envelopes than they began to assume their natural direction as before. He then filled another tube with earth, of a diameter so small, that an acorn, when placed in it, touched the sides of the tube. It was planted in its natural position, and allowed to remain so till the radicle appeared. The tube was then inverted and the radicle immediately began to bend itself downwards. The tube was again inverted, and the radicle resumed its original direction.*

How accounted for by the earlier phytologists.

Such is the invincible tendency of the radicle to fix itself in the soil, and of the plumelet to escape into the air. How is this tendency to be accounted for? A great many conjectures have been offered in reply to the inquiry, but without having done much to elucidate the subject. Some have attributed the phenomenon to the excess of the specific gravity of the juices of the radicle beyond that of the juices of the plumelet, which in their progress upwards were supposed to be reduced by elaboration to a light vapour. But this is not known to be the fact, or rather it is known not to be the fact, and consequently forms no ground of argument. Others have attributed it to the respective action of the sun and earth; the former attracting the leaves and stem, and the latter attracting the root. But it happens rather unfortunately for this conjecture, that the phenomenon is exactly the same, even when seeds are made to germinate in the dark. Du Hamel made the experiment in a dark room; and obtained the same result as in the light. The influence of the sun was then transferred to that of the air, which was thought to have some peculiar attraction for the plumelet, which the earth had not. But the attraction of the

* Phys. des Arb. liv. ii. chap. vi.

air was just as mysterious as that of the sun, and the subject as much in want of elucidation as before.

Dr. Darwin has, however, endeavoured to account for the phenomenon chiefly upon the principle now specified. Supposing the radicle to be naturally stimulated by moisture, and the cotyledons and plumelet by air, the difficulty is, as he thinks, easily solved; each being thus elongated in the direction in which it is most excited.* This hypothesis is no doubt sufficiently ingenious; but is by no means to be regarded as a satisfactory solution of the difficulty. For at this rate all cotyledons ought to rise above ground, which all cotyledons do not. And all seeds ought to germinate either in the earth or water; though many of them will germinate in neither; but on trunks or stumps of trees, and even on the surface of the bare and flinty rock. The radicle ought also to elongate itself in a vertical direction, if it could be but lodged in the lower surface of an insulated mass of mould, so as to have the moisture of the mass and grand exciting cause of its elongation placed above it. Now this must inevitably have happened in one or other of Du Hamel's repeated inversions, and yet the result was always the same; the radicle having uniformly bent itself downwards in the direction of the surface of the earth. By Darwin.

* Phytologia, sect. ix.

By Knight.

Mr. Knight has also more recently attempted to account for the descent of the radicle upon the old but revived principle of gravitation; strengthened, as he no doubt thinks, by the result of the following experiments. Beans which were made to germinate, after being fastened in all positions to the circumference of an upright and revolving wheel, that performed 150 revolutions in a minute, uniformly directed the radicle outwards from the centre, and the plumelet inwards to the centre. Beans that were so fastened to a horizontal and revolving wheel, protruded their radicles obliquely outwards and downwards; and their plumelets obliquely inwards and upwards. These effects Mr. Knight regards as resulting from the centrifugal influence of the wheel's motion counteracting that of gravitation, which is consequently in his opinion, in the natural situation of the seed, the cause of the radicle's descent.* But if gravitation acts so very powerfully upon the radicle, why will it not condescend to exert its influence upon the plumelet also, which, if not so heavy as the radicle, is at least specifically heaver than atmospheric air? And why does it make an exception in favour even of some radicles. The radicle of the seeds of the Missletoe, though adhering merely to the under surface of a bough and originally protruding itself, as it must sometimes unquestionably do, in the direction of the earth's surface, will yet in oppo-

* Nichol. Journ. vol. xiv. p. 410.

sition to the power of gravitation, bend itself upwards till it reaches the bough, and insinuate itself into the very substance of the bark above it? Till these questions can be satisfactorily answered they must be regarded as presenting an insurmountable obstacle to the adoption of Mr. Knight's hypothesis.

Ascribed to instinctive principles.

If I were to offer a conjecture in addition to the many that have been already offered, I should say that the invincible tendency of the radicle to fix itself in the earth or other proper soil; and of the plumelet to ascend into the air, arises from a power inherent in the vegetable subject, analogous to what we call instinct in the animal subject, infallibly directing it to the situation best suited to the acquisition of nutriment and consequent developement of its parts. And upon this hypothesis we include all varieties of plants whatever, parasitical as well as others; for let them attach themselves to whatever substance they will, to them it still affords a fit and proper soil.

SECTION III.

Chemical Phenomena.

As effected in the cotyledon.

The chemical phenomena of germination consist chiefly in the changes that are effected in the nutriment destined for the support and developement of the embryo till it is converted into a plant. It was already shown that this nutriment either passes through the cotyledons, or is contained in them; because the embryo dies when they are prematurely cut off. But the farinaceous substance of the cotyledons, at least in exalbuminous seeds, is a proof that they themselves contain the nutriment. They are to be regarded therefore as repositories of the food destined for the support of the embryo in its germinating state. And if the seed is furnished with a distinct and separate albumen, then is the albumen to be regarded as the repository of food, and the cotyledon or cotyledons as its channel of conveyance. But the food thus contained in the albumen or cotyledons is not yet fitted for the immediate nourishment of the embryo. Some previous preparation is necessary; some change must be effected in its properties. And this change is effected by the intervention of chemical agency.

Action of imbibed moisture. It has been shown in the foregoing section that a seed is no sooner placed in the earth than it begins to imbibe moisture. But the moisture thus imbibed is immediately absorbed by the cotyledons or albumen, which it readily penetrates, and on which it immediately begins to operate a chemical change, dissolving part of their farina, or mixing with their oily particles and forming a sort of emulsive juice; the consequence of which change is a slight degree of fermentation, induced, perhaps, by the mixture of the starch and gluten of the cotyledons in the water which they have absorbed, and indicated by the extraction of a quantity of carbonic acid gas as well as by the smell and taste of the seed.* This is the commencement of the process of germination, which takes place even though no oxygene gas is present.† But if no oxygene gas is present, then the process stops; which shows that the agency of oxygene gas is indispensable to germination.

Of inhaled oxygene.

Accordingly, when oxygene gas is present it is gradually inhaled by the seed; and the farina of the cotyledons is found to have changed its savour. Sometimes it becomes acid,‡ but generally sweet, resembling the taste of sugar; and is consequently converted into sugar or some substance analogous to it. This is a further proof that a degree of fermentation has been induced; because the result is precisely the same in the process of the fermentation of Barley when converted into malt, as known by the name of the saccharine fermentation; in which oxygene gas is absorbed, heat and carbonic acid evolved, and a tendency to germination indicated by the shooting of the radicle. The effect of oxygene therefore in the process is that of converting the farina of the albumen or cotyledons into a mild and saccharine food, fit for the nourishment of the infant plant.

But in what manner does the oxygene operate, and on what principles of the seed does it act? Does it act merely as a stimulant to principles which

* Seneb. Phys. Veg. vol. iii. p. 408.

† Thomson's Chemistry, vol. iv. p. 374.

‡ Saussure sur la Veget.

the seed already contains; or, does it form a combination with the substance of the seed, and identify itself with the germinating embryo? Or, does it abstract from the seed any particular principle of its composition, and so effect the change that follows? The suppositions contained in these questions have each had their defenders and opponents; though there is now, as I believe, but one opinion on the subject.

According to Humboldt.

Humboldt thought that the oxygene acts merely as a stimulant; and his opinion was founded upon the fact that seeds germinate faster, as it appeared, in pure oxygene gas than in common air; but particularly when steeped in water impregnated with oxymuriatic acid, according to his own discovery. But this conclusion, though sufficiently plausible at first sight, is by no means sufficiently warranted by the premises. In pursuit of facts, however, to establish and confirm it, Humboldt was fortunate enough to stumble upon discoveries of some importance. He found that seeds brought both from the East and West Indies, which had constantly refused to germinate at Vienna, germinated very readily when treated with oxymuriatic acid, even after having been kept for a period of from twenty to thirty years. This fact seemed extremely favourable to his opinion, and contributed no doubt to give it a considerable currency at the time, which on some part of the Continent perhaps it still retains, as we find his conclusion to have been adopted, and his language re-echoed, by Professor Willdenow of Berlin, without seeming to know any thing of the facts and experiments by which it has been disproved.*

M. Rollo was of opinion that the oxygene consumed in the process of germination is in part absorbed by the grain, and assimilated to its substance; and in part employed along with the carbon of the seed to form carbonic acid. His opinion was founded on the following fact which he had observed in watching the process of the germination of some grains of Barley confined in an artificial atmosphere. When the seeds were made to germinate in pure oxygene gas, the oxygene gradually disappeared, and its place was found to be occupied by carbonic acid gas.† The above conclusion seemed to follow almost necessarily from the premises; but as the phenomena had not yet been subjected to any particular analysis, it could only be regarded in the light of a conjecture. Rollo.

In this stage of inquiry Saussure the younger, having directed his attention to the subject, perceived that the only means of ascertaining the fact was that of comparing the quantity of oxygene gas consumed with the quantity entering into the composition of carbonic acid gas evolved during the process. If the quantity of the former proved to be greater than that of the latter, it was to be Saussure the younger.

* Princ. of Bot. Engl. Trans. p. 257.

† Annal. du Chim. vol. xxv. p. 37.

inferred that a portion of oxygene gas had been actually assimilated to the substance of the seed. But if the two quantities proved to be constantly equal, then it was to be inferred that the oxygene gas had not been assimilated to the substance of the seed, but only employed for the purpose of abstracting from it part of its carbon in the formation of carbonic acid gas.*

It was obvious that the ascertaining of the respective quantities must have given much elucidation to the subject; and the difficulty of ascertaining them seemed not to be great. Lavoisier had discovered that oxygene in combining with carbon by conbustion undergoes no perceptible alteration of volume, and that 100 cubic inches of carbonic acid gas contain 98 cubic inches of oxygene gas. But the result of combination from germinating seeds must be precisely the same as from combustion; it was easy therefore to ascertain the quantity of oxygene extricated along with the carbonic acid. Accordingly M. Saussure instituted a set of experiments to ascertain the proportion between the quantity of oxygene inhaled and the quantity evolved in the carbonic acid. The grains or seeds employed were those of Peas, Beans, Barley, Lettuce, and Cress, and the issue was as follows: In an atmosphere of 100 cubic inches of common air, known to contain about 21 cubic inches of oxygene, and 79 of nitrogen, when a number of these seeds

* Sur. la Veget. chap. i. sect. ii.

were made to germinate, it was found that if 14 cubic inches of carbonic acid gas were formed during the process, seven cubic inches of oxygene gas remained uncombined in the atmosphere; and if seven cubic inches of carbon acid gas were formed during the process, then 14 cubic inches of oxygene gas remained uncombined in the receiver. From which it followed, undeniably, that the quantity of carbonic acid gas that was evolved during the process of germination was precisely equal to the quantity that had been absorbed during the same process. There was consequently no actual fixation of oxygene in the seed; and the oxygene it had inhaled was employed solely for the purpose of diminishing the quantity of its carbon. The change then effected in the farina of the albumen or cotyledons, by which it is converted into a nutriment fit for the infant plant, consists in diminishing the proportion of its carbon, and in augmenting, by consequence, that of its oxygene and hydrogene principles which the seed is also known to possess.*

Whose opinion is confirmed.

It had been ascertained that seeds may be made to germinate in an atmosphere of pure oxygene gas; and from the experiments of M. Humboldt it appeared that their germination is thus sooner effected than in an atmosphere of common air. But Saussure, in repeating some comparative experiments on this subject, did not find any difference in the respective periods of germination. The only perceptible dif-

Germination in pure oxygene.

* Saussure sur la Vegetation, chap. i. sect. iii.

ference was in the comparative lengths of the radicles; the radicles of such as had been made to germinate in pure oxygene gas having made less progress in the same period of time than the radicles of such as had been made to germinate in atmospheric air.

Application of carbonic acid detrimental.

This may be accounted for in two ways; the oxygene in its pure state might have abstracted too great a quantity of carbon from the seed; or the carbonic gas evolved in too great abundance might have been prejudicial to the developement of the infant plant. For it has been found that carbonic acid gas is not useful to vegetables in general, except in proportion as they can decompose it; and seeds before the developement of the plumelet do not seem capable of effecting that decomposition; and in short it seems that the application of carbonic acid, in almost any proportion, retards rather than accelerates the commencement of germination.*

Dose of oxygene necessary.

It was further remarked, by Saussure, that different species of seeds require different doses of oxygene to excite germination. The quantity consumed by the seeds of Beans and Lettuce, before the commencement of germination, seemed to be equal, and was estimated at the 100dth part of their weight. But the quantity consumed before that period by grains of Wheat, Barley, and Purslain, which seemed also to be equal, was only about the 1000dth

* Saussure sur la Vegetation, chap. i. sect. iii.

part of their weight. The carbon lost at the same time is only about the one third part of these quantities; and the oxygene gas consumed is in proportion to the weight of the seeds, not in proportion to their size or number.

But it has been said that seeds will germinate even in mediums deprived of oxygene; and the germination of seeds placed in water, or in the vacuum of an air pump, has been adduced as a proof. It is plain, however, from the experiments already related, that the germination of the seeds so situated was effected merely in consequence of the uncombined oxygene yet remaining in the water, or of the oxygene remaining in the receiver that was not yet completely exhausted. These alleged proofs therefore are of no value. But Huber and Senebier give an account of experiments in which grains are said to have germinated in atmospheres even of pure nitrogene and hydrogene gas.* Carbonic acid gas was also evolved during the process as in other cases of germination; and it was asked, whence is the oxygene derived necessary to the formation of the carbonic acid? Senebier accounts for it upon the principles of the decomposition of the water contained in the seed; which he seems to be extremely anxious to establish. But Saussure, who being somewhat sceptical on the subject was induced to repeat the experiments that had given the above result, or to institute others of a similar nature, has

Experiments in mediums deprived of oxygene.

Unsuccessful.

* Senebier, Phys. Veget. vol. iii. p. 388.

proved in the most satisfactory manner that no seeds will germinate in an atmosphere of pure nitrogene or hydrogene gas; and that the seeming exceptions to the rule may be all accounted for from the action of the uncombined oxygene contained in the water in which the seed had been placed or previously steeped. For so far are seeds from germinating in an atmosphere of nitrogene gas that, even after germination has been begun and the radicle developed, they will die if put into it, at least unless the leaves of the plantlet have been developed also. The seeds of Peas, Water-cress, and of *Polygonum amphibium*, when put into an atmosphere of nitrogene gas in this state, all died without any further developement of parts.* Seeds immersed in water do indeed evolve a portion of carbonic acid, of carburetted hydrogene, and of nitrogene. But these are elements which separate from the substance of the seed during fermentation, and are observed only when it is found to be in a state of putrefaction. If seeds are placed in a small quantity of water and confined in hydrogene gas, the volume of their atmosphere is considerably diminished, and the remainder is carbonic oxide. But this also is the effect of putrefaction. The carbonic acid which they form of their own substance is decomposed by the hydrogene with the assistance of the caloric disengaged in fermentation. Water is thus formed, and the carbonic acid, deprived of

* Saussure sur la Vegetation, chap. vi. sect. i.

part of its oxygene, is converted into carbonic oxide.* The phenomena therefore observed by Huber and Senebier were not the result of germination, but of putrefaction; and there is no proof of the decomposition of the water contained in the seed during the progress of germination, because there is no hydrogene or oxygene evolved during that process.

Decomposition of water.

There were other grounds, however, on which the decomposition of water was supposed to be effected during the germination of the seed. M. Rollo had observed that many seeds during the process of germination are converted from a mucilage into a sort of sugar; but finding that this effect never took place in mediums deprived of oxygene, and knowing that sugar contains more oxygene than mucilage, he concluded that oxygene was in this case either abstracted from the atmosphere, or obtained from the decomposition of the water with which the seed was surrounded. It could not be abstracted from the atmosphere of the seed, because the quantity of oxygene in the atmosphere of the germinating seed remains the same; it was therefore of necessity obtained from the decomposition of water with which the seed was surrounded.†

Doubtful.

But the same effect will follow if we suppose, with Saussure, that the carbon of the seed is dimi-

* Saussure sur la Vegetation, chap. vi. sect. iv.

† Annal. de Chim. vol. xxv. p. 44.

nished; which will also agree better with the actual phenomena of germination. It must be confessed, however, that this explanation is still liable to some objection; because it has been found that any given weight of seeds dried after germination contain more carbon than the same weight of seeds dried before germination. But by the indefatigable industry and profound investigations of M. Saussure, this objection has been obviated also.

If a seed of any kind whatever, dried as much as possible, is weighed and made to germinate by the aid of water in a close vessel, and if after germination it is taken and dried again, it will be found to have lost in weight even beyond the allowance for carbon which it must have lost, and mucilage which it may have lost, during the process. This is the fact according to the repeated experiments of Saussure.* To what cause is it to be attributed? Saussure attributes it to a diminishing of the water formerly existing in the seed in a fixed state.

Disproved. A quantity of Peas which had been gathered for some years, and placed for some weeks in a stove heated to 20° of Reaumur, were found to weigh 200 grains. They were then made to germinate in a large vessel placed over mercury, containing about five times their weight of water with an atmosphere of common air. When the process of germination was completed, 4·5 cubic inches of carbonic acid gas were found to have been formed in the receiver, which, ac-

* Sur. la Veg. chap. i. sect. iii.

cording to the calculation of Lavoisier, contain 0·85 parts of a grain of carbon. The water which was now evaporated, left as a residue 0·75 parts of a grain of mucilage and extract; and the seeds, which were again dried, evolved during the process a quantity of carbon in the form of carbonic acid very nearly equal to the quantity lost in germination. The seeds therefore ought to have weighed $200 - 0{\cdot}85 + 2 - 0{\cdot}75 = 197{\cdot}5$ grains; but their actual weight was only 189 grains. Now besides the principles already mentioned they could have lost only water, and their loss in that respect amounted to 8·5 grains. It remained, however, to be determined whether the loss was occasioned by means of the process of fermentation, or by that of the drying of the seeds; and the result of the inquiry was that it was occasioned by the latter: because when the process of vegetation was allowed to proceed to double, or even triple the time, the loss of weight remained the same; but when the period of drying was prolonged the loss of weight was more considerable.

Seeds, then, during the process of drying after germination, lose under the modification of water part of their oxygene and hydrogene, which in effect augments the proportion of their carbon. It follows therefore of necessity, that any given weight of seeds dried after germination contains more carbon than the same weight of seeds dried before germination. But the direct agency of oxygene is still the same both in germination and putrefaction —namely, the abstraction of carbon. The results are indeed different. But their difference is easily accounted for; because in the latter case the seed loses a considerable quantity of water, or of hydrogene and oxygene, which in the former case it retains; and hence the proportion of its carbon is of necessity augmented.

Such are the phenomena, physical or chemical, observable in the germination of the seed; air and moisture are absorbed from the soil or atmosphere by the scar, foramen, or envelopes. Their agency is immediately exerted on the farina of the albumen or cotyledons; and a food is thus prepared for the nourishment of the tender embryo, to which it passes through the medium of the vessels of the cotyledons, or, as they have been also denominated, the seminal root. The radicle gives the first indications of life, expanding and bursting its integuments, and at length fixing itself in the soil: the plumelet next unfolds its parts, developing the rudiments of leaf, branch, and trunk: and finally the seminal leaves decay and drop off; and the embryo has been converted into a plant capable of abstracting immediately from the soil or atmosphere the nourishment necessary to its future growth.

CHAPTER II.

OF THE FOOD OF THE VEGETATING PLANT.

If the embryo when converted into a plant and fixed in the soil is now capable of abstracting from the earth or atmosphere the nutriment necessary to its growth and developement, the next object of the phytologist's inquiry will be that of ascertaining the substances which it actually abstracts, or the food of the vegetating plant.

Derived from the earth and atmosphere.

What then are the component principles of the soil and atmosphere? The investigations and discoveries of modern chemists have done much to elucidate this dark and intricate subject. Soil, in general, may be regarded as consisting of earths, water, vegetable mould, decayed animal substances, salts, ores, alkalies, gases, perhaps in a proportion corresponding to the order in which they are now enumerated; which is at any rate the fact with regard to the three first, though their relative proportions are by no means uniform. The atmosphere has been also found to consist of at least four species of elastic matter—nitrogene, oxygene, carbonic acid gas, and vapour; together with a multitude of minute particles detached from the solid bodies occupying the surface of the earth, and wafted upon the winds. The two former ingredients exist in the proportion of about four to one; carbonic acid gas in the proportion of about one part in 100; and vapour in a proportion still less.* Such then are the component principles of the soil and atmosphere, and sources of vegetable nourishment.

By selection.

But the whole of the ingredients of the soil and atmosphere are not taken up indiscriminately by the plant and converted into vegetable food, because plants do not thrive indiscriminately in all varieties of soil. Part only of the ingredients are selected, and in certain proportions; as is evident from the analysis of the vegetable substance given in the foregoing book, in which it was found that carbon, hydrogene, oxygene, and nitrogene, are the principal ingredients of plants; while the other ingredients contained in them occur but in very small proportions. It does not, however, follow that these ingredients enter the plant in an uncombined and insulated state, because they do not always so exist in the soil and atmosphere; it follows only that they are inhaled or absorbed by the vegetating plant under one modification or another. The plant then does not select such principles as are the most abundant in the soil and atmosphere; nor in the proportions in which they exist; nor in an uncombined and insulated state. But what are the substances actually selected; in what state are they taken up; and in what proportions? In order to give arrangement and elucidation to the subject, I

* De Luc on Evaporation.

shall consider it under the six following heads: Water, Gases, Vegetable Extracts, Salts, Earths, Manures.

SECTION I.

Water.

As water is necessary to the commencement of vegetation so also is it necessary to its progress. Plants will not continue to vegetate unless their roots are supplied with water; and if they are kept long without it the leaves will droop and become flaccid, and assume a withered appearance. Now this is evidently owing to the loss of water. For if the roots are again well supplied with water the weight of the plant is increased, and its freshness restored. But many plants will grow, and thrive, and effect the developement of all their parts, if the root is merely immersed in water, though not fixed in the soil. Lilies, Hyacinths, and a variety of plants with bulbous roots, may be so reared, and are often to be met with so vegetating; and many plants will also vegetate though wholly immersed. Most of the marine plants are of this description. It can scarcely be doubted therefore that water serves for the purpose of a vegetable aliment.

Absorbed by the root.

But if plants cannot be made to vegetate without water; and if they will vegetate, some, when partly immersed without the assistance of soil; and some even when totally immersed so as that no other food seems to have access to them; does it not follow that water is the sole food of plants, the soil being merely the basis on which they rest; and the receptacle of their food? This opinion has had many advocates; and the arguments and experiments adduced in support of it were at one time thought to have completely established its truth. It was indeed the prevailing opinion of the seventeenth century, and was embraced by several philosophers even of the eighteenth century; but its ablest and most zealous advocates were Van Helmont, Boyle, Du Hamel, and Bonnet, who contended that water, by virtue of the vital energy of the plant, was sufficient to form all the different substances contained in vegetables.

Thought to be the sole food of plants.

The opinion countenanced by the experiments of Van Helmont,

Van Helmont planted a Willow weighing 50lb. in an earthen vessel containing a known quantity of earth which had been previously dried in an oven. He moistened it with distilled water, or with rain water, and took care to prevent any accession of other earth. At the end of five years the plant was taken up and weighed. Its weight, together with that of all its leaves, was 169¼lb. and the weight of the earth, only two ounces less than at first, giving an accession of 119¼lb., which is to be accounted for only from the water with which the earth was moistened. Hence it was concluded that water is the sole food of plants; the two ounces of earth lost being regarded as bearing too small a proportion to the increased weight of the willow to deserve any notice in the calculation.*

Boyle dried a quantity of earth in an oven, which after having weighed he put into an earthen pot. He then sowed some Gourd seed in the earth and watered it with spring or rain water. A plant was ultimately produced that weighed three pounds; and in a subsequent experiment, a plant that weighed four pounds; and yet the weight of the earth, when dried and weighed again, was not perceptibly diminished. This seemed to give weight to the foregoing conclusion. Boyle.

Du Hamel placed some bulbous roots merely in moss or wet sponges, and they vegetated; and Beans and Peas when so treated even flourished and produced fruit.† Bonnet in repeating the experiments of Du Hamel had the same result; and in trying its operation upon vines, found that they produced excellent grapes. Nothing further seemed necessary to determine the point at issue; and it was accordingly believed that water is the sole food of plants, and that the other substances which they may contain are formed merely from the water, by virtue of the vital energy of the plant. DuHamel, and Bonnet.

But though these experiments have the appearance of being somewhat decisive, yet there are others by the same experimenters which are not quite so favourable to the opinion they were in-

* Phys. des Arb. liv. v. chap. i. † Ibid.

tended to support. Du Hamel reared in the above manner plants of the Horse-chesnut and Almond to some considerable size, and an Oak till it was eight years old.* And though he informs us that they died at last only from neglect of watering; yet it seems extremely doubtful whether they would have continued to vegetate much longer even if they had been watered ever so regularly: for he admits in the first place that they made less and less progress every year; and in the second place, that their roots were found to be in a very bad state.

Which are insufficient to decide the point.

But if they had even continued to vegetate, still the experiments were insufficient to decide the point in question. Their insufficiency was first pointed out by Bergman in 1773, who showed from the experiments of Margraff, that in one pound of rain water there is contained one grain of earth.† Earth, therefore, must have been absorbed along with the water; so that even the boasted experiment of Van Helmont, on which so much stress had been laid, amounted to nothing. For the rain-water employed in the experiment must have contained in it as much earth as could have been well expected to exist in the willow at the end of five years. And if not, then it is easy to point out an additional source of supply: for it has been shown by Hales and others, that unglazed earthen vessels when placed in the earth, will readily ab-

* Phys. des Arb. liv. v. chap. i. † Opusc. vol. v. p. 92.

sorb moisture;* so that, according to Mr. Kirwan's remark, the earthen vessel in which the willow was planted must have absorbed moisture from the surrounding soil, impregnated with whatever substances the earth contained. The access of earth therefore is accounted for without the joint efforts of the water and vital energy of the plant, and no satisfactory proof alleged of the similar formation of other substances.

Inference of Hassenfratz that water is not the sole food of plants.

The subject was afterwards investigated by Hassenfratz, who saw the insufficiency of the foregoing proofs, and objected to them because no account was given of the proportions of carbon at the commencement and termination of the respective experiments. Did not the carbon of the plant increase also as well as its other ingredients? And yet the carbon could not be supposed to be formed from the water. To clear up this point he analyzed the bulbs of the Hyacinth and of several other plants, together with a number of Kidney-beans, and Cress-seeds, with a view to discover the quantity of carbon they contained, and consequently by calculation the quantity contained in any given weight of similar bulbs or seeds. He then made a number of each to vegetate in pure water, some within doors, and others in the open air, having first ascertained their weight. They germinated, grew up, and flowered; but produced no seed. They were afterwards gathered, leaves and all, and

* Veg. Stat. vol. i. p. 5.

subjected to a chemical analysis, the result of which was, that the carbon contained in each was somewhat less than the quantity which existed in the bulb or seed from which the plant had sprung.* From this singular and unexpected result Hassenfratz concluded, as he was no doubt well entitled, that water is not the sole food of plants; because plants vegetating in pure water receive no accession of carbon, without which they cannot produce perfect seeds.

Deduced from erroneous premises,

But although this conclusion is certainly right, yet the premises from which it is deduced are as certainly wrong; and yet they seem to have been admitted by some phytological inquirers who were no doubt well qualified to judge of their value. But at this rate we must believe that Du Hamel's Oak of eight years old, and Van Helmont's Willow that was increased in weight by upwards of 100lb., contained no more carbon, even including all the leaves that fell annually, than the original acorn or original plant; and we must believe that the seeds of aquatics contain as much carbon as the plants they produce, together with all their seeds, which is an absurdity. Senebier saw the impossibility of admitting the premises, and rejected them;

Which Saussure rejects,

but Saussure put them again to the test of experiment. Having gathered some plants of the *Mentha piperita*, he found that 100 parts in weight of the green vegetable substance were reduced by

* Annal. de Chim. vol. xiii. p. 178.

drying to 40.29, which were found by experiment to contain 10.96 of charcoal. He then took a number of plants of the same species, and placed them by the roots in bottles filled with distilled water; exposing them to the sun on the outside of a window, but sheltering them from the rain. After ten weeks of vegetation the 100 parts of mint weighed in their green state to 216 parts, which were reduced by drying to 62. They had augmented therefore in dried vegetable matter 21.71 parts; but they had augmented also in their quantity of carbon: for the 62 parts of dried vegetable substance furnished 15.78 of charcoal.* A similar result was obtained from a similar experiment upon Beans, from which we may infer the accuracy of Saussure, and the consequent inaccuracy of Hassenfratz, who was no doubt misled by some circumstance not taken into the account. Perhaps the plants on which he made his experiments were not sufficiently exposed to the light of the sun; so that if he corrected one error he committed also another. While we maintain, therefore, that water is not the sole food of plants, and is not convertible into the whole of the ingredients of the vegetable substance, even with the aid of the vital energy; we must at the same time admit that plants, though vegetating merely in water, do yet augment the quantity of their carbon.

Though he establishes the same conclusion.

* Sur la Veg. chap. ii. sect. v.

SECTION II.

Gases.

WHEN it was found that water is insufficient to constitute the sole food of plants, recourse was next had to the assistance of the atmospheric air; and it was believed that the vital energy of the plant is at least capable of furnishing all the different ingredients of the vegetable substance, by means of decomposing and combining, in different ways, atmospheric air and water. But as this extravagant conjecture is founded on no proof, it is consequently of no value. It must be confessed, however, that atmospheric air is indispensably necessary to the health and vigour of the plant, as may be seen by looking at the different aspects of plants exposed to a free circulation of air, and plants deprived of it: the former are vigorous and luxuriant; the latter weak and stunted. It may be seen also by means of experiment even upon a small scale. If a plant is placed under a glass to which no new supply of air has access, it soon begins to languish, and at length withers and dies; but particularly if it is placed under the exhausted receiver of an air-pump; as might indeed be expected from the failure of the germination of the seed in similar circumstances. According to the experiments of Saussure, plants of Peas, though completely developed and furnished with their leaves, died in the

Atmospheric air indispensable to vegetation.

space of three days, when put into the exhausted receiver of an air-pump whether in the shade or the sun. But plants with thick and succulent leaves seem capable of supporting vegetation *in vacuo,* at least if exposed to the sun. A plant of the *Cactus Opuntia* lived more than a month in this state without showing any symptoms of decay, except that the epidermis seemed dry, which again recovered its freshness, however, in the atmospheric air.* And though plants with thin leaves generally died under the experiment, yet there were exceptions even among them. A plant of the *Polygonum Persicaria* lived for six months in the *vacuum* of an air-pump, and was at the end of the experiment as fresh and vigorous as at the beginning, with the exception of two or three leaves near the root, which were withered. The same was the case also with plants of the *Epilobium molle, Epilobium hirsutum, Lythrum Salicaria,* and *Inula dysenterica.* They were placed in the light, but not so as to receive the direct rays of the sun; to which when they were exposed they withered, even though the rays were but feeble.

It has been said indeed that roses will remain longer fresh in vacuo than in common air. But this is a mistake. In the latter case the petals, no doubt, fall sooner; but this is merely the natural effect of vegetation, and not a symptom of decomposition or decay, as is proved from the inspection

* Sur la Veg. chap. vi. sect. v.

of the petals even after they have fallen, in their still exhaling an agreeable though faint odour. But in the former case, though the petals remain longer attached to the plant, and retain their form and colour, and appear to be fresh and fragrant; yet when you put them to the test, they are found to exhale a strong and fetid odour, the sure symptom of inward putrefaction.*

But although we admit the great utility of atmospheric air, and even its absolute necessity to the support of vegetable life, we must not attribute to it more than is due, and conclude without proof that air, together with water, forms the whole of vegetable aliment. And yet in support of this doctrine it has been said that many plants do evidently effect the developement of their parts, without the aid of any other nourishment beyond that of air, rains, and dews; and the Mosses and Lichens, and some other tribes of plants have been quoted as affording examples.

It must be admitted no doubt that plants of slow growth and tenacious of life, such as many of the Mosses, and some of the succulent plants, do indeed effect the developement of their parts, without the aid of any other nourishment beyond what they derive from the atmosphere. But plants of rapid growth, such as annuals, can never effect that developement without the aid of nourishment derived from the soil. Saussure tried the experiment upon

* Saus. sur la Veg. chap. vi. sect. v.

Beans, Peas, and Cresses, by placing them in horse-hair, or in pure sand, and moistening them with distilled water. They grew indeed, and some of them even flowered, but never produced perfect seeds.* And Giobert and Hassenfratz, who had made similar experiments, had also similar results. It is plain therefore that some essential principle of nourishment was wanting, which is furnished by the soil; and that atmospheric air and water are not the only principles constituting the food of plants.

But as in germination so also in the progress of vegetation, it is part only of the component principle of the atmospheric air that are adapted to the purposes of vegetable nutrition, and selected by the plant as a food. Let us take them in the order of their reversed proportions.

Or at least some of its component parts.

SUBSECTION I.

Carbonic acid gas.—In the process of the germination of the seed, the effect of the application of carbonic acid gas was found to be altogether prejudicial. But in the process of subsequent vegetation its application has been found, on the contrary, to be extremely beneficial. Plants will not indeed vegetate in an atmosphere of pure carbonic acid, as was first ascertained by Dr. Priestley, who found that sprigs of mint growing in water, and

Beneficial to vegetation in the sun.

* Sur la Veg. chap. viii. sect. i.

placed over wort in a state of fermentation, generally became quite dead in the space of a day, and did not even recover when put into an atmosphere of common air.*

But Dr. Percival, of Manchester, observed that a plant of Mint, immersed in water by the root and exposed to a current of atmospheric air mixed with carbonic acid gas, was more vigorous and luxuriant than a plant of the same species similarly situated and exposed to a current of pure atmospheric air.†

Improving upon this hint, Saussure made some experiments with a view to determine the dose of carbonic acid gas which, being mixed with atmospheric air, is the most favourable to vegetation. Having made some Peas to germinate in water till they acquired the height of four inches and weighed about twenty grains, he then placed a number of them in glasses filled with water by threes, so as that the roots only were immersed, and introduced them into receivers filled with different mixtures of common air and carbonic acid gas. They were situated so as to receive the direct rays of the sun, moderated when too intense. The mean augmentation in weight of such as were placed in pure atmospheric air, and exposed during ten days to the sun, was eight grains to each plant. Such as were exposed to the sun, in an atmosphere of pure carbonic acid gas, faded and withered away without any further developement. In an atmosphere con-

* Priestley on Air, vol. i. p. 36. † Manch. Trans. vol. ii.

taining three-fourths or two-thirds of its volume of carbonic acid gas, they withered also; but in an atmosphere containing only one half of its volume of carbonic acid they lived seven days. And in an atmosphere containing but one fourth of its volume of the same gas, they lived ten days and augmented their weight by five grains. Lastly, the mean augmentation in weight of such as were placed in an atmosphere of common air, containing one-twelfth part of carbonic acid gas, was eleven grains. This experiment was repeated frequently, and was found to yield a uniform result; the plants always succeeding better than in pure atmospheric air. Carbonic acid gas, therefore, is of great utility to the growth of plants vegetating in the sun, as applied to the leaves and branches; and whatever increases the proportion of this gas in their atmosphere, at least within a given degree, forwards vegetation.*

But prejudicial to it in the shade.

But the result was not the same when the plant was placed in the shade; the smallest dose of carbonic acid gas, in addition to that of the atmospheric air, being then prejudicial to vegetation. This appears from the following experiments: Plants kept in the shade, and placed in an atmosphere containing one-fourth of its volume of carbonic acid gas, died on the sixth day; and when the atmosphere contained only one-twelfth of its volume of this gas, they lived indeed ten days, but weighed only three grains; while those in pure atmospheric

* Sur la Veg. chap. ii. sect. v.

air weighed five grains. Carbonic acid gas, therefore, as applied to the leaves and branches of plants, is prejudicial to their vegetation in the shade, if administered in a proportion beyond that in which it exists in atmospheric air.

Its influence as applied to the root.

But it is also beneficial to the growth of the plant when applied to the root. This Saussure ascertained by experiment also. Two boards pierced with a number of holes were made to float in two vessels filled, one with distilled water, and the other with water impregnated with carbonic acid gas. On each of these boards was placed a number of Peas that had been lately made to germinate in distilled water. Their radicles at the commencement of the experiment were two lines and a half long. At the end of ten days the roots in contact with the distilled water were longer by five inches, than those in contact with the acidulated water; and the stalks and leaves were developed in the same proportion. But at the end of a month the plants vegetating in the acidulated water had acquired the same dimensions as the others, and at the end of six weeks had considerably surpassed them. It follows, therefore, that carbonic acid gas as applied to the roots of plants is also beneficial to their growth, at least in the more advanced stages of vegetation.

SUBSECTION II.

Oxygene.—As oxygene is essential to the commencement and progress of germination; so also it is essential to the progress of vegetation. This is clearly proved by the following experiments of Saussure: Having pulled up some young plants of the Horse-chesnut, furnished with their leaves and weighing about 460 grains, he introduced their roots, which were nearly a foot in length, into receivers of about sixty cubic inches in capacity, and luted the base of the stem to the neck of the receiver. Into one of the receivers, each of which contained a quantity of distilled water, he introduced twenty-eight cubic inches of nitrogene which was in contact with the upper part of the root, while the under part was immersed in the water. Into another he introduced an equal quantity of hydrogene; and into a third an equal quantity of carbonic acid. The plant whose root was in contact with the carbonic acid died in the course of eight days: the others lived a fortnight, but had not diminished the volume of their atmosphere. But plants which were placed at the same time in a similar apparatus, furnished with atmospheric air, gave a very different result; for at the end of three weeks when the experiment was stopped, they were still fresh and vigorous, and the volume of their at-

Its beneficial influence as applied to the root.

mosphere was diminished.* It is obvious, then, that the presence of oxygene is beneficial to the growth of the vegetable, at least as applied to the root; because that is the only principle which had access to the root in the last experiment, which had not access to it in the former.

Branch and leaf.

But oxygene is beneficial to vegetation as applied also to the other parts of the plant as well as to the root. Branches of woody plants taken in the spring, immediately before the expansion of the bud, and enclosed in receivers filled with common air, together with a small quantity of water to supply them with moisture, developed their leaves as if attached to the parent plant. And this developement was effected solely by means of the oxygene contained in the receiver; for in mediums deprived of oxygene no developement took place.† The presence of oxygene therefore is necessary to the developement of the leaves.

Flower and fruit.

But it is necessary also to the developement of the flower and fruit. The flower-bud will not expand if confined in an atmosphere deprived of oxygene; nor will the fruit ripen. Flower-buds confined in an atmosphere of pure nitrogene faded without expanding. A bunch of unripe Grapes introduced into a globe of glass which was luted by its orifice to the bough, and exposed to the sun, ripened without effecting any material alteration

* Sur la Veg. chap. iii. sect. vi. † Ibid. sect. viii.

in its atmosphere. But when a bunch was placed in the same circumstance, with the addition of a quantity of lime, the atmosphere was contaminated, and the Grapes did not ripen.* Oxygene therefore is essential to the developement of the vegetating plant.

But how is this beneficial effect operated? Is the oxygene actually inhaled into the body of the plant and converted into a vegetable food; or is its operation merely external? Oxygene is actually inhaled, at least under certain circumstances. Saussure having suspended a plant of the *Cactus Opuntia,* after sun-set, in a receiver containing forty-eight cubic inches of atmospheric air deprived of its carbonic acid, but of which six cubic inches were displaced by the leaves, found early next morning, after making the necessary corrections relative to change of temperature and pressure, that the atmosphere of the plant had diminished in volume four cubic inches. The remaining air when examined contained but $\frac{14}{100}$ of oxygene, though before the introduction of the *Cactus* it had contained $\frac{21}{100}$ of the same gas. It follows, therefore, that the diminution of quantity had affected the oxygene only. But the oxygene did not exist in the atmosphere of the plant under any combination whatever; for the application of lime water gave no indications of the presence of carbonic acid. The oxygene of the atmosphere, therefore, must

How effected.

* Sur la Veg. chap. iii. sect. ix.

have been abstracted by the leaves of the *Cactus*. From which it also follows that the leaves of vegetating plants do actually inhale oxygene, at least in course of the night.

Similar experiments on vegetating plants gave similar results, but the quantity of oxygene abstracted was not always in the same proportion. In the present case it was very considerable, amounting to three-fourths of the volume of the leaves, while in other cases it was often not more than one-half of their volume.

SUBSECTION III.

Nitrogene.—Though nitrogene gas constitutes by far the greater part of the mass of atmospheric air, it does not seem capable of affording nutriment to plants; for as seeds will not germinate in it, so neither will plants vegetate. It was regarded, however, as constituting a vegetable food by some of the earlier pneumatic chemists, particularly by Priestley, who found, as it seems, that some sprigs of Mint on which he had made the experiment vegetated better in phlogisticated air than in either dephlogisticated or common air;* and hence he inferred that phlogisticated air, the nitrogene of modern chemists, serves as a vegetable food.† In this opinion he was followed by Ingenhoutz,‡

Not a vegetable food,

* Priestley on Air, vol. iv. p. 327. † Ibid. vol. v. p. 13.

‡ Exper. sur les Veg. vol. ii. p. 146.

whose experiments appear to have given a similar result; contradicted, however, by the result of the experiments of Senebier, Woodhouse, and Saussure, on the same subject.

Branches of *Populus nigra* and *Salix alba*, whose leaf-buds were just ready to open, were introduced by Saussure into an atmosphere of nitrogene both in the shade and sun. They effected no farther developement of parts, but were found to be in a state of putrefaction after a period of five days; but in an atmosphere of common air they readily effected their developement, and continued to vegetate for many weeks. Roses and Lilies gathered two or three hours before their expansion, and treated in the same manner, gave similar results.

Though some plants will live in it for a time.

It must be admitted, however, that many plants will continue to vegetate for a time in an atmosphere of nitrogene gas, when their leaves have been previously developed; but they are such plants only as present a great extent of surface, and consume but little oxygene in the shade. A plant of the *Cactus Opuntia*, nourished with water and placed in an atmosphere of nitrogene gas exposed to the influence of the sun, was found capable of supporting vegetation for the space of three weeks; but it was greatly injured by the experiment, and in the shade it lived only five days. A plant of the *Sedum telephium* when treated in the above

* Sur la Veg. chap. vi. sect. ii.

manner gave a similar result; and yet these plants vegetated to an indefinite time in an atmosphere of common air.

From the above experiments it seems to follow that nitrogene gas, at least in its pure state, is unfavourable to vegetation; but particularly in the shade. And yet there are some plants, such as the *Vinca minor*, *Lythrum Salicaria*, *Inula dysenterica*, *Epilobium hirsutum*, and *Polygonum Persicaria*, that seem to succeed equally well in an atmosphere of nitrogene gas as in an atmosphere of common air. A plant of the *Lythrum Salicaria*, selected for the purpose of experiment, was put into a receiver containing sixty-five cubic inches of nitrogene gas, of which it displaced about one-eighth of a cubic inch. It had its roots immersed in about an ounce of water, and was exposed to the rays of the sun, when it grew and became so luxuriant that it was more than once necessary to remove it into a larger receiver. But this luxuriance of growth seems incompatible with the previous conclusion. At the end of two months however, when the experiment was stopped, the receiver was found to contain the same quantity of nitrogene gas as at the beginning. The plant could have derived no nutriment, therefore, from its atmosphere. But this was the case also in all of the preceding examples. There was no diminution in the original quantity of nitrogene introduced into the receiver. It follows therefore that nitrogene gas, at least in its pure state, is not only incapable of affording a vegetable aliment, but is not even inhaled into the plant. But nitrogene is found in almost all vegetables, particularly in the wood, in extract, and in their green parts. Whence then is their nitrogene derived? From the extractive principle of vegetable mould.

SUBSECTION IV.

Hydrogene Gas.—A plant of the *Epilobium hirsutum*, which was confined by Priestley in a receiver filled with inflammable air or hydrogene, consumed one-third of its atmosphere and was still green.* Hence Priestley inferred that it serves as a vegetable food, and constitutes even the true and proper pabulum of the plant. But the experiments of later phytologists do not at all countenance this opinion. Saussure introduced a plant of the *Lythrum Salicaria* into a receiver containing sixty cubic inches of hydrogene gas, and exposed it to the sun. Its vegetation was perhaps somewhat more vigorous than that of plants confined in an atmosphere of nitrogene; but it had abstracted no nourishment from its atmosphere, nor effected any material change upon it. For at the end of five weeks of experiment, when its asmosphere was fired by the electric spark along with the proper quantity of oxygene, the result was the formation

Thought by Priestley to be a vegetable food.

* Priestley on Air, vol. iv. p. 323.

of water. The volume of its atmosphere was indeed diminished during the period of its vegetation; but this is to be accounted for by another cause, as will appear in the course of tracing the progress of vegetation.*

But found to be unfavourable to vegetation.

Our conclusion therefore must be that hydrogene is unfavourable to vegetation, and does not serve as the food of plants. But hydrogene is contained in plants as is evident from their analysis; and if they refuse it when presented to them in a gaseous state, in what state do they then acquire it? To this question it is sufficient for the present to reply, that if plants do not acquire their hydrogene in the state of gas, they may at least acquire it in the state of water, which is indisputably a vegetable food, and of which hydrogene constitutes one of the component parts.

SUBSECTION V.

Unfavourable to vegetation.

Carbonic Oxide.—When plants were confined by Saussure in atmospheres of carbonic oxide, they required nearly the same condition to support vegetation, and exhibited nearly the same phenomena as in nitrogene. Such as were deprived of their green parts died in the course of a few days. The vegetation of Peas whose leaves were completely developed was languid in the sun, and did not succeed at all in the shade. The *Epilobium*

* Sur la Veg. chap. vi. sect. 4.

hirsutum, *Lythrum Salicaria*, and *Polygonum Persicaria*, vegetated indeed as in common air: but at the end of six weeks of experiment, they had neither decomposed the oxide constituting their atmosphere, nor diminished its quantity.* It cannot, therefore, be regarded as favourable to vegetation.

SECTION III.

Vegetable Extract.

When it was found that atmospheric air and water are not even conjointly capable of furnishing the whole of the aliment necessary to the developement of the plant, it was then alleged that, with the exception of water, all substances constituting a vegetable food must at least be administered to the plant in a gaseous state. But this also is a conjecture unsupported by proof; for even with regard to such plants as grow upon the barren rock, or in pure sand, it cannot be said that they receive no nourishment whatever besides water, except in a gaseous state. Many of the particles of decayed animal and vegetable substances which float in the atmosphere and attach themselves to the leaves, must be supposed to enter the plant in solution with the moisture which the leaves imbibe; and so also similar substances contained in the soil must

* Sur la Veg. chap. vi. sect. iii.

be supposed to enter it by the root: but these substances may certainly contain vegetable nourishment; and they will perhaps be found to be taken up by the plant in proportion to their degree of solubility in water and to the quantity in which they exist in the soil. Now one of the most important of these substances is vegetable extract.

As a component part of vegetable mould.

When plants have attained to the maturity of their species, the principles of decay begin gradually to operate upon them, till they at length die and are converted into the dust from which they sprang, thus resembling the animal to whom they afford support. The substance to which they are finally converted has been denominated vegetable mould. And this, as might be expected, constitutes a considerable proportion of the soil. The chance then is, that it is again converted into vegetable nourishment, and again enters the plant. But it cannot wholly enter the plant because it is not wholly soluble in water. Part of it, however, is soluble and consequently capable of being absorbed by the root, and that is the substance which has been denominated extract.

Is soluble in water, and hence absorbable by the root.

Saussure filled a large vessel with pure mould of turf, and moistened it with distilled or rain water till it was saturated. At the end of five days, when it was subjected to the action of the press, 10,000 parts in weight of the expressed and filtered fluid yielded by evaporation to dryness 26 parts of extract. In a similar experiment upon the mould of a kitchen-garden which had been manured with dung, 10,000 parts of fluid yielded 10 of extract. And in a similar experiment upon mould taken from a well cultivated corn-field, 10,000 parts of fluid yielded four parts of extract.* Such was the result in these particular cases.

But the quantity of extract that may be separated from pure mould formed by nature upon the surface of the globe is not in general very considerable. After 12 decoctions, all that could be separated from mould of this sort was about $\frac{1}{11}$ of its weight; and yet this seems to be more than sufficient for the purposes of vegetation: for a mould containing this quantity was found by experiment to be less fertile, at least for Peas and Beans, than a mould that contained only one half or two thirds the quantity.†

But if the quantity of extract must not be too much, neither must it be too little. Plants that were put to vegetate in mould deprived of its extract, as far as repeated decoctions could deprive it, were found to be much less vigorous and luxuriant than plants vegetating in mould not deprived of its extract: and yet the only perceptible difference between them is, that the former can imbibe and retain a much greater quantity of water than the latter.‡

From this last experiment, as well as from the great proportion in which it exists in the living plant, it evidently follows that extract constitutes a

Constitutes a vegetable food containing nitrogene.

* Sur la Veg. chap. v. sect. ii. † Ibid. ‡ Ibid.

vegetable food. But extract contains nitrogene; for it yields by distillation a fluid impregnated with ammonia. The difficulty, therefore, of accounting for the introduction of nitrogene into the vegetating plant, as well as for its existence in the mature vegetable substance, is done away; for although the plant refuses it when presented in a gaseous state, it is plain that it must admit it along with the extract.

But it seems also probable that a small quantity of carbonic acid gas enters the plant along with the extractive principle, as it is known to contain this gas also. The mould analysed by Saussure was quite dry before the commencement of the experiment, and the water employed to moisten it contained no carbonic acid. But the solution contained some; for when it was mixed with lime-water, carbonate of line was precipitated, though not in a quantity much exceeding that of its precipitation by spring-water in general. 100 cubic inches of the solution yielded by experiment an air containing two cubic inches of carbonic acid gas. This is no doubt a small proportion: but it appears from a variety of considerations, that the quantity of this gas taken up by the roots of plants is not great; consequently they do not require a great supply from the soil.

SECTION IV.

Salts.

Most plants are found by analysis to contain a certain proportion of salts—such as nitrate, muriate, and sulphate of potass or soda—as has been already shown. How do plants acquire them? In the earlier periods of phytological investigation, when every effect was attributed to the agency of the vital principle as exerted upon the air and water which the plant inhales or absorbs, it was thought that the salts contained in vegetables are formed in the process of vegetation: but this is also one of those extravagant conjectures of which further research has exposed the absurdity. The salts which have been detected in vegetables are known to exist in the soil. It is most likely therefore that the root absorbs them in solution with the water by which the plant is nourished. It is at least certain that plants may be made to take up by the roots a considerable proportion of salts in a state of artificial solution. M. Saussure prepared 10 different solutions, consisting each of 40 cubic inches of distilled water, together with 12 grains of the peculiar salt or other substance on which the experiment of absorption was to be made. The first solution contained muriate of potass; the second, muriate of soda; the third, muriate of lime; the fourth, sulphate of soda; the

Absorbed in solution.

fifth, muriate of ammonia; the sixth, acetate of lime; the seventh, sulphate of copper; the eighth, crystallized sugar; the tenth, vegetable extract. Plants of *Polygonum Persicaria* and *Bidens cannabina* were then immersed in each of these solutions with the following result:—In the solutions of muriate of potass, muriate of soda, sulphate of soda, nitrate of lime, and extract, the former species vegetated in the shade for five weeks, developing their parts; but in the other solutions they died in the course of a few days. The latter species succeeded or failed in nearly the same way. It was afterwards found that a portion of the salts had been taken up along with the water by which they were held in solution; and if we suppose the quantity contained in each of the solutions to be divided into 100 parts, the ratio of their absorption may be shown as follows:—In consuming one half of the water assigned to the experiment, plants of the *Polygonum* had absorbed 14 parts of muriate of potass, 13 of muriate of soda, four of nitrate of lime, 14 of sulphate of soda, 12 of muriate of ammonia, eight of acetate of lime, 47 of sulphate of copper, nine of gum, 27 of sugar, and five of extract. Plants of the *Bidens* had absorbed the several salts in portions not very different.* But without minutely regarding proportions, the fact is thus clearly ascertained that plants are capable of taking up salts by the root, at least when presented to them in a state of artificial solution; and if so, there is then reason to

* Sur la Veg. chap. viii. sect. ii.

presume that salts are also taken up by the roots of plants vegetating even in their natural habitats.

But if salts are thus taken up by the root of the vegetating plant, does it appear that they are taken up as a food? Some plants, it must be confessed, are injured by the application of salts, as is evident from the experiments of Saussure; but others are as evidently benefited by it. Trefoil and Lucern have their growth much accelerated by the application of sulphate of lime, though many other plants are not at all influenced by its action. The *Parietaria* Nettle, and Borge, will not thrive except in such soils as contain nitrate of lime or nitrate of potass: and plants inhabiting the sea coast, as was observed by Du Hamel, will not thrive in a soil that does not contain muriate of soda. Whether a vegetable food.

It has been thought, however, that the salts are not actually taken up by the root, though converted to purposes of utility by acting as astringents or corrosives in stopping up the orifices of the vessels of the plant, and preventing the admission of too much water: but it is to be recollected that the salts in question are found by analysis in the very substance of the plant, and must consequently have entered in solution. It has been also thought that salts are favourable to vegetation only in proportion as they hasten the putrefaction of vegetable substances contained in the soil, or attract the humidity of the atmosphere. But sulphate of lime is not deliquescent; and if its action consist merely in accelerating putrefaction, why is its beneficial effect confined but to a small number of plants?

Lastly, some writers have contended that the salts which are found in vegetables are merely accidental in their occurrence, and not necessary to the health or perfection of the individual; because they are found to exist in but a very small proportion, both in the soil and plant: but as there are many species in which some salts are to be met with constantly and uniformly, at least if they have vegetated in a soil in which they are found to thrive, we can scarcely regard their occurrence as being merely accidental, or as producing no beneficial effect upon the plant. But the proportion of salts lodged in the soil is not so small as is generally believed. Re-agents do not indeed detect a great quantity in general; but that is because the alkaline salts of mould, like the alkaline salts of vegetables, are to be discovered chiefly in the remains of combustion; and because the ashes of the greater part of vegetable moulds do not readily part with their salts in boiling water. This difficulty of solution is thought by Saussure to be owing to a semivitrifaction that takes place in the mould when the ashes are abundant. An hundred parts of mould furnished by combustion 50 parts of ashes which did not give out their salts to boiling water. But 100 parts only of dried extract from the same mould yielded only 14 parts of ashes; and 100 parts of the ashes formed with boiling water a ley which contained

25 parts composed of potass in an uncombined state, and of alkaline sulphates and muriates; and yet, upon further analysis, it was found that the water had not extracted more than one half of the salts which the ashes contained. The soil, therefore, contains an abundant supply of salts for all the purposes of vegetation. It may even in some cases contain too much; for it is to be recollected that saline substances are beneficial to vegetation only when applied in very small quantities. If they are administered in great abundance they destroy the plant.

And the argument against their utility that has been drawn from the small proportion in which they are found to exist in the plant itself, is altogether inadmissible; because it is very well known that some particular ingredient may be essential to the composition of a body, and yet constitute but a very small proportion of its mass. Atmospheric air contains only about one part in the 100th of carbonic acid; and yet no one will venture to affirm that carbonic acid gas is merely an adventitious and accidental element existing by chance in the air of the atmosphere, and not an essential ingredient in its composition. Phosphate of lime constitutes but a very small proportion of animal bodies, perhaps not one part in 500; and yet no one doubts that it is essential to the composition of the bones. But the same salt is found in the ashes of all vegetables;* and who will say that it is not essential to their perfection?

SECTION V.

Earths.

As most plants have been found by analysis to contain a portion of alkaline or earthy salts, so most plants have been found to contain also a portion of earths: and as the two substances are so nearly related, and so foreign in their character to vegetable substances in general, the same inquiry has consequently been made with regard to their origin. Whence are the earths derived that have been found to exist in plants?

Whether formed in the process of vegetation.

It seems to have been the opinion of Lampadius that the earths contained in plants are merely the effect of vegetation, and altogether independant of the soil in which they grow: and extravagant as the opinion is, it has been made to assume the semblance of resting upon experiment. Lampadius prepared, in his garden, five small beds of four feet square in surface by one in depth; each bed consisted of a pure earth mixed with eight pounds of cow-dung. The earths were alumine, silica, lime, magnesia, and garden mould. They were sown with Rye, and the produce of each was separately

* Saus. sur la Veg. chap. viii. sect. iv.

reduced to ashes. But the same principles were found in them all; amongst which was a portion of silica;—whence Lampadius concluded that the silica found in plants is merely the result of vegetation, having no relation whatever to the soil in which the plants grow.

But this conclusion was by much too hasty, and has been since shown to be most palpably erroneous; because Lampadius does not take into the calculation the constitutent principles of the cow-dung with which his earths were mixed, the very substance from which his plants must have derived the greater part of their nourishment. If this precaution had been taken, his conclusion must have been very different: for it has been ascertained by Ruckert that dung does actually contain a portion of silica;* which in the case of cow-dung will appear the less surprising if it is only recollected that the plants which cows principally feed on do themselves contain a portion of silica. To the cow-dung, therefore, with which the different earths were manured, the origin of silica may be traced. It was thus of necessity found in them all, though not perhaps in an equal proportion.

The opinion absurd.

Saussure, in adverting to the experiment of Lampadius, exposes indeed the absurdity of his conclusion; but deduces from it another which is perhaps equally exceptionable—namely, that plants growing in calcareous and granitic sand, mixed with

* Sur. la Veget. chap. ix. sect. iii.

the same manure or mould, will produce equal quantities of ashes. But this supposes manures to have the same action upon all soils, which is surely not the fact: and if there be any manure that acts on a calcareous soil, without acting at all on a granitic soil, then the quantity of ashes will be altered in the former case, from that very circumstance; because the plant is now nourished not only by the manure that was committed to the soil, but from the original soil itself, in its state of combination with the manure.

Absorbed in solution.

The earths, then, that are contained in vegetables are derived chiefly from the soil: but in what peculiar state of combination do they enter the vessels of the plant? The state most likely to facilitate their absorption is that of their solution in water, in which all the earths hitherto found in plants are known to be in a slight degree soluble.

Lime is soluble in water with the aid of a little carbonic acid, in the proportion of about $\frac{1}{680}$ part of its weight; but it is also soluble even without the aid of the acid,* and the solution is known by the name of lime-water. Clay is soluble in water by means of the mineral acids; and also, though very sparingly, in pure water, from which even the filtre cannot abstract it.† Silica is soluble in water by means of carbonate of potass, as is evident from Black's analysis of the waters of Geyser in Iceland. It is soluble also in pure water according to the

* Seneb. Phys. Veg. vol. iii. p. 17. † Ibid.

analysis of Klaproth; and in that state of division in which it is precipitated from its solution in fixed alkalies, it is perfectly soluble in 1000 parts of water.* Magnesia is soluble in water by means of the mineral acids, and even in pure water, in very small quantities; requiring about 2000 times its weight to hold it in solution.

All the earths, then, found in plants are less or more soluble in water. And if it be said that the proportion in which they are soluble is so very small that it scarcely deserves to be taken into the account, it is to be recollected that the quantity of water absorbed by the plant is great, while that of the earth necessary to its health is but little, so that it may easily be acquired in the progress of vegetation.

Such is the manner in which their absorption seems practicable: but the following experiments afford a presumption that they are actually absorbed by the root. Woodward took three plants of Spearmint, one of which he made to vegetate in distilled or pure water; another in river water; and a third, in water mixed with mould. At the commencement of the experiment the first plant weighed 114 grains; at the end of the experiment it weighed 155 grains, being augmented by 41 grains. The water expended was 8863 grains, and the increase as 1·214 +. At the commencement of the experiment, the second plant weighed 28 grains, at the end 54 grains, being augmented by 26 grains.

* Kirwan's Miner. vol. i. p. 10.

The water expended was 2493 grains, and the increase as 1 : 95 +. At the commencement of the experiment the third plant weighed 92 grains, at the end 376 grains, being augmented by 284 grains. The water expended was 14950 grains, and the increase as 1 : 52 + .*

From the greater proportional augmentation of the plant to which the mould had access, we may infer the beneficial effect of the earths as applied to the root, and perhaps the absorption of a part; particularly as it is known that the proportion of earths contained in the ashes of vegetables depends upon the nature of the soil in which they grow. The ashes of leaves of the *Rhododendron ferrugineum*, growing on Mount Jura, a calcareous mountain, yielded 43·25 parts of earthy carbonate, and only 0·75 of silica. But the ashes of leaves of the same plant, growing on Mount Breven, a granitic mountain, yielded two parts of silica, and only 16·75 of earthy carbonate.

It is probable, however, that plants are not indebted merely to the soil for the earthy particles which they may contain. They may acquire them partly from the atmosphere. Margray has shewn that rain water contains silica in the proportion of a grain to a pound; which, if it should not reach the root, may possibly be absorbed along with the water that adheres to the leaves.

But although the earths are thus to be regarded

* Phil. Trans. vol. xxi. p. 200. Saus. sur la Veg. chap. ix. sect. iii.

as constituting a small proportion of vegetable food, they are not of themselves sufficient to support the plant, even with the assistance of water. Giobert mixed together lime, alumine, silica, and magnesia, in such proportions as are generally to be met with in fertile soils, and moistened them with water. Several different grains were then sown in this artificial soil, which germinated indeed, but did not thrive; and perished when the nourishment of the cotyledons was exhausted. It is plain, therefore, that the earths, though beneficial to the growth of some vegetables, and perhaps necessary to the health of others, are by no means capable of affording any considerable degree of nourishment to the plant.

Whether nutritive.

SECTION VI.

Manures.

As the object of the preceding sections has been that of exhibiting a brief view of the different species of vegetable food, whether it be regarded as derived from the soil or the atmosphere; so the object of the present section will be that of showing how the food necessary to the support of the vegetating plant may be supplied when defective, or restored when exhausted: but this unavoidably involves the subject of manures, or artificial preparations of vegetable food, so important to the advancement of agriculture, and consequent interest of mankind.

What implying.

With regard to the food of plants derived from the atmosphere, the supply is pretty regular; at least, in as far as the gases are concerned; for they are not found to vary materially in their proportions on any part of the surface of the globe: but the quantity of moisture contained in the atmosphere is continually varying, so that in the same season you have not always the same quantity, though in the course of the year the deficiency is perhaps made up. From the atmosphere, therefore, there is a regular supply of vegetable food kept up by nature for the support of vegetable life, independent of the aid of man: and if human aid were even wanted, it does not appear that it could be of much avail.

Composition of fertile soils.

But this is by no means the case with regard to soils; for if soils are less regular in their composition, they are at least more within the reach of human management. We have already seen the materials of which soils are composed: but what are the proportions of the materials in soils best suited for culture? According to the analysis of Bergman, the soil best suited for culture contains four parts of clay, three of sand, two of calcareous earth, and one of magnesia: and, according to the analysis of Fourcroy and Hassenbratz, 9216 parts of fertile soil contained 305 parts of carbon, together with 279 parts of oil; of which, according to the calculations of Lavoisier, 220 parts may be regarded as carbon: so that the whole of the carbon contained in the soil in question may be estimated at about

525 parts, exclusive of the roots of vegetable—or to about $\frac{1}{16}$ of its weight.

Mr. Young observed that equal weights of different soils, when dried and reduced to powder, yielded by distillation quantities of air somewhat corresponding to the ratio of their values. The air was a mixture of fixed and inflammable airs, proceeding probably from decomposition of the water; but partly, I should presume, from its capacity of abstracting a portion of air from the atmosphere, which the soil at least is capable of doing.

The following is the analysis of a fertile soil, as occurring in the neighbourhood of Bristol. In 400 grains, there were of

Water	52
Silicious sand	240
Vegetable fibre	5
——— extract	3
Alumine	48
Magnesia	2
Oxide of iron	14
Calcareous earth	30
Loss	6
Total	400*

But Mr. Kirwan has shown in his Geological Essays, that the fertility of a soil depends in a great

Fertility dependant upon tenacity.

* Agricultural Magazine, April, 1808.

measure upon its capacity for retaining water: and if so, soils containing the same ingredients must be also equally fertile, all other circumstances being the same; though it is plain that their actual fertility will depend ultimately upon the quantity of rain that falls, because the quantity suited to a wet soil cannot be the same that is suited to a dry soil. And hence it often happens that the ingredients of the soil do not correspond to the character of the climate. Silica exists in the soil under the modification of sand, and alumine under the modification of clay. But the one or the other is often to be met with in excess or defect. Soils in which the sand preponderates retain the least moisture; and soils in which the clay preponderates retain the most: the former are dry soils; the latter are wet soils. But it may happen that neither of them is sufficiently favourable to culture; in which case their peculiar defect or excess must be supplied or retrenched before they can be brought to a state of fertility.

Poor or exhausted soils ameliorated,

But soils in a state of culture, though consisting originally of the due proportion of ingredients, may yet become exhausted of the principle of fertility by means of too frequent cropping, whether by repetition or rotation of the same, or of different crops. And in this case, it should be the object of the phytologist, as well as of the practical cultivator, to ascertain by what means fertility is to be restored to an exhausted soil; or communicated to a new one.

By draining, par-

In the breaking up of new soils, if the ground has been wet or marshy, as is frequently the case, it is often sufficient to prepare it merely by means of draining off the superfluous and stagnant water, and of paring and burning the turf upon the surface. This mode of preparation is at present much practised throughout England, but particularly in Yorkshire and Lincolnshire, as being the best suited to the character of the soil of these counties that remains to be taken into cultivation.

ing, burning,

If the soil has been exhausted by too frequent a repetition of the same crop, it often happens that a change of crop will answer the purpose of the cultivator; for although a soil may be exhausted for one sort of grain, it does not necessarily follow that it is also exhausted for another. And accordingly, the practice of the farmer is to sow his crops in rotation, having in the same field a crop, perhaps, of wheat, barley, beans, and tares in succession; each species selecting in its turn some peculiar nutriment, or requiring, perhaps, a smaller supply than the crop that has preceded it. But even upon the plan of rotation, the soil becomes at length exhausted, and the cultivator obliged to have recourse to other means of restoring its fertility.

Repose, fallowing, trenching.

In this case, an interval of repose is considerably efficacious, as may be seen from the increased fertility of fields that have not been ploughed up for many years, such as those used for pasture; or even from that of the walks and paths in gardens when they are again broken up. Hence also the practice of fallowing, and of trenching or deep ploughing, which must have nearly the same effect.

If any one asks how the fertility of a soil is restored by the means now stated, it will be sufficient for the object of the present section to reply that, in the case of draining, the amelioration is effected by means of its carrying off all such superfluous moisture as may be lodged in the soil, which is well known to be prejudicial to plants not naturally aquatics, as well as by rendering the soil more firm and compact. In the case of burning, the amelioration is effected by means of the decomposition of the vegetable substances contained in the turf, and subjected to the action of the fire, which disperses part also of the superfluous moisture, but leaves a residue of ashes favourable to future vegetation. In the case of the rotation of crops, the fertility is not so much restored as more completely developed and brought into action; because the soil, though exhausted for one species of grain, is yet found to be sufficiently fertile for another, the food necessary to each being different, or required in less abundance. In the case of the repose of the soil, the restored fertility may be owing to the decay of vegetable substances that are not now carried off in the annual crop, but left to augment the proportion of vegetable mould; or to the accumulation of fertilizing particles conveyed to the soil by rains; or to the continued abstraction of oxygen from the atmosphere. In the case of fallows, it is owing undoubtedly to the action of the atmospheric air upon the soil, whether in rendering it more friable, or in hastening the putrefaction of noxious plants; or, it is owing to the abstraction and accumulation of oxygene. In the case of trenching, it is owing to the increased facility with which the roots can now penetrate to the proper depth; and in the case of deep ploughing, it is owing, as it would appear, to the same cause.

By the application of manures.

But it often happens that the soil can no longer be ameliorated by any of the foregoing means, and in this case there must be a direct and actual application made to it of such substances as are fitted to restore its fertility. And hence the indispensable necessity of manures, which consist chiefly of animal and vegetable remains that are buried and finally decomposed in the soil, from which they are afterwards absorbed by the root of the plant, in a state of solution.

But as carbon is the principal ingredient furnished by manures as contributing to the nourishment of the plant, and is not itself soluble in water, nor even disengaged by fermentation in a state of purity; under what state of chemical combination is its solution effected? Is it effected in the state of charcoal? It has been thought, indeed, that carbon in the state of charcoal is soluble in water; because water from a dunghill, when evaporated, constantly leaves a residuum of charcoal, as was first ascertained by the experiments of Hassenfratz. But there seem to be reasons for doubting the legitimacy of the conclusion that has been drawn from it; for Senebier found that plants whose roots were immersed in water took up less of the fluid in proportion as it was mixed with water from a dunghill.* Perhaps then the charcoal of water from a dunghill is held merely in suspension, and enters the plant under some other modification.

But if carbon is not soluble in water in the state of charcoal, in what other state is it soluble? It is soluble in the state of carbonic acid gas. But is this the state in which it actually enters the root?

Opinion of Senebier.

On this subject phytologists have been somewhat divided in opinion. Senebier endeavours to prove that carbonic acid gas, dissolved in water, supplies the roots of plants with almost all their carbon, and founds his arguments upon the following facts:—In the first place it is known that carbonic acid gas is soluble in water; in the second place it is known to be contained in the soil, and generated by the fermentation of the materials composing manures;† and in the next place it is known to be beneficial to vegetation when applied artificially to the roots, at least in a certain degree. This is evident from the following experiment of Ruckert, as well as from several experiments of Saussure's, previously related. Ruckert planted two beans in pots of equal dimensions, filled with garden mould; the one was moistened with distilled water, and the other with water impregnated with carbonic acid gas. But the latter

* Phys. Veg. vol. iii. p. 154. † Ibid. vol. iii. p. 55.

appeared above ground nine days sooner than the former, and produced 25 beans; while the former produced only 15. Now the result of this experiment, as well as the preceding facts, is evidently favourable to the presumption of Senebier, and shows that if carbonic acid is not the state in which carbon enters the plant, it is at least a state preparatory to it; and there are other circumstances tending to corroborate the opinion, resulting from the analysis of the ascending sap of plants. The tears of the Vine, when analysed by Senebier, yielded a portion of carbonic acid and earth;* and as the ascending sap could not be supposed to have yet undergone much alteration, the carbonic acid, like the earth, was probably taken up from the soil.

Controverted by Hassenfratz.

But this opinion, which seems to be so firmly established upon the basis of experiment, Hassenfratz strenuously controverts. According to experiments which he had instituted with an express view to the investigation of this subject, plants which were raised in water impregnated with carbonic acid differed in no respect from such as grew in pure water, and contained no carbon that did not previously exist in the seed. Now if this were the fact, it would be decisive of the point in question. But it is plain from the experiments of Saussure, as related in a preceding section, that Hassenfratz must have been mistaken both with regard to the utility of carbonic acid gas as furnishing a vegetable

* Seneb. Phys. Veg. vol. iii. p. 55.

aliment, and with regard to the augmentation of carbon in the plant. The opinion of Senebier, therefore, may still be correct.

Conjecture of Dr. Thomson.

It must be acknowledged, however, that the subject is not yet altogether satisfactorily cleared up; and that carbon may certainly enter the plant in some state different from that, either of charcoal in solution, or of carbonic acid gas. Is not the carbonic acid of the soil decomposed before entering the plant? This is a conjecture of Dr. Thomson's, founded upon the following facts:—The green oxide of iron is capable of decomposing carbonic acid; and many soils contain that oxide. Most soils indeed contain iron, either in the state of the brown or green oxide, and it has been found that oils convert the brown oxide into green.* But dung and rich soils contain a quantity of oily substance. One effect of manures, therefore, may be that of reducing the brown oxide of iron to the green, thus rendering it capable of decomposing carbonic acid gas, so as to prepare it for some new combination, in which it may serve as an aliment for plants. All this, however, is but a conjecture; and it is more probable that the carbonic acid of the soil enters the root in combination with some other substance, and is afterwards decomposed within the plant itself.

* Thomson's Chemistry, vol. iv. p. 394.

CHAPTER III.

OF THE PROCESS OF NUTRITION.

In the foregoing chapter I have enumerated the substances constituting the principal food of plants, as deducible from the observations and experiments of the best phytological chemists. But this enumeration serves only as a step to conduct us to further inquiries. For it is necessary to know not only in what the food of plants consists, but also by what means that food, whether lodged in the soil or wafted through the atmosphere, is taken up by the plant, conveyed to its different parts, and elaborated so as to prepare it for final assimilation. The investigation of these topics shall form the subject of the several following sections.

SECTION I.

Intro-susception.

Effected by the pores of the epidermis.

As plants have no organ analagous to the mouth of animals enabling them to take up the nourishment necessary to their support, by what means do they effect the intro-susception of their food? In our anatomical analysis of the vegetable structure, it was found that the whole of the parts of the plant, the root, stem, branches, leaves, flower, and fruit, are covered with an epidermis, or fine and transparent pellicle, which has been described by some phytologists as being of so close and compact a texture that the eye, aided even by the best microscopes, is unable to discover in it the slightest vestige of pores or apertures. Hedwig and Decandolle have, however, detected pores in the epidermis of the leaves of many plants, and they may readily be detected by any one who will be at the trouble of employing the same means. It does not appear that any pores have been yet detected in the epidermis of the root; though we must not on that account conclude that it is not porous. We must even, on the contrary, admit that it is furnished also with pores, as well as the epidermis of the leaf; because the whole of the nourishment which the plant derives from the soil must of necessity pass through it.

Absorbing or inhaling fluids.

But if the pores of the epidermis are so very fine as either to elude the sight, or to be discoverable only by the application of the highest magnifying powers, they can be permeable only to fluids; and if so, then the food of the plant can be taken up only by absorption or inhalation, as the chyle into the animal lacteals, or the air into the lungs. The former term will be applied to the intro-susception of non-elastic fluids; the latter, to that of gaseous fluids.

Non-elastic fluids absorbed

Of the fact of the absorption of non-elastic fluids by the epidermis of plants any one may easily satisfy himself, merely by immersing in water a plant of almost any species of moss that has been some time gathered, or long exposed to drought, so as to have had its leaves shrivelled up. The moisture will immediately begin to ooze through the epidermis, and the plant to resume its original form and verdure.

By the root,

But has any of the moisture thus absorbed passed through the root? If the bulb of a hyacinth is placed on the orifice of a glass bottle filled with water, so as that the radicles only shall be immersed, the water is imperceptibly exhausted, and the plant grows: the moisture must consequently have passed through the root. The following experiment of Hales proves not only the fact, but also the extraordinary energy of the absorbent power of the root. Having laid bare the root of a pear tree half an inch in diameter, and luted to it a tube of glass, one inch in diameter and eight inches long, to which was luted also another tube a quarter of an inch in diameter and 18 inches long, he filled both with water, and immersed the extremity in a cistern of mercury. The result was, that the absorption of water by the root was so rapid that the mercury rose eight inches in the space of six minutes.*

By the leaf.

But moisture is absorbed also by the leaves as well as root. Du Hamel cut off several branches from several trees of different species, and covered the surface of the section with mastic. The consequence was that the branches soon began to exhibit

* Veg. Stat. Exper. xxi.

a faded and sickly appearance. Some of them were then removed to damp situations, and others to dry situations, to know what the effect of such removal might be. The former gave indications of recovery, the latter of continued decay.* It is plain, therefore, that in the former case moisture must have been absorbed from the atmosphere by means of the epidermis of the leaf, or at least of the branch. Mariotte cut off from a tree a branch terminating in two boughs, which he suspended upon the edge of a vessel filled with water, so as that the one was within and the other without the vessel. The former preserved its verdure for several days, but the latter began almost immediately to wither.

Experiments of Bonnet,

But the most complete set of experiments upon the absorbent power of leaves which has hitherto appeared is that of M. Bonnet, of Geneva. Satisfied that leaves are furnished with absorbent organs for the purpose of the intro-susception of moisture, as deducible from the experiments of Hales and Guettard, his object was that of ascertaining whether the absorbent power of both surfaces was alike. With this view he filled several vessels with water, on the top of which he placed a number of leaves, some having the upper, and others the under surface applied to the water, so as that they only floated in it but were not immersed. If the leaf retained its verdure longest with its upper surface applied to the water, the absorbing power of the upper surface was

* Phy. des Arb. liv. ii. chap. iii.

to be regarded as the greatest; but if it retained its verdure longest with the under surface applied to the water, then the absorbing power of the under surface was to be regarded as the greatest. The experiment was made in the spring and autumn, the temperature being between five and ten of Reaumur; and the leaves employed being such as were fully expanded. The result was as follows:—

On the detached leaves of herbs.

Out of fourteen herbs of different genera selected for the purpose of experiment, the leaves of six—the *Arum maculatum*, Kidney Bean, Sun-flower, Cabbage, Spinach, and small Mallow—were indifferent to the mode in which they were applied to the water, and were found to retain their verdure equally long whether moistened by the upper or under surface. The rest—the Plantain, white Mullein, great Mallow, the Nettle, Cockscomb, purple-leaved Amaranth, Marvel of Peru, and Balm—were not indifferent to the mode in which they were applied to the water, but retained their verdure longest when moistened by the upper surface.

The following are the most remarkable examples of the relative capacity of their different surfaces:— The leaf of the Nettle when moistened by the upper surface lived two months, but when moistened by the under surface only three weeks. The leaf of the Amaranth when moistened by the upper surface lived three months, and when moistened by the under surface only seven or eight days. The leaf of the Mullein when moistened by the upper surface lived five weeks, and when moistened by the under surface only five days. A leaflet of the French Bean absorbed also a sufficient quantity of moisture to nourish another leaflet that was still attached to the same footstalk, though not touching the water.

On the detached leaves of trees.

Out of sixteen trees or shrubs of different genera selected for the purpose of experiment, the leaves of only two, the Lilac and Aspen, retained their verdure equally long by whatever surface they were moistened. But the leaves of the rest—the Vine, Pear, Cherry, Prune, Apricot, Walnut, Mulberry, Oak, Hazel, Rose, &c.—retained it longest when moistened by the under surface. The following are the most remarkable examples of relative capacity: —The leaves of the white Mulberry when moistened by the under surface retained their verdure for nearly six months, but when moistened by the upper surface they retained it for only five or six days. The leaves of the Vine, Poplar, and Walnut faded almost as soon when moistened by the upper surface, as when left without water altogether. The leaves of the Hazel and Rose when moistened by the under surface absorbed a sufficient quantity of moisture to nourish also other leaves on the same branch, though not touching the water.*

Such was the result of the experiments of M. Bonnet; and the only thing to be regretted is that he has not always been sufficiently accurate in specifying, beyond the chance of mistake, the plant on

* Recherches sur les Usages des Feuilles.

which his experiments were made; which are on this account the less satisfactory, as well as the less valuable. And hence it is now impracticable to ascertain what particular species of Mallow are intended by the Great and Little Mallows; or what particular species of Poplar it is that differs so much in its capacity of absorption from the *Populus tremula* or Aspen. But the inference deducible from the whole, and deduced accordingly by Bonnet, is that the leaves of herbs absorb moisture chiefly by the upper surface, and the leaves of trees chiefly by the under surface.

Their different capacities accounted for

But what is the cause of this direct opposition between the absorbing surface of the leaf of the herb and of the tree? The immediate cause must be, that there exists a greater number of absorbents in the upper surface of the one, and in the under surface of the other. But what is the cause in the economy of the vegetable subject, or state of surrounding bodies, that requires this arrangement?

By Du Hamel.

Du Hamel thought the lower surface of the leaf of the tree was endowed with the greater capacity of absorbing moisture, chiefly for the purpose of catching the ascending dews and exhalations that must necessarily come into contact with it as they rise, but which might possibly still escape if absorbable only by the upper surface, as being now considerably rarefied, as well as more rapid in their ascent;* presuming, as it appears, that absorption

* Phys. des Arbres, liv. ii. chap. iii.

by the upper surface is all that is necessary to herbs, being but low in stature, and near the surface of the earth, where the dews and exhalations are yet so much condensed and so slow of ascent, that absorption by the under surface of the leaf would but drench and destroy them. There may possibly be some truth in this conjecture, though it rests on a foundation rather too slight to be much trusted to; as the same mode of argumentation would have suited a reversed order of the absorbing capacity of surfaces, if viewed with regard to the rains that descend from the atmosphere.

Observations on living plants. But as the foregoing experiments upon leaves were made on such only as were detached from the plant, it may be said that they are not well calculated to become the ground of any general conclusion, and that they do not represent to us the actual phenomena of vegetation. To the actual phenomena of vegetation therefore let us now appeal, in as far at least as they are applicable to the present subject. They will be found fully to confirm the fact of the absorption of moisture by the leaf. If, after a long drought, a fog happens to take place before any rain falls, so as to moisten the surface of the leaves, the plant begins to revive and to resume its verdure long before any moisture can have penetrated to the root. Hence it follows incontestably that moisture has been absorbed by the leaf: because it is impossible to account for the change that has been effected, except by such absorption. But the efficacy of rains themselves and of artificial waterings may be accounted for upon the same principle; for they have not always penetrated to the root when they are found to have given freshness to the plant; and indeed many plants will thrive merely by having their leaves kept moist, though no water should reach the root at all. The same thing might be said of the immersed *Fuci*, many of which being totally destitute of root, and constituting merely a sort of frond or leaf, absorb the nourishment necessary to their support by the whole of their surface. The moisture then entering the plant as a food is taken up by means of the absorbent pores of the epidermis, not only of the root and leaf, but often, as it is to be believed, of the other parts of the plant also, at least when they are in a soft and succulent state.

Elastic fluids may be inhaled by the same,

But by what means do the gaseous fluids enter the plant? From what has been already ascertained concerning the vegetable structure, it follows unavoidably that the gases which may be inhaled as a food must enter the plant in a manner similar to that of moisture, that is, they must also pass through the pores of the epidermis. Perhaps the pores by which moisture is absorbed are fitted also for the inhalation of air; but this cannot be regarded as altogether certain; if it is not rather altogether certain that each of the two fluids enters the plant by a peculiar set of pores.

Or by different pores.

Bonnet has shown that most leaves absorb moisture better by the one surface than the other: and it is known that some surfaces do actually repel it; as may be seen in the case of Cabbage-leaves in the time of rains and dews, when the drops roll along the upper surface without wetting it, or lodge in its folds and hollows like globules of quicksilver, conglomerated together without being absorbed. This is the case also with all such plants as are covered with bloom. It is probable therefore that all such surfaces as repel moisture are fitted rather for the inhalation of air which they have long been regarded as capable of effecting; and in times in which it was fashionable to look for analogies between the plant and animal in every thing whatever, leaves were even regarded as being the lungs of plants. The notion seems to have arisen as follows: Grew thought he had discovered in the leaves a number of little bags or bladders filled with air: the air was supposed to have entered by inhalation; and the bags or bladders were supposed to be analogous in their office to the cells of the lungs of animals. This was at the time a sufficiently plausible conjecture, but was not enough to prove that leaves are lungs. Accordingly it became necessary to look out for some further arguments in defence of the doctrine, and one of the first that was discovered was that of the experiment of M. Papin, who, with a view to ascertain the point in question, introduced into the receiver of an air-pump an entire plant, root, stem, and leaf. The

Though leaves are not lungs.

consequence was that it very soon died. He then introduced a plant by the root and stem only, while the leaves were still exposed to the influence of the air. But in this case the plant lived much longer than in the former, and warranted him, as he thought, to conclude that leaves are the lungs of plants. It is plain, however, that this conclusion was by much too hasty; because the life of the plant might have been protracted merely by the absorption of the moisture of the atmosphere through the medium of the leaf, and not by the inhalation of any gaseous principle. And before venturing upon such a conclusion, the experiment should have been also reversed, to show the result of enclosing the leaves only in the receiver, and of leaving out the stem and root: and if it had even been proved that atmospheric air is actually inhaled by the leaf and indispensable to the health of the plant, still it would have been necessary to show that it is again expired also, in order to make good the analogy of leaves to lungs.

Another argument in support of the doctrine was deduced from Du Hamel's experiment of besmearing the surface of the leaf with oil, in consequence of which treatment it soon died, owing, as it appeared, to the exclusion of air.* But this argument is also insufficient to establish the fact, and is here introduced, together with that of M. Papin, not merely for the purpose of show-

* Phys. des Arb. liv. ii. chap. iii.

ing its inadequacy, or of making it appear that there is any absurdity in the doctrine it was intended to support; but rather that the doctrine, though founded in truth, could not have been satisfactorily proved by any experiments that were practicable at the time.

Their inhalation proved by pneumato-chemical experiment.

It is to the modern improvements in pneumatic chemistry, and to them alone, that we are indebted for our knowledge of the real functions of the leaves of plants; from which it is proved indisputably, that the leaves not only contain air, but do actually inhale it. It was the opinion of Priestley that they inhale it chiefly by the upper surface. Has this been confirmed? And it has been shown by Saussure that their inhaling power depends entirely upon the organization. A bough of the *Cactus Opuntia*, when placed, as it was detached from the plant, in an atmosphere of common air, inhaled in the course of a night four cubic inches of oxygene; but when it was placed in a similar atmosphere after being cut to pieces and pounded in a mortar so as to destroy the organization of its parts, no inhalation took place. The inhalation of air, therefore, is no doubt effected by the pores of the epidermis of the leaf.

It has been a question, however, among phytologists, whether it is not also effected by the epidermis of the other parts of the plant. We can scarcely suppose it to be effected by the dry and indurated epidermis of the bark of aged trunks, of which the original organization is obliterated; nor by that of the larger and more aged branches. But it has been thought there are even some of the soft and succulent parts of the plant by which it cannot be effected, because no pores are visible in their epidermis. M. Decandolle found no pores in the epidermis of fleshy fruits, such as Pears, Peaches, and Gooseberries; nor in that of roots, or scales of bulbs; nor in any part not exposed to the influence of air and light. It is known, however, that fruits will not ripen, and that roots will not thrive, if wholly deprived of air; and hence it is probable that they inhale it by their epidermis, though the pores by which it enters should not be visible. In the root, indeed, it may possibly enter in combination with the moisture of the soil; but in the other parts of the plant it enters no doubt in the state of gas. Herbs, therefore, and the soft parts of woody plants, absorb moisture and inhale gases from the soil or atmosphere by means of the pores of their epidermis, and thus the plant effects the intro-susception of its food,

SECTION II.

Ascent of the Sap.

In tracing out the means by which the plant effects the intro-susception of its food, it was found to be chiefly that of absorption by the root. But the fluids existing in the soil when absorbed by the root, are designated by the appellation of sap or lymph; which, before it can be rendered subservient to the purposes of vegetable nutrition, must either be intermediately conveyed to some viscus proper to give it elaboration, or immediately distributed throughout the whole body of the plant. The object, therefore, of the present section will be that of tracing out the progress of its distribution or ascent.

Sap proved to be in motion,

A very simple experiment will be sufficient to show that the sap is in motion in one direction or other, at least at occasional periods. If the branch, or trunk, or even root of a tree, is laid open or fractured in the course of spring, whether by intentional incision or accidental wound, the sap will immediately begin to flow, and will, in some cases, continue to be copiously discharged perhaps for several days, or at least till the wound is cicatrized; and if the wound is again opened the sap will flow afresh. This is what is usually denominated the bleeding of plants, and is well exemplified in the Vine, Birch, Maple, and Walnut, as affording a most copious discharge.

By the bleeding of plants.

But what is the most to be wondered at in the case of the bleeding of plants, is that the most copious discharge does not seem to injure the individual in any material degree. Du Hamel selected several strong and healthy Vines as the subject of experiment, some of which were trimmed in the usual way, and others made to bleed copiously; but the latter were afterwards as vigorous and productive as the former. The American Maple will also continue to yield its usual quantity of sap in the spring for many years; though it requires now and then an interval of rest.

Which is periodical;

The plant always bleeds most freely about the time of the opening of the bud; for in proportion as the leaves expand the sap flows less copiously, and when they are fully expanded it entirely ceases. But this suspension is only temporary, for the plant may be made to bleed again in the end of the autumn, at least under certain conditions. If an incision is now made into the body of the tree after the occurrence of a short but sharp frost, when the heat of the sun or mildness of the air begins to produce a thaw, the sap will again flow. It will flow even where the tree has been but partially thawed, which sometimes happens on the south side of a tree, when the heat of the sun is strong and the wind northerly. At the seasons now specified, therefore, the sap is evidently in motion; but the plant will not bleed at any other season of the year. Are we to conclude, therefore, that the motion of the sap is at such other season wholly suspended; or that it only flows with diminished velocity? It has been the opinion of some phytologists, indeed, that the motion of the sap is wholly suspended during the winter. But though the great cold of winter, as well as the great heat of

Though the sap's motion is never wholly suspended.

summer, is by no means so favourable to vegetation as the milder though more changeable temperature of spring and autumn, yet it does not wholly suspend the movement of the sap. Palms may be made to bleed at any season of the year. And although this is not the case with plants in general, yet there is proof sufficient that the colds of winter do not, even in this climate, entirely prevent the sap from flowing. Buds exhibit a gradual development of parts throughout the whole of the winter, as may be seen by dissecting them at different periods. So also do roots. Evergreens retain their leaves; and many of them, such as the *Arbutus*, *Laurustinus*, and the beautiful tribe of the Mosses, protrude also their blossoms, even in spite of the rigour of the season. But all this could not possibly be accomplished if the motion of the sap were wholly suspended.

Its direction that of ascent.

The sap then is in perpetual motion with a more accelerated or more diminished velocity throughout the whole of the year: but still there is no decided indication, exhibited in the mere circumstance of the plant's bleeding, of the direction in which the sap is moving at the time; for the result might be the same whether it was passing from the root to the branches, or from the branches to the root. But as the great influx of the sap is effected by means of the pores of the epidermis of the root, it follows that its motion must, at least in the first place, be that of ascent; and such is its direction at the season of the plant's bleeding, as may be proved by the following experiment :—If the bore or incision that has been made in the trunk is minutely inspected while the plant yet bleeds, the sap will be found to issue almost wholly from the inferior side. If several bores are made in the same trunk one above another, the sap will begin to flow first from the lower bore, and then from those above it. If a branch of a Vine be lopped, the sap will issue copiously from the section terminating the part that remains yet attached to the plant; but not from the section terminating the part that has been lopped off. This proves indubitably that the direction of the sap's motion, during the season of the plant's bleeding, is that of ascent.

But if the sap flows so copiously during the season of bleeding, it follows that it must ascend with a very considerable force; which force has accordingly been made the subject of calculation. To the stem of a Vine cut off about two feet and a half from the ground, Hales fixed a mercurial gauge which he luted with mastic; the gauge was in the form of a syphon, so contrived that the mercury might be made to rise in proportion to the pressure of the ascending sap. The mercury rose accordingly, and reached, as its maximum, to a height of 38 inches.* But this was equivalent to a column of water of the height of 43 feet $3\frac{1}{3}$ inches; demonstrating a force in the motion of the sap that, without

Its velocity.

* Veg. Stat. Exper.

the evidence of experiment, would have seemed altogether incredible.

The sap then in ascending from the lower to the upper extremity of the plant is propelled with a very considerable force, at least in the bleeding season. But is the ascending sap propelled indiscriminately throughout the whole of the tubular apparatus, or is it confined, in its course, to any particular channel? Before the anatomy of plants had been studied with much accuracy, there was a considerable diversity of opinion on the subject.

Channel of ascent.

Some thought it ascended by the bark; others thought it ascended by the bark, wood, and pith, indiscriminately; and others thought it ascended between the bark and wood.

According to Malpighi.

The first opinion was maintained and supported by Malpighi; who seems to have taken it for granted that the sap ascends by the bark, merely because the fibres of the bark (which he describes under the appellation of *fibræ ligneæ, seu vasa tubulosa*)* had been found to be tubular, and hence permeable to fluids: but this is a very lame argument indeed; for although the bark is of a vascular texture and permeable to fluids, yet this is no proof that the sap in its natural course ascends through it, because the vessels contained in it may possibly be destined for purposes very different from that of the transmission of the sap. But it was said that when a horizontal in-

* Anat. Plant. v.

cision is made in the bark, a fluid is found to exude from the lower lip, and that consequently the sap ascends by it. But in order to make this argument good, the fluid must first be proved to be sap, or at least to afford the presumptive evidence of continuing to flow for a considerable length of time; as it is known that the vessels of plants will empty themselves at both ends when cut horizontally, as any one may see merely by cutting in two the stem of any species of Spurge; so that the mere exudation of a fluid from the lower lip, is no proof that it proceeds from the ascending sap since the vessels might thus empty themselves if they but contained even a fluid descending.

It was further contended that old Willows and several other sorts of trees will still continue to vegetate even when the whole of the woody part of the trunk is decayed, and nothing but bark remaining. But this is not exactly the fact; for in the case alluded to, there will always be found to be more or less of wood immediately under the bark; so that the ascent of the sap through the channel of the bark is by no means established.

The second opinion does not seem to have been entertained by any very distinguished phytologists —namely, that of its ascending between the bark and wood; but it seems to have been entertained by those who held it, because much juice is found there; because the wood is formed there; and because the graft takes effect there.

According to Grew.

Grew has shown this opinion to be altogether erroneous, and has substituted a third in its place—namely, that the sap ascends by the bark, wood, and pith indiscriminately.* It ascends by the pith, as he says, during the first year of the plant's growth, and during the first year only; because the pith is always found succulent during that time, whether in the sprout from a seed, or sucker from a root, or scion from a branch; but dry ever after. And it ascends by the wood and bark, because upon cutting a branch a liquid issues from both either spontaneously or by pressure. But we must not too hastily conclude that any particular part of the plant serves as the channel of the sap's ascent, merely because it has been found to be moist, or to give out a liquid by pressure; for one might just as well say, because the soil of a meadow situated by the banks of a river contains a great deal of moisture, that it is therefore the channel of the descent of the water as well as the bed of the river itself. The sap is no doubt conveyed to all the parts of the plant, and is consequently to be found in them all under one modification or another, but still its ascent is confined to a peculiar channel. Let us try to ascertain by experiment what that channel is.

If a tree is to be subjected to the operation of bleeding, there will be no notable discharge of sap unless the bore or incision penetrates beyond the bark; and if the operation is performed on the

* Veg. of Trunks, chap. i.

trunk of the Poplar-tree, there will be no notable discharge till it penetrates almost to the centre.* These facts afford a strong presumption that the sap does not ascend by the bark; but the following experiments afford an indubitable proof.

Experiments of Du Hamel and Knight.

Du Hamel stript several trees of their bark entirely, which continued, notwithstanding, to live for many years, protruding new leaves and new branches as before.† Mr. Knight stript the trunk of a number of young Crab-trees of a ring of bark half an inch in breadth, but the leaves were protruded, and the branches elongated, as if the operation had not been performed.‡ It is evident, therefore, that the sap does not ascend by the bark.

But it is equally evident that it does not ascend by the pith, at least after the first year; for then, even upon Grew's own supposition, it becomes either juiceless or wholly extinct: and even during the first year it is not absolutely necessary, if at all subservient to the ascent of the sap, as is proved by an experiment of Mr. Knight's. Having contrived to abstract from some annual shoots a portion of their pith, so as to interrupt its continuity, but not otherwise *materially to injure* the fabric of the shoot, he found that the growth of the shoots which had been made the subject of experiment was not at all affected by it.

* Exper. par Coulomb. † Phys. des Arb. liv. v. chap. ii.
‡ Phil. Trans. 1801.

Proving its ascent through the wood,

The sap then ascends neither by the bark nor pith, but by the wood only. But the whole mass of the wood throughout is not equally well adapted for the purpose of conveying it. The interior and central part, or part that has acquired its last degree of solidity, does not in general afford it a passage. This is proved by what is called the girdling of trees, which consists in making a circular gap or incision quite round the stem, and to the depth of two or three inches, so as to cut through both the bark and alburnum. The operation is very generally performed by the American farmer on trees whose further growth might be prejudicial to his crop, but which he does not yet find it convenient to cut down: and indeed there are but few trees that will long survive the operation, particularly if performed early in the spring.* An Oak-tree on which Mr. Knight had performed the operation with a view to the very object in question, namely, that of ascertaining the channel of the sap's ascent, exhibited not the slightest mark of vegetation in the spring following.† The sap then does not ascend through the channel of the matured wood.

Or rather alburnum.

But if the sap ascends neither through the channel of the bark, nor pith, nor matured wood; through what other channel does it actually ascend? The only remaining channel through which it can possibly ascend is that of the alburnum. But though the object of our inquiry has been thus so

* Barton's Elem. of Bot. p. 155. † Phil. Trans. 1805.

far obtained, another inquiry remains yet to be instituted. In passing through the channel of the alburnum, does the sap ascend promiscuously by the whole of the tubes composing it, or is it confined in its passage to any peculiar set?

Vessels through which it ascends,

The earliest conjectures recorded on this subject are those of Grew and Malpighi, who, though they maintained that the sap ascends chiefly by the bark, did not yet deny that it ascends also partly by the alburnum or wood: but their opinions do not at all coincide with regard to the peculiar set of vessels through which the sap ascends the alburnum. Malpighi thought it ascended through the channel of the tubes formed by the woody fibre, which he describes under the appellation of *fistulæ ligneæ*;* regarding the tracheæ, which he represents as constituting also part of the wood, as being confined merely to the function of conducting air. But Grew thought it ascended the alburnum only through the channel of the tracheæ,† which he represents as being numerous both in the stem and root, and capable of conducting not only air but sap. Such were the primitive conjectures entertained on this subject, at a time when phytological inquiry was but yet in its in-

According to Grew and Malpighi.

* Sunt autem hæ fistulæ ejusdem naturæ cum exaratis corticem compingentibus, et consimilem admittunt succum qui ex naturæ legibus sursum pellitur. Anat. Plant. xi.

† In the wood the sap ascendeth only by the air vessels. Veg. of Trunks, chap. i.

fancy; and as the obscurity as well as importance of the subject demanded, so it gave origin to further investigations.

It occurred to succeeding phytologists that the progress of the sap, and the vessels through which it passes, might be traced or ascertained by means of making plants to vegetate in coloured infusions; and accordingly plants were made so to vegetate. The earliest experiments on the subject seem to be those of Magnol, instituted about the beginning of the eighteenth century, though it does not appear that his object was any thing beyond that of merely demonstrating the ascent of the sap to the very summit of the plant. The colouring matter he made use of was the juice of *Phytolacca;* and when the extremity of a stem of the Tuberose was moistened in an infusion of this juice, it was found to mount up to the summit and to give a red colour to the flower. M. De la Baisse, improving upon this hint, instituted a number of experiments, with the same juice, upon a great variety of different plants, and found that the infusion always left behind it some evident traces of its ascent in the form of longitudinal streaks or threads. In the root, it was found that the smaller divisions were always tinged more deeply, and the larger divisions more faintly; the tinge being also deeper as it approached the centre. In stems of the Peach and Elm, of from three to four feet in length, the coloured tubes were traced to the extremity of the

De la Baisse.

branches pervading the wood only, but not the pith or bark, the tinge being always deepest at the origin of the leaf and branch. But in the immersed portion of the stem, the bark was tinged where the epidermis was wanting; and in branches of the Fig-tree, the medullary sheath, or sheath surrounding the pith, was tinged also. On inspecting the surface of a transverse section of a branch of the Lime-tree that had been made the subject of experiment, the wood was found to be variegated with alternate zones of white and red; but there was no colouring in the bark or pith.

In herbaceous plants the case was nearly the same, the streaks being found only in the bundles of woody fibre, though in some examples the upper part of the plant had assumed a reddish tinge, even where no traces of fibre were perceptible. In the leaves the infusion was found to have passed through two different sets of vessels, the one large and longitudinal, the other undulating and twisted; the former abounding chiefly in the under surface, and the latter in the upper surface. In repeating the experiment of Magnol, M. De la Baisse was completely successful; and in extending it to a species of *Antirrhinum*, the infusion was found to have tinged not only the corolla, but also all the other parts of the fructification.

Bonnet instituted a set of similar experiments on this subject also, in which he used for colouring matter, ink, and tincture of Madder-root. The Bonnet.

central part of the root was, as in the foregoing experiments, tinged the deepest; but the wood of a branch of the Apricot was tinged the deepest towards the circumference, the pith and bark being unaltered; and in a case in which the branch was stripped of a ring of bark, the wood was tinged as before. On the surface of a transverse section of a bud three black spots were distinguishable, indicating the ascent of the coloured fluid; and when the experiment was made upon the stalk of French Beans the tincture was found to have ascended only by the bundles of woody fibre.

DuHamel. Du Hamel, in pursuit of the same object, mixed a quantity of powdered Madder-root with the earth in which a plant vegetated, hoping he might thus succeed in detecting and tracing out the sap vessels, in the same manner as he had succeeded in colouring the bones of some animals by means of mixing Madder-root with their food. But the experiment failed. He then adopted the plan of Bonnet, namely, that of steeping the extremity of a branch or stem in a coloured fluid. The fluid he used was ink; and the subject of experiment branches of the Fig, Elder, Honeysuckle, and Filbert. In examining some branches of the two former after being steeped for several days, the part immersed was found to be black throughout, but the upper part was tinged only in the wood, which was coloured for the length of a foot, but more faintly and partially in proportion to the height. The pith indeed exhibited some traces of ink, but the bark and buds none. In some other examples the external layers of the wood only were tinged. In the Honeysuckle the deepest shade was about the middle of the woody layers; and in the Filbert there was also observed a coloured circle surrounding the pith; but none in the pith itself, nor in the bark.

Such were the experiments of the earlier phytologists with a view to discover the vessels conducting the sap in its ascent, which, though they do not exactly determine the point in question, do yet very much circumscribe the limits of inquiry, showing that it ascends through the vessels of the longitudinal fibre composing the alburnum of woody plants, and through the vessels of the several bundles of longitudinal fibre constituting the woody part of herbaceous plants. But it has been already shown that the vessels composing the woody fibre are not all of the same species. There are simple tubes, porous tubes, spiral tubes, mixed tubes, and interrupted tubes. Through which of these, therefore, does the sap pass in its ascent?

The best reply to this inquiry has been furnished by Mr. Knight and M. Mirbel, whose experiments on the subject are considerably more luminous than the preceding. Mr. Knight prepared some annual shoots of the Apple and Horse-chesnut, by means of circular incisions, so as to leave detached rings of bark with insulated leaves remaining on the

Mirbel and Knight.

stem. He then placed them in coloured infusions obtained by macerating the skins of very black grapes in water; and on examining the transverse section at the end of the experiment, it was found that the infusion had ascended by the wood beyond his incisions, and also into the insulated leaves, but had not coloured the pith nor bark, nor the sap between the bark and wood.* From the above experiment Mr. Knight concludes that the sap ascends through what are called the common tubes of the wood and alburnum, at least till it reaches the leaves. This is no doubt the fact, though still it is but vaguely stated. M. Mirbel is somewhat more explicit. From a variety of experiments made by immersing branches of the Elder in coloured infusions, he finds himself entitled to conclude that the sap ascends through the medium of what he calls the small tubes; because they only were found to be coloured with the infusion, while the large tubes were not; and because in the bleeding season they are found to contain a limpid juice, which the large tubes do not.

Who traces it even into the leaf.

Thus the sap is conveyed to the summit of the alburnum. But Mr. Knight's next object was to trace the vessels by which it is conveyed into the leaf. The Apple-tree and Horse-chesnut were still his subjects of experiment. In the former the leaves are attached to the plant by three strong fibres, or rather bundles of tubes, one in the middle

* Phil. Trans. 1801.

of the leaf-stalk, and one on each side. In the latter they are attached by means of several such bundles. Now the coloured fluid was found in each case to have passed through the centre of the several bundles, and through the centre only, tinging the tubes throughout almost the whole length of the leaf-stalk. In tracing their direction from the leaf-stalk upwards, they were found to extend to the extremity of the leaves; and in tracing their direction from the leaf-stalk downwards, they were found to penetrate the bark and alburnum, the tubes of which they join, descending obliquely till they reach the pith which they surround.* From their position Mr. Knight calls them central tubes, thus distinguishing them from the common tubes of the wood and alburnum, and from the spiral tubes with which they were every were accompanied as appendages; as well as from a set of other tubes which surrounded them, but were not coloured, and which he designates by the appellation of external tubes.

The experiment was now transferred to the flower-stalk and fruit-stalk, which was done by placing branches of the Apple, Pear, and Vine, furnished with flowers not yet expanded, in a decoction of Logwood. The central vessels were rendered apparent as in the leaf-stalk. When the fruit of the two former was fully formed, the experiment was then made upon the fruit-stalk, in **Flower and fruit.**

* Phil. Trans. 1805.

which the central vessels were detected as before; but the colouring matter was found to have penetrated into the fruit also, diverging round the core, approaching again in the eye of the fruit, and terminating at last in the stamens. It was by means of a prolongation of the central vessels, which did not however appear to be accompanied by the spiral tubes beyond the fruit-stalk.

Such then are the parts of the plant through which the sap ascends, and the vessels by which it is conveyed. Entering by the pores of the epidermis, it is received into the longitudinal vessels of the root by which it is conducted to the collar. Thence it is conveyed by the longitudinal vessels of the alburnum, the small tubes of Mirbel, and common tubes of Knight, to the base of the leaf-stalk and peduncle; from which it is further transmitted by the central vessels of Knight to the extremity of the leaves, flower, and fruit. It is to be regretted, however, that Mr. Knight's description of the central vessels is not altogether so explicit as could be wished. In trying to illustrate the subject by synonym, he says indeed, that his central vessels are M. Mirbel's tubular tissue.* But this unhappily tends to obscure rather than to elucidate the subject. For M. Mirbel's tubular tissue consists of not less than five or six different species, large tubes, small tubes, simple tubes, porous tubes, spiral tubes, and mixed tubes. But to which of

* Phil. Trans. 1807.

them do the central vessels correspond? If we regard their respective functions they can correspond only to the small tubes, as it is by them alone, according to M. Mirbel, that the sap ascends.

Function of the spiral tubes, according to Grew and Malpighi.

And yet after all the elucidation that has been thrown on the subject, the function of the spiral tubes is as much involved in obscurity as ever. Grew, who, together with Malpighi, regarded them originally as being destined to the transmission of air, is known to have retracted his opinion, or at least to have very much modified it; so that, instead of regarding them as being solely air-vessels, he afterwards regarded them as being also sap-vessels, and as being even the sole sap-vessels of the wood or alburnum. But this opinion is evidently contradicted by the fact that no tracheæ are to be found in the wood or alburnum, except in the annual shoot immediately surrounding the pith; for they are not generated in the succeeding and annual layers by which the stem and trunk are augmented in width, and are obliterated by age in the vicinity even of the pith itself. It is impossible, therefore, that they should be the channel of the sap's ascent through the wood or alburnum of an aged trunk.

According to Darwin, Knight, and Smith.

And yet this opinion seems to have been adopted even by Dr. Smith, upon the authority as it appears of Dr. Darwin and Mr. Knight, whom he represents as having proved in the most satisfactory manner that the spiral vessels are the channel through which the sap ascends.* That this was the conclusion deduced from Dr. Darwin's experiment, there is no doubt. But it does not appear that Mr. Knight has deduced any such conclusion from any experiments of his own; but certainly not inasmuch as regards the ascent of the sap through the alburnum, in which he denies the existence of the spiral tubes altogether, except as already stated. And although his central tubes, which conduct the sap through the leaf and leaf-stalk of woody plants, as well as through the stem of herbaceous plants, are accompanied with spiral tubes as appendages; yet these spiral appendages are represented as conveying no fluid. How then Dr. Smith came to regard it as Mr. Knight's opinion that the sap ascends through the channel of the spiral tubes, it is not easy to say, except from Mr. Knight's occasional obscurity and perplexity of expression, if not of thought, that so often embarrass the reader, even in his most luminous papers.

Which the most probable opinion.

But what is the office of the spiral tubes where they are certainly known to exist? The opinion of Grew and Malpighi, as it is the most ancient, is perhaps also the most correct: at least we may fairly regard Grew's reformed opinion in this light; namely, that they transmit not only air but sap. It is indeed the opinion of Knight that they are altogether incapable of transmitting moisture: but

* Introduction, p. 49.

this can refer only to their uncoiled state, in which they do not form a tube, but merely a loosely spiral line; for in the coiled up state in which they exist in the living vegetable and in which the spires are united, they form a perfect tube, which we cannot regard as incapable of transmitting moisture without some proof. On the contrary it seems to have been ascertained that they do contain moisture. Hedwig, who examined with great care, and with a view to the very point in question, the stem of the *Cucurbita Pepo*, and *Momordica Elaterium*, in which the spiral tubes are comparatively large, affirms that the juices may be seen issuing from their orifices, if the horizontal section is inspected immediately after the stem is divided.* Senebier gives a similar account of their appearance in the stem of the *Sagus farinifera*, which he had chosen for the subject of his observations.† And Hedwig appears to have succeeded even in inspecting them by means of steeping a portion of the stem in coloured infusions; and also in measuring their diameter, which he estimates at about the $\frac{1}{290}$ of a line. In these observations there may certainly have been error; but from the known accuracy of the observers we are warranted in regarding them as correct; and consequently in concluding that the tracheæ or spiral tubes, where they exist, do also conduct sap.

But still there remains a question to be asked

* Fund. Hist. Nat. Musc. p. 55. † Phys. Veg. vol. i. p. 107.

Lateral communication.

intimately connected with the sap's ascent. Do the vessels conducting the sap communicate with one another by inosculation or otherwise, so as that a portion of their contents may be conveyed in a lateral direction, and consequently to any part of the plant; or do they form distinct channels throughout the whole of their extent, having no sort of communication with any other set of tubes, or with one another?

Denied.

Each of the two opinions implied in the question has had its advocates and defenders. At the head of those embracing the former we find Malpighi; and at the head of those embracing the latter we find Grew; who, in speaking of what he calls the succiferous and air vessels of the bark and wood of the root, describes them as being no where inosculated or twisted one about another, but only tangent or collateral.* This was regarded as a proof that the vessels of plants do not communicate laterally, so as to distribute their sap in all directions, but are destined merely to the nourishment of a particular part. It was also urged in support of the same opinion, that if a tree is planted so as to have cultivated ground on the one side, and uncultivated ground on the other, the roots and branches will be the most vigorous and the most luxuriant on the cultivated side; and that if a tree having two or more principal branches, with the same number of principal roots, has one of the roots

* Anatomy of Roots, Part II. chap. iii. and iv.

cut off, the branch corresponding to it will be considerably affected by the loss.

But a very little reflection will serve to show that the above premises are by no means sufficient to justify the conclusion that has been drawn from them: for although the tracheæ as they exist in the living plant are not twisted one about another, but only tangent or collateral, yet the longitudinal film of which they are composed is itself perforated with pores, or interrupted with meshes, as may be seen by inspecting those of the leaf-stalk of the Artichoke; so that it is very possible they may be occasionally tangent, where the pores or meshes shall meet. And the degeneration of the root or branch in the cases above stated is just what would have taken place, upon the supposition that a lateral communication exists: for if by default of nutriment in the direct line, any root or branch is supposed to be nourished merely by means of the sap that may be transmitted to it through the lateral apertures, its growth will of necessity be less luxuriant, because its supply is now rendered both more scanty and more difficult of access. So that we have in this argument a refutation of the very doctrine it was meant to support; for if the root or branch thus treated does not absolutely die, it is a proof that lateral communication must exist. Proved.

But the existence of a lateral communication between the vessels conducting the sap has been also further elucidated by means of direct experiment. Du Hamel having selected a tree for the purpose of experiment, made two incisions at different heights and on opposite sides of the trunk, each penetrating to the centre. He then closed up the gaps with a mixture of wax and turpentine, in order to prevent the action of the air from interfering in the result; and found accordingly that the tree continued to vegetate as before the experiment. Hales took two branches of equal size, in one of which he made four incisions, answering to the four cardinal points, and each penetrating to the centre; in the other he made no incision. He then put the extremities of both into water, and found that the branch that was cut with incisions absorbed moisture as copiously as the one that was not so cut. Knight has also shown that a branch will still continue to live though the tubes leading directly to it are cut in the trunk.* From all which it follows that the sap, though flowing the most copiously in the direct line of ascent, is at the same time also diffused in a transverse direction. But this seems to have been acknowledged even by Grew himself, in spite of all he has advanced in support of a contrary opinion: for he says that the sap vessels of the bark being the first year adjacent to the pith, do all that time transfuse part of their sap into it, and so keep it always succulent;† which, whether we regard as being the fact

* Phil. Trans. 1801. † Veg. of Trunks, chap. i.

or not, is at any rate admitting a species of lateral communication.

SECTION III.

Causes of the Sap's Ascent.

FROM the evidence exhibited in the foregoing section the ascent of the sap is demonstrated, and the channel of its distribution ascertained. But what is the cause of that ascent; or by what power is the sap propelled?

The great and almost impenetrable obscurity in which this subject is unavoidably involved has occasioned much diversity of opinion among phytologists. Grew states two hypotheses which he seems to have entertained at different periods, though it is not quite certain to which of them he finally gave the preference. In one of them he attributes the ascent of the sap to its volatile nature and magnetic tendency, aided by the agency of fermentation.* But this hypothesis is by much too fanciful to bear the test of serious investigation. In the other he attributes the entrance and first stage of the sap's ascent to the agency of capillary attraction, and accounts for its progress as follows: The portion of the tube that is now swelled with sap, being surrounded with the vesiculæ of the

Hypothesis of Grew.

* Anat. of Veg. chap. iii.

Parenchyma, swelled also with sap, which they have taken up by suction or filtration, is consequently so compressed, that the sap therein is forced upwards a second stage, and so on till it reaches the summit of the plant.* But if the vesiculæ of the Parenchyma receive their moisture only by suction or filtration, it is plain that there is a stage of ascent beyond which they cannot be thus moistened, and cannot consequently act any longer upon the longitudinal tubes. The supposed cause, therefore, is inadequate to the production of the effect.

Of Malpighi.

Malpighi was of opinion that the sap ascends by means of the contraction and dilatation of the air contained in the air-vessels. This supposition is perhaps somewhat more plausible than either of Grew's; but in order to render the cause efficient, it was necessary that the tubes should be furnished with valves, which were accordingly supposed;† but of which the existence has been totally disproved by succeeding phytologists. If the stem or branch of a plant is cut transversely in the bleeding season, it will bleed a little from above, as well as from below: and if the stem of any species of

* Veg. of Trunks, chap. i.

† Subintrans itaque humor, sursum ascendit et quasi suspenditur; singula namque portio, quæ invicem fibrarum frustula unit, cum parum interius emineat, valvulæ vices supplet, et ita minima quælibet guttula, veluti per funem, seu per gradus, ad ingens deducitur fastigium. Anat. Plant. vol. v.

spurge is cut in two, a milky juice will exude from both sections, in almost any season of the year. Also if a plant is inverted, the stem will become a root, and the root a stem and branches, the sap ascending equally well in a contrary direction through the same vessels; as may readily be proved by planting a willow twig in an inverted position. But these facts are totally incompatible with the existence of valves; and the opinion of Malpighi proved consequently to be groundless.

Of De la Hire.

The next hypothesis is that of M. De la Hire, who seems to have attempted to account for the phenomenon by combining together the theories of Grew and Malpighi. Believing that the absorption of the sap was occasioned by the spongy parenchyma which envelopes the longitudinal tubes, he tried to illustrate the subject by means of the experiment of making water to ascend in coarse paper, which it did readily to the height of six inches, and by particular management even to the height of eighteen inches. But in order to complete the theory, valves were also found to be necessary, and were accordingly summoned to its aid. The sap which was thus absorbed by the root was supposed to ascend through the woody fibre, by the force of suction, to a certain height, that is, till it got above the first set of valves, which prevented its return backwards; when it was again supposed to be attracted as before, till it got to the second set of valves, and so on till it got to the top of the plant.

Borelli. This theory was afterwards adopted by Borelli, who endeavoured to render it more perfect by bringing to its aid the influence of the condensation and rarification of the air and juices of the plant as a cause of the sap's ascent. And on this principle he endeavoured also to account for the greater force of vegetation in the spring and autumn; because the changes of the atmosphere are then the most frequent under a moderate temperature; while in the summer and winter the changes of the atmosphere are but few, and the air and juices either too much rarified or too much condensed, so that the movement of the sap is thus at least prejudicially retarded, if not perhaps wholly suspended. But as this theory, with all its additional modifications, is still but a combination of the theories of Grew and Malpighi, it cannot be regarded as affording a satisfactory solution of the phenomenon of the sap's ascent.

DuHamel. With this impression upon his mind, and with the best qualifications for the undertaking, Du Hamel directed his efforts to the solution of the difficulty, by endeavouring to account for the phenomena from the agency of heat, and chiefly on the following grounds: * Because the sap begins to flow more copiously as the warmth of spring re-

* Phys. des Arb. liv. v. chap. ii.

turns; because the sap is sometimes found to flow on the south side of a tree before it flows on the north side, that is, on the side exposed to the influence of the sun's heat sooner than on the side deprived of it; because plants may be made to vegetate even in the winter, by means of forcing them in a hot-house; and because plants raised in a hot-house produce their fruit earlier than such as vegetate in the open air.

There can be no doubt of the great utility of heat in forwarding the progress of vegetation; but it will not therefore follow that the motion and ascent of the sap are to be attributed to its agency. On the contrary, it is very well known that if the temperature exceeds a certain degree, it becomes then prejudicial both to the ascent of the sap and also to the growth of the plant. Hales found that the sap flows less rapidly at mid-day than in the morning; * and every body knows that vegetation is less luxuriant at mid-summer than in the spring. So also in the case of forcing it happens but too often that the produce of the hot-house is totally destroyed by the unskilful application of heat; and if heat is actually the cause of the sap's ascent, how comes it that the degree necessary to produce the effect is so very variable even in the same climate. For there are many plants, such as the *Arbutus*, *Laurus Tinus*, and the Mosses, that will continue not only to vegetate, but to protrude their

* Veg. Stat. Exper. 36.

blossoms and mature their fruit, even in the midst of winter, when the temperature is at the lowest. And in the case of submarine plants the temperature can never be very high; so that although heat does no doubt facilitate the ascent of the sap by its tendency to make the vessels expand, yet it cannot be regarded as the efficient cause; since the sap is proved to be in motion even throughout the whole of the winter. Du Hamel endeavours, however, to strengthen the operation of heat by means of the influence of humidity, as being also powerful in promoting the ascent of the sap, whether as relative to the season of the year or time of the day. The influence of the humidity of the atmosphere cannot be conceived to operate as a propelling cause, though it may easily be conceived to operate as affording a facility to the ascent of the sap in one way or other; which under certain circumstances is capable of most extraordinary acceleration, but particularly in that state of the atmosphere which forebodes or precedes a storm. In such a state a stalk of Wheat was observed by Du Hamel to grow three inches in three days; a stalk of Barley six inches; and a shoot of a Vine almost two feet; but this is a state that occurs but seldom, and cannot be of much service in the general propulsion of the sap.

Linnæus. On this intricate but important subject Linnæus appears to have embraced the opinion of Du Hamel, or an opinion very nearly allied to it; but does not seem to have strengthened it by any new accession of argument; so that none of the hitherto alleged causes can be regarded as adequate to the production of the effect.

Saussure.

Perhaps the only cause that has ever been suggested as appearing to be at all adequate to the production of the effect, is that alleged by M. Saussure. According to Saussure the cause of the sap's ascent is to be found in a peculiar species of irritability inherent in the sap vessels themselves, and dependant upon vegetable life; in consequence of which they are rendered capable of a certain degree of contraction, according as the internal surface is affected by the application of stimuli, as well as of subsequent dilatation according as the action of the stimulus subsides; thus admitting and propelling the sap by alternate dilatation and contraction. In order to give elucidation to the subject let the tube be supposed to consist of an indefinite number of hollow cylinders united one to another, and let the sap be supposed to enter the first cylinder by suction, or by capillary attraction, or by any other adequate means; then the first cylinder being excited by the stimulus of the sap, begins gradually to contract, and to propel the contained fluid into the cylinder immediately above it. But the cylinder immediately above it, when acted on in the same manner, is affected in the same manner; and thus the fluid is propelled from cylinder to cylinder till it reaches the summit of the plant. So also when the first cylinder has discharged its contents into the second, and is no longer acted upon by the stimulus of the sap, it begins again to be dilated to its original capacity, and prepared for the introsusception of a new portion of fluid. Thus a supply is constantly kept up, and the sap continues to flow.

Knight. The above is by far the simplest as well as most satisfactory of all theories accounting for the ascent of the sap. But Mr. Knight has presented us with another which, whatever may be its real value, merits at least our particular notice, as coming from an author who stands deservedly high in the list of phytological writers. This theory rests upon the principle of the contraction and dilatation, not of the sap vessels themselves, as in the theory of Saussure, but of what Mr. Knight denominates the *silver grain*, assisted perhaps by heat and humidity, expanding or condensing the fluids. The appellation of the *silver grain* seems to be synonymous with that of the medullary rays already explained. On the transverse section of the trunk of woody plants, particularly the Oak, they appear in the form of the radii of a circle extending from the pith to the bark, and on the longitudinal cleft or fissure of the trunk of most trees, but particularly the Elm; they appear in the form of fragments of thin and vertical lamina or plates, interlacing the ascending tubes in a transverse direction, and touching them at short intervals, so as to form with them a sort of irregular wicker work, or to exhibit the resemblance of a sort of web. Such then being the close and complicated union of the plates and longitudinal tubes, the propulsion of the sap in the latter may be easily accounted for, as it is thought, by means of the alternate contractation and dilatation of the former, if we will but allow them to be susceptible to change of temperature; which susceptibility is proved, as it is also thought, from the following facts:—On the surface of an oaken plank that was exposed to the influence of the sun's rays, the transverse layers were observed to be so considerably affected by change of temperature as to suggest a belief that organs which were still so restless, now that the tree was dead, could not have been formed to be altogether idle while it was alive. Accordingly on the surface of the trunk of an Oak deprived of part of its bark, the longitudinal clefts and fissures which were perceptible during the day were found to close during the night. But in the act of dilating they must press unavoidably on the longitudinal tubes, and consequently propel the sap; while in the act of contracting they again allow the tubes to expand and take in a new supply. This, as I think, is the substance of the theory.*

But in drawing this grand and sweeping conclusion, it should have been recollected that change of temperature cannot act upon the transverse layers of a tree that is covered with its bark in the same manner as it acts upon those of a tree that is stripped

* Phil. Trans. 1801.

Insufficient.

of its bark, or upon those of a plank; and if it were even found to act equally upon both, still its action would be but of little avail. For according to what law is the machinery of the plates to be contracted and dilated, so as to give impulse to the sap? According to the alternate succession of heat and humidity. But this is by much too precarious an alternation to account for the constant, and often rapid, propulsion of the sap, especially at the season of bleeding. For there may be too long a continuance of heat, or there may be too long a continuance of humidity; and what is to become of the plant during this interval of alternation? If we are to regard it as happening only once in the space of four and twenty hours, as in the case of the Oak, it can never be of much efficacy in aiding the propulsion of the sap. But if we should even grant more, and admit the alternate contraction and dilatation of the vessels to be as frequent as you please, still their effect would be extremely doubtful, owing to a want of unity or co-operation in the action of different plates, or of different portions of the same plate. If heat, like humidity, entered the plant by the root, and proceeded gradually upwards like the ascending sap, perhaps it might be somewhat efficacious in carrying a portion of sap along with it; but as this is not the case, and as the roots of plants are but little affected by change of temperature, while the trunk and upper parts may be affected considerably, it can scarcely be supposed that the action of the plates will be uniform throughout the whole plant; or rather, it must be supposed that it will often be directly in opposition to that which is necessary to the propulsion of the sap. But admitting that the sap is propelled by the agency of the plates in question, and admitting that it has been thus raised to the extremity of the woody part of the plant, how are we to account for its ascent in such parts as are yet higher—the leaf-stalk and leaf, the flower-stalk and flower; as well as in the herb also, and in the lofty Palm, in which no such plates exist? Here it will be necessary to introduce the agency of a new cause to complete the work that has been thus begun, and of a new set of machinery to supply the deficiency or absence of the machinery that has been already invented. In short the theory of Mr. Knight is beset with so many difficulties, and the agency of the alleged cause so totally inadequate to the production of the effect to be accomplished, that of all theories on the subject it is perhaps the least satisfactory.

SECTION IV.

Elaboration of the Sap.

THE moisture of the soil is no sooner absorbed into the plant than it begins to undergo a change. This is proved by the experiment of making a bore or incision in the trunk of a tree during the season

Commences in the root or stem.

of bleeding; the sap that issues from the wound possesses properties very different from the mere moisture of the soil, as is indicated by means of chemical analysis, and sometimes also by means of a peculiar taste or flavour, as in the case of the Birch-tree. Hence the sap has already undergone a certain degree of elaboration; either in passing through the glands of the cellular tissue, which it reaches through the medium of a lateral communication, or in mingling with the juices contained in the cells, and thus carrying off a portion of them; in the same manner, we may suppose, that water by filtering through a mineral vein becomes impregnated with the mineral through which it passes.

But this primary and incipient stage of the process of elaboration must always of necessity remain a mystery to the phytologist, as being wholly effected in the interior of the plant, and consequently beyond the reach of observation. All he can do, therefore, is to trace out its future progress, and to watch its succeeding changes, in which the rationalé of the process of elaboration may be more evident.

But is chiefly effected in the leaf by perspiration,

The next, and indeed the principal, part of the process of the elaboration of the sap is operated in the leaf: for the sap no sooner reaches the leaf than part of it is immediately carried off by means of perspiration, perceptible or imperceptible; effecting a change in the proportion of its component parts, and by consequence a change in its properties.

Imperceptible perspiration is an excretion of sap thrown off by the *Epidermis* of the leaf or other tender parts of the plant, in consequence of the healthy action of the vegetable organs. It is not discoverable by the external senses, as the name indeed implies, but is legitimately inferred from the following fact:—If the branch of a tree is lopped, and the section of the part lopped off covered with mastick, the branch will be found in the course of a few days to have lost in weight. This was originally an experiment of Mariotte's, and the loss in weight is to be accounted for only on the principle of the perspiration of the sap escaping through the pores of the epidermis. This conclusion may perhaps be regarded as not altogether satisfactory, as being founded on an experiment made only on a lopped-off branch. But the same conclusion follows from experiments on the living plant:—Hales reared a Sun-flower in a pot of earth till it grew to the height of three feet and a half; he then covered the mouth of the pot with a plate of lead, which he cemented so as to prevent all evaporation from the earth contained in it. In this plate he fixed two tubes, the one nine inches in length and of but small diameter, left open to serve as a medium of communication with the external air; the other two inches in length and one in diameter, for the purpose of introducing a supply of water; but kept always shut except at the time of watering. The holes of the bottom of the pot were also shut, and

Imperceptible,

the pot and plant weighed for fifteen successive days in the months of July and August; hence he ascertained, not only the fact of transpiration by the leaves, from a comparison of the supply and waste; but also the quantity of moisture transpired in a given time, by subtracting from the total waste the amount of evaporation from the pot. In a dry and hot day it transpired the most, and in a damp and wet day it transpired the least; the mean rate of transpiration being 1 lb. 4 oz.—17 times more in proportion than that of the human body. In a hot and dry night without dew it transpired 3 oz.; in a dewy night it did not transpire at all; and in a rainy night, or night of much dew, its weight was increased by 3 oz.

Hales suspected that the quantity transpired was in proportion to the extent of the surface of the leaves, which he regarded as the principal organs of transpiration; and ascertained also the relative proportion of the capacity of the leaves for transpiration as compared to the capacity of the root for absorption. The surface of the leaves and stem of the plant which was the subject of experiment was found to be equal to about 5616 square inches; and the surface of the root of the same plant, or rather, as I believe, of a plant of nearly the same size, was found to be about 2286 square inches, the latter being to the former in the proportion of two to five; from which it follows that the absorbing power of the root is greater than the transpiring power of the leaves, in the proportion of five to two. Similar experiments were also made upon some species of cabbage, whose mean transpiration was found to be 1 lb. 3 oz. per day; and on some species of evergreens, which were found, however, to transpire less than other plants. The same is the case also with succulent plants, which transpire but little in proportion to their mass, and which as they become more firm transpire less. It is known, however, that they absorb a great deal of moisture, though they give it out thus sparingly; which we cannot but regard as a wise institution in nature for the purpose of resisting the great droughts to which they are generally exposed, inhabiting, as they do for the most part, the sandy desert or the sunny rock.

Along with his own experiments Hales relates also some others that were made by Mr. Millar, of Chelsea; the result of which was that, other circumstances being the same, transpiration is in proportion to the transpiring surfaces; and is affected by the temperature of the air, sunshine, or drought promoting it, and cold and wet diminishing or suppressing it entirely. It is also greatest from six o'clock in the morning till noon, and is least during the night. But when transpiration becomes too abundant owing to excess of heat or drought, the plant immediately suffers and begins to languish; and hence the leaves droop during the day, though they are again revived during the night. For the same or for a similar reason, transpiration has been found also to increase as the heat of summer advances; being more abundant in July than in June, and still more in August than in either of the preceding months, from which last period it begins again to decrease.

But the most remarkable instance of rapid transpiration yet observed is that which is related by Guettard, who found that a small sprig of the Corneil-tree or Cornelian Cherry, *Cornus mascula*, transpired in the course of a day 1 oz. $3\frac{3}{4}$ drams, a quantity almost double its own weight. He found also in general that branches deprived of their leaves afford but little transpired matter, and that branches furnished with their leaves afford a great deal; it follows, therefore, of necessity that the leaves, as Hales suspected, are the principal organs of transpiration.

The substance thus transpired by the plant may be obtained by enclosing a bough in a glass vessel of proper dimensions luted to the branch. Its properties have not yet been very minutely investigated; Hales and Guettard could discover in it nothing different from common water except that in some cases it had the odour of the plant; but Du Hamel found that it became sooner putrid than water.

Or perceptible.

Such then are the facts that have been ascertained with regard to the imperceptible perspiration of plants, from which it unavoidably follows that the sap undergoes a very considerable modification in its passage through the leaf. But it often undergoes also a further modification in consequence of what may be called perceptible perspiration, which is an exudation of sap too gross or too abundant to be dissipated immediately, and which hence accumulates on the surface of the leaf. It is very generally to be met with in the course of the summer on the leaves of the Maple, Poplar, and Lime-tree; but particularly on the surface exposed to the sun, which it sometimes wholly covers. Its physical as well as chemical qualities are very different in different species of plants; so that it is not always merely an exudation of sap, but of sap in a high state of elaboration, or mingled with the peculiar juices or secretions of the plant.

Sometimes it is a clear and watery fluid conglomerating into large drops, such as are said to have been observed by Mr. Millar, of Chelsea, exuding from the leaves of the *Musa arbor*, or Plantain-tree; and such as are sometimes to be seen in hot and calm weather, exuding from the leaves of the Poplar, or Willow, and trickling down in such abundance as to resemble a slight shower. This Phenomenon was observed by Dr. Smith under a grove of Willows, in Italy,* and is said to occur sometimes even in England.† Sometimes it is glutinous, as on the leaf of the Lime-tree; sometimes it is waxy, as on the leaves of Rosemary; sometimes it is saccharine, as on the Orange leaf, according to the account of M. De la Hire, as re-

* Lectures at R. Inst. † Introd. p. 188.

lated by Du Hamel; who having observed under some Orange-trees a saccharine substance somewhat resembling *Manna,* found upon further investigation that it had fallen from the leaves.* Sometimes it is resinous, as on the leaves of the *Cistus creticus,* from which the resin known by the name of *Labdanum* is obtained, by means of beating it gently with leathern thongs to which the exudation adheres;† as also on the leaves of the *Populus dilatata,* or Lombardy Poplar, the exudation from which Ovid in his metamorphosing flights regards as the tears of Phaeton's sisters, whom he transforms, as it is supposed, into this species of Poplar.‡ Their tears were now gum.§ The leaves of Fraxinella or *Dictamnus albus* are also said to be often covered with a sort of resinous substance. And after a hot day, if the air is calm, the plant is even found to be surrounded with a resinous atmosphere, which may be set on fire by the application of the flame of a candle. This, as I think, was the discovery of a daughter of the celebrated Linnæus.

The cause of this excess of perspiration has not yet been altogether satisfactorily ascertained; though it seems to be merely an effort and institution of nature to throw off all such redundant

* Phys. des Arb. vol. i. p. 130. † Voyage de Tournefort.

‡ Smith's Introd. p. 189.

§ Inde fluunt lachrymæ, stillataque sole rigescunt
De ramis electra novis, quæ lucidus amnis
Excipit, et nuribus mittit gestanda Latinis. Ovid. Met. ii. 376.

juices as may have been absorbed, or secretions as may have been formed, beyond what are necessary to the due nourishment or composition of the plant, or beyond what the plant is capable of assimilating at the time. Hence the watery exudation is perhaps nothing more than a redundancy of the fluid thrown off by imperceptible perspiration, and the waxy and resinous exudations nothing more than a redundancy of secreted juices; all which may be still perfectly consistent with a healthy state of the plant. But there are cases in which the exudation is to be regarded as an indication of disease, particularly in that of the exudation known by the name of Honey-dew, a sweet and viscid substance covering the leaves like a varnish, and sometimes occasioning their decay. Such at least seems to be the fact with regard to the honey-dew of the Hop, which, according to the observations of Linnæus, is the consequence of the attacks of the caterpillar of the ghost moth injuring the root. And such seems also to be the fact with regard to the honey-dew of the Beech-tree, which Dr. Smith regards as the consequence of an unfavourable wind.* But whether the honey-dew of the Oak is to be regarded as an indication of disease I cannot say, as I have often met with it on trees and leaves that seemed perfectly healthy. The sap then in the progress of its ascent from the extremity of the root to the extremity of the leaf undergoes a con-

* Introduction, p. 189.

siderable change, first in its mixing with the juices already contained in the plant, and then in its throwing off a portion at the leaf. Perhaps it is also further affected by means of the gases entering into the root along with the moisture of the soil, but certainly, by means of the gases inhaled into the leaf; the action and elaboration of which I shall now proceed to elucidate.

SECTION V.

Elaboration of Carbonic Acid.

Carbonic acid gas inhaled by the leaf during the day.

THE utility of carbonic acid gas as a vegetable food has been already shown in the preceding chapter, in which plants were found not only to absorb it by the root along with the moisture of the soil; but also to inhale it by the leaves, at least when vegetating in the sun or during the day. But how is the elaboration of this gas effected? Is it assimilated to the vegetable substance immediately upon entering the plant, or is its assimilation effected by means of intermediate steps? The gas thus inhaled or absorbed is not assimilated immediately, or at least not wholly: for it is known that plants do also evolve carbonic acid gas when vegetating in the shade, or during the night. The circumstance that led to this discovery was as follows:—About the year 1771, Priestley in his experiments on air found that a cabbage leaf which was

Evolved during the night.

placed under a glass vessel filled with common air for the space of one night only, had so affected its atmosphere by next morning that a candle would not burn in it, and yet the leaf showed no symptoms of putrefaction.* This fact he did not at the time attempt to account for; so that it was not yet known whether the change produced in the atmosphere of the leaf was occasioned by the abstraction of any constituent part, or by the addition of any extraneous substance. The true cause was afterwards ascertained by Saussure: into a receiver containing only atmospheric air, Saussure introduced some plants of *Vicia Faba*, and placed the apparatus in the shade; but at the end of six days when the experiment was stopped, the atmosphere of the receiver was found by the application of lime water to contain $\frac{1}{100}$ of carbonic acid. Into another receiver containing also atmospheric air he introduced at the same time several other plants of the same species, together with a small quantity of lime, and placed the receiver over lime water, leaving the apparatus in the shade. At the end of the six days of experiment the atmosphere of the receiver contained $\frac{3}{100}$ of carbonic acid, though a great deal must have been abstracted also by the lime; but in both these experiments the excess of carbonic acid gas found in the atmosphere of the plants, could have been derived only from the plants themselves. Plants, then, vegetating in con-

* Priestley on Air, vol. i. p. 51.

fined atmospheres evolve carbonic acid gas in the shade, or during the night; and the vitiated state of their atmospheres after experiment is owing to that evolution.

Process of its elaboration.

But in this alternate inhalation and extrication of carbonic acid, is any part of it assimilated to the plant? or is the quantity extricated always equal to the quantity inhaled? From the continual increase of the carbon of the vegetating plant, the assimilation of carbonic acid is unquestionable; and the supply inhaled by the leaves indispensable to vegetation. For if the carbonic acid that is evolved in the night is withdrawn from the artificial atmosphere as soon as it is formed, the leaves wither and the plant dies. Into a receiver containing atmospheric air deprived of its carbonic acid, in which a Pea had been made to vegetate, Saussure introduced a small quantity of lime, placing the receiver over lime water, and exposing the apparatus to the sun. On the second day the atmosphere of the plant had diminished in volume. On the third day the lower leaves began to fade; and on the fifth or sixth day the stem was completely stript of its leaves; the atmosphere when examined was found to be vitiated, containing only $\frac{16}{100}$ of oxygene. But there had been an absorption of carbonic acid by the lime, and consequently a formation of that acid, the component parts of which could have been derived only from the plant. The elaboration of carbonic acid gas, therefore, is essential to vegetation in the sun.

But plants which were made to vegetate at the same time in receivers filled with common air without lime had effected no change in their atmosphere either in purity or volume; this seems to contradict the necessity of the elaboration of carbonic acid. But the process was here imperceptible only because the plant again inhaled the gas in the same proportion in which it had previously evolved it, or formed it with the surrounding oxygene.

It should be added, however, that the action of lime water did not produce the same destructive effect upon the leaves of succulent plants, such as the *Cactus*, when treated as above; which, owing to their very thick parenchyma and less porous epidermis, thus seem to retain more obstinately the carbonic acid which they form.

But the result of such experiments as were conducted in the shade was very different; for so far were plants thus exposed from showing any symptoms of langour or decay when placed under receivers containing lime and lime water, that their growth was even more vigorous than that of others which were placed under receivers containing only common air. The mean augmentation of each plant in the receiver containing lime was seven grains in the space of six days; and the proportion of carbonic acid remaining after the experiment $\frac{3}{100}$; while the mean augmentation in the receiver without lime was only five grains in the same space of time, though it appeared by the application of lime water that the proportion of carbonic acid remaining after the experiment was $\frac{1}{100}$. Saussure explains the phenomenon thus: the great quantity of carbonic gas evolved by plants in the shade is prejudicial to their vegetation at least in confined atmospheres; but a partial privation of the gas thus produced is beneficial to their vegetation.*

The foregoing experiments were made upon plants vegetating in pure water; but Saussure made some experiments also on plants vegetating in the earth, by means of enclosing part of a bough in a large globe of glass. The results obtained were upon the whole similar to the former; but in the case of the decay of the leaves by their exposure to the sun and to the action of lime, the effect was produced more slowly. It should be recollected, however, that the cases are not precisely similar; for though the plants were in both cases equally deprived of the external action of carbonic acid gas upon their leaves; yet there was a supply of that gas communicated to the plant from the oil in the one case that could not have been communicated to it in the other.

By the assimilation of its carbon and evolution of its oxygene.

The elaboration of carbonic acid then in plants exposed to the sun is unquestionable: but in what state is it actually assimilated to the plant? Is it

* Sur. la Veg. chap. ii.

assimilated in the state in which it is inhaled? or is it previously decomposed? It had been observed by Ingenhoutz that the leaves of plants, if placed in water and exposed to the action of the sun's rays, will evolve a quantity of oxygene gas. It was afterwards ascertained by Senebier that this process takes place only when the leaves are fresh and the water impregnated with carbonic acid. For when the water was deprived of its carbonic acid by boiling, or in the course of experiment, there was no more oxygene evolved. But when the water was again impregnated with carbonic acid, the extrication of oxygene recommenced as before; the conclusion, therefore, is obvious and the phenomenon satisfactorily accounted for. The carbonic acid gas contained in the water is abstracted and inhaled by the leaf, and immediately decomposed; the carbon being assimilated to the substance of the plant, and the oxygene evolved.

Such was the important discovery of Senebier, affording an undoubted proof of the decomposition of carbonic acid. But the effects of that decomposition had not yet been analysed; nor was it yet ascertained whether the quantity of oxygene evolved was more or less than the quantity contained in the composition of the carbonic acid, or equal to it. The solution of the question was reserved for Saussure, who after a variety of experiments obtained the most satisfactory results.

In an artificial atmosphere composed of common air and carbonic acid gas the eudiometer indicated $\frac{21}{100}$ of oxygene; and lime water $7\frac{1}{2}$ of carbonic acid gas. Into the receiver containing this atmosphere there were introduced several plants of the *Vinca minor*; the apparatus was exposed during six days to the direct rays of the sun, from five o'clock in the morning till eleven. On the seventh day the plants were taken out; they had undergone no alteration, nor had their atmosphere sustained any perceptible change of volume. But the lime water gave no longer any indication of the presence of carbonic acid gas, and the eudiometer indicated 24·5 parts in the hundred of oxygene. The capacity of the receiver was 290 cubic inches; it contained, therefore, before the experiment 211·92 inches of nitrogene, 56·33 of oxygene, and 21·75 of carbonic acid. But after the experiment it was found to contain 218·95 cubic inches of nitrogene, and 71·05 of oxygene, which were the whole of its contents; the carbonic acid gas had disappeared. The plant then had decomposed or elaborated $21\frac{3}{4}$ cubic inches of carbonic acid gas. Now, if in the process of decomposition the whole of the oxygene had been disengaged, there would have been also a quantity of oxygene produced equal in volume to the carbonic acid that had disappeared; but the quantity of oxygene disengaged was only $14\frac{3}{4}$ cubic inches. The plants then had assimilated seven cubic inches of oxygene in decomposing the carbonic acid; and had at the same time produced seven cubic inches of nitrogene. Several plants of the same species were made to vegetate in a similar apparatus, at the same time and in the same exposure, in pure atmospheric air. Their atmosphere was not altered either in purity or volume, but their carbon instead of augmenting had rather diminished; whilst the carbon of the others, as was found by comparative analysis had augmented very considerably. Experiments of the same kind were made upon *Mentha aquatica*, *Lythrum Salicaria*, *Pinus genevensis*, and *Cactus Opuntia*; and the results were always similar, from which it follows also that plants decomposing carbonic acid gas assimilate at least a part of the oxygene which it contains.

Of which also they assimilate a part.

Such are the several results obtained from experiments, concerning the accuracy of which there can be no doubt. The decomposition of carbonic acid gas takes place only during the light of day, though Saussure has made it also probable that plants decompose a part of the carbonic acid gas which they form with the surrounding oxygene even in the dark. But of this there does not yet exist any satisfactory proof; how the light acts is not certainly known. But the effect is operated chiefly by means of the leaves and other green parts of vegetables, that is, chiefly by the parenchyma; the wood, roots, petals, and leaves that have lost their green colour not being found to exhale oxygene gas. It may be observed, however, that the green colour is not an absolutely essential character of the parts decomposing carbonic acid; because the leaves of a peculiar variety of the *Atriplex hortensis*, in which all the green parts change to red, do still exhale oxygene gas. But all leaves have not the same facility in decomposing carbonic acid; a plant of *Lythrum Salicaria* has been known to decompose in one day seven or eight times its volume, while many other plants cannot decompose the one-fifth or even the one-tenth of that quantity; though in general it may be observed that the leaves and other green parts of the plant decompose it in proportion to their surface, and not in proportion to their volume. But how is the formation of nitrogene to be accounted for, which is always found to be extricated along with the oxygene thus evolved by the leaves? The subject is yet enveloped in much obscurity, as well as the means of giving it the elucidation to be desired; but perhaps it is abstracted from the interior of the leaf through the medium of the evolved oxygene for which it is known to have an affinity.

SECTION VI.

Elaboration of Oxygene.

In treating of the utility of the gases as a vegetable food it has been already shown that the leaves of plants abstract oxygene from confined atmospheres, at least when placed in the shade, though they do not inhale all the oxygene that disappears; but it has been further proved from experiment, that the leaves of plants do also evolve a gas in the sun. This phenomenon was first observed by Bonnet, who gave indeed a wrong explanation of it; beliving it to be the extrication of the air that might have entered the plant along with the sap, or believing it to come directly from the water. His method was to expose the leaves to the sun, in an inverted glass vessel filled with water; air bubbles began immediately to disengage themselves from the surface of the leaves, and to ascend to the summit of the water.

The leaves of plants disengage a gas in the sun, as shown by Bonnet,

The next experiments on this subject are those of Priestley, who discovered that the leaves of plants in a state of vegetation have the property of ameliorating vitiated air. On the 17th of August 1771, he put a sprig of Mint into a quantity of atmospheric air in which a candle had burnt out, and found after confining it till the 27th of the same month, that the air was again ameliorated and ca-

And Priestley,

pable of supporting combustion: another candle burned in it perfectly well.* The experiment succeeded also with sprigs of Balm, and with plants of Groundsel and Spinach, and the process seemed to depend on the vegetating state of the plant; for when detached leaves only were introduced, they did not ameliorate the air, though they were yet perfectly fresh. The ameliorating of a quantity of vitiated air by means of confining a sprig of Winter Savoury in it for five or six days was ascertained also by the application of the eudiometer. Equal measures of the confined atmosphere and nitrous gas occupied a space equal only to 1·275. Hence the vitiated air was evidently ameliorated by the plant. It does not, however, appear that Priestley had yet discovered the rationale of the above amelioration, whether it was by abstraction or extrication; but he discovered some years afterwards that plants, when placed in water and exposed to the light of the sun, give out what was then called pure or dephlogisticated air. In the course of his experiments on the growth of plants in water impregnated with fixed air, he had observed air-bubbles issuing spontaneously from the stalks and roots of several plants growing in water that was not so impregnated; believing that the air thus extracted had percolated through the plant, he thought he had now discovered the clue that was to lead him infallibly to the ascertaining of the fact of the ameliora-

Who finds it to be pure or dephlogisticated air.

* On Air, vol. i. p. 50.

tion or contamination of the air of the atmosphere effected by the vegetating plant. For if this air was purer than that of the atmosphere, then it seemed to afford a proof that the phlogiston of the atmospheric air had been retained by the plant as its true food, and the pure part liberated, agreeable to the hypothesis by which he supposed phlogiston to be the pabulum of plants.

In order, therefore, to ascertain the fact he plunged into water a number of phials containing sprigs of Mint, so as that the air discharged might be retained in them, the bottoms being a little elevated. The sprigs thus placed continued to vegetate and also to evolve air, so that in the course of a few days he procured an ounce measure of it, which proved to be so pure that when mixed with equal measures of nitrous gas the mixture occupied but the space of one measure. In repeating the experiment he found that many of his phials became lined with a green vegetable matter, *Conferva minima*, which also gave out bubbles of pure air when exposed to the light of the sun, but never except in such exposure.

From the above experiments, made in the month of June 1788, Priestley inferred that the air of the atmosphere is ameliorated through the process of vegetation, and purged of the impurities with which it is loaded by the putrefaction of vegetable and animal substances, the noxious part being assimilated to the substance of the plant, and the remaining part evolved pure; so that the atmosphere even of bogs and marshes is purified, and rendered at least less insalubrious by means of the plants that grow in them, such as the *Confervæ* and Duck-meat, which last thrives, as he says, better in inflammable than even in dephlogisticated air.

Or oxygene, as proved by Ingenhoutz;

Whatever may be the legitimacy of this conclusion, upon the whole the facts from which it is drawn prove incontrovertibly that plants vegetating in the sun exhale an air purer than that of the atmosphere. But the air thus exhaled was afterwards ascertained by Ingenhoutz to be pure oxygene gas; plants then in the process of vegetation inhale oxygene gas in the shade or during the night, and exhale it in the light of the sun or during the day. But the detail and rationale of the different processes remained yet to be inquired into, as also, whether any part of the oxygene inhaled was assimilated to the plant; or whether plants evolve in the day exactly what they inhale in the night.

Which is inhaled during the night and evolved during the day.

Experiments of Saussure.

It was at first supposed that plants assimilate the whole, or at least the greater part, of the oxygene they inhale in the night; but this opinion was soon found to be erroneous, as will appear from the experiments of Saussure, whose view of the whole process of the influence of oxygene on the vegetating plant is so full and satisfactory as to leave but little unexplained; of which view the following is an abstract:—a *Cactus* of six cubic inches in volume, which had inhaled during the night four cubic inches of oxygene, was exposed on the following morning to the action of the sun's light in a receiver containing 48 cubic inches of atmospheric air deprived of its carbonic acid. In the succeeding evening its atmosphere was found to be augmented by 4·4 cubic inches, but without any accession of carbonic acid; 27¼ parts in the hundred being oxygene, as indicated by the eudiometer, and the remainder being nitrogene. Before the experiment the receiver contained 10·1 cubic inches of oxygene, and 37·9 of nitrogene; after the experiment it contained 14·28 of oxygene, and 38·1 of nitrogene. The amount of the difference, then, or the quantity of gas extricated was 4·18 cubic inches of oxygene, and 0·2 of nitrogene. The experiment was continued with the same plant during seven successive days and nights. In the course of the second night the quantity of oxygene inhaled was equal to 3¾ cubic inches; and in the course of the following day the quantity of gas evolved was equal to four cubic inches of oxygene, and ⅓ cubic inch of nitrogene. In short it was found during the seven days of experiment, that the quantity of oxygene alternately inhaled and evolved, during the night and day, was always diminishing; and the quantity of nitrogene extricated, always increasing; the quantity of oxygene inhaled upon the whole being 21¾ cubic inches, and the quantity of gas evolved upon the whole being 29½ cubic inches, of which 23¼ were oxygene and 6¼ nitrogene.

Saussure varied the experiment upon the extrication of oxygene gas, by means of immersing a plant in distilled water during the day, which had been placed under a receiver filled with atmospheric air deprived of its carbonic acid during the night. The general result was the same as in the foregoing experiment; but owing to the constrained and unnatural situation of the plant the process did not go on so rapidly, and the oxygene given out was contaminated by a considerable quantity of nitrogene.

Leaves saturated with oxygene.

When the leaves were kept constantly in the shade and in a confined atmosphere, without being at all exposed to the light of day, they continued to inhale oxygene slowly till they were saturated; when they refused to inhale any more. The quantity necessary to their saturation was about $1\frac{1}{4}$ their volume; and the time necessary to complete the process from 36 to 40 hours. But still they continued to act upon the surrounding oxygene, with which and with the carbon they contained they formed carbonic acid, consuming about $\frac{1}{8}$ the oxygene they consumed by inhalation, but not thus altering the volume of their atmosphere. But when they were again exposed to the sun, they evolved, in the space of seven or eight hours, a much greater quantity of oxygene than when they were confined in the receiver only for one night. Six cubic inches of the *Cactus Opuntia* which by remaining 36 successive hours under a receiver in the dark had inhaled $7\frac{1}{2}$ cubic inches, while in the course of one night it inhaled only four; evolved

Still continue to consume it.

during the succeeding day, when exposed to the light of the sun, $7\frac{1}{2}$ cubic inches also.

The quantity of oxygene, therefore, which is exhaled during the day is proportional and nearly equal to the quantity inhaled during the night, or during the time of the plant's confinement in the shade up to its saturation. The former quantity is generally indeed somewhat more than the latter, which Saussure regards as being probably owing to the decomposition of water in the plant. But whatever may be the true explication of this particular phenomenon, it is evident that no permanent assimilation of oxygene is effected in the alternate process of its inhalation and extrication by the leaves, so as to increase materially the quantity of dried vegetable substance.

The quantities alternately inhaled and extricated nearly equal.

But as plants vegetating in the shade and in confined atmospheres become so soon saturated with oxygene and refuse to absorb more, one might be apt to conclude that plants vegetating even in the open air, if situated in the shade, must become saturated with it also, and refuse to absorb more when placed in an artificial atmosphere. This, however, is not the fact. Saussure tried the experiment repeatedly, and found that plants confined in an artificial atmosphere, after having been exposed to the open air in the shade, always inhaled oxygene as in other cases; so that by frequently changing their exposure from the natural to the artificial atmosphere and the contrary, they were capable

Saturation occurs only in confined atmospheres.

of being made to inhale an unlimited quantity of oxygene many times the size of their own volumes.

Why? What is the cause of this apparent anomaly? Why are not leaves which are made to vegetate in the dark, saturated with oxygene in the open air as well as in confined atmospheres? and why does their alternate exposition in the receiver and in the open air give them the property of inhaling an unlimited quantity. The truth is, that the inhalation of this unlimited quantity is a mere deception, produced by the action of the atmospheric air upon the carbonic acid contained in the leaves. The air of the atmosphere has a chemical affinity for carbonic acid gas, as has been already shown upon the authority of Bertholet,* and abstracts, by consequence, a portion of it from the leaf which it thus prepares for commencing anew the process of inhalation; so that however long the alternate change of exposition may be continued, there is no accumulation of carbonic acid or of oxygene.

Inhalation dependant on the structure of the leaf. But the inhalation of oxygene seems to depend upon the structure and organization of the leaf; for Saussure found with regard to the *Cactus* what Senebier found with regard to other leaves—that when they were cut into pieces and pounded in a mortar, so as to destroy their organization, and then placed under a receiver filled with common air, no inhalation took place; though they formed carbonic acid gas, by the combination of the carbon which

* Mem. de l'Inst. Nat. tom. iii.

they contained with the oxygene of their atmosphere; the juice of the plant was coagulated. Hence the oxygene inhaled by the leaf of the vegetating plant seems also to form carbonic acid gas with the carbon which the leaf already contains; and in this state it probably remains in the parenchyma, till exposed to the action of light. There is not indeed any direct proof that this is the fact; but there is no other supposition that will explain the phenomena of the process so well.

Affinity by which oxygene is retained,

But if the oxygene inhaled by the plant is thus converted into carbonic acid, and condensed in the parenchyma, by what affinity is it retained? It cannot be extricated by placing the plant in the vacuum of an air pump. Six cubic inches of a *Cactus* which had inhaled during one night four cubic inches of oxygene, gave out in the vacuum of an air pump only one cubic inch of air containing not more than $\frac{15}{100}$ of oxygene gas. A heat, without light, sufficiently moderate not to destroy the vegetable, produced no better effect. It is retained therefore by an affinity too strong to be overcome by such means. It is overcome, however, by means of the action of the sun's light, as is demonstrated by the clearest evidence; but how the light acts is not known.

The property then which plants possess of inhaling and evolving oxygene in the night and day is analogous, and seems to be subordinate, to that by which they decompose carbonic acid. The green parts which effect the decomposition of the latter effect also the alternate inhalation and extrication of the former; which two operations seem to be the cause, the one of the other. When a leaf is put into the shade immediately after having been exposed to the light of the sun, it contains no carbonic acid gas, because that gas has been decomposed by the light; but the oxygene of the atmospheric air which now penetrates and traverses the leaves, is seized in its passage by the carbon of the plant. And hence carbonic acid gas is again formed, which loses its elasticity by its union with the water of vegetation; and which undergoes also a compression by means of the vegetable structure, bounded however by certain limits, since plants which absorb the most do not absorb more than $\frac{1}{14}$ of their volume. They are now therefore saturated, and evolve by consequence carbonic acid gas; but the action of the oxygene is in both cases the same—namely, that of forming carbonic acid with the carbon of the plant. Before saturation the carbonic acid is condensed in the plant; but after saturation it is evolved, because the plant can contain no more. Hence it follows also that leaves do not immediately assimilate the oxygene of the atmosphere which they inhale during the night, unless they then decompose part of the carbonic acid which is thus formed, and of which experiment affords no proof. There can be no conclusion drawn from the phenomena produced in the dark, in atmospheres deprived of oxygene; because

Overcome by light,

But forms carbonic acid.

in this case vegetation is altogether suspended. The plant soon discovers a tendency to putrefaction, furnishing from its own substance, and in consequence of its decomposition, the two elements of carbonic acid gas.

Conditions of successful experiment on the leaves.

The foregoing results which were obtained from experiments on *Cactus Opuntia* are equally applicable to the leaves of other plants, though not in so perceptible a degree. But the more succulent any leaf is the better it is fitted for experiment; because succulent leaves contain a great quantity of green herbaceous matter within a small volume, while the leaves of most other plants are so thin and fine, and their surface so much extended that the experiment is not only more difficult but the result less striking. According to Saussure, the following conditions are necessary in order to ensure success in experiment:—the leaves must be perfectly sound and fresh, and they must displace from about the $\frac{1}{10}$ to $\frac{1}{20}$ part of their volume of the air contained in the receiver, for if they displace less, the effect is not sufficiently perceptible, and if more, there is too little oxygene left. When the apparatus is placed in the sun, the leaves must not touch the sides of the receiver, which is then so hot as to disorganize the structure of the plant.

But although the quantity of oxygene extricated in the day is proportional to the quantity inhaled in the night, yet the specific quantity inhaled is very different in the leaves of different plants. The leaves of succulent plants consume less than most others, but they retain it also more obstinately—that is, they give out less carbonic acid—perhaps because they present fewer points of contact to the surrounding air, and are furnished with fewer pores in their epidermis; hence they lose but little of their carbon, even when vegetating in the open air, and can live for a long time under the privation of that part of their nourishment: and hence also their peculiar aptitude to the different sorts of soil in which they naturally grow—sand, clay, or the barren rock, as in the case of *Sedum, Saxifraga, Sempervivum.*

Plants inhabiting marshes consume less oxygene than other herbaceous plants, which proceeds no doubt from an institution in nature fitting them for the situation in which they vegetate, and in which they are deprived of the free access of oxygene, owing to the vapours that surround them; hence the herbaceous plants of the mountain, where the supply of oxygene is but little, are often to be found in the marshes of the plain.

The leaves of ever-greens consume also but little oxygene gas, and are consequently found to thrive in a barren soil, and in a rarefied atmosphere; as in the case of *Pinus, Juniperus,* and *Rhododendron.*

But plants which shed their leaves in the winter contain in general the most oxygene, and lose, by consequence, the most carbon; and hence they are not to be met with in such lofty situations as herbaceous plants.

The general conclusion to be drawn from these observations is, that the quantity of oxygene consumed by the leaves is relative to the situation in which the plant naturally vegetates; and that plants vegetating in a barren soil, or in a rarefied atmosphere, or in a marshy situation, consume, in the same circumstances, less oxygene than such as vegetate in a fertile soil with an abundant supply of atmospheric air.

But in saying that any quantity of oxygene was consumed, it is not meant that it was all inhaled by the plant; the greater part of it was often employed in the formation of carbonic acid gas in the atmosphere of the receiver: for it does not appear that the actual inhalation of oxygene had in any instance perceptibly exceeded the volume of the leaves. It was for the most part less.

Such then is the detail and rationale of the alternate processes of the inhalation and extrication of oxygene by the leaves of the vegetating plant. Do any of the other parts of the plant perform similar functions?

If a sound and fresh root deprived of its stem is put into a receiver filled with atmospheric air and placed over mercury, it inhales indeed a small portion of oxygene and hence diminishes the volume of its atmosphere, but it consumes, and seems also to inhale a much larger portion; while the oxygene that thus disappears is employed in the formation of carbonic acid with carbon, which it abstracts from the root. If the root is immediately removed into

On the root.

another receiver, no futher change is produced in the volume of its atmosphere; and consequently no further inhalation of oxygene. But if it is allowed to remain some time in the open air it inhales oxygene as before. But the quantity of oxygene thus inhaled is always inferior to the volume of the root. A Radish, which consumed in the space of 24 hours, a quantity of oxygene equal to its own volume, inhaled one-fourth part. But a Carrot, which consumed in the same space of time, a quantity equal to its own volume, inhaled only $\frac{1}{100}$ part. And a Potatoe, which consumed only 0·04 of its own volume, inhaled only 0·08.

This regular inhalation of a quantity less than the root, with the faculty of inhaling oxygene anew after a short exposure to the open air, seems to prove that the root does not immediately assimilate the oxygene which it inhales, but converts it into carbonic acid gas, which the atmospheric air again abstracts. Thus the action of oxygene on the root, whether in the sun or shade, resembles its action upon leaves vegetating in the shade; though in the former case, the inhalation is less perceptible. Roots therefore do not evolve oxygene at all.

But if the experiment is made upon roots to which the stem is still attached, the result is very different, at least if the root only is confined in the receiver, while the stem and leaves are left exposed to the open air. For, in this case, the root seems to inhale more than its volume of oxygene gas; though the gas thus inhaled is not yet assimilated to the vegetable, but is given out to the atmosphere by the leaves. Hence also, if an entire plant—root, stem, and branch, is introduced into the receiver, so as that the root shall be immersed in the water by the extremity only, and in contact, for the most part, with the atmosphere of the receiver, the oxygene of this atmosphere is not diminished; because the portion which is abstracted by the root is restored again by the leaves. The oxygene inhaled by the root, then, is not again evolved by it, nor immediately assimilated to the vegetable substance; but is conducted to the leaves in the state of carbonic acid, and there elaborated, or given out to the atmosphere.

If the branch of a woody plant, taken and lopped off in the spring immediately before the expansion of the buds, is inclosed in a receiver filled with common air together with a small quantity of water to supply it with nourishment, it will develope its leaves as if vegetating in the open air. But it will not effect this developement if it is placed in a receiver filled with nitrogene or hydrogene gas; in which it will, on the contrary, soon exhibit symptoms of putrefaction, by giving out a quantity of nitrogene and carbonic acid gas. The developement then, in the former case, must consequently have been effected by means of the inhalation of oxygene, which it thus appears that the stem and branches are capable of effecting, even though stripped of

On the branch.

their leaves; for they are then found to vitiate common atmospheric air, whether in the sun or shade, without changing the volume of their atmosphere; replacing the oxygene which they consume by an equal quantity of carbonic acid, and by consequence not assimilating it immediately. Branches of *Salix alba*, *Populus nigra*, and *Quercus Robur* consumed in the space of 24 hours, in the spring and summer, at 15° of Reamur, a quantity of oxygene, equal to more than half their volume; while branches of the Apple and Pear consumed, in equal circumstances, two or three times their volume.

The oxygen which the stem and branches inhale in the shade they give out again in the sun, in proportion to the quantity of green vegetable substance contained in their bark; by means of which they perhaps assimilate a small quantity of oxygene in decomposing the carbonic acid which they form with that of their atmosphere, though the effect is not perceptible.

But if a portion of the stem remains in the receiver whilst the root remains in the soil, and the leaves in the open air, then the oxygene gas which the stem consumes is not replaced by an equal quantity of carbonic acid; because the carbonic acid, after being formed, follows the course of the branch, and is decomposed by the leaves in the open air. Into a glass tube, containing 6·3 cubic inches of atmospheric air, Saussure introduced the extremity of the branch of an Apple tree, stripped of its leaves, but still attached to the stem, which he luted to the neck of the tube. The tube was then placed over mercury. Two hours after sun-set the mercury rose one and a half line within the tube, corresponding to about half the volume of the branch. On the following morning the mercury was found to have descended a little; and about two hours after the rising of the sun it was as at the commencement of the experiment. The air contained $\frac{3}{100}$ of carbonic acid, and $\frac{13}{100}$ of oxygene. The branch had then consumed in the space of 24 hours more than five times its volume of oxygene, which it had replaced by scarcely three times its volume of carbonic acid. Now the disproportion between the quantity of oxygene and carbonic acid remaining, was evidently owing to the circumstances of the latter's being carried off to the leaves by means of the branch.—But there was formed also in the tube a very considerable quantity of nitrogene, for the volume of its contained atmosphere was not changed, which Saussure accounts for as follows:—As the branch inhales the oxygene of its atmosphere a vacuum is formed in the tube, in consequence of which the external atmospheric air penetrates the porous substance of the branch, and insinuates itself into the atmosphere within; the oxygene of this new atmosphere is absorbed by the branch, and its nitrogene left behind: and hence the proportion of nitrogene in the atmosphere of the tube is of necessity augmented.

If this experiment is made with water instead of atmospheric air, and the water placed over mercury, then the phenomenon occurs by which Hales was deceived into the opinion that plants inhale air during the day and give it out during the night. Whilst the water is rapidly absorbed by the branch, owing to the more copious perspiration of the leaves during the day, the air is yet prevented from rushing in by means of this very perspiration, although a vacuum may be formed in the tube. The mercury then ascends. But at night when the perspiration and consequent absorption is but small, there is no cause to counteract the intrusion of the atmospheric air; the mercury again descends, and gives rise to the apparently alternate inhalation and extrication of air by day and by night, as understood by Hales. But it is thus obvious, that the phenomenon is to be ascribed to the nature of the apparatus.

On the flower.

The action and influence of oxygene are equally conspicuous in the developement of the flower as in the other parts of the plant. The flower-bud will not expand if confined in an atmosphere of pure nitrogene, and will fade much sooner than in an atmosphere of common air. But in a confined atmosphere of common air, if placed in the shade, although it does not alter the volume of its atmosphere, at least in a perceptible degree, yet it replaces the oxygene it absorbs by nearly an equal quantity of nitrogene; and in this respect the flower differs from the other parts of vegetables, which when vegetating in the dark give out but little nitrogene, and consequently diminish for the most part the volume of their atmosphere. Some flowers of *Lilium album,* which were introduced into a receiver filled with common air, and placed over mercury in the shade, consumed in the space of 24 hours a quantity of oxygene equal to 1·1 of their own volume, of which they inhaled 0·15, replacing it by 0·15 of nitrogene. In a similar experiment on a Rose it was found to have consumed 1·8 of its own volume, inhaling 0·43 parts of oxygene, and giving out at the same time 0·43 parts of nitrogene. There seems then to be some ground for the commonly received opinion of the unwholesomeness of sleeping in an apartment which may happen to contain a great many flowers; for the nitrogene which they give out will no doubt have some effect upon the atmosphere of the apartment, if there is not a free circulation of air in it, though the consequences said to result from this circumstance have certainly been much exaggerated.

On the fruit.

The action and influence of oxygene are in like manner essential to the maturity of the fruit. Saussure introduced a bunch of grapes, not yet ripe, into a globe of glass, which he luted by its orifice to the bough and exposed to the rays of the sun; the bunch ripened without having effected any material alteration in its atmosphere, except that it contained rather more oxygene than at first. But when a bunch was placed in the same circumstances, with the addition of a quantity of lime, the atmosphere was contaminated, and the grapes did not ripen: hence we may infer, that the elaboration of oxygene is necessary to the maturity of the fruit.

General results.

From the whole then of the foregoing experiment, as relative to the action and influence of oxygene on the plant, and the contrary, the following is the sum of the results.

The green parts of plants, but especially the leaves, when exposed in atmospheric air to the successive influence of the light and shade, inhale and evolve alternately a portion of oxygene gas mixed with carbonic acid. But the oygene is not immediately assimilated to the vegetable substance; it is first converted into carbonic acid by means of combining with the carbon of the plant, which withers if this process is prevented by the application of lime or potass. The leaves of aquatics, succulent plants, and ever-greens consume, in equal circumstances, less oxygene than the leaves of other plants.

The roots, wood, and petals, and in short all parts not green, with the exception of some coloured leaves, do not effect the successive and alternate inhalation and extrication of oxygene; they inhale it indeed, though they do not again give it out, or assimilate it immediately, but convey it under the form of carbonic acid to the leaves, where it is decomposed.

Oxygene is indeed assimilated to the plant, but not directly, and only by means of the decomposition of carbonic acid; when part of it, though in a very small proportion, is retained also and assimilated along with the carbon.

Hence the most obvious influence of oxygene, as applied to the leaves, is that of forming carbonic acid gas, and thus presenting to the plant elements which it may assimilate; and perhaps the carbon of the extractive juices absorbed even by the root is not assimilated to the plant till it is converted by means of oxygene into carbonic acid.

But as an atmosphere composed of nitrogene and carbonic acid gas only is not favourable to vegetation, it is probable that oxygene performs also some other function beyond that of merely presenting to the plant, under the modification of carbonic acid, elements which it may assimilate. It may effect also the disengagement of caloric by its union with the carbon of the vegetable, which is the necessary result of such union.

Influence of oxygene on soil.

But oxygene is also beneficial to the plant from its action on the soil; for when the extractive juices contained in the soil have become exhausted, the oxygene of the atmosphere, by penetrating into the earth and abstracting from it a portion of its carbon, forms a new extract to replace the first. Hence we may account for a number of facts observed by the earlier phytologists, but not well explained. Du Hamel remarked that the lateral roots of plants are always the more vigorous the nearer they are to the surface, ;* but it now appears that they are the

* Phys. des Arbres, liv. i. chap. v.

most vigorous at the surface because they have there the easiest access to the oxygene of the atmosphere, or to the extract which it may form. It was observed also by the same phytologist that perpendicular roots do not thrive so well, other circumstances being the same, in a stiff and wet soil as in a friable and dry soil; while plants with slender and divided roots thrive equally well in both: but this is no doubt owing to the obstacles that present themselves to the passage of the oxygene in the former case, on account of the greater depth and smaller surface of the root. It was further observed, that roots which penetrate into dung or into pipes conducting water divide into immense numbers of fibres, and form what is called the fox-tail root; but it is because they cannot continue to vegetate, except by increasing their points of contact, with the small quantity of oxygene found in such mediums. Lastly, it was observed that plants whose roots are suddenly overflowed with water remaining afterwards stagnant, suffer sooner than if the accident had happened by means of a continued current. It is because in the former case the oxygene contained in the water is soon exhausted, while in the latter it is not exhausted at all.

And hence also we may account for the phenomenon exhibited by plants vegetating in distilled water under a receiver filled with atmospheric air, which having no proper soil to supply the root with nourishment, effect the developement of their parts only at the expence of their own proper substance; the interior of the stem, or a portion of the root, or the lower leaves decaying and giving up their extractive juices to the other parts.

Concluding remarks.

Thus it appears that oxygene gas, or that constituent part of the atmospheric air which has been found to be indispensable to the life of animals is also indispensable to the life of vegetables; on both which accounts it seems to have well merited the appellation of *vital air*, by which it was at one time designated. But although the presence and action of oxygene is absolutely necessary to the process of vegetation, plants do not thrive so well in an atmosphere of pure oxygene, as in an atmosphere of pure or common air. This was proved by an experiment of Saussure's, who having introduced some plants of *Pisum sativum* that were but just issuing from the seed into a receiver containing pure oxygene gas, found that in the space of six days they had acquired only half the weight of such as were introduced at the same time into a receiver containing common air. From whence it follows that oxygene, though the principal agent in the process of vegetation is not yet the only agent necessary to the health and growth of the plant, and that the proportion of the constituent parts of the atmospheric air is just what it ought to be, as well for the purposes of vegetable as of animal life; being at once an indication both of the wisdom and goodness of Him by whom it was established.

SECTION VII.

Decomposition of Water.

Inferred by Senebier,

ALTHOUGH the opinion was proved to be groundless, by which water had been supposed to be convertible into all the different ingredients entering into the composition of the vegetable substance by means of the action of the vital energy of the plant; yet when water was ultimately proved to be a chemical compound, it was by no means absurd to suppose that plants may possess the power of decomposing part, at least, of what they absorb by the root, and thus acquire the hydrogene as well as a portion of the oxygene which, by analysis, they are found to contain. This opinion was accordingly pretty generally adopted, but was not yet proved by any direct experiment. Senebier pointed out several phenomena from which he thought it was to be inferred, but particularly that of the germination of some seeds moistened merely with water, and so situated as to have no apparent contact with oxygene. But to this it was objected by Saussure that the seeds in question might have germinated in consequence of the action of the air contained in the water, independent of that of its component principles.

And Ingenhoutz.

The decomposition of water was inferred also by Ingenhoutz, from the amelioration of an atmosphere of common air into which he had introduced some succulent plants vegetating in pure water; but the degree of amelioration is not stated; and on this account Saussure is of opinion that no conclusion should be founded on the fact, as he had never observed any example in which a plant deprived of the contact of carbonic acid had augmented the quantity of oxygene contained in its atmosphere by a quantity exceeding that of its own volume, which he regards as being too little to establish the above conclusion.

From insufficient data.

It was next ascertained that plants vegetating in pure water augment their weight, at least in a green state, even though confined in an atmosphere of oxygene, or of common air deprived of its carbonic acid. This was thought to be a fact of great importance, but it does not yet prove the decomposition of water by the plant, nor the fixation of its oxygene, or hydrogene; because the augmentation in weight may have been occasioned by the mere introduction of the water into the sap vessels, or cellular tissue: and hence the question can be determined only by the evidence of the augmentation of the solid substance of the vegetable in a dried state.

Experiments of Saussure,

The first experiments that were instituted with a view to this object are those of Saussure; his method was as follows:—Having gathered a number of plants of the same species, as nearly alike as possible in all circumstances likely to be affected by the experiment, he dried part of them to the temperature of the atmosphere, and ascertained their weight; the rest he made to vegetate in pure water, and in an atmosphere of pure oxygene, for a given period of time, at the end of which he dried them as before, and ascertained their weight also, which it was thus only necessary to compare with the weight of the former in order to know whether the plants had increased in solid vegetable substance or not. But after many experiments on a variety of plants, the result always was, that plants when made to vegetate in pure water only, and in an atmosphere of pure oxygene, or of common air deprived of its carbonic acid, scarcely added any thing at all to their weight in a dried state; or if they did, the quantity was too small to be appreciated. Particularly he made the experiment on three plants of the *Lysimachia vulgaris*, which he introduced into a receiver containing 250 cubic inches of common air deprived of its carbonic acid, the roots were immersed in about one cubic inch of distilled water, and the plants weighed in their green state 129¼ grains, displacing half a cubic inch of their atmosphere; three other plants of the same species and weight when green, were found to weigh when dried to a certain degree of the thermometer and hygrometer 38½ grains: at the end of eight days the plants which had been confined in the receiver were taken out; they had increased considerably in length and were in a perfectly sound state, but had made no perceptible change upon the atmosphere in which they vegetated either in purity or volume. They now weighed in their green state 141 grains, but when dried to the proper degree 40½ grains: they had thus augmented their solid vegetable substance by somewhat more than two grains, which could have been acquired only by the assimilation of the oxygene and hydrogene of the water, which they had consequently decomposed. But when the experiment was prolonged to double or treble the time, the weight of the dried vegetable substance of the plants was not farther augmented; for which reason, added to that of the small amount of their augmentation, Saussure did not regard the proof from these experiments as being altogether complete, and began to suspect that the oxygene and hydrogene of the plant cannot, perhaps, be assimilated by the plant in any considerable degree, unless the augmentation of its carbon is effected in the same proportion.

The next thing to be done, therefore, was to place his plants in a mixture of common air and carbonic acid gas, that they might have the privilege of assimilating carbon at the same time; the results were now more perceptible and more decided, the solid vegetable substance of the plant was evidently increased in a greater proportion than could have arisen from the mere presence of carbonic acid. Seven plants of the *Vinca minor*, vegetating in pure water in a receiver filled with common air and carbonic acid gas, assimilated in the space of six days the carbon contained in 21¾ cubic inches, or a quantity equal to 4·2 grains; they assimilated at the same time seven cubic inches of oxygene, but as that was replaced by an equal quantity of nitrogene, it goes for nothing in the weight of the plants.

Before the experiment they weighed in their green state 168¾ grains, which were ascertained to be equal to 51 grains of dried vegetable matter; but after the experiment the quantity of dried vegetable matter was equal to 61 grains. There was consequently an augmentation of weight of 10 grains, of which 4·2 only can be attributed to the formation of carbon; hence it follows most evidently, that there had been a decomposition of the water, and an assimilation of its component parts, by means of which 5·8 grains were added to the weight of the plant.

Who infers also the decomposition of water.

The decomposition then, and fixation of water by the vegetating plant is thus, according to Saussure, legitimately inferred; but it does not appear that plants do in any case decompose water directly—that is, by appropriating its hydrogene and at the same time disengaging its oxygene in the form of gas, which is extricated only by the decomposition of carbonic acid. Plants vegetating in nitrogene gas and exposed to the alternate influence of night and day, do, indeed, extricate a quantity of oxygene equal to many times their volume; but this is because being deprived of the contact of that gas in the first period of experiment, they form of their own substance a supply of carbonic acid gas, which they afterwards decompose: and hence the origin of the oxygene found in their atmosphere. They do not, however, by a similar exposure augment the volume of oxygene when confined in an atmosphere of oxygene gas, or of common air deprived of its carbonic acid, because the carbonic acid which they now form is the result of the combination of their carbon only with the oxygene that surrounds them, and not the entire produce of their own substance: hence it is only in such atmospheres that the experimenter can form any judgment concerning the direct decomposition of water by the plant.

Succulent plants form indeed an exception with regard to the augmentation of the volume of oxygene when vegetating in an atmosphere of common air deprived of its carbonic acid, but afford no proof of the direct decomposition of water. Into a receiver containing $42\frac{3}{4}$ cubic inches of atmospheric air previously deprived of its carbonic acid, a leaf or articulation of the *Cactus Opuntia* was introduced, so as that part of it was immersed in a glass containing distilled water, which was to serve as its nourishment; at the end of 31 days, when the experiment was stopped, the leaf was still sound and vigorous; it had even formed roots of an inch in length, and augmented its atmosphere $3\frac{1}{2}$ cubic inches. The eudiometer indicated the presence of 25 parts in the hundred of oxygene, and the application of lime water showed that it contained no carbonic acid; hence the leaf had extricated in the space of a month $3\frac{1}{2}$ times its volume of oxygene, which could be attributed to no other cause but that of the decomposition of water. Still, however, there is no proof that this decomposition was direct; but rather there is reason to suppose that the leaf formed in the sun carbonic acid gas from its own substance, and that the oxygene was extricated by the decomposition of this gas: for in the progress of a similar experiment, when a vessel filled with potass was suspended in the receiver, the formation of oxygene was stopped; it is plain, therefore, that carbonic acid gas was forming, and that the oxygene which appeared was produced from its decomposition.

SECTION VIII.

Descent of the Proper Juice.

Its analogy to the blood of animals,

When the sap has been duly elaborated in the leaf by means of the several processes that have just been described, it now assumes the appellation of the *Cambium*, or Proper Juice of the plant. In this ultimate state of elaboration it is found chiefly in the bark, or rather between the bark and wood, and may very often be distinguished by a peculiar colour, being sometimes white, as in the several species of Spurge, and sometimes yellow, as in Celandine. It is said to be the principal seat of the medical virtues of plants; and was regarded by Malpighi as being to the plant what the blood is to the animal body—the immediate principle of nourishment, and grand support of life; which opinions he endeavours to establish by the following analogies :—if the blood escapes from the vessels of the animal body, it forms neither flesh nor bone, but tumors; if the proper juices of the plant are extravasated, they form neither bark nor wood, but a lump of gum, resin, or inspissated juice. The disruption of the blood vessels and consequent loss of blood, injures and often proves fatal to the animal. The extravasation of the proper juice injures and often proves fatal to vegetables, unless the evil is prevented by the skill and management of the gardener. Whatever may be the value of these remarks as tending to establish the analogy in question, it cannot be doubted that the *cambium* or proper juice constitutes at least the grand principle of vegetable organization; generating and developing in succession the several organs of the plant, or furnishing the vital principle with the immediate materials of assimilation.

And distribution throughout the plant.

But how is the proper juice, which is thus so indispensable to the process of vegetation, conveyed to the several parts or organs of the plant? As the sap in its ascent to the summit of the leaf is conducted by an appropriate set of vessels, so also is the proper juice in its descent to the extremity of the root. One of the earliest and most satisfactory experiments on this subject, at least as far as regards the return of the proper juice through the leaf and leaf-stalk, is that of Dr. Darwin, which was conducted as follows :—A stalk of the *Euphorbia helis-*

copia, furnished with its leaves and seed-vessels, was placed in a decoction of Madder-root, so as that the lower portion of the stem and two of the inferior leaves were immersed in it. After remaining so for several days the colour of the decoction was distinctly discerned passing along the midrib of each leaf. On the upper side of the leaf many of the ramifications, going from the midrib towards the circumference, were observed to be tinged with red; but on the under side there was observed a system of branching vessels, originated in the extremities of the leaf and carrying not a red but a pale milky fluid, which after uniting in two sets, one on each side the midrib, descended along with it into the leaf-stalk. These were the vessels returning the elaborated sap.* The vessels observable on the upper surface Darwin calls arteries, and those on the under surface he calls veins; the propriety of which appellations is questionable, though the discovery of the different sets of vessels conducting the sap and proper juiceis important; because it points out the intention of the peculiar structure of the leaf as discoverable in the skeleton, which has been already described as consisting of two, or, as in the case of the Orange-leaf, of three layers of net-work.

Channel of its conveyance through the leaf and leaf-stalk, according to Darwin.

According to Mr. Knight.

To this may be added the more recent discoveries of Mr. Knight who in his experiments, instituted with a view to ascertain the course of the sap, detected in the leaf-stalk, not only the vessels which

* Phytologia, sect. iv.

he calls central tubes through which the coloured infusion ascended, together with their appendages, the spiral tubes; but also another set of vessels surrounding the central tubes, which he distinguishes by the appellation of external tubes, and which appeared to be conveying in one direction or other a fluid that was not coloured, but that proved upon further investigation to be the descending proper juice. In tracing them upwards they were found to extend to the summit of the leaf; and in tracing them downwards they were found to extend to the base of the leaf-stalk, and to penetrate even into the inner bark. According to Mr. Knight, then, there are three sets of vessels in leaves, the central tubes, the spiral tubes, and the external tubes. And yet Sir J. E. Smith* represents him as meaning to speak of two sets of vessels only, admitting that his language seems to imply three, but cautioning the reader against falling into the mistake.

But whatever Mr. Knight's meaning may have been, he positively speaks of and specifies three distinct sets of vessels, the central, the spiral, and the external, as is plain from the circumstance of his trying to ascertain the respective functions of each. The first, he says, conducts the ascending sap from the tubes of the alburnum to the leaf-stalk and leaf; the second does not seem to conduct any fluid; the third contains the proper juice and con-

* Introduction, p. 51.

ducts it in its descent from the summit of the leaf down to the base of the foot-stalk.*

Channel of its conveyance through the stem.

But by what means is the proper juice conducted from the base of the leaf-stalk to the extremity of the root? This was the chief object of the inquiry of the earlier phytologists who had not yet begun to trace its progress in the leaf and leaf-stalk; but who were acquainted with facts indicating at least the descent of a fluid in the trunk. If the stem, or branch, or even root of a woody plant is encircled by a strong ligature, a tumor is formed immediately above the ligature, but no tumor is formed below it.† Hence they inferred the descent of a fluid that was now stopped; but this descending fluid was proved also to be the *Cambium* or proper juice. If a branch of any tree abounding in a conspicuous proper juice, such as the Fig or Fir-tree, is cut transversely in two, the proper juice will flow much more copiously from the upper portion next the leaves than from the under portion next the trunk, even though their positions should be inverted. If trees are stripped wholly of their bark, they will often form new productions from the leaf downwards, but none or scarcely any from the root upwards. Du Hamel stript 60 trees of their bark in the course of the spring, laying them bare from the upper extremity of the sap and branches to the root; the experiment proved indeed fatal to them, as they all died in the course of three or four years.

* Phil. Trans. 1806.

† Phys. des Arb. liv. v. chap. ii.

But many of them had made new productions both of wood and bark from the buds downwards, extending in some cases to the length of a foot; though very few of them had made any new productions from the root upwards. Hence it is that the proper juice not only descends from the extremity of the leaf to the extremity of the root, but generates also in its descent new and additional parts.

But although the above experiments prove in general the descent of the proper juice, yet they do not decide in particular by what peculiar channel it descends—that is, whether by the bark or wood. It was the opinion of Du Hamel that it descends through the channel of the bark, in favour of which there is indeed an original presumption in the fact of its being always found in the greatest abundance in the bark when analysed; or of its flowing the most copiously from it when cut; as well as a direct and positive proof in the result of the following experiments:—In the time of the flowing of the sap Du Hamel stripped the trunk of a Cherry-tree of a ring of bark, and covered the wound with a piece of canvass to let nothing escape, wrapping it up at the same time with an additional covering of straw to prevent its becoming dry: the result was that the upper lip yielded a most copious exudation of gum, while the lower lip yielded none; but the tree did not long survive the experiment. The proper juice then descends through the channel of the bark, and cannot be made to descend through the medium

According to Du Hamel.

of any other channel. But although such experiments as the foregoing do generally prove fatal to the life of the plant on account of the interruption of the channel of the descent of the proper juice, and consequent privation of nutriment; yet there are some plants to which the experiment even communicates a temporary and preternatural fertility. If a ring of bark is detached in the spring from the trunk of an Olive-tree, it will produce that year a double quantity both of blossoms and of fruit, though it will soon afterwards die;* but the phenomenon is easily accounted for. The preternatural fertility of the plant is owing to the unusual accumulation of proper juice in the leaves and branches, in consequence of the interruption of the descent of the proper juice; and the subsequent death of the plant is owing to the privation of nutriment sustained by the root, in consequence of the same cause.

According to Hales.

But Hales did not admit the bark to be the channel of the descent of the proper juice, alleging in support of his objections the evidence of the following experiment:—Having stripped a trunk of its bark, so as to leave a number of insulated rings still remaining, of which some were furnished with buds, and some not, the trunk still lived, and the buds protruded both leaves and branches; the lower lips of such rings as were furnished with buds producing tumors, and the lips of such as were without buds

* La Nature Devoilée.

producing none. Hence he inferred that the bark cannot be the channel of the descent of the sap or proper juice, since the plant still lived in spite even of the abstraction of several circular portions. But if some plants are so very tenacious of life as to survive even the violent operation of girdling, it will not appear surprising if some plants should survive also the gentler operation of a partial barking. For in this case the proper juice may find a partial and temporary channel even in the alburnum where it is naturally of a very loose texture, and not too much indurated by exposure to air; and as the sap ascends, at any rate by the alburnum, there is no difficulty in accounting for the developement of the buds in the above experiment. For wherever a bud is formed the ascending juice will find its way to it, from which the elaborated sap or proper juice will again descend by the bark, its natural channel, at least till it meets with some interruption; where it will form tumors as in the above case. And if no tumors were formed on the lower lips of the rings without buds, it is because there was no particular determination of sap towards such rings on the very account of their want of buds, and consequently no room for the process of elaboration and return of proper juice. This was accordingly Du Hamel's reply, and subsequent experiment has shown it to be correct; for the experiments of Mr. Knight on this subject are, if possible, more convincing than even those of Du Hamel. From the trunks of a number of young Crab-trees

According to Knight.

Mr. Knight detached a ring of bark of half an inch in breadth. The sap rose in them, and the portion of the trunk above the ring augmented as in other subjects that were not so treated, while the portion below the ring scarcely augmented at all. The upper lips of the wounds made considerable advances downwards, while the lower lips made scarcely any advances upwards; but if a bud was protruded under the ring, and the shoot arising from it allowed to remain, then the portion of the trunk below that bud began immediately to augment in size, while the portion between the bud and incision remained nearly as before. When two circular incisions were made in the trunk so as to leave a ring of bark between them with a leaf growing from it, the portion above the leaf died, while the portion below the leaf lived; and when the upper part of a branch was stripped of its leaves the bark withered as far as it was stript. Whence it is evident that the sap which has been elaborated in the leaves and converted into proper juice, descends as air through the channel of the bark, or rather between the bark and alburnum to the extremity of the root, effecting the developement of new and additional parts.*

Vessels conducting it.

But not only is the bark thus ascertained to be the channel of the descent of the proper juice, after entering the trunk; the peculiar vessels through which it immediately passes, have been ascertained

* Phil. Trans. 1803.

also. In the language of Mr. Knight they are merely a continuation of the external tubes already noticed, which after quitting the base of the foot-stalk he describes as not only penetrating the inner bark, but descending along with it and conducting the proper juice to the very extremity of the root. In the language of M. Mirbel they are the large or rather simple tubes so abundant in the bark of woody plants, though not altogether confined to it; and so well adapted by the width of their diameter to afford a passage to the proper juice.

SECTION IX.

Causes of Descent.

THE proper juice then, or sap elaborated in the leaf, descends by the returning vessels of the leaf-stalk, and by the longitudinal vessels of the inner bark, the large tubes of Mirbel and external tubes of Knight, down to the extremity of the root. What is the cause of its descent? It appears that the descent of the proper juice was regarded by the earlier phytologists as resulting from the agency of gravitation, owing perhaps more to the readiness with which the conjecture suggests itself than to the satisfaction which it gives. But the insufficiency of this cause was clearly pointed out by Du Hamel, who observed in his experiments with ligatures that the tumor was always formed on the side next to

According to the earlier phytologists.

the leaves, even when the branch was bent down whether by nature or art, so as to point to the earth,* in which case the power propelling the proper juice is acting not only in opposition to that of gravitation, but with such force as to overcome it. This is an unanswerable argument; and yet it seems to have been altogether overlooked, or at least undervalued in its importance by Mr. Knight, who has more recently investigated the subject of the descent of the proper juice; but without having been able to offer any thing that can be at all regarded as satisfactory. He endeavours, however, to account for the effect by ascribing it to the joint operation of the four following causes:—gravitation, capillary attraction, the waving motion of of the tree, and the structure of the conducting vessels; but, like charity among the virtues, the greatest of these is gravitation.

According to Knight.

Gravitation.

Gravitation.—A vertical shoot of a Vine was forcibly bent down in nearly a perpendicular direction, and its succulent extremity introduced into a pot as a layer, without wounding the stem or depriving it of any of its leaves. Two circular incisions were made in the bark of the inverted part and the intervening portion of bark was stript off. But there was more wood formed at the lip now uppermost, than at the lip opposite to it; which in the opinion of Mr. Knight was owing to the force of gravitation, since the result would have

* Phys. des Arb. liv. iv. chap. v.

been quite the contrary if the shoot had been allowed to remain in its natural position. This conclusion seems at first sight to be plausible enough; but was afterwards acknowledged even by Mr. Knight himself to be rather too hastily drawn, as it occurred to him upon further reflection that the proper juice which in other cases would have generated the greatest quantity of wood at the lip now undermost, was, in the present case, employed in the formation of roots. But although the argument drawn from the above fact is thus rendered invalid, the opinion of the efficacy of gravitation is by no means given up by Mr. Knight, as is evident from a subsequent attempt to account for the descent of the radicle upon the same principle as stated in a foregoing chapter.

Capillary attraction.—This though enumerated by Mr. Knight as a cause of the descent of the proper juice, is not much insisted on. And indeed it is plain from the known laws by which this species of attraction is regulated, that it could be but of little avail in operating the alleged effect; if indeed, it is at all applicable to the case in question. Capillary attraction.

The waving motion of the plant.—Part of the stem of a number of young seedling Apple-trees was bound by means of stakes and bandages of hay, so as to prevent all motion from the action of winds to the height of three feet. The upper part of the stem which was kept in motion by the winds Waving motion of the plant.

increased in size very considerably; but the under part which remained motionless increased very little. A plant which was compelled to move in a circle was found to have the greatest diameter of its trunk in the direction of its motion. Hence Mr. Knight inferred that the motion communicated to plants by the action of the winds facilitates the descent of the proper juice and consequently growth of the plant; and that plants deprived of that motion do not thrive so well, as in the case of trees nailed to walls. It seems probable, however, that the small degree of augmentation in the under part of the stem of the seedling Apple-trees was as much owing to its exclusion from air and light, as to its want of motion; as in the case of what is termed the etiolation of plants, exemplified in the slender stem of potatoes that may happen to protrude their shoots in cellars or other dark places. And trees nailed to walls are often as healthy as standards, particularly Vines.

To the waving motion of the plant Mr. Knight also attributes the facility with which plants adapt themselves to the habitats in which they grow. In lofty and exposed habitats they will increase most near the root, owing to the accelerated descent of the proper juice; they will send out many lateral branches and diminish gradually towards the top; they will be low and sturdy. In groves and vallies where they are crowded and less exposed they will extend in length without extending proportionally in breadth, owing to the retarded descent of the proper juice from want of motion. But this remark though founded on the general aspect of plants affecting the habitats in question is not without its exceptions. For the Pine though inhabiting the most lofty mountains is still a lofty tree; and the Oak though inhabiting the recesses of the grove may still, even in respect of diameter, be the monarch of the wood. In aged subjects the descent of the proper juice, or at least the augmentation of the plant is sometimes promoted by means of paring off the lifeless part of the bark, owing, as Mr. Knight thinks to the increased pliancy or flexibility which is thus communicated to the stem; and the excision of any decayed part is always of benefit to the plant. But the operations in question will communicate but little flexibility to a stiff and aged trunk where the mass of wood has become already firm and indurated; so that the good effect produced is perhaps more properly ascribed to the facility which is thus given merely to the access of air.

Structure of the vessels.—Mr. Knight is of opinion that the vessels of the bark are better fitted by their conformation to convey the proper juice towards the root than in any other direction, which opinion he founds upon the following experiment:—Four strong shoots of a Vine were selected and converted into layers; and were in the end of the autumn disengaged from their parent stock, at the

Structure of the vessels.

distance of five inches from the layer corresponding to the length of wood left on the opposite side. The buds on each end were by previous management made to stand at equal distances from the root, and an inch of wood was left at each end beyond the buds. If Mr. Knight's hypothesis was true it was to be expected that the proper juice would be impelled less forcibly towards the extremities that had originally formed the summit of the shoots than towards the opposite extremities, beyond which it was presumed that new wood might even be formed. The result was as follows: —At the proper and natural extremities the wood above the buds became dry and lifeless, while the wood below increased as usual. But at the inverted extremities the result was also inverted, new wood being accumulated above the buds, and even numerous roots protruded, while no sensible augmentation took place between them. Inverted cuttings of Gooseberries and Currants were also made the subject of experiment; the former not succeeding at all; but the latter succeeding for the most part, and exhibiting phenomena similar to that of the inverted ends of the layers, with the exception of their not emitting roots. Whence Mr. Knight concludes that the vessels of the bark through which the proper juice descends are better adapted by their structure to transmit their contents towards the original root, than in any other direction; by which means the motion of the returning fluid in the pendant branches of the Weeping Willow, and other such plants, is enabled to counteract the power of gravitation, though not wholly to destroy it; as its agency is still discernible in the pliancy and feebleness of the pendant shoot, and in the occasional success of inverted cuttings.

This is certainly as complete a jumble as could well have been made if it had even been attempted on purpose; and a notable example of the unphilosophical practice of multiplying causes without necessity. First we are told of the paramount influence of gravitation, and then we are introduced to a cause which is capable of counteracting it, if the branch happens to be pendant. And what is this transcendant cause that overcomes even the agency of gravitation? It is the supposed existence of valves in the tubes of the bark; an opinion entertained with regard to the sap vessels, at least by some of the earlier phytologists, but long ago exploded, as has been already seen upon the almost uniform evidence of the success of inverted cuttings of the Willow and Poplar. But the same arguments are applicable to the tubes of the bark, in which if valves even existed they could seldom be of any use, as the motion of the proper juice is almost always downwards. Would not Mr. Knight have found a cause better calculated to account for the phenomenon of the inverted layer in the force of habit, on which he lays so much stress, in a variety of other cases? as for example in that of the susceptibility of the plant to the action of heat as necessary to the protrusion of the bud, according to the temperature in which it has formerly been kept. And should it not have been also recollected that the phenomenon might have been owing merely to the difficulty attending the admission of the proper juice into any vessels capable of conveying it in the inverted direction? there being no set of vessels leading from the bud directly upwards, as there are vessels leading from it directly downwards, in the original position of the shoot. So that the entrance of the proper juice could be effected only by lateral communication, which in this case the structure of the vessels may very possibly not admit of.

Insufficient to account for the fact.

Such are the causes assigned by Mr. Knight for the descent of the proper juice. They are each perhaps of some efficacy; and yet even when taken altogether they are not adequate to the production of the effect. The greatest stress is laid upon gravitation; but its agency is obviously over-rated, as is evident from the case of the pendant shoot; and if gravitation is so very efficacious in facilitating the descent of the proper juice, how comes its influence to be suspended in the case of the ascending sap. The action of the *Silver grain* will scarcely be sufficient to overcome it; and if it should be said that the sap ascends through the tubes of the alburnum by means of the agency of the vital principle, why may not the same vital principle conduct also the proper juice through the returning vessels of the bark. In short if, with Saussure, we admit the existence of a contracting power in the former case sufficient to propel the sap from ring to ring, it will be absolutely necessary to admit it also inthe latter. Thus we assign a cause adequate to the production of the effect, and avoid at the same time the transgression of that most fundamental principle of all sound philosophy which forbids us to multiply causes without necessity.

CHAPTER IV.

PROCESS OF VEGETABLE DEVELOPEMENT.

Different in different classes of plants.

When the sap has been elaborated in the leaf, and converted into proper juice, it is now finally prepared for immediate assimilation, and for the production of such parts and organs as are peculiar to the species, or necessary to the perfection of the individual. The next object of our inquiry, therefore, will be that of tracing out the order of the developement of the several parts, together with the peculiar mode of operation adopted by the vital principle. But this mode of operation is not exactly the same in herbaceous and annual plants, as in woody and perennial plants. In the former, the process of developement comprises as it were but one act of the vital principle, the parts being all unfolded in immediate succession and without any perceptible interruption till the plant is complete. In the latter, the process is carried on by gradual and definite stages easily cognizable to the senses, commencing with the approach of spring, and terminating with the approach of winter; during which, the functions of the vital principle seem to be altogether suspended, till it is aroused again into action by the warmth of the succeeding spring. The illustration of the latter, however, involves also that of the former; because the growth of the first year exemplifies at the same time the growth of annuals, while the growth of succeeding years exemplifies whatever is peculiar to perennials.

SECTION I.

Elementary Organs.

If the embryo, on its escape from the seed and conversion into a plant, is taken and minutely inspected, it will be found to consist of a root, plumelet, and incipient stem, which have been developed in consecutive order; and if the plant is taken and dissected at this period of its growth it will be found to be composed merely of an epidermis enveloping a soft and pulpy substance, that forms the mass of the individual; or it may be furnished also with a central and longitudinal fibre; or with bundles of longitudinal fibres giving tenacity to the whole. These parts have been developed no doubt by means of the agency of the vital principle operating on the proper juice; but what have been the several steps of operation?

Formed out of the proper juice.

Some phytologists have attempted to account for the formation of the above parts by supposing the proper juice to consist of multitudes of organic fibres, which being united together by the vegetable gluten constitute the cellular and tubular tissue, and thus form the mass of the plant. But this supposition leaves us just where we were before. For if it were even proved to be the fact, the next question would be, how are the organic fibres themselves formed? But as it is an assumption founded on no proof, it merits of course no further consideration. Perhaps no satisfactory explication of the phenomenon has yet been offered, though M. Mirbel, in the want of all plausible conjecture, submits the following:—He supposes the proper juice to be at first converted into a fine membrane, which he calls the membranous tissue, from which the cellular tissue of the pulp is afterwards formed, by means of the foldings and doublings of the original membrane, so as to present an hexagonal appearance similar to that of the cells of the Bee. The tubular tissue he supposes to be in like manner formed out of the cellular tissue, by means of such openings and perforations as may be accidentally effected in the tissue itself, from the

Which has been supposed to consist of organic fibres.

Or to be convertible into membranous tissue.

From which the tubes and cells are generated.

bursting of the vertical partitions of the cells, the tubes having no existence till the membrane is lacerated.*

But if the tubes are generated in the manner here supposed, that is by the accidental bursting of the partitions of the cells, it will be difficult to account for the known regularity with which they are formed. The only circumstance giving plausibility to the conjecture is that of the occasional occurrence of a transverse membrane interrupting the continuity of the small tubes, which M. Mirbel regards as a proof of their cellular origin. But allowing this to be a sufficient proof of the truth of the supposition, which few will be disposed to admit; how is the formation of the tracheæ to be accounted for, which retain no traces of a cellular origin, and are besides twisted spirally throughout the whole of their extent? They cannot be supposed to be merely the result of the accidental bursting of the cells of the pulp; because there is too much of regularity both in their form and distribution to be the result of accident. If M. Mirbel had even contended that the cells burst open in a regular and determinate manner, and thus give to the tube its spiral or vertical direction, his hypothesis would still have been clogged with difficulties; but on the position he assumes the difficulties are doubled. The most that can be said for it is, that it is perhaps not impossible; but it cannot

The supposition beset with difficulties.

* Traité d'Anat. et de Phys. Veget. liv. i.

certainly be said to be founded on any known facts.

It is much more likely, however, that the rudiments of all the different parts of the plant do already exist in the embryo in such specific order of arrangement as shall best fit them for future developement, by the intro-susception of new and additional particles, than that the vital principle should first manufacture a membrane which it then converts into cells, which are afterwards partially and accidentally converted into tubes, and the plant so patched up. For if this were the fact, there would be no such thing as saying what species of plant any particular seed might produce when committed to the soil.

Formation of the epidermis.

The only portion of the infant plant now remaining is the epidermis, which although it is in some cases to be regarded as a composite organ, in consequence of its consisting of more than one layer; yet as it cannot in the incipient stages of vegetation be divided into distinct layers, it may with sufficient propriety be introduced into the present section. How then is the epidermis generated, in which the body of the infant plant is invested as in a sheath?

According to Grew and Malpighi.

The pellicle constituting the vegetable epidermis has generally been regarded as a membrane essentially distinct from the parts which it covers, and as generated with a view to the discharge of some particular function. Some phytologists, however, have viewed it in a light altogether different, and have regarded it as being merely the effect of accident, and nothing more than a scurf formed on the exterior and pulply surface of the parenchyma indurated by the action of the air. This was the opinion of Grew and Malpighi, which, though it does not seem to have ever met with any very general reception, has been, however, revived of late by M. Mirbel; who, professing to be dissatisfied with the analogy that has generally been thought to exist between the epidermis of the animal and vegetable, contends that the latter is nothing more than the indurated surface of the parenchyma, from which it differs only in such circumstances as are occasioned by position. If it is more or less transparent—if it is tougher and firmer in its texture than the parenchyma or any of its parts, it is only because it is constantly exposed to the influence of light and air, and to the contact of such bodies as float in the atmosphere; but it is not to be regarded as constituting a distinct organ or membrane, or as exhibiting any proof of its being analogous to the epidermis of animals.* Such is the substance of M. Mirbel's opinion, to which he is aware that objections may still be urged. For it may be said, if this is the true origin of the epidermis, how comes it to separate so easily from the interior parts in the spring? To this objection M. Mirbel furnishes the following reply—namely, that its facility of detachment is owing to the

According to Mirbel.

* Traité d'Anat. et de Phys. Veg. tom. i. p. 87.

disorganization occasioned in the epidermis by means of its exposed position, which has even the effect of ultimately detaching it from the plant altogether, as may be seen in the instances in which it bursts and exfoliates when it is not able to expand in proportion to the internal parts. And thus M. Mirbel presumes he has established his position. But this is by no means the most formidable objection to which his hypothesis is liable; for if it be true that the epidermis is nothing more than the pellicle formed on the external surface of the parenchyma, indurated by the action of the air, then it will follow that an epidermis can never be completely formed till such time as it has been exposed to that action. But it is known that the epidermis exists in a state of complete perfection in cases where it could not possibly have been affected by the external air. If you take a rose-bud, or bud of any other flower, before it expands, and strip it of its external covering, you will find that the petals and other inclosed parts of the fructification are as completely furnished with their epidermis as any other parts of the plant, and yet they have never been exposed to the action of the air. The same may be said of the epidermis of the seed while yet in the seed vessel, or of the root, or of the paper birch, which still continues to form and to detach itself, though defended from the action of the air by the exterior layers. In herbs and in the annual parts of woody plants, such as the leaves and flowers, the epidermis never detaches itself at all; which circumstance M. Mirbel adduces as an additional argument in favour of his hypothesis, though to me it seems an argument against it. For if the air produces such violent effects upon the trunk and branches of woody plants, why should it not produce similar effects upon other plants, or upon other parts of the same plant? and why is the epidermis of the leaf and fruit incapable of being again regenerated if accidentally destroyed? Till a satisfactory answer can be given to these questions it is impossible to admit the hypothesis of M. Mirbel.

Whose hypothesis is inadmissible.

But so far is the action of the external air from being the cause and origin of the epidermis, that it is even detrimental to its formation. For the reproduction of a part that has been destroyed, in cases capable of reproduction, is always more easily effected if the wound is covered closely up. And hence it is extremely improbable that the epidermis is merely a modification of the external surface of the parenchyma effected by the influence and action of the air; if rather it is not evidently an organ formed by the agency of the vital principle, even while the plant is yet in embryo, for the very purpose of protecting it from injury when it shall have been exposed to the air in the process of vegetation. Its growth or developement is accordingly found to keep pace with that of the plant which it invests as a sheath, extending in all its dimensions and accommodating itself with wonderful facility to the expansion of the interior parts; as may be seen in large trees, and fruits of rapid growth. Its expansion, however, is circumscribed by certain bounds or limits which it cannot pass; for when vegetation is too rapid, or when the parts have become indurated with age, it refuses or is unable to expand further, and consequently cracks, as in the bark of aged trees, or in Melons of luxuriant growth; the fissure being for the most part perpendicular, though sometimes, as in the Cherry-tree, horizontal. It is also much more capable of expansion in some trees than others, and remains longer smooth; and where it does not expand freely it is thought to retard in some degree the developement of the interior parts, as in the case of the Cherry-tree, the epidermis of which the gardener is often obliged to lay open by a longitudinal incision, in order to facilitate the growth of the plant.

With regard to the disavowed analogy between the animal and vegetable epidermis, it is of no consequence to the above argument whether it holds good or not. But there are several respects in which an analogy between the two cuticles is sufficiently striking: they are both capable of great expansion in the growth of the subject; they are both easily regenerated when injured (with the exceptions already stated), and seemingly in the same manner; they are both subject, in certain cases, to a constant decay and repair; and they both protect from injury the parts enclosed.

SECTION II.

Composite Organs.

The elucidation of the developement of the Composite Organs involves the discussion of the two following topics:—the formation of the annual plant, and of the original shoot of the perennial; and the formation of the subsequent layers that are annually added to the perennial.

SUBSECTION I.

Annuals and Annual Shoots.—If a perennial of a year's growth is taken up in the beginning of winter when the leaves, which are only temporary organs, have fallen, it will be found to consist of a root and trunk, surmounted by one or more buds. The root is the radicle expanded into the form peculiar to the species, but the trunk and buds have been generated in the process of vegetation. Let the root or trunk be now taken and cut into two by means of a transverse section, and it will be found to consist already of bark, wood, and pith. The pith is spongy and succulent, being an assemblage of hexagonal cells filled with a limpid juice. The wood is tender and brittle, being an assemblage of longitudinal tubes, or fibres, forming in the aggregate a cylinder or circular layer that invests the pith. The bark is soft and flexible, being also an assemblage of tubes that form in their

Section of the caudex.

aggregate a cylinder or circular layer closely investing the wood. If the root or trunk of an annual is taken and treated in the same manner, it will be found to consist of an epidermis, pulp, and interspersed fibre. Here then is the termination of the growth of the annual, and of the first stage of the growth of the perennial: how have their several parts or organs been formed?

Formation of the pith.

As the pith seems only a modification of the original pulp, the same hypothesis that accounts for the formation of the one will account also for the formation of the other, at least in as far as they are found to consist of cellular tissue; the cells and membrane composing them being in both cases alike, though somewhat modified by situation. If the cells of the pith are the largest, it is perhaps because being lodged in the centre they are there the farthest removed from the compression and action of the air: and if the cells of the exterior pulp are the smallest, it is perhaps because being situated towards the circumference they are there the most exposed to the influence of the same causes. But their character is also affected by the character of the juices they contain; the pulp containing a resinous juice, as being mingled from its position with the proper juice of the plant; and the pith containing a watery juice, as having more communication with the ascending sap. Hence the pith and pulp, or parenchyma, are ultimately converted into organs essentially distinct from one another; though phytologists have been much puzzled to assign to each its respective functions.

Function.

According to the earlier phytologists, In the earlier ages of phytological inquiry, or rather in ages in which phytological opinions were formed without inquiry, one of the vulgar errors of the time seems to have been an opinion by which the function of the pith was supposed to be that of generating the stone of fruit, and by which it was thought that a tree deprived of its pith would produce fruit without a stone.* But this opinion is by much too absurd to merit a serious refutation. Another early opinion, exhibiting however indications of legitimate inquiry, is that by which the pith was regarded as being analogous to the heart and

Malpighi, brain of animals,† as related by Malpighi; who did not himself adopt it, but believed the pith to be like the cellular tissue, the viscera in which the sap is elaborated for the nourishment of the plant, and

Magnol, for the protusion of future buds.‡ Magnol thought that it produces the flower and fruit, but not the

Du Hamel, wood. Du Hamel regarded it as being merely an extension of the pulp or cellular tissue, without being destined to perform any important function in

Linnæus. the process of vegetation.§ But Linnæus was of opinion that it produces even the wood; regarding it not only as the source of vegetable nourishment,

* Phys. des Arb. liv. i. chap. iii.

† Medullæ usus olim insignis cordi et cerebro analogus credebatur. Anat. Plant. 13.

‡ Concoquitur itaque in horizontalibus utriculis, et medulla ipsa succus, ut futuris et proxime erupturis gemmis, et tenellis foliis præsto sit. Anat. Plant. 13.

§ Phys. des Arb. liv. i. chap. iii.

but as being also to the vegetable what the brain and spinal marrow are to animals, the source and seat of life. In these opinions there may be something of truth, but they have all the common fault of ascribing to the pith either too little or too much.

Lindsay, Plenk, and Knight,

Mr. Lindsay, of Jamaica, suggested a new opinion on the subject, regarding it as being the seat of the irritability of the leaves of the Mimosa, and Sir J. E. Smith says he can see nothing to invalidate the arguments on which this opinion is founded. Plenk in his Physiologia Plantarum, and Mr. Knight in one of his papers published in the Philosophical Transactions, regard it as destined by nature to be a reservoir of moisture to supply the leaves when exhausted by excess of perspiration; which opinion Sir J. E. Smith combats, contending that the cause assigned is wholly inadequate to the effect, as the moisture of the pith would in many cases be insufficient to supply one hour's perspiration of a single leaf, and as it is not found to be affected even when the leaves are flaccid by drought. The objections thus alleged are fatal to Mr. Knight's hypothesis, which is regarded however as deriving considerable support from the phenomena of the roots of some of the Grasses, as from that of *Phleum pratense*, which in moist situations has a fibrous root, while in dry situations it has a bulbous root, the interior of which is moist and spongy like the pith of the young shoot. But this, instead of proving the pith to be a reservoir of moisture, proves rather the capacity inherent in plants of adapting themselves to their situation, by means of an extraordinary exertion of their vital energies.

And Sir J. E. Smith.

Sir J. E. Smith professes to hold an intermediate opinion between that of Du Hamel, who ascribes to it no peculiar function at all, and that of Linnæus, who ascribes to it almost every thing important in vegetation—regarding it not as a source of nourishment, but as a reservoir of vital energy or life, analogous to the spinal marrow and nerves in animals, which do not nourish the individual, but give life and vigour to the whole by being diffused throughout the whole of its parts.* But in thus adopting the golden mean, through which, while he guards against ascribing to it too much, he avoids also at the same time the opposite extreme of ascribing to it too little, and steers equally clear both of Charybdis and Scylla, there does not seem to have been much gained in the present instance. The analogy between the pith of vegetables and spinal marrow of animals is not very well made out: if the spinal marrow is injured the parts are immediately paralyzed, and if it is broken the animal dies; but Mr. Knight has shown that a portion of the pith may be abstracted from the shoot so as to effect a disruption of continuity without occasioning any material injury to the plant. It should be recollected, however, in justice to the phytology of ancient Greece, that this

* Introduction, p. 40.

experiment had been performed, and the result ascertained, even in the time of Theophrastus.* The pith cannot therefore be regarded as corresponding to the spinal marrow of animals, in any thing at all essential.

Hence it appears that the peculiar function of the pith has not yet been altogether satisfactorily ascertained; and the difficulty of ascertaining it has been thought to be increased from the circumstance of its seeming to be only of a temporary use in the process of vegetation, by its disappearing altogether in the aged trunk. But although it is thus only temporary as relative to the body of the trunk, yet it is by no means temporary as relative to the process of vegetation; the central part of the aged trunk being now no longer in a vegetating state, and the pith being always present in one shape or other in the annual plant, or in the new additions that are annually made to perennials. The pith then is essential to vegetation in all its stages: and from the analogy of its structure to that of the pulp or parenchyma which is known to be an organ of elaboration, as in the leaf, the function of the pith is most probably that of giving some peculiar elaboration to the sap, according to the hypothesis of Malpighi; which seems to me to be the best founded of all the foregoing, with the exception of that part of it by

* Ἡ δε μήτρα, μέχρι τινος ἐξαιρουμένη ὀυ φθείρει, δἰ ὁλου δε φθείρει. Περι φυτων ἀιτιων. το. E.

which the pith is supposed to be also peculiarly destined to give origin to the future buds.

Origin and formation of the wood in layers or otherwise.

The next topic of inquiry is that of the generation of the layer of wood in our example of woody plants, or of the parts analogous to wood in the case of herbaceous plants; a topic that has been hitherto but little attended to. Phytologists have indeed offered many conjectures concerning the origin of the annual layer that is added to perennials; but they do not seem to have taken up the subject at the proper point. They have generally made their observations on trees of considerable age and magnitude, and have contended, some that the wood is formed from the pith, others that it is formed from the bark, and others that it is formed from the alburnum of the former year; thus evading the subject of the formation of the original layer altogether, which is the foundation of all the rest, and the most essential step in the process of vegetation; though it is a step concerning which there can be but little said that is at all satisfactory. If we suppose the rudiments of all the different parts to exist already in the embryo, then we have only to account for their developement by means of the intro-susception and assimilation of sap and proper juice; but if we suppose them to be generated in the course of vegetation, then the difficulty of the case is augmented: and at the best we can only state the result of operations that have been so long continued as to present an effect cognizable to the sense of sight, though the detail of the process is often so very minute as to escape even the nicest observation. All, then, that can be said on the subject is merely that the tubes, however formed, do, by virtue of the agency of the vital principle operating on the proper juice, always make their appearance at last in an uniform and determinate manner, according to the tribe or species to which the plant belongs, uniting and coalescing so as to form either a circular layer investing the pith, as in woody plants; or a number of divergent layers intersecting the pith, as in some herbaceous plants; or bundles of longitudinal and woody fibre interspersed throughout the pith, as in others; though in some of the less perfect plants no longitudinal fibres are at all discernible, and consequently no part analogous to wood.

In the same manner we may account for the formation of the layer of bark, either by supposing that it is merely the developement of some original elements existing in the embryo, or that it is generated by means of the agency of the vital principle in the process of vegetation and out of the proper juice, so as to form an exterior layer distinct in its character and properties, and separate or easily separable from the wood or interior part of the plant. And bark.

Such is a slight sketch of the process of the developement of the elementary and composite organs of annuals, or of perennials of a year's growth.

SUBSECTION II.

Formation of the layers of the second year,

Perennials, and their Annual Layers.—If a perennial is taken at the end of the second year and dissected as in the example of the first year, it will be found to have increased in height by the addition of a perpendicular shoot consisting of bark, wood, and pith, as in the shoot of the former year; and in diameter by the addition of a new layer of wood, and of bark generated between the wood and bark of the former year, and covering the original cone of wood, like the paper that covers a sugar-loaf: this is the fact of the mode of augmentation about which phytologists have not differed, though they have differed widely with regard to the origin of the additional layer by which the trunk is increased in diameter.

According to Malpighi and Grew.

Malpighi was of opinion that the new layer of wood is formed from the liber of the former year; the layer of liber being by degrees assimilated to the woody substance, and ultimately converted into a layer of wood, which attaches itself to the layer that was previously formed.* But the defect of this hypothesis is, that it does not account for the formation of the new layer of liber itself. Grew was of opinion that a new ring of sap vessels is first generated on the inner furface of the liber of the former year, which is gradually converted into a new layer of liber that ultimately splits into two portions,

* Anat. Plant. 11.

taking contrary directions; the outer portion extending towards the circumference of the plant, and forming the new layer of bark; and the inner portion receding towards the centre and forming the new layer of wood. This hypothesis is preferable to Malpighi's, inasmuch as it accounts for the new layer both of wood and bark; and perhaps no hypothesis whatever is more conformable to fact. Hales was of opinion that the new layer of wood is formed from the layer of the preceding year, both by means of the dilatation of the horizontal vessels of the former layer, and also of the extension of the longitudinal tubes. The opinion was founded upon the phenomena of a tumor that seemed evidently formed from the wood; whence he inferred that the new layer is also formed from the wood. But as it could never have been deemed logical from the phenomena of the diseased action of a part to infer the mode of procedure in the sound action of the whole, the hypothesis does not appear to have met with many advocates. Linnæus thought the new layer of wood was formed from the pith, which is absurd, because the opinion goes to the inversion of the very order in which the layer is formed, the new layer being always exterior to the old one. But according to the most general opinion, the layer was thought to be formed from a substance oozing out of the wood or bark—first, a limpid fluid, then a viscid pulp, and then a thin layer attaching itself to the former; the substance thus exuding from the wood or bark was generally

According to Linnæus.

regarded as being merely an extravasated mucilage, which was somehow or other converted into wood and bark: but Du Hamel regarded it as being already an organized substance, consisting of both cellular and tubular tissue, which he designated by the appellation of the *Cambium*, or Proper Juice. This opinion seems to have been entertained also by Grew, though it does not appear to rest on any very convincing evidence; the fact, however, of its exudation was evident, though it was not yet ascertained whether it exuded from the wood or from bark of the former year; and whether the new layer, when formed, separated into two, as Grew had conjectured. This inquiry was undertaken by Du Hamel,* whose experiments on the subject are indeed most luminous, and are, though seldom quoted, an anticipation of almost every thing that has been done by the most distinguished of our modern phytologists.

Du Hamel,

Who ascertains that it is formed

In order to ascertain whether the new layer of wood is formed from the former layer of wood or of bark, his first experiment was that of a graft *par l'ecusson;*† which is done by means of detaching a portion of bark from the trunk of a tree and supplying its place exactly by means of a portion of bark detached from the trunk of another tree, that shall contain a bud. In this way he grafted the Peach on the Prune tree, because the appearance of the wood which they respectively form is so very different that it could easily be ascertained whether the new layer

* Phys. des Arb. liv. iv. chap. iii. † Ibid. chap. 4.

was produced from the stock or from the graft. Accordingly at the end of four or five months after the time of grafting the tree was cut down, and as the season of the flowing of the sap was past, a portion of the trunk including the graft was now boiled to make it part more easily with its bark; in the stripping off of which there was found to be formed under the graft a thin plate of the wood of the Peach, united to the Prune by its sides, but not by its inner surface, although it had been applied to the stock as closely as possible: hence Du Hamel concluded that the new layer of wood is formed from the bark, and not from the wood of the preceding year. The same experiment was repeated with the same result upon the Willow and Poplar; when it was also found that if a portion of wood is left on the graft it dies, and the new wood formed by the bark is exterior to it. The above conclusion, therefore, is perfectly legitimate, which Du Hamel also strengthens by the following experiment:—Having detached a cylinder of bark from its trunk, and covered the wood below it with a thin plate of tin-foil, he then replaced the bark as before, reducing the case to the following dilemma; if the new layer of wood was formed from the old layer of wood, then it was plain that the new layer would be deposited within the tin-foil; and if it was formed from the bark, it was also equally plain that it would be deposited without the tin-foil: the result accordingly was, that a new layer of wood was deposited between

From the bark of the preceding year.

the bark and tin-foil, but none between the tin-foil and the interior layer. This experiment was completely decisive of the point in question; and yet there is an experiment of Dr. Hope's on the same subject, which is, if possible, still more convincing:— Having made a longitudinal incision in the trunk of a Willow of three or four years old so as to penetrate through the bark, he laid bare a portion of the stem by slipping the bark to the one side, which was, however, still attached to the stem at the upper and lower extremities of the decorticated part; the detached portion of bark was then bent into the form of a hollow cylinder, by uniting its edges as closely as possible, and the whole well secured from the action of the atmosphere.* The plant was then allowed to remain undisturbed for several years, when the result of dissection was, that new layers of wood were generated within the lateral cylinder of bark, while the decorticated portion of the stem remained unaugmented; the portions above and below being augmented, as in other ordinary cases of vegetation. It is evident, therefore, that the additional layer by which the plant increases in diameter is generated from the bark.

Not by the conversion of a layer of liber into wood,

But it was not yet accurately ascertained whether the newly formed layer of wood was merely indurated liber, as was the opinion of Malpighi; or a production formed from it, as was the opinion of Grew. This Du Hamel thought he might ascertain

* Smith's Introduction, p. 35.

by means of passing through the bark of a tree several small threads of silver in a horizontal direction, so as to penetrate the liber.* If the *liber* was converted into wood, the threads, it was to be presumed, would be found ultimately imbedded in the wood; and if it was not converted into wood, they would be found still in the bark. Accordingly when a trunk which had been so treated was at the end of several years opened up and inspected, the threads were found to be deeply imbedded in wood: it is plain, therefore, that the new layer of wood forms originally a layer of *liber*, according to the common acceptation of the term. But to try also the value of Grew's conjecture with regard to the separation of the liber into two parts, the one expanding towards the circumference and forming new bark, and the other condensing towards the centre, and forming new wood, Du Hamel varied the above experiment so as that some of the threads were passed through the outer part of the bark, near the epidermis; others through the inner part of it, near the liber; others through the liber itself; and others between the liber and wood. At the end of several years, when the trunk was opened up and inspected, the threads that were originally placed in the outer bark and near the epidermis were now covered only with a thin and decayed crust, which broke readily into pieces; those that were originally placed in the outer bark, but near the liber, were

* Phys. des Arbres, liv. iv. chap. iii.

now among the external cortical layers; those that were originally passed through the liber were now imbedded in wood; and those that were orginally passed between the liber and wood were still more deeply imbedded. The conjecture of Grew, therefore, is confirmed, at least in substance. For either the layer that is formed separates into two distinct portions, the one tending towards the circumference and forming bark, and the other tending towards the centre and forming wood; or, two distinct layers are originally generated receding in opposite directions, and forming respectively wood and bark: which last part of the alternative is the most likely to be the fact; because we can scarcely refer two substances so distinct in their character and properties as the wood and bark, precisely to the same origin.

But by immediate generation from the proper juice;

But although the above experiments are correct and convincing, beyond all controversy, as far as they go, yet they do not include the whole of the case; for the annual layer, which seems thus to be formed merely from the bark, is in fact formed only from the proper juice descending from the leaf through the tubes of the liber. The bark then is not to be regarded as the generating cause of the new layer, but merely as the medium of the transmission of the materials from which it is formed. For the proof of this most important fact we are chiefly indebted to the well-known experiments of Mr. Knight, by which he has thrown the highest degree of elucidation on one of the most obscure and intricate processes of the vegetable economy, in having shown that the sap is elaborated, so to render it fit for the formation of new parts, in the leaf only. If a leaf or branch of the vine is grafted even on the fruit-stalk or tendril, the graft will still succeed;* but if the upper part of a branch is stripped of its leaves the bark will wither as far it is stripped; and if a portion of bark furnished with a leaf is insulated by means of detaching a ring of bark above and below it, the wood of the insulated portion that is above the leaf is not augmented:† this shows evidently that the leaf gives the elaboration necessary to the formation of new parts, and that without the agency of the leaf no new part is generated.

As elaborated in the leaf,

There seems indeed to be an exception to this law in one of Mr. Knight's own experiments, in which he found that when the fruit-stalk was grafted on the leaf-stalk, the graft succeeded also; and in which case the wood was deposited on the external sides of his central vessels, having oozed, no doubt, from the external tubes by which the proper juice descends. Now this seems to prove that the parts of the flower are capable of generating wood as well as the leaf: but it should be recollected that flowers in general are furnished with a calyx, which is certainly very nearly allied to the leaf, and that some parts of the flower are even convertible into leaves;

Or even flower,

* Phil. Trans. 1803. † Ibid. 1801.

as may be seen in the case of the common Cherry when the blossom becomes double, the stamens being converted into petals, and the pistil not unfrequently into a leaf. A similar phenomenon may be sometimes observed also in the fruit of the Pear-tree. In the summer of 1809, I observed in the garden of Rendlesham Parsonage two Pears, of nearly the size of the rest on the same tree, having each several leaves growing out of it, with the insertion about half way between the base and apex of the fruit: I do not know what particular variety of Pear it was, but it was a large and dusky-coloured winter Pear, perhaps the winter rousselet. The same thing may be also observed in the fruit of the Medlar; and hence the success of Mr. Knight's graft of the fruit-stalk on the leaf-stalk is the less surprising. The fact then is that the new layer is formed not absolutely from the bark, as the experiments of Du Hamel might seem to indicate, but from the proper juice descending through the returning vessels of the leaf, leaf-stalk, and inner bark, from the summit to the lower extremity of the plant; in tracing which vessels Mr. Knight thought he could even discover two distinct sets, one for forming the new layer of wood, and another for forming the new layer of liber. This is a very nice distinction indeed, and a fact, if it is the fact, difficult to ascertain. It is certain, however, that either two layers are originally generated; or that the ori-

And descending through the returning vessels.

ginal layer separates into two parts, as Grew supposed; because the bark is augmented by a new layer as well as the wood.

Peculiar notions of Knight.

Such is the final result of the experiments of Du Hamel, Hope, and Knight. And yet Mr. Knight, who has done so much to elucidate the subject, entertains at the same time some minor opinions that are altogether incompatible with his general theory. He is of opinion that the new bark, as well as the leaf, possesses also the power of preparing proper juice and of generating new wood; because it was observed that a small quantity of wood was generated even at the lower extremity of an insulated portion of bark on which there was neither bud nor leaf.* But is it not possible to account for this small production from the quantity of proper juice that might exist in the bark at the time it was left in its insulated state? He is also of opinion that the wood or alburnum already formed extends itself laterally without any radicles as he calls them, descending from the leaves and shoots above; because trees of different species, when grafted on one another, preserve their wood distinct in quality immediately above and below the graft. But if the wood extends itself laterally, why did it not so extend itself above his insulated leaf, where we are told it made no augmentation? And is it not also possible that the proper juice may receive its final degree of modification in the bark itself?

* Phil. Trans. 1803.

This is much more likely to be the case than that the alburnum should extend itself laterally, which is contrary to the experiments of Du Hamel, and also to the scope of Mr. Knight's general conclusion; namely, that the sap after being exposed to the action of the light in the leaves, and converted into proper juice, is again carried back by the returning vessels of the leaf and leaf-stalk to the bark, by which it is conveyed throughout the whole extent of the plant, to add new matter and to compose the different organs for the succeeding year.*

Augmentation in succeeding years.

Such then is the mode of the augmentation of the plant in the second year of its growth. It extends in width by a new layer of wood and of bark insinuated between the wood and bark of the former year; and in height by the addition of a perpendicular shoot or of branches, generated as in the shoot of the first year. But if the plant is taken and dissected at the end of the third year, it will be found to have augmented in the same manner; and so also at the end of the succeeding year as long as it shall continue to live; so that the outermost layer of bark, and innermost layer of wood, must have been originally tangent in the first year of the plant's growth; the second layer of bark, and second layer of wood, in the second year; and so on in the order of succession till you come to the layer of the present year, which will

* Phil. Trans. 1805.

in like manner divide into two portions, the outer forming one or more layers of bark, and the inner forming one or more layers of wood. And hence the origin of the concentric layers of wood and of bark of which the trunk was found to be composed in treating of the anatomy or internal structure of the plant; where it was shown that the layers of wood are by no means regularly developed, being often thicker on the one side of the stem than on the other, and often also more in number; owing, as it appeared, to a superabundance of sap flowing from a large root, or to a large branch, or to sudden changes of temperature. And even where the individual layers are of equal thickness throughout, they are not always equal in thickness to one another; owing no doubt to the age and vigour of the plant; for the larger the diameter of the tree, the thinner the layer, even if the same quantity of wood should be formed. But the layers of wood are always thicker than the layers of bark; though the bark of sickly trees is thicker in proportion to the wood than that of healthy trees. But how are the different layers united together so as to form but one body in the aggregate? Malpighi thought the layers of liber, as formed by the longitudinal tubes, are united or cemented together by means of a plate of cellular tissue interposed between them. In the formation of the layers it is true that tubes and utricles are always conjoined, the former being apparently united together by the latter. But it does not appear that an intervening layer of cellular tissue is always to be found distinct. There are some plants, however, which do exhibit the layer in question distinctly enough according to the description of Malpighi. Between every two layers of the bark of the Fir-tree, there is a thin layer of a substance evidently different in texture, which might have been the ground of Malpighi's remark. And even in the body of the trunk there is alternately a layer of wood that is hard and white, and a layer that is brown and resinous, from which the resinous drops exude when the trunk is cut.

Formation of the divergent layers according to DuHamel,

But how are we to account for the formation of the divergent layers, which Du Hamel erroneously supposed to proceed from the pith? If Du Hamel had but happened to attend to the phenomena relative to the point in question, which some of his own experiments were the best calculated to exhibit, he would readily have found the true solution of the difficulty. This, however, has been furnished by Mr. Knight, who, in tracing the result of the operation of budding, observed that the wood formed under the bark of the inserted bud unites indeed confusedly with the stock, though still possessing the character and properties of the wood from which it was taken, and exhibiting divergent layers of new formation which originate evidently in the bark, and terminate at the line of union between the graft and stock.* Also, if a portion of the stem of a tree is decorticated so as to leave the surface of the alburnum exposed to the air for any considerable length of time, there is no farther vegetation on that part of the alburnum. But if the wound is not very large it will again close up, first by means of the production of a new bark issuing from the edges, and gradually narrowing the extent of the wound; and then by the production of new layers of wood formed under the bark as before. The new wood will not indeed unite with the portion of alburnum that had been exposed to the air; but it will exhibit on a horizontal section, the same traces of divergent layers as before, extending from the bark in which they originate to the lifeless surface of the old wood within. It is evident, therefore, that the divergent layers are formed, not from the pith, but from the proper juice descending through the channel of the bark, and are synchronous in their formation with that of the concentric layers through which they pass.

Whose opinion is erroneous.

It seems indeed impossible that the divergent layers should be an extension of the pith, at least beyond the first or second year of the plant's growth, whether on account of the gradual induration of the wood, or of its own natural diminution. For as the trunk increases in size the pith

* Phil. Trans. 1803.

gradually diminishes in diameter, till it is at length totally obliterated, and its place supplied with wood, as in the case of the aged trunk; existing only in the smaller branches, or in the annual shoot.

Conversion of the pith into wood.

But how is the formation of the wood to be accounted for, that now occupies the place of the pith? In the chapter on the anatomy of the plant it was observed that the pith of the young shoot is surrounded by a set of longitudinal tubes, forming in the aggregate a cylinder, in which it is invested as in a sheath, which M. Mirbel designates by the appellation of the *Tubular sheath;* but which I have thought to be better designated by the appellation of the *Medullary sheath;* inasmuch as the latter term suggests the use of the organ in question, which the former term does not. But by whatever appellation it may be designated, it appears that the tubes of which it is composed do, in the process of vegetation, deposit a *Cambium*, which forms an interior layer that is afterwards converted into wood for the purpose of filling up the medullary canal. Such is the account given by M. Mirbel,* of the origin of the wood occupying the place of the pith in the aged trunk, which is countenanced by the fact of the developement and existence of longitudinal tubes that are to be found even within the body of the pith, as stated also in the above-mentioned chapter.

* Traité d'Anat. et de Phys. Veg. liv. iii.

Conversion of alburnum into wood.

But in consequence of the increase of the trunk by means of the regular and gradual addition of an annual layer, the layers whether of wood or of bark are necessarily of different degrees of solidity in proportion to their age; the inner layer of bark, and the outer layer of wood, being the softest; and the other layers increasing in their degree of solidity till you reach the centre on the one hand, and the circumference on the other, where they are respectively the hardest, forming perfect wood or highly indurated bark, which sloughs or splits into chinks, and falls off in thick crusts, as in the Plane-tree, Fir, and Birch. What length of time then is requisite to convert the alburnum into perfect wood, or the liber into indurated bark; and by what means are they so converted?

There is no fixed and definite period of time that can be positively assigned as necessary to the complete induration of the wood or bark, though it seems to require a period of a good many years before any particular layer is converted from the state of alburnum to that of perfect wood; and perhaps no layer has received its final degree of induration till such time as the tree has arrived at its full growth. But this is not a subject of any peculiar importance. It is a subject of some importance, however, to trace the cause of the induration of the wood, concerning which there seems to have existed, or to exist now, some diversity of opinion.

Attributed to loss of sap.

An opinion pretty generally entertained was that by which the induration of the alburnum, and its consequent durability, are attributed to the loss of sap which the layer sustains after the period of its complete developement; when the supply from the root diminishes, and the waste by evaporation or otherwise is still kept up, inducing a contraction or condensation of its elementary principles that augments the solidity of the layer, in the first degree, and begins the process that future years finish.

Theory of Mr. Knight.

But Mr. Knight believes the induration of the alburnum as distinguishable in the winter to be owing rather to some substance deposited in it in the course of the preceding summer, which he regards as being the proper juice in a concrete or inspissated state, but which is carried off again by the sap as it ascends in the spring. This was originally a conjecture which he thinks he has proved to be also the truth. The first argument is founded on the fact that the sap as it ascends becomes gradually more and more mixed with the proper juice of the plant, as may be seen in the case of the Maple, and Birch, the sap of which last when extracted near the root, being almost without taste; but when extracted at the height of seven or eight feet, or less, being sensibly sweet. The second argument is founded on the fact that the specific gravity of the sap in the bleeding season increases according to the height at which it is extracted.

The specific gravity of the sap of the Sycamore-tree extracted close to the ground was 1·004; at the height of seven feet, 1·008; and at the height of twelve feet, 1·012. The same proportion was also observed in the sap of the Birch; but after the sap had flowed for some days from the same incision the specific gravity was reduced to 1·002; which shows that the matter carried off from the alburnum had diminished at least in the vicinity of the incision. The third argument is founded on the fact that the specific gravity of the alburnum is greater in the winter than it is either in the preceding or following summer. Two equal portions were taken from two poles that had sprung from the same shoot, the one having been felled in December, and the other in the May following. They were kept for the space of seven weeks near a warm fire, at the end of which period they both appeared to be perfectly dry. The specific gravity of the winter felled wood was 0·679; while that of the summer felled wood was only 0·609. Still it was possible that the apparent difference of specific gravity might have proceeded merely from a greater degree of contraction in the winter felled wood; in consequence of which doubt it was thought that the comparison of their respective quantities of extractive matter would be the best means of avoiding all mistake. Accordingly six ounces of boiling water were poured on 1000 grains of each, reduced to small fragments; and at the end of twenty-four hours it was found that the winter felled wood had communicated a much deeper tinge to the water, and raised its specific gravity to 1·002, the specific gravity of the other being only 1·001. Whence Mr. Knight concludes that the cause of the superior quality of winter felled timber, and of the conversion of the alburnum into wood, is attributable to the matter deposited in the alburnum during the preceding summer, and partially, though not totally, carried off in the succeeding spring.*

Throws but little light on the subject.

But after all this parade of experiment, it must still be confessed that there is but little elucidation thrown on the subject. For the truth of the facts may be admitted, and yet the legitimacy of the ultimate conclusion deduced from them may be denied. It may be true that an extraordinary quantity of proper juice is deposited in the alburnum in the course of the summer, which is again partly carried off in the succeeding spring; and this will no doubt account for its superior quality in the intervening winter. But how is it to account for its final conversion into wood, a change that is effected only by slow degrees, the layer becoming every year more and more indurated till it acquires in the end its last degree of solidity. For if the matter deposited in the summer is almost wholly carried off again in the succeeding spring, no progress has been made in the

* Phil. Trans. 1805.

process of induration; unless you suppose that the matter carried off from the alburnum in the spring, by means of the ascending sap, is again deposited in it in the course of the summer. And this is indeed what Mr. Knight supposes; for he thinks that the proper juice in descending from the leaf is expended not only in forming a new epidermis, where that is wanted, and a new layer of liber and of alburnum, but partly also in entering the pores of the former alburnum and mingling again with the ascending sap. But if this second accession of proper juice were even allowed, its effect could be but very trifling. For if it mingles again with the ascending sap, it must also be again for the most part carried off, and can consequently be of no great advantage to the induration of the wood. And if it should even leave behind it a considerable deposit, now, in the second year, still you have to account for its further induration in the third and fourth and subsequent years, when it will hardly be contended that the descending proper juice enters it. It cannot, therefore, be admitted upon Mr. Knight's principles that the alburnum is converted into wood by means of any matter deposited in it during the summer; because the matter thus deposited is again carried off in the succeeding spring; and is not proved, but conjectured, to be again restored in the summer following: on which very slender foundation Mr. Knight has, however, thought proper to erect the superstructure of the doctrine of the circulation of the vegetable juices.

SECTION III.

Circulation of Vegetable Juices.

According to the earlier phytologists.

AFTER the discovery of the circulation of the blood of animals, phytologists, who were fond of tracing analogies between the animal and vegetable kingdoms, began to think that there perhaps existed in plants also, a circulation of fluids. There was not indeed any visible apparatus corresponding respectively to the stomach, intestines, lacteals, and heart of animals, the main spring of the circulation of the blood; but the defect was supplied in the best way possible. The root was regarded as corresponding both to the mouth and stomach of animals, and as effecting some peculiar change upon the fluid absorbed, that fitted it for the direct nourishment of the plant; as well as possessing also the power of propelling the digested fluid, impregnated with the principles of nutrition, growth, and developement, to the very summit of the leaf, from which it was again returned to the root, where, mingling with the newly digested fluid, it was again propelled to the summit as before, and a circulation thus kept up. The vessels in which it was propelled to the summit of the plant were denominated arteries; and the vessels in which it is again returned to the root were denominated veins.

Such was the original theory of the circulation of the vegetable fluids, which was soon found to rest on a very slender basis, and to stand in need of the support and sanction of some substantial argument. Accordingly the best arguments that could be obtained were mustered up in defence of the doctrine.

In proof of the digestive power of the root it was said that if a tree is cut down level with the earth, it will still continue to live and to send out new shoots; which it could scarcely be thought capable of doing, except upon the supposition that the sap is elaborated in the root, and not in the upper part of the plant. But the Fir-tree uniformly dies under this operation; and although most trees do indeed survive it, yet it is only by virtue of new buds which are formed on the upper part of the root, or remaining part of the stem, and in which the process of elaboration is continued. For if you constantly destroy the buds as they make their appearance the root will to a certainty die. It was urged, however, that the root is the organ of elaboration, because, in plants having a conspicuous proper juice, it may be readily perceived by means of a transverse section oozing from the very extremity of the root. But so also it may be observed to ooze even from the extremity of the stem. The argument then proves nothing;

and if it does prove the root to be the organ of elaboration, why does the sap in general ascend the stem unaltered?

In proof of the fact of the circulation of the sap it was said that it is impossible to conceive how the important and complicated operations of the secretion of the peculiar juices of the plant could be effected all at once; and that, therefore, it was reasonable to suppose them to be effected by repeated efforts, which the circulation of the sap, as of the blood in the animal system, was the best fitted to produce. But this is obviously an argument of convenience, which proves nothing. For if the cause of vegetable developement should remain unexplained, we are not to assume that of the process of circulation without any adequate proof, merely because it seems to account for it the most easily. But it was added that different sorts of vessels are distinguishable in the structure of the plant, some for conducting the sap in its ascent, and others for conducting it in its return. This it was admitted might be the fact, without proving the one set to be arteries and the other veins. It was then said that they must still be supposed to exist, though we should not be able to distinguish or ascertain them; in the same manner as we must suppose their existence in the wing of a butterfly without being able to distinguish them. This is no doubt possible; but it is contradicted by the fact that an inverted plant grows. It was also added that a malignant humour has been sometimes found to pervade the whole of the vascular system; which could not have happened, as it was thought, except upon the supposition of a circulation of fluids. But it is also known that diseases of the trunk do not always affect the root; and that if a tree diseased in the trunk or branches is cut down to the root, it will send up new shoots as sound and vigorous as at first. Finally, it was said that parasitical plants are injurious to the tree on which they grow, by throwing into the circulation some noxious principle. But it is not proved that parasitical plants are always injurious to the tree on which they grow; and if they are so sometimes, the effect may be very well accounted for by attributing it to the privation of a part of its due nourishment, rather than by the introducing of some noxious principle into an assumed circulation.

Such are the principal arguments that were advanced by the earlier phytologists in support of the circulation of the sap, as stated and refuted by Du Hamel,* who, while he admits the ascent of the sap, and descent of the proper juice, each in peculiar and appropriate vessels, does not however admit the doctrine of a circulation; which seems, about the middle of the last century, to have fallen into disrepute. For Hales, who contended for an alternate ascent and descent of fluids in the day and night, and in the same vessels, or for a sort of vibratory motion as he also

* Phys. des Arbres, liv. v. chap. ii.

describes it, gave no countenance whatever to the doctrine of a circulation of juices.

According to Hedwig, Corti, and Willdenow.

But the doctrine, as it appears, has been again revived, and has met with the support of some of the most distinguished of modern phytologists. Hedwig is said to have declared himself to be of opinion that plants have a circulation of fluids similar to that of animals. But as I am not acquainted with the arguments on which his opinion is founded, I can say nothing with regard to them. Corti is said to have discovered a species of circulation in the stem of the *Chara*, confined as I believe, within the limits of the internodia. But perhaps it was nothing more than a sort of vibratory motion of the contained fluids, similar to that which I observed in the peduncle of the *Marchantia*, as related in the Analysis of the Internal Structure. Willdenow has also introduced the subject, and defended the doctrine, in his Principles of Botany; * but only by saying he believes a circulation to exist, and that it is impossible for the leafless tree to resist the cold if there be not a circulation of fluids; which, as it is no argument, merits no particular reply.

According to Knight.

But as Mr. Knight has given his reasons somewhat more in detail, we will also be somewhat more particular in endeavouring to ascertain their value. The experiments by which Mr. Knight accounts for the conversion of the alburnum into wood have been already stated in detail. But he is of opinion

* English Trans. p. 285.

that they prove at the same time the circulation of the vegetable fluids. For if it is admitted that the descending proper juice forms not only a new epidermis where wanted, and a new layer of liber and of alburnum, but enters also, partly, the alburnum of the preceeding year, where it mingles and is again carried up with the ascending sap, it is obvious that a sort of circulation is completed. But this last and most essential part of the process rests merely on the foundation of conjecture; for there is no proof offered in support of the fact: it is only said that you cannot conceive how in a body so porous as wood, the several fluids should remain unmixed. It is, however, no proof of the truth of any opinion to say that you cannot conceive the thing to be otherwise, as the same thing has been said in support of ten thousand absurdities which have still continued to be absurdities. It is no doubt difficult to conceive how the fluids should remain unmixed; but what if they should remain unmixed after all? The alburnum of the former year being now considerably condensed, will not afford that ready reception to the proper juice which is here alledged: in the same manner we shall suppose that the returning vessels of the leaves do not admit coloured infusions; but if the vessels of the alburnum should admit some part of the descending proper juice by means of lateral communication as is here supposed, can any one be certain that it is the juice which was formerly carried up, and not rather part of that which has been newly generated? And if the circulation is completed only by the entrance of the descending fluid into the alburnum of the former year, what becomes of the circulation of juices during the first year of the plant's growth, when there is yet no alburnum of a former year to enter? which if it does enter in future years, why is it carried up again? It does not seem necessary to complete the circulation, and upon Mr. Knight's principles it ought rather to remain for the purpose of effecting the induration of the wood. In short there seems to be a great deal of confusion and contradiction in Mr. Knight's hypothesis. For first he supposes that the superior specific gravity and superior quality of winter felled wood depends upon a substance deposited in the *alburnum* during the preceeding summer and autumn, and yet he abstracts the major part of it in the succeeding spring without replacing it by a quantity sufficient to account for its increased solidity in the subsequent winter, or at least, without replacing it by any quantity which is to remain permanent

Whose hypothesis is indefensible.

Mr. Knight's hypothesis, therefore, cannot be true in its whole extent; for if the supposed circulation exists, then the superior quality of winter felled wood does not ultimately depend upon any substance deposited in the alburnum in summer, because it is all or in great part carried off in the succeeding spring and not absolutely proved to be replaced in the subsequent summer; and if the matter supposed to be deposited in the alburnum is the true cause of the wood's superior quality, then the circulation cannot take place; because on the supposition in question the matter that is thus deposited ought not to be again abstracted. Indeed it seems to be doubtful whether the hypothesis is good in any of its parts; for as on the one hand the circulation rests on no admissible proof, so on the other the superior quality of the winter felled wood is well enough accounted for by its becoming more condensed.

It is no proof of the circulation of the vegetable fluids to say that the proper juice may even be seen in the alburnum of some plants, as in the example of the Fig. For since the alburnum is itself originally formed of the proper juice, it cannot during the first year be otherwise than mixed with it; and if it should occasionally be found even in the matured wood, it may be nothing more than the portion that was originally deposited, now in an inspissated state. Nor is the induration of the wood to be regarded as proceeding from the introsusception of some additional substance into the alburnum, rather than from its condensation, owing to the mere change of colour which takes place in that process from a white to a dark brown; or to its increased durability, as supposed by Mr. Knight,* because the effects in question may be easily accounted for, even upon the principle of condensa-

* Phil. Tran. 1806.

tion. The leaf changes its colour in the autumn, and wood when felled not only changes its colour, but becomes also more durable without the introsusception of any new substance.

Though his general views are luminous.

But although the doctrine of a circulation as maintained by Mr. Knight should be false, yet the account which he gives of the progress and agency of the sap and proper juice, short of circulation, may be true. The sum of the account is as follows:—When the seed is deposited in the ground under proper conditions, moisture is absorbed and modified by the cotyledons, and conducted directly to the radicle, which is by consequence first dedeveloped. But the fluid which has been thus conducted to the radicle, mingling no doubt with the fluid which is now also absorbed from the soil, ascends afterwards to the plumelet through the medium of the tubes of the alburnum. The plumelet now expands and gives the due preparation to the ascending sap, returning it also in its elaborated state to the tubes of the bark, through which it again descends to the extremity of the root, forming in its progress new bark and new alburnum; but mixing also, as Mr. Knight thinks, with the alburnum of the former year, where such alburnum exists, and so completing the circulation.

But in this account of the process of vegetation, though sufficiently perspicuous, one or two links of the chain are obviously omitted; no conjecture being offered with regard to the origin of the tubes of the alburnum and bark. Their existence is assumed but not accounted for. We are told, indeed, that the tubes of the alburnum are not discoverable at a very early period of vegetation, as coloured infusions are not absorbed by the plant till it is some weeks old, even when part of the root is cut off, at least in the case of the Horse-chesnut,* though they begin to be discoverable soon after that period. But we are not favoured even with a conjecture with regard to the probability of their actual origin; whether as being wholly generated in the progress of vegetation, or as being merely developed by the intro-susception of nutriment into some primordial and duly organized element pre existing in the embryo.

SECTION IV.

Decomposite Organs.

To the above brief sketch of the agency of the vital principle in the generation or growth of the elementary and composite organs, there now remains to be added that of the progress and mode of the growth of the decomposite organs, or organs immediately constituting the plant, as finishing the process of the vegetable developement. This will include the phenomena of the ultimate developement of the root, stem, branch, bud, leaf, flower, and fruit.

* Phil. Trans. 1806.

SUBSECTION I.

The Root.

The Root.—From the foregoing observations and experiments it appears that the roots of plants, or at least of woody plants, are augmented in their width by the addition of an annual layer, and in their length by the additon of an annual shoot, bursting from the terminating fibre. But how is the developement of the shoot effected? Is it by the intro-susception of additional particles throughout the whole of its extent; or only by additions deposited at the extremity? In order to ascertain the fact, with regard to the elongation of the root, Du Hamel instituted the following experiment:—Having passed several threads of silver transversely through the root of a plant, and noted the distances, he then immersed the root in water. The upper threads retained always their relative and original situation, and the lowest thread which was placed within a few lines of the end was the only one that was carried down. Hence he concluded that the root is elongated merely by the extremity.* Mr. Knight who from a similar experiment obtained the same result deduced from it also the same conclusion.† We may regard it then as certain that the mode of the elongation of the root is such as is here represented, though in the progress of its developement it may affect a variety of directions.

Elongated by the extremity only.

* Phys. des Arb. liv. i. chap. v.

† Phil. Trans, 1806.

The original direction of the root is generally perpendicular, in which it descends to a considerable depth if not interrupted by some obstacle. In taking up some young Oak-trees that had been planted in a poor soil, Du Hamel found that the root had descended almost four feet, while the height of the trunk was not more than six inches. If the root meets with an obstacle it then takes a horizontal direction, not by the bending of the original shoot, but by the sending out of lateral shoots. The same effect also follows if the extremity of the root is cut off. It grows in length no longer. Du Hamel made some Cherry-stones, Almonds, and Acorns to germinate in wet sponges; and when the roots had grown to the length of two inches, he then placed them in glasses as bulbous roots are placed, so as that the extremity of the root only touched the water. Some were previously shortened by the cutting off of a small bit from the point; others were put in entire. The former immediately sent out lateral shoots, but elongated no farther in a perpendicular direction; the latter descended perpendicularly to the bottom of the glass. He cut off also the tips of some roots vegetating in the earth, and had the same result; the wound citatrized, and the root sent out lateral divisions. Its direction.

When a root ceases of its own accord to elongate, it sends out also lateral fibres, though less vigorously and with less rapidity than in the above cases. The lateral branches of perpendicular roots are Lateral fibres.

always the more vigorous the nearer they are to the trunk, but the lateral branches of horizontal roots are the less vigorous the nearer they are to the trunk. In the former case the increased luxuriance is perhaps owing to the easy access of oxygene in the upper divisions; but in the latter case the increased luxuriance of the more distant divisions is not so easily accounted for, if it is not to be attributed to the more ample supply of nutriment which the fibres meet with as they recede from the trunk, particularly if you suppose a number of them lying horizontally and diverging like the *radii* of a circle.

Accidental deviations.

But the direction of roots is so liable to be affected by accidental causes, that there is often but little uniformity even in roots of the same species. If plants were to be sown in a soil of the same density throughout, perhaps there might be at least as much uniformity in the figure and direction of their roots, as of their branches; but this will seldom happen. For if the root is injured by the attacks of insects, or interrupted by stones, or earth of too dense a quality, it then sends out lateral branches, as in the above cases; sometimes extending also in length by following the direction of the obstacle, and sometimes ceasing to elongate, and forming a knot at the extremity. But where the soil has been loosened by digging or otherwise, the root generally extends itself to an unusual length. This Du Hamel has illustrated by the following cases:—If a trench is opened at a small distance from a young tree and immediately filled up again with loose earth, the roots which enter the trench will continue to follow its direction, and will send out but few lateral branches. And if part of the trench is filled up with earth of a superior quality, or with earth mixed with manure, the greater number of divisions will be directed to that quarter. Trees also that are planted by the banks of a river extend their branches chiefly in the direction of the river without sending out many lateral brances; where the earth is very loose the roots are generally weak, because having no obstacle to overcome they have extended to an undue length. Hence the roots of plants vegetating in pots, but especially in water, are the weakest; but where roots have some considerable obstacle to overcome they will often acquire a strength proportioned to the difficulty: sometimes they will penetrate through the hardest soil to get at a soil more nutritive, and sometimes they will insinuate their fibres into the crevices even of walls and rocks which they will burst or overturn. This of course requires much time, and does much injury to the plant. Roots consequently thrive best in a soil that is neither too loose nor too dense; but as the nourishment which the root absorbs is chiefly taken up by the extremity, so the soil is often more exhausted at some distance from the trunk than immediately around it.

Terminal fibres compared

Du Hamel regards the small fibres of the root which absorb the moisture of the soil as being to the lacteals of animals.

analogous to the lacteals of the animal system, which absorb the food digested by the stomach. But at this rate we must also regard the earth as being the stomach of plants, which analogy, as I think, will not hold good. For the root is rather to be regarded as the mouth of the plant, selecting what is useful to nourishment and rejecting what is yet in a crude and indigestible state; the larger portions of it serving also to fix the plant in the soil and to convey to the trunk the nourishment absorbed by the smaller fibres, which ascending by the tubes of the alburnum, is thus conveyed to the leaves, the digestive organs of plants.

Said to die annually like the leaves.

Du Hamel thinks that the roots of plants are furnished with preorganized germes by which they are enabled to send out lateral branches when cut, though the existence of such germes is not proved; and affirms that the extremities of the fibres of the root die annually like the leaves of the trunk and branches, and are again annually renewed; which last peculiarity Professor Wildenow affirms also to be the fact,* but without adducing any evidence by which it appears to be satisfactorily substantiated. On the contrary Mr. Knight, who has also made some observations on this subject, says, it does not appear that the terminating fibres of the roots of woody plants die annually, though those of bulbous roots are found to do so.†

* Princ. of Bot. Eng. Trans. 262. † Phil. Trans. 1809.

SUBSECTION II.

The Stem.—The stem, like the root, or at least the stem of woody plants, is also augmented in width by the addition of an annual layer, and in length by the addition of an annual shoot bursting from the terminating bud. Is the developement of the shoot issuing from the stem effected in the same manner also? The developement of the shoot from the stem is not effected in the same manner as that of the root—by additions to the extremity only—but by the intro-susception of additional particles throughout its whole extent, at least in its soft and succulent state: the longitudinal extension diminishing in proportion as the shoot acquires solidity, and ceasing entirely when the wood is perfectly formed; though often continuing at the summit after it has ceased at the base. Du Hamel divided a shoot of the Horse-chesnut into several equal parts, distinguished by coloured varnish; and on inspecting it some time afterwards, found that all the marks were removed from one another to a greater distance than at first; but on inspecting it after a second interval, he found that the upper marks only had continued to increase in distance. Hales made a number of similar experiments on shoots of the Vine, and obtained similar results; from which it seems to follow, as Du Hamel had observed, that the extension of the shoot is inversely as the indura-

Mode of augmentation.

The new shoots augmented by the intro-susception of particles throughout its whole extent;

tion, rapid while it remains herbaceous, but slow in proportion as it is converted into wood. Hence moisture and shade are the most favourable to its elongation, because they prevent or retard its induration; and hence the small cone of wood which is formed during the first year of the plant's growth increases no more after the approach of winter, neither in height nor thickness. But the plant is augmented in height by the addition of a new cone protruded from the terminating bud in the succeeding spring, that rises to a certain height above the former cone, which it invests entirely with a new layer of wood originating in the descending proper juice, and augmenting the width of the trunk, and is at last terminated by a bud which sends out a new shoot in the spring following, and so on till the tree ceases to vegetate; so that at the end of a hundred years the tree has been augmented in length by a hundred longitudinal shoots, and at the base by a hundred layers of wood, diminishing in number as you ascend; and yet the trunk is sometimes augmented in thickness by the addition of a new layer, after the shoot has ceased to elongate.*

In length by a new shoot,

In thickness by twice the annual layer.

The trunk then is annually augmented in length by the length of the terminating shoot; and in diameter by twice the thickness of the layer. If the induration of the trunk is effected slowly, then the growth of the plant is rapid; and if it is effected rapidly, then the growth of the plant is slow, as in

* Phys. des Arb. liv. iv. chap. iii.

the respective examples of the Horse-chesnut and Box, though the growth and induration of the plant are also liable to be affected both by soil and exposure.

Sometimes the one side of a shoot will remain in a state capable of extension longer than the other; and hence the tree is liable to become deformed. But gardeners correct or prevent the deformity by making a number of oblique incisions in the bark of the shoot on the side to which it is inclined, which, by occasioning an irruption of the cellular tissue, forces it back again to an erect posture.

At the junction of the root and stem, which I have denominated the collar, there is generally to be observed a sort of irregular and circular protuberance, similar to that which is occasioned by the operation of grafting. This is owing—first, to its being the point of the insertion of the seed leaves—secondly, to its being the point in which the divisions of the roots often originate, causing a deflection of the longitudinal fibres—and lastly, by the different degrees of augmentation which take place in the root and stem, the latter augmenting more than the former, and consequently occasioning a bulge. The collar.

Such is the mode of the growth and developement of the trunk of perennial and woody plants, to which there exists indeed a striking exception in the growth of the trunk of Palms. Their internal structure has been already taken notice of as pre- Growth of the trunk of Palms.

senting no concentric or divergent layers, and no medullary canal but merely an assemblage of large and woody fibres, interspersed without order in a pulp or parenchyma, softer at the centre and gradually becoming harder as it approaches the circumference; which structure they possess indeed in common with many animals. But the grand and peculiar feature by which they are distinguished from all other plants is that of the origin and mode of the annual augmentation of their stem.

When the seed of the Palm-tree germinates it protrudes a circular row of leaves, or of fronds, which crowns the radicle, and is succeeded in the following year by a similar row issuing from the centre or bosom of the former leaves, which ultimately die down to the base. This process is continued for four or five years successively without exhibiting as yet any appearance of a stem, the remaining bases of the leaves or frond forming by their union merely a sort of knob or bulb. At last, however, they constitute by their union an incipient stem, as thick the first year as it ever is after; which in the following year is augmented in height as before, and so on in succession as long as the plant lives, the leaves always issuing from the summit and crowning the stem which is a regular column, but decaying at the end of the year, and leaving circular marks at their points of insertion, which furrow the surface of the plant, and indicate the years of its growth.

SUBSECTION III.

The Branches.—The Branches, in their mode of growth and developement, exhibit nearly the same appearances as the trunk from which they issue. They originate in a bud, and form also a cone that consists of pith, wood, and bark; or rather they form a double cone. For the insertion of the branch into the trunk resembles also a cone whose base is at the circumference, and whose apex is at the centre, at least if it is formed in the first year of the plant's growth, or on the shoot of the present year; but falling short of the centre in proportion to the lateness of its formation, and number of intervening layers. Their origin and insertion.

Like the trunk and root it increases also in width by the accession of new layers, and in length by the addition of new shoots, at least in as much as regards its external portion; exhibiting however some slight peculiarities in as far as regards its insertion, the apex being never carried nearer to the centre than at the period of its first formation, and the inserted portion elongating only in consequence of the accumulation of the new layers by which the diameter of the trunk is increased. In its width, however, it increases like the external portion by the addition of new layers pervading the alburnum of the trunk, to which it is intimately united by the interplexus of their respective fibres, forming a firm They increase like the trunk.

and compact knot, as may be seen by truncating a stem immediately above or below a small branch, but particularly in the case of the Fir-tree. For the branches are not formed merely by means of a horizontal extension of the longitudinal tubes of the trunk, but are each as it were a distinct individual, of which the external cone is the trunk, and the internal cone the root. Hence the trunk is to the branch what the soil is to the plant, the source of its nourishment and stability. The branches in their developement assume almost all varieties of position from the reflected to the horizontal and upright; but the lower branches of trees are said to be generally parallel to the surface of the soil on which they grow, even though that surface should be the sloping side of a hill—owing, as it has been thought, to the evolution of a greater number of buds on the side that forms the obtuse angle with the soil, in consequence of its being exposed to the action of a greater mass of air.*

Their position.

SUBSECTION IV.

The Bud.—The Bud which in the beginning of spring is so very conspicous on the trees of this country as to be obvious to the most careless observer, is by no means common to all plants, nor to plants of all climates; shrubs in general, and annuals universally, are destitute of buds as well as

Not common to all plants.

* La Nature Devoilée. Dialog. xiv.

all plants whatever growing within the tropics, the leaf being in them immediately protruded from the bark. It is only in the woody plants of cold climates therefore that we are to look for buds, and in them no new part is added, whether proper to the leaf or flower, without the intervention of a bud. For when the young shoot is produced, it is at the same time furnished with new buds which are again extended into new shoots in the following spring; and thus the bud is to be regarded as forming not only the cradle but also the winter quarters of the shoot, for which its coat of tiled and glutinous scales seems admirably well adapted. It is found chiefly in the extremity, or on the surface of the young shoot or branch and but rarely on the stem, except it be at the collar where it produces suckers. It is also generated for the most part in the axil of the leaves, as may be seen by inspecting the annual shoot of almost any tree at random, though not universally so; for to this rule there exists a curious and singular exception in the bud of the Plane-tree, which is generated in the very centre of the base of the foot-stalk, and is not discoverable till after the fall of the leaf.

Where situated.

But how are the buds formed which are thus developed? Pliny thought they were formed from the pith, but without adducing any substantial reason.* Malpighi thought they were formed from

Their origin according to Pliny and Malpighi.

* Medulla, sive illa vitalis anima, ante se tendit longitudinem impellens, quamdiu nodi pervia patet fistula, cum vero

the pith or cellular tissue which he regarded as viscera destined for the elaboration of the sap and protrusion of future buds;* but this opinion has not been supported by subsequent observation. Du Hamel thinks the exterior scales of the bud originate in the interior part of the bark, of which they seem to be only a prolongation, and that the young branch or flower contained within the scales seems to be a prolongation of the wood and pith of the former year. And yet this opinion seems to be altogether inconsistent with an opinion which he also advances, and by which he supposes the buds of the plant to originate in what he denominates pre-organized germes, existing in the proper juice, and deposited by it in its descent so as to pervade the whole of the plant. If these germes are understood to be the result of the agency of the vital principle, their existence is not impossible; though it must, at the same time, be acknowledged that it is by no means proved. Perhaps the opinion arose from the facility with which buds are protruded in given circumstances, in almost any part of the plant. If a branch is lopped, or if the stem is truncated, new buds containing the rudiments of new shoots will soon after make their appearance near the section; so that they seem to be dispersed without number throughout the whole extent of the plant.

repercussa juxta nodos, hoc vocatur in vite gemma. Nat. Hist. liv. xvii. chap. 21.

* Anat. Plant. 13.

But the Fir-tree will send out no bud at all if cut down near to the root. It may be said that this is merely the exception to the rule; but we cannot, after all, place much reliance on the doctrine of pre-organized germes.

Mr. Knight relates an experiment from which he thinks it follows that the buds are formed from the descending proper juice. He intersected the runners connecting the tubers of a potatoe with the stem, and immersed both portions in a decoction of logwood. The decoction passed along in both directions, but did not enter the stem, because in that direction the communication is kept up only by the bark through which the proper juice descends from the leaves, and which admits not coloured infusions: but in the opposite direction it was found that the infusion had passed through an elaborate assemblage of vessels between the bark and alburnum, the ramifications of which were seen to approach the skin at the base of the buds, to which they were thought to convey nourishment.* But allowing the experiment to be correct, it does not prove that buds are formed from the proper juice but only nourished by it; as the experiment must have given precisely the same result if the buds had proceeded from the pre-organized germes of Du Hamel. According to Mr. Knight.

But whatever may be the actual origin of the bud, it is evident that its developement does not take place except through the medium of the proper

* Phil. Trans. 1803.

juice, which has been elaborated in the leaves of preceding buds, and originally in those of the plumelet; as the young bud does not make its appearance till the leaves of the preceding buds have expanded, and will not ultimately succeed if deprived of them too soon.

But from the period of its first formation during the course of that summer to that of its final expansion in the following spring it continues gradually and constantly to augment in size; its progress being visible by dissection even in the course of the winter, and accelerated as the spring advances, till at last its bonds are loosened and the scales expanded, protruding both leaf and flower.

Bulbs.

Bulbs are so very similar to buds both in their origin and developement as to require no specific investigation. The parent bulb produces an offset analogous to the bud, which attains to maturity about the time of the maturity of the flower, and which finally detaches itself and forms a new individual; in which last property it differs essentially from the bud, which does not detach itself spontaneously, and can but rarely be made to vegetate if detached by art.

SUBSECTION V.

How augmented.

The Leaf.—When the leaves burst from the expanding bud, and even long before that period, as may be seen by the dissection of the bud in the winter, they are complete in all their parts—all the nerves, and all the indentations of the margin, being distinctly perceptible, at least by the assistance of a good glass, together with the fibres, or bundles of fibres connecting the branch and foot-stalk, and thus presenting in the aggregate a miniature representation of the future leaf. Hence it is obvious that the leaf, like the young shoot, effects its final developement by means of the intro-susception of new particles throughout the whole of its dimensions: and yet this law of developement is not common to all leaves whatever, for the leaves of liliaceous plants extend chiefly at the point of their junction with the bulb. This fact was ascertained by Du Hamel by means of graduating the leaves of the Hyacinth with transverse lines of coloured varnish; the lines near the point of the leaf maintained their original and relative distances, but the lines below were removed to a considerable distance, and the nearer the bulb the distance was the greater. Perhaps this peculiarity of developement is the effect of their peculiarity of structure, in being formed of parallel tubes which extend throughout their whole length, without those transverse and branching fibres that constitute what are called the nerves of the leaves of woody plants.

SUBSECTION VI.

The Flower and Fruit.—When the flower bursts from the expanding bud, and even long before that

Complete in all its parts long

before its evolution.

period, it is already complete in all its parts, as may be seen also by the dissection of the bud in winter. Du Hamel, who dissected the bud of a Pear-tree in the month of January describes it as follows:—The scales were from twenty-five to thirty in number, enveloping from eight to ten flowers, attached to a common foot-stalk of half a line in length; the flowers resembled rose-buds set with hairs; the stamens were distinguished with ease, together with their anthers which were white; the petals were distinguished with some difficulty; but the pistils were not yet to be discerned. In the month following the pistils were now discernible, and the anthers had begun to assume a reddish tinge; the ovary was not perceptible at this early period, but it was perceptible before the evolution of the bud.* Hence, as Grew had before observed, the flowers which are protuded in the spring have been actually formed in the preceding year, being generally of a whitish colour till toward the time of their expansion, when the several parts begin to assume their own peculiar shade, and to exhibit their essential traits of character. The calyx and corolla exhibit a structure similar to that of the leaf. The stamens consist merely of a parenchyma enveloped by a fine epidermis, but the filaments are sometimes tubular, as in the Tulip, and sometimes furnished with spiral threads, according to Senebier;† though I have not

* Phys. des Arb. liv. iii. chap. i.

† Phys. Veg. vol, ii. p. 60.

been able to discover them in any specimens I have yet examined. The pollen is now capable of being distinguished into three parts, according to the description of Gærtner—a cuticle, a cellular tissue, and a parenchyma;—and the pistil, which is at first merely a gelatinous mass, begins now to be distinguishable into germen, style, and stigma, the germen being the Linnæan name for what Malpighi calls the uterus, and Gærtner the ovarium.

Developement of the ovary. The ovary in its first stage of growth exhibits also the appearance of a homogeneous mass of parenchyma, without any division into distinct parts; but in a more advanced state it exhibits also the rudiments of distinct organs, and finally the embryo occupying the centre.

Style. The style, which is not a constant part of the pistil, originates generally in the substance of the ovary, and sometimes though rarely in the receptacle; as in leguminous, malvaceous, and rough-leaved plants. It agrees in its fabric and texture with the ovary or receptacle, being merely an extension of the one or the other; its vessels communicating with the ovary from which it ascends, and containing a fluid which occasionally exudes and moistens the surface of the stigma.

Stigma. The stigma, which according to Gærtner is present in the flower of all plants except *Aphrodites,* originates in the style if the style is present, and if not, in the upper extremity of the ovary; assuming generally some peculiar figure, and being sometimes smooth and sometimes hispid, but always beset with a number of pores or papillæ through which the moistening fluid exudes.

Origin of the several parts according to Linnæus,

Such is the order of the developement of the several parts of the flower, concerning the origin of which there have been several different opinions. Linnæus represents the pistil as originating in the pith, the stamens in the wood, and the corolla and calyx in the inner and outer bark respectively: but this account of their origin though extremely plausible at first sight, will not bear the test of minute examination, being contradicted by the anatomy of the parts themselves; particularly in the case of compound flowers. But with all its imperfections it seems to have obtained at least a partial and temporary celebrity, and to have been adopted in substance by Hill, who refined upon it indeed very considerably, describing the flower-cup as originating in the outer bark; the petals in the rind and blea (alburnum); the nectaries in the vascular series; the filaments in the flesh; the receptacle in the conic clusters; and the seeds and capsule in the pith; and thus amusing the reader with the arrangements of his own fancy instead of the arrangements of the Divine Mind.

Gærtner,

Gærtner regards the ovary as proceeding from the wood and bark, in superior flowers; and from the receptacle in inferior flowers.

Knight.

Mr. Knight in investigating the organization of the Apple and Pear endeavoured to ascertain the origin of the several parts by tracing the several parts of the fruit-stalk to their termination. In the fruit-stalk he thought he could discover the pith, the central tubes, spiral tubes, and tubes of the bark, together with its epidermis: and in tracing them to their termination he thought the pith seemed to end in the pistils; the central vessels in the stamens, after diverging round the core and approaching again in the eye of the fruit; and the bark and epidermis, in the two external skins.* Hence he infers that the flower is a prolongation of the pith, wood, and bark in nearly the same way as Linnæus, though he adduces arguments from dissection with which Linnæus was not acquainted. But although central vessels are found in the stamens, it is no proof that the stamens are a prolongation of the wood, unless the central vessels of the fruit-stalk and common tubes of the alburnum are proved to be one and the same, which remains yet to be done. It seems also doubtful whether the fruit-stalk contains any thing that can be absolutely regarded as pith; and it is evident from a very little inspection that the two external skins of the Apple are not very well accounted for by deriving them from the bark and epidermis of the fruit-stalk.

Nourishment of the flower and fruit.

But another question of some considerable importance has arisen out of this subject: does the flower or fruit elaborate sap for its own developement, or is it supplied with nourishment from the leaf?

* Phil. Trans. 1801.

By placing small branches of the Apple, Pear, and Vine with blossoms not expanded in a decoction of logwood, Mr. Knight found that the central vessels were coloured by the decoction. By means of a similar experiment on the same subjects after the fruit was formed, the colouring matter was traced through the mass of the fruit to the base of the stamina.* And hence it appears to me that the flower and fruit do possess the power of elaborating sap for their own developement; since it seems that the sap ascends to them only in an unelaborated state, as is to be inferred from the ascent of the coloured decoction, which tubes conveying elaborated sap do not seem capable of admitting.

Inferences of Mr. Knight.

Mr. Knight infers, however, from the foregoing data that the blossom is nourished from the alburnum,† by means, as I should suppose, of the mingling of the proper juice which the alburnum may be supposed to contain with the sap in its ascent. There may perhaps be something of truth in this remark; but it is to be observed that most blossoms are accompanied with a calyx, which may be supposed from its similarity to the leaf to perform to the flower or fructification similar functions: and so are the petals themselves similar in structure to the leaves, and may perhaps be capable of performing similar functions. It may be objected, however, to this opinion that when the leaves are by any accident stripped off or destroyed,

* Phil. Trans. 1805. † Ibid. 1805.

the fruit does not come to maturity; but still the calyx or corolla may perform some peculiar and indispensable function to such flowers as are furnished with them: and there are also plants in which the flower is completely developed before ever the leaves expand; as in the case of *Daphne Mezereon* and the Apricot, which seems to imply that they are capable of elaborating the sap necessary to their own developement.

But the office of the tubes of the bark does not seem to have been ascertained in the fruit-stalk, though Mr. Knight thinks it cannot be the same with that of the tubes of the leaf-stalk—namely, the conducting of the returning proper juice for the purpose of forming new parts below; and this he thinks he has proved by the following experiment:—When the end of a shoot of the Vine immediately above a bunch of grapes was pinched off as soon as it had made its appearance, and the leaf opposite allowed to remain, the wood below increased as usual; but when the leaf opposite was taken off also, then the wood below ceased to elongate, and remained in form and substance similar to the fruit-stalk. Hence Mr. Knight concludes that the tubes of the bark do not in the fruit-stalk conduct a fluid downwards that is capable of forming wood; and yet, as it is likely that the motion of the tubes of the bark is in all cases retrograde, he supposes that the function of the tubes of the bark of the fruit-stalk may be that of carrying off from the fruit any superfluous humours formed in it from excess of humidity, or other causes.* It must be confessed that this is but a very clumsy contrivance for the carrying off of superfluous humours, which might be much more easily got rid of by means of transpiration; though it must, at the same time, be admitted that we are but bad judges of the facility with which nature effects her operations. But it appears from an experiment of Mr. Knight's that the fruit and fruit-stalk do actually generate wood in certain circumstances, for he says expressly that he succeeded at last in grafting the fruit-stalk of the Vine on the leaf-stalk; in which case the fruit or fruit-stalk must have formed wood.†

But how are these two contradictory experiments to be reconciled? Perhaps in the natural process of vegetation there is but little juice returned by the bark of the fruit-stalk; while in the case of the graft it might have been an extraordinary effort of the vital principle by which the part grafted was adapting itself to the circumstances in which it was placed.

CHAPTER V.

ANOMALIES OF VEGETABLE DEVELOPEMENT.

In the growth of the vegetable subject as well as in that of the animal, it often happens that a deviation from the general laws of developement is occa-

* Phil. Trans. 1801. † Ibid. 1803.

sioned by the intervention of some accidental cause; or of some cause operating permanently in certain subjects. Hence the anomaly may regard the developement either of an individual or a species, and may occur either in the root, stem, branch, leaf, bud, flower, or fruit, according to the circumstances in which it is placed; or it may affect the habit, duration or physical virtues of the plant.

SECTION I.

The Root.

According to the general laws of vegetable developement, plants of the same species are furnished with the same species of root—not producing at one time a woody or fibrous root, and at another time a bulbous root. And yet it is found that there are cases in which changes of this kind do occur.

The fox-tail root.

If part of the root of a tree planted by a pond or river is accidentally laid bare on the side next the water, or if in the regular course of its growth it protrudes beyond the bank, so as to be now partially immersed, the future developement of the part is considerably affected; for the root which was formerly firm and woody instead of augmenting in the regular way—that is, by the accession of new layers insinuated between the wood and bark enlarging the individual mass, divides now at the extremity into innumerable ramifications, or sends out innumerable fibres from the surface, which become again subdivided into fibres still more minute, and give to the whole an appearance something resembling that of the tail of a fox; which has accordingly been denominated by Du Hamel the fox-tail root. (*Pl. IX. Fig.* 5.) This anomaly I have frequently observed in the root of Willows growing by ponds, of which the main offset has been about eighteen inches in length, and the terminal and lateral subdivisions six or eight inches. Du Hamel relates an example of the same anomaly, which he had observed in the case of a root that had insinuated itself into a water-pipe, where it increased by the sending out of a prodigious number of small fibres, till at last it occupied the whole diameter of the pipe and stopped the current of the water.* Perhaps the above anomaly is merely the result of an extraordinary effort of the vital principle to adapt itself to the circumstances in which it is placed, by extending the surface and multiplying the subdivisions of the root, for the purpose of the more easy abstraction of the oxygene of the water.

Roots that from being fibrous become bulbous. But sometimes an anomaly takes place which is directly the reverse of the above. The *Phleum pratense* when growing in a moist soil, which it naturally affects, is uniformly furnished with a fibrous root; but when growing in a dry soil, where it is also often to be found, it is furnished with a bulbous root. The same is the case also with the *Alope-*

* Phys. des Arb. liv. i. chap. v.

curus geniculatus; which, when growing in its native marshes, protrudes a fibrous root, though when growing in a very dry situation, as on the top of a dry wall, it is found to be furnished with an ovate and juicy bulb.* This anomaly also seems to be merely the result of a provision of nature by which the plant is endowed with the capacity of collecting a supply of moisture suited to existing circumstances, and hence of adapting itself to the soil in which it grows.

Bladder, bearing roots. The roots of *Utricularia minor,* which consist of a number of slender and hair-like filaments exhibit the singular anomaly of being furnished with a multitude of small and membraneous bladders, each containing a transparent and watery fluid, and a small bubble of air, by means of which the plant is kept floating in the water.

The descending root. Some perennials present the anomaly of what has been called the descending root, which is at first spindle-shaped and perpendicular sending out some lateral fibres; but dying at the lower extremity in the course of the succeeding winter, and protruding new fibres from the remaining portion, and even from the lower portion of the stem, in the course of the following spring, which by descending into the soil, draw down the plant with them, so that part of what was formerly stem is now converted into root. This process is repeated every year, and by consequence a portion of the

* Smith's Introduction, p. 113.

stem is made to descend every year into the earth. The anomaly may be exemplified in the roots of *Valeriana dioica, Tanacetum vulgare,* and *Oxalis acetosella;* and will also account for the bitten and truncated appearance of *Scabiosa succisa* or Devil's-bit.

Anomaly of Beet-root.

The Beet root, a biennial plant, if dissected when about a year old, presents the singular anomaly of being already furnished with from five to eight distinct and concentric circles of longitudinal tubes or sap vessels, imbedded at regular intervals in its pulp; whereas other biennial roots form only an individual circle each year, and are, consequently at no time furnished with more than two.*

Migratory roots.

There are also some roots that may be called migratory, upon a principle similar to the foregoing. If the stem of a descending root happens to be creeping or procumbent instead of being erect, then the lateral shoots from above are carried forward in the direction of that procumbency, so that in the course of a few years the plant has actually changed its place by so much as the stem has been converted into a root. This is well exemplified in the genus Iris. But the migratory plant is perhaps best exemplified in the case of some aquatics, which have actually no fixed habitat, but float about on the surface of the water as they happen to be driven by the winds, as in the case of the genus *Lemna* and some marine plants.

* Willdenow, p. 260.

But one of the most curious and singular anomalies throughout the whole of the vegetable kingdom is that by which a plant may be made to grow though inverted, the root being transformed into a stem and branches; and the stem and branches into a root. If the stem of a young Plum or Cherry-tree, but particularly of a Willow, is taken in the autumn and bent so as that one half of the top may be laid in the earth, one half of the root being at the same time taken carefully out, but sheltered at first from the cold and then gradually exposed to it, and the remaining part of the top and root subjected to the same process in the following year; the branches of the top will become roots, and the ramifications of the root will become branches, protruding leaves, flowers, and fruit in due season. Inverted roots.

But it has been already seen in treating of the germination of the seed, that no power or art is capable of converting the radicle into the plumelet; or the plumelet into the radicle. How then is the anomaly of the inversion of the plant to be accounted for, at a future stage of its growth? Perhaps it may be accounted for thus. The embryo of the seed is an individual germe, whose developement is necessarily effected in a determinate manner, owing to its peculiar structure and organization. But that happens to be by the descent of the radicle into the earth, and ascent of the plumelet into the air. It could not, therefore, succeed by being inverted, because the plumelet contains as yet no vegetative principle whose developement could be effected by being placed in the earth. But this is not the case with the inverted plant; because its leaves or branches contain buds or germes that have been acquired in the process of vegetation. But these germes are plants in miniature, containing the rudiments of every thing necessary to the perfection of the species. Consequently they contain a part equivalent to the radicle of the embryo, and capable of being converted into a root when placed in a proper situation. Now the earth affords them that situation, and the inverted plant grows.

SECTION II.

The Stem.

The radicating stem.

If the stem of a tree planted by a pond or river is so bent in its growth as to come near to the surface of the water and to be occasionally immersed in it, it will sometimes send out from the under surface a multitude of shoots that will descend into the water and develope themselves in the manner of the Fox-tail-root. They are often rendered conspicuous in the summer by means of the subsiding of the water from the under surface of a stem that may have been partially immersed in the winter, such as that of Willows overhanging ponds or ditches, and are produced, no doubt, by the agency of the same cause that gives a similar figure to the root.

Sometimes it happens that a stem instead of assuming the cylindrical form common to the species, assumes a compressed and flattened form similar to the herbage of the *Cactus*. Of this anomaly I have occasionally observed a specimen in the stem of the *Tamus communis*, which from a cylinder of about a quarter of an inch in diameter, its natural size and shape, was converted into a flattened and oblong production of about an inch in breadth. But the best specimen of the anomaly I have ever met with, was in the case of the stem of an Ash-tree (*Pl.* IX. *Fig.* 6). The tree stood in a hedge row in the parish of Stow Upland, Suffolk, and in the autumn of 1809, seemed to be about twelve or fifteen years of age, or at any rate to be about twelve or fifteen feet in height. Of this tree the top and perpendicular shoot which had in the preceding summer extended to the length of twenty inches, was compressed into a flattened and oblong production, fluted on both sides as well as furnished with some buds, and of about an inch in breadth, but expanding at the summit to the breadth of nearly two inches, and surmounted with a row of buds of between twenty and thirty in number; the shoot of the preceding year having been cylindrical and now measuring about half an inch in diameter.

The flattened stem.

Accounted for.

Du Hamel accounts for the anomaly of the flattened stem by supposing that an unnatural graft must have taken place in the leaf bud; and so united shoots that would otherwise have been distinct. But if shoots should be thus united by means of an unnatural graft, why should they be compressed or flattened in their aggregate growth.

Affected by tumors or bunches.

Sometimes the stem is disfigured by accidental tumors or bunches projecting from the surface, and forming ultimately what are called knots in the wood. They are very common in the Oak and Elm, and are produced perhaps by means of some obstruction in the channel of the sap's motion, by which the vessels become convoluted and swell up into a bunch.

But bunches are also to be met with on the stem of herbaceous plants, as on that of the *Carduus pratensis;* of which you will often find a portion near the top swollen out into an egg-shaped or egg oblong bunch extending from an inch to two inches in length and about an inch across. If this bunch is cut open in the month of August, it will be found to contain several large and white maggots. It has consequently been occasioned by the puncture of the parent insect depositing its eggs; but it does not seem to affect the general health of the plant. Sometimes a number of trees growing together are affected with a longitudinal protuberance all on the same side. This Du Hamel attributes to a *Coup de soleil vif*, or to frost. Sometimes the bark of the stem becomes rough and scabby and the wood underneath full of knots and inequalities, this Du Hamel attributes to frost also.

The fasciculated stem.

Sometimes two or more contiguous stems, extending in the process of their growth till they meet and press against one another, become incorporated at length into one, and form a sort of bundle. This is what may be termed a natural graft, in opposition to an artificial graft, of which it is the model and prototype, the whole of the art of grafting being founded upon the capacity inherent in plants of uniting together by the stem, in given circumstances, and in a given mode. But the natural graft is always affected by means of the union of the liber of the respective stems composing it; so that the perfection of the art of grafting consists in applying the liber of the graft and stock together in such a manner as shall the most facilitate their incorporation. And hence the graft will not succeed unless the two libers are brought into contact, and closely bound together. Nor will it succeed well unless the plants ingrafted have some natural affinity to one another, such as that subsisting between the Plum, and Cherry; in which, and in all other cases, the union is effected by means of a granular and herbaceous substance exuding from between the wood and bark, and binding and cementing together the stock and graft; though not uniting the former layers of wood. But after the graft has been effected, the new layers of wood are produced entire as before so that it is sometimes difficult to point out the place of the graft, which is generally discoverable, however, by means of a tumor that is formed round it.

SECTION III.

The Branch.

Producing brush-like shoots. IF the branch of a tree is situated as in the foregoing case of the stem, so as to be partially or periodically immersed in water, it will send out also the same sort of brush-like shoots. Like the stem it is also liable to be disfigured by bunches or knots; exhibiting, however, an occasional variety of structure which I have not observed in those of the stem. The variety to which I allude seems as if formed from a plexus of young shoots issuing from nearly the same point, and crossing in all directions, and finally incorporating together by means of a sort of natural graft. Or perhaps the knot is first formed, and then sends out a multitude of shoots all over its surface, forming a batch interwoven all together, and exhibiting at a little distance something like the appearance of a pigeon's nest. With witches' knots. These bunches are frequently to be met with on the branches of the Birch-tree, rarely on the Slow-thorn (*Pl.* IX. *Fig.* 7), and are known among the peasantry of Scotland by the name of witches' knots. They are occasioned, like the bunches of the stem, by some obstruction in the channel of the sap or proper juice. A peculiar sort of knot or bunch is also often formed on the branches of the Dog-rose (*Pl.* IX. *Fig.* 9). The nucleus, which is generally from an inch to an inch and a half in diameter, is covered with a long and winged shag, first of a green and then of a purple colour, presenting the appearance of a small bunch of moss. It has been occasioned like that of the stem of the thistle, by the puncture of an insect depositing its eggs in the tender shoot; for if it is cut open about the month of August, it contains maggots.

With moss-like bunches.

SECTION IV.

The Bud.

THE regular developement of the bud is also often prevented by means of the puncture of insects, and converted into a large globular tumor. This is very often effected by a species of Cynips that lances its piercer into the heart of the bud while yet tender, and penetrates with its saw into the very pith; injecting at the same time a drop of the corroding liquor contained in its bag, and then laying its egg. The bud being thus wounded, and the juices corrupted by the injected poison, the circulation is not only impeded, but a fermentation is induced which burns the contiguous parts and changes their colour. The extravasated juice flows

Forming galls.

round the egg and is there accumulated and converted into a sort of spongy lump which vegetates and augments till it forms what is called a gall. The gall thus formed affords both shelter and nourishment to the young maggot, which after being converted into a fly pierces its enclosure and launches into the open air.

Oak apples.

The most remarkable of such galls are those produced on the Oak-tree, and known in this country by the vulgar name of Oak-apples; of the origin and growth of which I think Malpighi gives a detailed account, but of which I cannot now offer any abstract to the reader, not being at present furnished with a copy of his works. The following are some observations which may in the mean time serve as a substitute. About the end of May, 1808, having observed upon an Oak-tree some of the galls in question (*Pl.* IX. *Fig.* 8.) I had some of them gathered for the purpose of examination. The largest was then about the size of a Golden Pippin, soft and spongy to the touch, and covered with a fine and glossy epidermis of a white colour, but changing in some places to red, and hence not much belying in appearance its vulgar name. At its base it was furnished with a number of scales or leaves resembling a calyx, which proved upon examination to be the outer scales of the original bud. On cutting the gall open whether by a longitudinal or transverse section, a number of oval or cylindrical bodies of a whitish colour were found to be imbedded in its centre. They were the eggs of the insect by which the bud had been punctured. But on some trees of the same species there was found a gall of a very different aspect, which, though nearly of the same size, was covered with a long and white shag, and did not exhibit the same fleshy texture when cut open. It was occasioned, however, in the same manner; the eggs of the insect, which was no doubt of a different species, being crowded together in the centre like a cluster of small seeds, united by the lower extremity, and covered with the wool. Having cut open some of both sorts about the end of the month of June following, the maggots were now distinguishable in the former by the aid of the microscope, complete in all their parts; and in the latter each egg was found to contain a fly. On the extremity of some of the branches a few fragments of galls of the former sort were still to be found, which seemed to have stood from the preceding summer, and in which the holes or perforations were still to be seen through which the maggots or flies had escaped. The fragments were quite charred by means of the action of the atmosphere.

Deformities of the Willow bud.

The bud of the Willow, particularly *Salix Helix*,* is apt also to be punctured by insects and converted into a gall. But the conversion is not always complete; and in this case the shoot remains dwarfish, and the leaves which are now protruded

* Smith's Introduction, p. 346.

from nearly the same point assume something of the figure of a rose. Hence it has obtained the common name of the Rose Willow; and so also in the case of the Lime-tree; the bud is often punctured, and the egg deposited, and the gall formed into a round and fleshy substance about the size of a garden Pea, with a good deal of complexion on the side exposed to the sun, while the apex is yet crowned with the rudiments of a leaf or leaves.

The galls of the *Salvia pomifera* formed in the above manner are said to be of a very pleasant flavour, and are esteemed a great delicacy in eastern countries.*

SECTION V.

The Leaf.

Nut galls. THE leaves, like the buds, are also frequently chosen for the nidus of insects, and disfigured with galls or excrescences. But the most remarkable gall produced on the leaf, and indeed the most remarkable and important of all galls, is that which is so extremely useful in the arts of dyeing and making ink, the nut-gall of the shops. It is generated on the leaf of a species of Oak that grows plentifully in the Levant, and is so well known in commerce as to require no particular description. It is occasioned by the puncture of the *Cynips querci folii*, which deposits its egg in the substance

* Willdenow, p. 346.

of the leaf, by making a small perforation on the under surface.* So also various other excrescences are generated on the leaves of Oaks of other species. If the leaves of *Quercus Robur*, or the common Oak of this country, are inspected in the beginning of summer they will often be found to be disfigured by a small purple coloured excrescence, about the size of a Whortle-berry, partly imbedded in the parenchyma, but chiefly swelling above the surface. Having cut open one of these excrescences in the month of May I found it to consist of a white and glary fluid. And on inspecting some others in the month of June following, nothing now remained but the shrunk and withered bag in which the fluid was contained. I had not an opportunity of observing the intermediate stages; but it had undoubtedly been the nidus of some species of insect.

Galls of the Willow-leaf.

On the leaf of *Salix alba* there is often also to be found about the month of June an oblong and glandular tumor, sometimes at the apex, and sometimes about the middle region, of about the size of a Pea, assuming a reddish appearance with age, and a villous exterior, which if cut open is found to envelope a single maggot.

And Clinopodium vulgare.

On the leaf of the *Clinopodium vulgare* I have also found in the month of June a sort of gall or excrescence of about the size of a Kidney Bean, but larger at the one end, of a deep purple, and

* Withering, vol. ii. p. 388.

covered with a hoary down. The skin was easily separated from the nucleus, which when cut open was found to be of a firm and solid consistence containing a maggot.

Almost all leaves indeed are liable to similar deformities arising from similar causes, giving them a blistered, wrinkled, or curled appearance; and often producing disease. But sometimes the anomaly consists in the excess or deficiency of the usual number of leaves protruded in a group. Thus in the case of the Trefoils, in which the leaves are regularly protruded in trees, you will often find them protruded in sets of four, five, or even six. This anomaly is often to be met with at least in the case of the commonly cultivated Clover, in which you will sometimes find also a set consisting but of a single pair.

Anomalies of figure.

But the anomaly may also consist even in the natural figure of the leaf itself. Most leaves exhibit in their general aspect a sort of compressed and flattened surface, whatever may be their specific figure; but to this rule there exists a variety of exceptions. The leaves of *Asparagus officinalis* are bristle-shaped; the leaves of *Salsola Kali* are awl-shaped; and the leaves of *Allium Cepa* are tubular, tapering to a point. But one of the most remarkable anomalies of figure is that which occurs in the leaves of the genus *Sarracenia*, of which the lower portion is tubular, ascending and approaching to funnel-shaped, or rather pitcher-

Exemplified in the genus Sarracenia.

shaped reversed, with a flattened and concave limb attached by the one side to the orifice of the tube, and constituting the upper portion of the leaf. Linnæus, who was acquainted with this singularity of structure, accounted for it by supposing that it was an institution of nature, meant for the purpose of furnishing the plant with a supply of water, which it could thus catch and retain in the leaf. But as some species of the genus do not readily admit water notwithstanding their capacity to retain it, this hypothesis is regarded by Dr. Smith as being extremely doubtful, who accordingly offers a different solution founded upon the following facts.

An insect of the *Sphex* or *Ichneumon* kind had been observed by one of the gardeners of the botanic garden at Liverpool, to drag several large flies to a leaf of *Sarracenia adunca*, and to force them into the tubular part of it. On examination the leaf was found to be about half filled with water, in which the flies were now struggling; the other leaves were also examined, and were found crammed with dead or drowning flies. The leaves of *Sarracenia purpurea* are said to exhibit also the same phenomena, and seem peculiarly well adapted to entrap and confine flies, by having the margin beset with inverted hairs rendering the escape of such insects as may have accidentally fallen into the watery tube, or are intentionally forced into it, impracticable; so that the putrid exhalation from the dead insects contained in the leaf often offends the nostrils, even in passing near the plant. Hence Sir J. E. Smith infers that the growth of the plant is perhaps benefited by means of the air evolved by the dead flies, which the water has been intended to tempt, and the leaves to entrap and retain.* This ingenious conjecture is no doubt sufficiently plausible as far as the plant may be affected; but cannot be regarded as quite satisfactory till such time as it shall have been shown that the health of the plant is injured when insects are prevented from approaching it.

And Nepenthes distillatoria.

The celebrated *Nepenthes distillatoria* exhibits also an anomaly similar to that of *Sarracenia*, but more striking if possible. The leaf, which is itself lanceolate, terminates at the summit in a thread-shaped pedicle supporting a pitcher-shaped process, surmounted with a lid, and holding an ounce or two of a fluid which appears to be secreted from the leaf, and to be intended as a lure to insects, which gain admission either by the spontaneous opening of the lid, or by forcibly raising it themselves. The consequence is that they fall into the fluid and are drowned, no insect being capable of living in it except a certain small squilla or shrimp with a protuberant back, which, according to Rumphius, sometimes crawls into it and can live there.† To this phenomenon Sir J. E. Smith applies the same explication as above, which is of course liable to the same objection.

* Smith's Introduction, p. 196. † Ibid. p. 197.

But the figure of the leaf, however singular, is generally the same throughout the same individual, except in the case of accidental deformity, and yet there are exceptions even to this rule. For sometimes the lower leaves of a plant are entire while the upper leaves are divided, as occurs in a variety of mountainous plants, such as Barnet, Saxifrage, Anise, Coriander; and sometimes the lower leaves are divided while the upper leaves are entire, as in the case of a variety of aquatics, particularly *Ranunculus aquaticus*, in which the lower leaves are capillary and immersed, and the upper leaves flat and circular, floating on the surface of the water. But sometimes the dissimilitude of the leaves is still more remarkable. The Chinese Mulberry has not two leaves alike in form on the whole plant. And lastly, there are some plants, as in the case of the *Fungi*, that are wholly destitute of leaves, and hence called aphyllous; while there are others, as in the case of the *Fuci*, that seem to be wholly leaf.

SECTION VI.

The Flower.

THE principal anomaly relative to the flower is that by which one of its parts is unduly augmented, to the exclusion or diminution of some of the rest. The flower is then said to be luxuriant, and comprises the three following varieties:—the multiplicate, the full, and the proliferous flower.

The Multiplicate flower.

The multiplicate flower is sometimes, though rarely, accasioned by an unusual multiplication of the divisions of the calyx, as exemplified by Linnæus in *Dianthus Caryophyllus* and some of the Alpine Grasses.* But the anomaly most generally consists in the undue multiplication of the divisions of the corolla, by the conversion of part of the stamens into petals, which is occasionally to be met with both in monopetalous and polypetalous flowers. It occurs but seldom however in flowers growing in their natural state and habitat, though you will now and then meet with a double flower even in such circumstances. I have met with several specimens of the *Ranunculus Acris* in which the corolla consisted of a double row of petals, even when growing wild in the fields; but double flowers are for the most part the effect, and often also, the object of cultivation.

The following is a species of multiplicate flower that does not come under any of the foregoing cases, though it is perhaps not altogether a solitary example; it is that of an individual flower of *Primula veris*, containing two ovaries, two styles, two stigmas, and eight anthers complete, with the rudiments of a ninth, and the calyx and corolla divided into nine segments. It was gathered by

* Phil. Bot. 80.

Mrs. Keith in the church-yard of Stow Maries, near Maldon, Essex, on the 10th of May, 1812.

The full flower is generally described to be that in which the divisions of the corolla are so multiplied as to exclude the stamens and pistils wholly, by means of their conversion into petals; which conversion is most readily effected in polypetalous flowers, such as the Tulip, Poppy, Pink, and *Ranunculus;* monopetalous flowers seldom being found full. This complete metamorphose is, I believe, always the effect of cultivation, and is indeed, one of the principal objects of the art of the florist; the beauty of the flower, according to general estimation, being thus much augmented. In the full flower the stamens are always converted into petals, whence we should perhaps infer their identity of origin. But the pistil is often converted into a leaf, as may be seen by inspecting the flower of the double blossomed Cherry, which generally protrudes from the centre, a leaf in miniature. But a flower may become full also by the multiplication of the parts of the nectary, as is sometimes the case in the genus *Aquilegia*, which produces full flowers in three different ways,* by the multiplication of the petals to the exclusion of the nectaries, by the multiplication of the nectaries to the exclusion of the petals, and by the multiplication of the nectaries while the proper petals remain. There are also some peculiarities in the manner in which com- The full flower.

* Phil. Bot. 80.

pound flowers become full. Radiated flowers become full sometimes by the multiplication of the floscules of the ray to the exclusion of the floscules of the disk, as in *Helianthus*, *Anthemis*, and *Centaurea*; and sometimes by the multiplication of the floscules of the disk to the exclusion of those of the ray, as in *Matricaria* and *Bellis*.*

The proliferous flower.

The proliferous flower is that out of which another flower or another shoot is produced. It is seldom found but in flowers already full; from the centre of which, that is, from the ovary or pistil, it sometimes happens that a new flower and foot-stalk is produced if the flower is simple, as in the *Ranunculus*, *Anemone*, and Pink; or several flowers and foot-stalks, issuing from the common calyx, if the flower is compound, as in the Daisy (*Pl.* IX. *Fig.* 11.), Hawkweed and Marigold; or a new umbel issuing from the centre of the original umbel, if the flower is umbellate, as in *Cornus*. Sometimes the proliferous issue of the full flower is not itself a flower, but a shoot furnished with leaves, as has been sometimes, though rarely, observed in the case of the Anemone and Rose.† And hence we should perhaps infer, with Du Hamel, the identity of the origin of the pistil and woody shoot, of which he thinks the bundles of woody fibres found in the fruit of the Pear is also a presumptive proof.‡

Such are the several varieties of luxuriant flowers, constituting anomalies of excess; but it sometimes

* Phil. Bot. 84. † Ibid. 82. ‡ Phys. des Arb. liv. iii. chap. ii.

happens that there is also in the flower an anomaly of defect in the absence of one of its parts. Examples of this sort are occasionally to be met with in the flowers of *Campanula pentagonia* and *Tussilago Anandria,* in which the corolla is altogether wanting, though proper to the species; and in this case the flower is said to be mutilated. Anomalies of defect.

Sometimes the anomaly consists in the situation of the flower, which is generally protruded from the extremity or sides of the branches. But the flower of the *Ruscus* is protruded from the surface of the leaf; or it may consist in the relative situation of the several parts of the flower. In simple flowers the pistil is invariably central with regard to the stamens; but in compound flowers the pistils are often situated in the circumference and the stamens in the centre. This seems to be the case also with some monœcious plants having their flowers on the same peduncle, as in the examples of the *Carex* and *Arum,* in which the stamens are more central than the pistils.

Sometimes the anomaly consists in the colour of the corolla, which will often deviate even in the same species. The general colour of the common Cowslip, *Primula veris,* is a bright yellow; but an individual is occasionally to be met with, though very rarely, in which the limb or expansion of the corolla is purple with a line of yellow around the border. A Cowslip answering to this description was gathered by the Lady Francis Douglas, at Of colour.

Goldsborough Hall near Knaresborough, Yorkshire, in the spring of 1800. I have met also with a similar anomaly in the flower of the Oxlip, and in various other flowers.

In the season of flowering.

Sometimes the anomaly consists in the time of flowering. The season proper for the flowering of the Apple and Pear-tree is the month of May; but trees of that sort have been known to protrude both bud and blossom even in the month of November. Some plants, however, blow only in the winter, as is the case of the *Laurus Tinus* and *Arbutus Unedo;* while others blow only in the night, and refuse to expand their petals to the light of the sun. Such is the case of the *Cactus Grandiflorus* that produces one of the most magnificent of flowers, but blows only in the night; and is hence known also by the appellation of the Night-blowing *Cereus.*

Cryptogamous flowers.

Some plants, such as the Ferns, *Algæ,* and *Fungi,* are altogether destitute of conspicuous flowers; and are hence called *Cryptogamous*: but in this respect the Fig is perhaps the most singular. The flowers which in other cases uniformly precede the fruit, are in this case concealed within what is generally denominated the fruit; as may be proved by cutting open a ripe Fig by means of a longitudinal section passing through its axis. Great numbers of flowers are then discovered lining a sort of cavity in the axis of the fruit; and hence what is called the Fig in common language is rather the receptacle of the flower than any thing else.

Most plants have their flowers furnished both with stamens and pistils, and are hence hermaphrodites; but there are also many genera that have the stamens in one flower and the pistils in another, both on the same individual. These are denominated *Monœcious* plants, and are exemplified in the Oak and Hazel. Other genera have the flowers with stamens on one plant, and the flowers with pistils on another; these are denominated *Diœcious*, and are exemplified in the Hop and Willow. Others have flowers of all the previous kinds on one and the same plant; these are denominated *Polygamous*, and are exemplified in the genus *Atriplex*. Diœcious flowers.

SECTION VII.

The Fruit.

THE anomalies of the fruit may affect either its number, figure, colour, or appendages. The common Hazle-nut produces in general but one kernel in one shell; but in the course of opening up a considerable number, you will now and then meet with one containing two or three kernels in a shell. This is perhaps best accounted for by supposing, with Du Hamel, that it is the result of an unnatural graft effected in the bud; though, I think, the fact is that the shell does always contain the rudiments of two or more kernels, although it rarely happens that more than one is developed. But if Multiplicate fruit.

two Apples or Pears are developed in an incorporated state, which is a case that now and then occurs, it is no doubt best accounted for by the graft of Du Hamel.

Anomalies of figure.

Sometimes the anomaly consists in the figure of the fruit which is deformed by the tumours or excrescences, in consequence of the bite of insects or injuries of weather producing warts, moles, or specks.

Colour.

Sometimes it consists in the colour, producing green Melons and white Cucumbers.*

Appendages.

Sometimes it consists in an appendage of leaves, as in the following examples:—In the autumn of 1809, when gathering some fruit in the garden of Rendlesham parsonage, I observed some Pears of rather an unusual appearance; they had grown to nearly the size of the species, and were nearly of the usual shape. But the anomaly consisted in their being each furnished with several leaves resembling the proper leaves of the tree, but not so large, having their insertion about half way between the base and apex of the fruit, and growing directly out of the fleshy part of it. This anomaly, which I have not found to be mentioned by any botanist except Du Hamel,† may with propriety be designated by the name of the *Foliated fruit*. (*Pl.* IX. *Fig.* 12.)

The foliated fruit.

* Phil. Bot. 212. † Mem. de l'Acad. Royal, 1755.

SECTION VIII.

Habit.

SOME plants which, when placed in a rich soil grow to a great height, and affect the habit of a tree, are when placed in a poor soil, converted into dwarfish shrubs. This may be exemplified in the case of the Box-tree; and so also in the case of herbaceous plants, as in that of *Myosotis,* which in dry situations is but short and dwarfish, while in moist situations it grows to such a size as to seem to be altogether a different plant. The habit of the plant is sometimes totally altered by means of cultivation; the *Pyrus sativa* when growing in a wild and uncultivated state is furnished with strong thorns; but when transferred to a rich and cultivated soil the thorns disappear. This phenomenon, which was observed by Linnæus, was regarded as being equivalent to the taming of animals. But this explication is, like some others of the same great botanist, much more plausible than profound, in place of which Professor Willdenow substitutes the following:—The thorns protruded in the uncultivated state of the plant, are buds rendered abortive from want of nourishment, which when supplied with a sufficiency of nourishment, are converted into leaves and branches.

SECTION IX.

Physical Virtues.

When plants are removed from their native soil and taken into a state of culture, it alters not only their habit but their physical virtues. Thus the sour Grape is rendered sweet, the bitter Pear pleasant, the dry Apricot pulpy, the prickly Lettuce smooth, and the acrid Celery wholesome. Pot-herbs are also rendered more tender by means of cultivation, and better fitted for the use of man; and so also are all our fine varieties of fruit.

SECTION X.

Duration.

Plants are either annuals, biennials, or perennials, and the species is uniformly of the same class. But it has been found that some plants which are annuals in a cold climate, such as that of Sweden, will become perennials in a hot climate, such as that of the West Indies. This anomaly has been exemplified in *Tropæolum*, Beet-root, and *Malva arborica*; and on the contrary some plants, which are perennials in hot climates, are reduced to annuals when transplanted into a cold climate; this has been exemplified in *Mirabilis* and *Ricinus*.*

* Phil. Trans. 216.

CHAPTER VI.

OF THE SEXUALITY OF VEGETABLES.

THE doctrine of the sexuality of vegetables and foundation of the Linnæan system, though but lately established upon the basis of logical induction, is by no means a novel doctrine. It cannot, however, be said that the original notion of a sexual distinction as existing in vegetables was at all correct. It was a conjecture formed at random, rather than an opinion founded upon the evidence of fact; which maintained its ground, however, for a period of many ages, though wholly unsupported by any convincing argument, till at last the elucidations of Linnæus established it beyond a doubt. The following brief sketch of the origin, progress, and proofs of the doctrine, from the earliest notices recorded in history down to the present times, will furnish the reader with the evidence on which the above remarks are founded.

SECTION I.

Anticipations of the Ancients.

IT cannot now be ascertained with whom or at what particular period the notion of vegetable sexu-

Empedocles. ality originated. But its antiquity is unquestionably great; as it appears to have been entertained even among the original Greeks, from the antiquity of their mode of cultivating Figs; and to have been made the subject of the speculations of some of their earliest philosophers, from the fact of its having been a doctrine taught by Empedocles, that the sexes are united in plants; a doctrine involved indeed in that of Anaxagoras by which the desires and passions of animals are attributed to vegetables.* Herodotus. It was evidently a prevalent notion throughout Greece, and the nations to the east of Greece, in the time of Herodotus, who recognises it in his account of the cultivation of the *Phœnix dactylifera* or Babylonian Palm; which he represents as being cultivated in the country around Babylon in the manner of Figs, the cultivator taking the flower of that Palm which the Greeks call the male Palm, and binding it around the flowers of the fruit-bearing Palm, that the fruit may not fall immature.† Whether the beneficial effect resulting from this practice was produced by the agency of insects, generated in the male plant, as Herodotus asserts, it is not our object at present to inquire. It is enough to have ascertained that the notion of

* Arist. Περι φυτων. το.Α.

† Τοὺς φόινικες συκέων τροπόν θεραπέυουσι τά, τε ἄλλα, καὶ φοινίκων, τοὺς ἔρσενας Ἕλληνες καλλέουσι, τόυτον τὸν καρπον περιδέουσι τῆσι βαλανηφόρουσι τῶν φοινίκων, ἵνα πεπάινη τέ σφι ὀψὴν τὴν βάλανον ἐσδύνων, καὶ μὴ ἀποῤῥεη ὁ καρπὸς ὁ τοῦ φοινίκος. Herodot. Porsoni Clio, 193.

a sexual distinction in plants existed, or rather was a general and prevalent notion in the age of Herodotus, that is at least 400 years before the Christian æra.

Our next authority is that of Aristotle, who maintains the doctrine of a distinction of sex in plants as well as in animals,* though he admits that some plants are altogether without sex; and represents the beneficial effect of the practice adopted in the cultivation of the Palm, as resulting from the action of the dust of the male flower, quickening the maturity of the fruit, which it is said to effect also equally well if it is but wafted to the female flower by means of the wind. Aristotle.

Theophrastus, the disciple and successor of Aristotle, who pursued his phytological investigations to a much greater length than his master, maintains also the doctrine of the sexuality of vegetables, which he illustrates with more of detail, and exemplifies not only in the case of the Palm-tree, but in that also of the Fig, and a variety of others. The barren Palm he calls the male, and the fruit-bearing Palm, the female; pointing out, at the same time, the ground of this distinction as consisting in the indispensable necessity of the co-operation of the flower of the barren Palm, to the ripening of the fruit of the fertile Palm; the fruit of the fertile Palm being otherwise extremely apt to fall off before it becomes ripe. But if the spathe of the male Theophrastus.

* Αριστοτελους περι γενεσιος Ζωων. Το. Α.

plant containing the male flowers is cut off, and shook over the flowers of the female plant, the fruit does not fall, but is preserved till it is mature; in which case, he adds, there is a sort of *coitus* of the male and female.*

But beyond the example of the Date Palm and such other plants as produce barren and fertile flowers on distinct individuals, Theophrastus does not seem to have entertained any correct notions of vegetable sexuality. For although he institutes the distinction of sex in other genera also, yet it is by no means on the same principle, but rather upon that of the habit or aspect of the plant, or upon the quality of the timber when felled; the male being represented as shorter and stouter, and the female as taller and more slender, as erroneously exemplified in the case of the *Pinus Larix*, which is well known to produce no individuals that are exclusively male or female; † as well as in the case of the Lime-tree, of which it is also added that the male plant is not only barren, but destitute even

* Ὅταν ἀνθῇ το ἀῤῥεν ἀποτεμνόντες τὴν σπάθην, ἀφ' ἧς το ἄνθος, εὐθὺς ὥσπερ ἔχει τον τε χνοῦν και τὸ ἄνθος, και τὸν κονίορτον, κατασείουσι κάτα τὸν καρπον της θελεῖας; κἄν τουτο πάθη, διατηρεῖ καὶ ὀυκ ἀποϐάλλει; φάινεται δ' ἀμφοῖν απο ἀῤῥενος τοῖς θήλεσι βοήθειαν γίνεσθαι (θῆλυ γὰρ καλοῦσι τον καρποφόρον) αλλ' ἡ μὲν ὁιον μιξις, ἡδὲ κατ' αλλον τρόπον. Περι φυτῶν ἱστοριας. το. Β.

† Φασὶ δὲ οἱ πὲρι Μακεδονίαν καὶ ἄκαρπον τι γένος ὁλως ειναι πεύκης, καὶ το μεν ἄῤῥεν ϐραχύτερόν τε καὶ σκληρότερον; τὸδὲ θῆλυ εὐμηκέστερον. Περι φυτων ἱστοριας, το. Γ.

of flowers.* And to complete the mystery in which the doctrine was yet involved, the male plant is, in some cases, said to bear fruit as well as the female.† From all which it follows that the doctrine of vegetable sexuality was but very imperfectly understood in the time of Theophrastus.

After a long blank in the annals of phytological research, the next traces of inquiry relative to the sexuality of vegetables, are such as occur in the works of Pliny, Dioscorides, and Galen, who also adopted the division by which plants were then distributed into male and female; but chiefly upon the erroneous principle of habit or aspect, and without any reference to a distinction absolutely sexual; the fertile plant being sometimes denominated the male, and the barren plant the female, as in the example of male and female mercury, in which the true notion of vegetable sexuality was altogether reversed. Pliny seems, however, to admit the distinction of sex in all plants whatever, and quotes the case of the Palm-tree as exhibiting the most striking example.‡

Pliny, Dioscorides, and Galen.

* Τας δὲ φιλυρας, ἡ μὲν ἄῤῥεν εστὶν ἡ δε θήλεια; τὸ μεν δε τὴς ἄῤῥενος ξυλον ἐυωδέστερον τȢ τὴς θήλειας, καὶ ἡ μεν ἀκάρπος και ἀκανθος, ἡ δε θήλεια καὶ ἄνθος εχει, και καρπόν. Περι φυτων ἱστοριας. το. Γ.

† Διαφοραὶ πλέιους είσίν, ἡ μὲν κõινη πᾶσιν ῃ διᾶιρουσιν τὸ θήλυ και το ἄῤῥεν, ὦν το μὲν καρπόφορον, το δε ἀκαρπον ἐπὶ τινων; έν ὂις δε ἄμφω καρπόφορα, το θῆλυ καλλικαρποτερον. Ibid.

‡ Arboribus imo potius omnibus quæ terra gignit herbisque etiam utrumque sexum esse diligentissimi naturæ tradunt. Lib. xiii. p. 4.

SECTION II.

Discoveries of the Moderns.

Cæsalpinus.

Cæsalpinus, who follows next in order, though not till after an interval of many centuries, enters more into the detail of the doctrine, and speaks with more confidence on the subject than any preceding phytologist. Trees which produce fruit only he denominates females; and trees of the same kind which are barren, he denominates males; adding that the fruit is found to be more abundant and of a better quality where the males grow in the neighbourhood of the females, which is, as he says, occasioned by certain exhalations from the males dispersing themselves all over the females, and by an operation not to be explained, disposing them to produce more perfect seed. Still it seems doubtful whether any conjecture had been yet formed with regard to the peculiar and appropriate organs by which the sexual intercourse is conducted. Zeluzianski, a native of Poland, who lived about the end of the sixteenth century, is said to have made some considerable discoveries with regard to the sexuality of vegetables. But as his book, if he ever published one, is not now to be met with, no one seems able to say what his discoveries were, if rather they are not a transcript of the discoveries of Cæsalpinus.*

Zeluzianski.

* Pulteney's Sketches, p. 335.

At last, however, about the middle of the seventeenth century, when the improved philosophy of Bacon had begun to be adopted even in phytology, and phytologists to be directed by observation and experiment rather than by hypothesis and conjecture, the doctrine of the sexes of plants began also to assume a more fixed and determinate character, and to exhibit the legitimate evidence of being founded upon fact. Still it is difficult to say who first discovered and pointed out the peculiar organs by which the sexes are respectively characterized; not that these organs had been overlooked in the description of the flower, but that their functions had been misunderstood. Malpighi, who describes not only the stamens and anthers but also the pollen contained in them, regards the former as excretory organs contributing to the perfection of the seed, and the latter as the substance excreted.* The true use of the pollen, therefore, was not yet discovered; but the merit of suggesting its true use seems to be between Sir T. Millington, Savilian Professor at Oxford, and the celebrated Dr. Grew, who represents the suggestion as originating with the Professor, and consisting in the expression of an opinion that the stamens serve as the male

Opinion of Malpighi,

Of Sir T. Millington and Grew.

* Vegetantium igitur, uteri gratia, reliquæ floris partes, folia scilicet, stamina et calyx circumlocantur in fæcundis floribus. Anat. Plant. 55.

Ita determinata succi portio per stamina et floris folia excernitur. Ibid. 56.

4

organs of the vegetable for the purpose of the generation of the seed; which opinion he seems himself to have previously entertained, or at the least to have acquiesced in as soon as it was suggested.* This we may regard as the first glimpse that was ever caught of the true and proper use of the stamens, and may date at about the year 1676.

Published. But the opinion, if not first suggested, was at least first published by Dr. Grew, in his Anatomy of Plants, together with the grounds on which he had adopted it, and the illustrations which its novelty demanded or his researches had furnished; so that he does not merely ascribe a peculiar function to the stamens, but points out also the mode in which he thinks that function is discharged, and which is represented to be as follows:—When the summits of the stamens, or anthers surmounting the filaments, burst open in the process of vegetation, the inclosed pollen falls upon the pistil and impregnates the embryo; not by actually entering the pistil, but by means of a subtle and vivific *effluvium:* hence the stamens are the male, and the pistil or pistils the female organs of vegetable impregnation. But this was the very discovery that furnished the clue for

* Our learned Savilian Professor Sir T. Millington told me that he conceived the attire (stamens) doth serve as the male for the generation of the seed. I immediately replied that I was of the same opinion, gave him some reasons for it, and answered some objections which might oppose them.—Grew's Anat. b. iv. chap. 5.

the unravelling of the whole of the mystery overhanging the subject, because it is equally applicable to all sorts of vegetables whatever, whether producing the organs in question in separate flowers and on separate plants, as in the case of the Palm-tree; or in separate flowers and on the same plant, as in the case of the Hazel-nut-tree; or lastly, in the same flower, as in the case of the Lily, which is by far the most general mode of vegetable sexuality.

Adopted by Ray.

The opinion of Grew was adopted also by Ray, at first with some appearances of doubt, but finally without any sort of reservation, as being founded on evidence which appeared to him sufficiently convincing, and which he was even induced to illustrate.*

Experiments of Camerarius,

Hitherto the doctrine of the sexuality of vegetables had been supported chiefly upon the ground of its probability as arising from careful observation, or upon that of the necessity of the case, and had not yet been confirmed by the evidence of actual experiment: but this confirmation which was so devoutly to be wished, and without which all other arguments must have remained insufficient, was at length also happily undertaken. The first example of experiment recorded on this subject is that of Camerarius, Professor of Botany at Tubingen, who having adopted the opinions of Grew and Ray, though without perhaps regarding their arguments

* Sylloge Stirpium Europæarum Præf. 1694.

as the best that could be adduced, conceived that the subject might be still further illustrated by means of depriving the plant of its male flowers altogether, or of removing the individuals of different sex to a distance from one another. Accordingly having selected some plants of *Mercurialis*, *Morus*, *Zea Mays*, and *Ricinus*, and stripped them of their stameniferous flowers, or removed the male plant to a great distance from the female, he found that the fruit did not now ripen; the inference from which was that the generation of plants is analogous to that of animals, and that the stamens of the flowers of the former correspond to the sexual organs of the males of the latter.*

But though the fact of the sexuality of vegetables seemed thus unequivocally ascertained, the peculiar mode of their fecundation was still left undetermined. Some conjectures had been offered with respect to it by Cæsalpinus and Grew, the former regarding it as being effected by means of an exhalation from the male flower; and the latter, by means of an effluvium from the pollen: but Morland, who published a paper on the subject in the Philosophical Transactions for 1703, in which he adopts indeed the opinion of Grew with regard to the functions of the stamens, contends, however, that the pollen is a congeries of seminal plants, one of which at least must be conveyed through the style into the ovary, before it can become prolific. This conjecture

* Epistola de Sexu Plantarum, 1695.

seems to have arisen out of the theory of Leuwenhoeck on animal generation, which was then popular, but it is not corroborated by any experiments. It seems, however, to have had the effect of keeping alive the discussion of the subject; for Geoffroy, in his memoir presented to the Royal Academy of Sciences in 1711, on the structure and use of the principal parts of flowers, endeavours, as it appears, to reconcile the discordant theories of Grew and Morland; and maintains that the germ is never visible in the seed till the anthers have shed their pollen: adding, that if the stamens are cut off before the anthers burst, the seeds remain barren. In this we have a step in advance beyond the point that had been gained by means of the experiments of Camerarius, which relate only to monœcious and diœcious plants, in which the proof is less difficult than in hermaphrodites, to which Geoffroy's experiments apply. Of Geoffroy,

From the spirit of inquiry that was thus excited new discoveries could not but be expected to follow; for although the doctrine was discountenanced and rejected by some of the leading botanists of the time, and even by the illustrious Tournefort, yet it was too well established in fact to be overthrown by any argument or any authority. Accordingly its evidence was becoming every day more irresistible, and its advocates more confident. Vaillant, in a dissertation on the structure of flowers read at the opening of the Of Vaillant.

Royal Garden at Paris in 1717, supports the doctrine of the sexes of vegetables by new accessions of experiment, and throws additional elucidation both on the structure of the pollen and manner of its explosion; which he represents indeed in terms too glowing for the style of sober narrative, but by which he appears according to the remarks of a cotemporary author, to have been the first eye-witness of that secret operation of nature—the sport that passes between the flowers of plants in the mysterious process of vegetable generation.

Of Linnæus.

But the doctrine of the sexes of vegetables, which was thus daily acquiring new accessions of proof, was destined to receive its last degree of elucidation from the pen of Linnæus. This great and illustrious botanist, reviewing with his usual sagacity the evidence on which the doctrine rested, and perceiving that it was supported by a multiplicy of the most incontrovertible facts, resolved to devote his labours peculiarly to the investigation of the subject, and to prosecute his inquiries throughout the whole extent of the vegetable kingdom; which great and arduous enterprize he not only undertook but accomplished with a success equal to the unexampled industry with which he pursued it. So that by collecting into one body all the evidence of former discovery or experiment, and by adding much that was original of his own, he found himself at length authorized to draw the important conclusion—that no seed is perfected without the previous agency of the pollen, and the doctrine of the sexes of plants is consequently founded in fact.

SECTION III.

Induction of Particular Proofs.

THE evidence on which the above conclusion depends is substantially comprised in the following brief induction of particulars—first, as resulting from observation; and secondly, as resulting from experiment.

SUBSECTION I.

Observation 1.—In all plants hitherto discovered it has been observed that the fruit is uniformly preceded by the blossom; and that without blossom there is no fruit. This is a remark that can scarcely fail to be made even by the most inattentive observer, at least with regard to such plants as come within the sphere of his notice; as every school-boy knows that unless the Cherry-tree blossoms in the spring, he will gather no fruit from it in the summer. This proves that the organs necessary to the production of the fruit exist in the flower; and is one step at least towards the general conclusion. But to this rule there exists a seeming exception in the case of the *Colchicum autumnale*, which produces its fruit

From the precedency of parts.

in the spring and its flower in the autumn, so that the former has the appearance of being the cause of the latter; but the truth is that the fruit, which ripens in the spring, is the natural result of the flower of the preceding autumn, and not the cause of the flower of the following autumn: for if the flower is cut off in the autumn, before its expansion, you will have no fruit in the succeeding spring; and yet if the fruit is cut off any time in the spring, you will still have blossom in the following autumn.

There exists also another seeming exception in the case of the Pine apple, in which the part that is commonly called the fruit is formed before the flower expands; but when it is recollected that this alleged fruit is merely a fleshy receptacle, and that the seed, the only essential part of the fruit, is not developed till after the expansion of the flower, the seeming exception vanishes.

From the fecundation of diœcious plants.

Obs. 2.—The fruit bearing individuals of such species as have their barren and fertile flowers on distinct plants do not perfect their fruit except where individuals of both sorts are sustained in the vicinity of one another. This observation is confirmed not only by the testimony of the ancients, and their manner of cultivating the Palm and Fig-tree, but also by the additional observations of the moderns. Father Labat, a French ecclesiastic, who had undertaken a voyage to the West Indian islands about the year 1745, says that when he was in the island of Martinique there was then growing near the monastery of the order to which he belonged, a female Date-tree, which bore fruit though single, there being no other tree of the same species within two leagues of it; but he adds that the stones of the Dates it produced did not germinate: it is plain, therefore, that the fruit was not perfect, though it might have been externally complete. A female plant of the *Cycas revoluta*, in the possession of the Bishop of Winchester, produced also fruit though single; but the drupe, which was externally and apparently complete, was found when dissected by Sir J. E. Smith to be internally very defective: for in place of the embryo, the most important part of the whole, all that could be discovered was only a minute cavity, which defect Sir J. E. Smith rightly attributes to the want of the vicinity of a plant furnished with male flowers; which he adds was, perhaps, not to be found nearer than Japan.*

The fruit then is perfected by means of some substance conveyed from the barren to the fertile flower, and capable, as it appears, of being transmitted through the medium of the atmosphere, if the respective plants are situated in the vicinity of each other.

But in the case of the Fig-tree vicinity is not even enough, the structure of the fruit being such as to require a peculiar mode of transmission; for the

* Trans. Lin. Soc. vol. vi.

fruit of the Fig is not, as in most other cases, a pericarp enveloping the seed, but a common calyx or receptacle enclosing the flowers: this may be readily seen by means of cutting a fig in two in the direction of the longitudinal axis of the fruit, in the centre of which there will be found a cavity lined with a multitude of flowers, the male and female blossoms being generally in different Figs and on distinct plants, and the medium of communication between them being only a small aperture at the summit of the receptacle. Hence the access of the substance necessary to impregnation is rendered impracticable in the ordinary mode of transmission. But nature is not without a resource even in this difficulty; for in Greece and Italy, and the islands belonging to them, the native country of Figs, a species of insect of the genus Cynips, which is continually fluttering about from Fig to Fig for the purpose of depositing its eggs in the cavity, carries the substance necessary to impregnation from the male to the female flower. But the substance which it carries is the pollen of the anthers, with which it becomes covered all over in rummaging through a variety of receptacles till it finds one to please it. The pollen then is the substance by which the impregnation of the female flower is effected; and the whole of the phenomena of the growth and economy of flowers tends to corroborate the fact. In Italy and the Levant, where the Fig is much cultivated, the cultivator ensures or facilitates the agency of the insect by presenting it to the Fig at the time proper for impregnation; and the service he thus performs is called Caprification.

Obs. 3.—If the stamens or pistils are obliterated by cultivation, or injured by rain or frost, or by the operation of any other natural cause, the process of impregnation is interrupted or prevented, and the fruit deteriorated or diminished in quantity or quality.

From injuries done to the stamens or pistils.

Sometimes they are wholly obliterated by means of cultivation, as in the case of double flowers; in which the stamens degenerate into petals, and the pistil not unfrequently into a leaf: but in this case it is well known that no flower produces perfect seed. Sometimes they are injured by accidents arising from weather, and even in such vegetables as are the most serviceable for the food of man, particularly in crops of grain; but some sorts of grain are much more liable to be injured by such accidents than others:—Crops of rye, for example, are much more liable to be injured by heavy and continued rains than crops of Barley, because the anthers are better sheltered by the husks of the latter than of the former. But shrubs and trees are affected in the same manner as the plants now mentioned. It was observed by Linnæus that the Juniper produces few or no berries in Sweden if the flowering season is wet; and that the Cherry-tree is much less liable to come short of its annual crop than the Pear-tree, because in the latter the blossoms are unfolded and the stamens and pistils matured all about the same period, so that the whole of them might be blasted by the dews or frosts of a single night; whereas in the former the blossoms are unfolded, and the stamens and pistils matured, by gradual and successive steps, so that if part of them should happen to be destroyed by the occurrence of a frosty morning, the rest may escape. But the fruit is equally blasted whether the injury is done to the stamens or to the pistil; the stamens being the organs in which the impregnating substance is contained, and the pistil being the channel through which it is conveyed to the ovary. Hence we may account for the peculiar care with which these organs have been guarded by the hand of nature from external injury: sometimes this is effected by means of a nodding or pendant flower, as in the case of the Crown Imperial and Cowslip, in which the intention of nature is the more evident in that the flower-stalk after the time of flowering becomes gradually erect, even though loaded with fruit; sometimes it is effected by means of a capacity inherent in the petals of folding themselves together in the night and opening themselves out again in the morning, as in the case of many of the *Papilionaceous* and *Compound* flowers, particularly the Pea and Dandelion. But one of the best examples of this capacity is that of the *Nymphæa alba* of Linnæus, which closing its petals as the sun begins to get low, and shrinking into itself, reposes its lovely blossom upon the surface of the water till the morning, when it again rears its head, sometimes to the height of several inches, and presents its expanded petals to the culminating sun. A phenomenon still more singular is related by Theophrastus as occurring in what he calls the Lotus, perhaps the *Nymphæa Lotus* of Linnæus; of which he says, though only on report, that in the Euphrates the flower keeps sinking till midnight, when it again begins to ascend, but more rapidly as day advances, elevating itself to the surface about sun-rise, and afterwards expanding and rearing its head high above the water.* Some flowers are so very susceptible to changes of atmosphere as to shut up their petals even upon the approach of rain. One of the most remarkable examples of this sort is that of the *Anagallis arvensis*, or Poor Man's Weather-glass, which appellation it seems to have obtained from its peculiar susceptibility, always shutting up its blossoms even upon the slightest symptoms of approaching rain, except in the case of a sudden thunder-storm, when it happens to be taken by surprise: but Sir J. E. Smith says he has reason to think that its susceptibility is apt to be impaired, and sometimes totally destroyed by long continued wet;† and Linnæus remarks that flowers in general lose this susceptibility when the anthers have discharged their pollen.

Obs. 4.—The pollen is generally discharged from the anther in such a manner as to ensure its dis- From the discharge of the pollen.

* Theoph. 106. Aldi. † Introduction, p. 329.

persion, at least to any pistil that is near it, and at such a time as pistils of the same species are best fitted to receive it. When the anther has given indications of maturity by the distended appearance of its cells; the valves of which the cells consist become daily more and more indurated till at last they fly open with a sudden jerk, and discharge the contained pollen as if by the force of an elastic spring. The phenomenon exhibited in this case by the Cypress-tree affords a good example, in which the pollen is thrown out with such force and in such abundance as to resemble a little cloud of smoke; but the same phenomenon may be observed in the discharge of the pollen from the male catkins of the Birch and Willow, particularly if they are suddenly shaken or agitated by the wind; in which cases a portion of the pollen can scarcely fail to alight upon the pistil-bearing and contiguous flowers, or to be wafted to them if even at some distance.

But at the season of the discharge of the mature pollen, the pistil is also peculiarly adapted to receive it, as is evident from the state of the stigma. Sometimes this adaptation consists in the stigma's then assuming a peculiar form or shape, as may be exemplified in the case of the *Gratiola*, *Martynia*, and *Viola tricolor* or Pansy, all of which are furnished with what botanists call a gaping stigma, opening as if to receive the pollen, yet not in the early stage of its growth, nor during its decline; but in the intermediate stage only, when the pollen is ripe. But the adaptation generally consists in the stigmas being then moistened with an exuding and viscous fluid; except in the case of a hispid stigma in which no such exudation is discoverable, as is peculiarly well exemplified in the case of the *Amaryllis formosissima*. This beautiful flower, which when fully expanded is pendulous, exhibits the curious phenomenon of the exuding of a fine and limpid fluid from the surface of the stigma every morning, which augments as the day advances, and forms about noon a drop so large that one would think it in danger of falling to the ground. It is re-absorbed, however, by the style about three or four o'clock in the afternoon, and again protruded about ten o'clock on the following morning. This limpid drop, which is thus regularly exuded and absorbed, is intended no doubt in the economy of the flower to facilitate the process of impregnation, by catching a portion of the pollen as it is discharged from the anther, and conducting it to the ovary. It is at least certain that the pollen reaches it, and is detained by it; as a number of drenched and disfigured particles may generally be seen adhering to the surface of the stigma, after the drop has been absorbed. Perhaps it may even have some effect in forwarding the explosion of the pollen, which is known to be also strongly effected by moisture.

As the stamens and pistils grow and come to maturity together, so they also decay together; the stamens shrinking and withering immediately after the anthers have discharged their pollen, and the stigma withering also and falling off much about the same time, even when the style remains an appendage to the fruit.

From the proportion of the stamens of the pistils.

Obs. 5.—The relative proportion, situation, and mutual sympathies, of the stamens and pistils are such as seem expressly calculated to facilitate the process of impregnation. In pendulous flowers the pistil is generally longest, as in the case of the Lily; but in upright flowers the stamens are generally the longest, as in the case of the *Ranunculus*. In simple and hermaphrodite flowers the situation of the pistil is invariably central with regard to that of the stamens, as may be seen by inspecting almost any flower at random. In plants of the class *Monœcia*, the barren blossoms stand generally above the fertile blossoms, even when situated on the same foot-stalk, as may be seen in the case of the *Carex* and *Arum*. And in plants that have their barren and fertile flowers on distinct individuals, the blossom is generally protruded before the leaves expand. But a very little reflection will serve to show that all the above arguments are institutions of nature, by which the pollen, when it explodes from its envelopes, shall possess the best possible chance of coming into contact with the pistil or stigma.

And where such means are wanting, nature displays a variety of other contrivances to effect the same end. The style of the *Gloriosa superba* is bent towards the stamens at a right angle even from the very base, and for no other conceivable purpose but that of throwing itself in the way of the pollen when discharged. The stamens of the genus *Saxifraga* bend down to the pistil, one or two at a time; if two, the two opposite, and discharge their pollen directly over the stigma, returning afterwards to their former position and giving place to one or two others successively, which also retire in their turns, till all of them have discharged their pollen.* Similar phenomena have been observed in the flowers of *Parnassia, Celosia,* garden Rue, and others.

But the most singular phenomenon of this kind is that which is exhibited in the stamens of the flower of the Berberry Bush; the stamens which are six in number lie sheltered under the concave tips of the petals as long as they are allowed to remain undisturbed; but if any extraneous body, whether by accident or design, is made to touch a stamen at the base of the filament, it immediately collapses with a sudded jerk and bends inward till the anther strikes against the summit of the pistil, discharging its pollen if ripe, and again retiring. This curious and singular fact seems to have been first discovered by Sir J. E. Smith,† of the truth of which any one may easily satisfy himself by applying the point of any instrument sufficiently delicate to the

* Withering, vol. i. p. 239. † Phil. Trans. 1788.

inner side of the base of a stamen; it will immediately spring forward till it strikes against the pistil. Whence it is to be presumed that the effect is often produced in the natural order of things, by means of the feet or trunks of insects rummaging the flower in quest of honey.

From the economy of aquatics.

Obs. 6.—The economy of many of the aquatics seems also expressly intended to facilitate the process of impregnation. Many plants of this class that vegetate for the most part wholly immersed in water, and often at a considerable depth, gradually begin to elevate their stems as the season of flowering advances when they at last rear their heads above the surface of the water, and present their opening blossoms to the sun, till the petals have begun to fade, when they again gradually sink down to the bottom to ripen and to sow their seeds. This very peculiar economy may be exemplified in the case of *Ruppia maritima*, and several species of *Potamogeton*, common in our ponds and ditches; from which we may fairly infer that the flowers rise thus to the surface merely to give the pollen an opportunity of reaching the stigma uninjured.

But the most remarkable example of this kind is that of the *Valisneria spiralis*, a plant that grows in the ditches of Italy. The plant is of the class *Diœcia*, producing its fertile flowers on the extremity of a long and slender stalk twisted spirally like a cork-screw, which uncoiling of its own accord, about the time of the opening of the blossom, elevates the flowers to the surface of the water, and leaves them to expand in the open air. The barren flowers are produced in great numbers upon short upright stalks issuing from a different root, from which they detach themselves about the time of the expansion of the female blossom, mounting up like little air bubbles, and suddenly expanding when they reach the surface, where they float about in great numbers among the female blossoms, and often cling to them in clusters so as to cover them entirely; thus bringing the stamens and pistils into immediate contact, and giving the anthers an opportunity of discharging their pollen immediately over the stigma. When this operation has been performed, the now uncoiled stalk of the female plant begins again to resume its original and spiral form, and gradually sinks down as it gradually rose, to ripen its fruit at the bottom of the water.

SUBSECTION II.

Experiments.—The above are the proofs of the sexuality of vegetables, arising from the observation of the natural phenomena exhibited in the economy of flowers. It remains now to exhibit such proofs as arise from experiment.

Experiment 1.—If the anthers of an hermaphrodite flower, or the stameniferous flowers of a monœcious plant, are cut off before they shed their pollen, and care taken to prevent the access of the

The anthers of hermaphrodites cut off.

pollen of any other plant of the same species, the fruit will prove abortive. From a flower of the *Chelidonum corniculatum*, or red-horned Poppy, which was detached from all other individuals of the same species, Linnæus removed all the anthers upon the first opening of the blossom, and stripped off at the same time all the rest of the flowers; but the result of the experiment was that the flower produced no seed.

A gardener who cultivated Melons and Cucumbers, but was no botanist, thinking that the staminiferous flowers of the plant only exhausted the nourishment due to the other flowers without being of any utility in themselves, fancied that his plants would be rendered more vigorous and his fruit of superior flavour, and his profits consequently increased, by means of tearing them off altogether. But, like the boy who cut open his goose that laid golden eggs in the hope of getting rich all at once, he soon found cause to repent of his rash experiment; for the consequence was that his plants produced no fruit.

Supplied from other plants of the same species.

Exper. 2.—If after the anthers have been removed, as in the foregoing experiment, the pollen of another plant of the same species is shook over the pistil, then the fruit will still ripen. This Linnæus proved by first treating a flower of the *Chelidonum corniculatum* as in the foregoing experiment, and then sprinkling over the pistil pollen borrowed from another plant of the same species. The flower produced perfect seeds. Upon this principle gardeners now assist the impregnation, or what they call the setting of the fruit, at least in the case of their Melons and Cucumbers, by means of sprinkling the pollen of the male flowers over the pistils of the females. But if a plant has more than one pistil, and you apply the pollen only to that one, then that one only will ripen seed.

The stigma cut off.

Exper. 3.—If the stigma of the pistil is cut off before the discharge of the pollen, no fecundation ensues; and the fruit is inferior both in quantity and quality. Of this experiment I have not been able to procure the proper examples: but it shows that it is by no means a matter of indifference to what part of the pistil the pollen is applied; for unless it enters by the stigma it cannot be conveyed to the ovary.

Sprinkled with pollen from plants of a different species.

Exper. 4.—If the stigma of a flower that has been stripped of its stamens before the bursting of the anthers is sprinkled with the pollen of a plant of a different species, then the seeds will not only ripen and produce perfect plants when sown, but these plants will partake of the qualities both of the fecundating and fecundated species. The pollen of the *Tragopogon pratensis,* whose petals are yellow, when sprinkled on the stigmas of the flower of the *Tragopogon purpureus,* whose petals are purple, yielded seeds that produced plants with both purple and yellow flowers. Hence botanists account for the existence of what are called spurious plants, attributing them to the accidental intermixture or access of the pollen of a different species. Thus *Veronica spuria* is thought to have sprung from *Veronica maritima*, impregnated by the pollen of *Verbena officinalis;* agreeing in its fructification with the former, and in its leaves with the latter. So also *Delphinum hybridum* is thought to have sprung from *Delphinum elatum* and *Aconitum Napellus*, by its combining together the features of both. But this spurious impregnation seems to be confined within very narrow limits, and takes place only among plants that are nearly related by natural affinity.

A male plant introduced, or pollen carried to a distance.

Exper. 5.—If a male plant is placed in the vicinity of a female plant which, from its having been formerly insulated, had produced no perfect seed; or if the pollen of a male plant of the same species is conveyed to it from a distance and sprinkled over the stigma, it will now produce perfect seed. A plant of the *Datisca Cannabina*, which came up in the garden of Linnæus from seed about the year 1750, and which produced afterwards many flowers, yielded however no perfect seed, as the flowers happened to be all female; a few perfect seeds were now procured and sown with a view to raise some male plants, but still they were all female. At last, however, in 1757 a parcel of seed was procured, from which a few male plants were obtained that flowered in the following year. They were removed to a distance from the females, and when their flowers were ready to discharge the pollen, it was collected by means of shaking the panicle with the finger over a piece of paper, till the paper was covered with a fine yellow powder. The pollen thus obtained was immediately carried to the female plants, which were growing in another part of the garden, and sprinkled over them; in consequence of which they now produced perfect seeds.

But the best example of this kind yet exhibited is that of the famous experiment of Linnæus upon the Berlin and Leipsic Palms. About the period of the foregoing experiment, or rather a few years prior to it, there grew at Berlin an individual female Palm-tree which had never perfected any fruit, so as that the seeds would germinate; while there grew at the same time, at Leipsic, a male plant of the same species. Hence it occurred to Linnæus that the impregnation of the female flowers of the former was still practicable, even by means of the pollen that might be procured and carried from the male flowers of the latter. Accordingly a flowering branch of the male plant was dispatched by post from Leipsic to Berlin, a distance of twenty German miles, and shook or suspended over the flowers of the female plant. The consequence was that the fruit was ripened and the embryo perfected, and young plants raised from the seed.

Exper. 6.—If the male plant is again removed from the vicinity of the female plant to which it had given fecundity, the fruit of the female plant is

The male plant withdrawn.

again produced imperfect as before. About the year 1755, there grew in the garden of M. De la Serre, at Paris, a female Pistachio-tree which blossomed every summer, but without producing any fruit capable of germinating; as M. De la Serre had frequently sown the seeds it yielded in the hope of raising more plants, though without success. At last, however, he was advised by Bernard De Jussieu and Du Hamel to endeavour to procure a male plant and place it near it: accordingly a male plant was procured in the following year, full of flowers, and placed near the female; the result being, as in other cases of a similar kind, that the seed now produced was capable of germinating when sown. But when the male plant was afterwards removed, the fruit of the female plant was found to be again incapable of germinating as before.*

In the month of April, 1752, Linnæus sowed a few grains of Hemp-seed in two different pots, in both of which it came up very well. In the one pot he left the male and female plants together, which flowered and produced fruit that was ripe in July; from the other pot he removed all the male plants as soon as they could be distinguished from the females, which grew indeed very well and presented their long pistils in great abundance, as if in expectation of their mates. But when the calyxes were afterwards inspected, about the time that the pistils began to decay through age, though they were large indeed

* Phys. des Arb. liv. iii. chap. iii.

and luxuriant, yet the seed-buds were brown, compressed, and membranaceous, without exhibiting any appearance of cotyledons or pulp.

Two plants of *Clutia tenella* were, in like manner, kept growing in a window of Linnæus's house or apartments during the months of June and July of 1753, the male plant being in one pot and the female plant in another. The latter abounded with flowers, not one of which proved abortive; the pot containing the male plants was after some time removed to a different window in the same apartment, and still the flowers that were protruded under such circumstances were found to be fruitful. The pot containing the male plant was at last removed into a different apartment, and the female plant left alone, after being stripped of all the flowers already expanded. It continued indeed to produce new flowers every day from the axils of every leaf, but they proved to be all abortive. For after remaining on the plant for the space of eight or ten days, till the foot-stalks began to turn yellow, they all fell barren to the ground.

Such is the amount of the great body of evidence, whether resulting from observation or experiment, on which Linnæus has established the doctrine of the sexes of vegetables, and on which the important and irresistible conclusion depends—namely, that no seed is perfected without the previous agency of the pollen.

SECTION IV.

Objections.

ALTHOUGH the proofs contained in the foregoing section seem to be altogether irresistible when taken in their aggregate effect, yet it will readily be admitted with regard to several of them that they do not amount to much in their individual weight. And hence we can easily account for the doubts that were entertained on the subject, and the opposition that was given to the doctrine of vegetable sexuality, at a time when the preceding proofs were not yet all discovered nor collected into a body.

Anticipated by Camerarius.

Camerarius, who had inferred the truth of the doctrine from the result of actual experiment, which he was indeed the first to institute on the subject, seems after all to have found cause to doubt the legitimacy of his conclusion, in observing that some of the female plants on which his experiments were made —namely, Hemp, Mercury, and Spinach, produced also ripe and perfect seeds even when placed altogether beyond the reach of the influence of the male plant. This fact looked no doubt extremely hostile to the doctrine he was endeavouring to establish, and perhaps remained with him to be an insuperable objection; but the fact has been now accounted for, and the objection done away. For it has been ascertained, by means of more minute and accurate observation, that the fertile plants of the genera in question have often some latent male flowers interspersed among their female flowers, so that the former, though difficult of detection, are sufficient to secure the impregnation of the latter, even when the individual producing them is solitary.

Tournefort, who denied the doctrine of the sexes altogether, though I do not know upon what precise grounds, admitted, however, the utility of the stamens in the economy of fructification, regarding them as organs both of secretion and excretion—the substance excreted being the pollen, and the substance secreted being a peculiar fluid that was conducted by the filaments to the germen. But if the pollen is merely an excrement, how comes it to be so very curiously organized? And if the stamens secrete a fluid which they afterwards conduct to the germen, by what means do they conduct it when placed on a different plant?

Advanced by Tournefort.

Answered,

Pontedera, who was one of the most zealous disciples of Tournefort, and willing to defend him even where least defensible, not only adopted the opinions of his master on this subject, but endeavoured to establish them by additional argument; contending that if the stamens and pistils were even destined to the discharge of the functions ascribed to them by the sexualist, yet there are many cases of perfect fructification in which they could not possibly co-operate to the production of the effect; adducing the example of the *Umbelliferæ*,

Urged by Pontedera.

in which the style, as he rightly remarked, does often not appear till after the stamens have fallen. But although the styles remain often inconspicuous till the period assigned by Pontedera, yet the stigma is previously mature, and consequently capable of the necessary co-operation.

Answered. But if the fact had been precisely what it appears to be in the objection, still it would have afforded no formidable argument against the doctrine of the sexes. For as the several flowers of the same plant, and much more the flowers of different plants, do not all come to maturity precisely at the same time, the flower whose stamens have fallen before the maturity of its pistil, may still be impregnated by the pollen of another flower or plant with which the period of its maturity is identical, and to which it may be contiguous. And in this way, we may believe, the impregnation of many flowers is effected, particularly in the case of *Zea Mays* or India Corn, the barren flowers of which upon the same plants have generally quite decayed before the fertile flowers have burst from the bosom of the leaves, at least as it grows in this country; as also in the case of the *Jatropha urens*, the barren floweres of which are generally protruded either several weeks sooner or several weeks later than the fertile flowers, and are consequently either decayed or not yet come to maturity at the time the style is perfect.

But if the fertile flower or plant should not be contiguous to the barren flower or plant, the pollen may yet be wafted to it by means of the wind, which curious phenomenon may sometimes be distinctly seen. On the 14th of June 1808, as I was accidently looking at a field of Rye-grass situated to the south of the spot on which I then stood, the atmosphere being clear, and the wind blowing gently from the west, I was surprized to observe a thin and sudden cloud, as if of smoke or fine dust, sweeping briskly along the surface of the Grass, and gradually disappearing. This cloud was soon followed by a second from a different quarter of the field, and that by a third, and so on in succession for several minutes. It was a general discharge of pollen from thousands of anthers bursting at the same moment, so that no stigma ready to receive the pollen could possibly fail of being supplied, either from the anthers proper to the flower of which it formed a part, or from those of some other flower discharging their contents into the general mass. The distance to which the pollen may be conveyed, on a short exposure to the action of a fine atmosphere, is not likely to do it any damage. Linnæus kept some of the pollen of the *Jatropha urens* in paper for more than a month, which even then fertilized the pistils it was shook over.

Insisted on by Dr. Alston.

The foregoing doubts or objections were entertained by the scrupulous or sceptical prior to the elucidations of Linnæus; and indeed they arose almost naturally out of the darkness in which the subject was then involved. But as the elucidations of Linnæus, though capable of affording conviction to the mind of the impartial inquirer, were not able to subdue passions, or to eradicate prejudices imbibed by education or excited by comparison, the doctrine of the sexes of vegetables met also with many opponents even in the time of Linnæus. The most zealous and redoubtable of these was Dr. Alston of Edinburgh; who, professing to be dissatisfied with every thing that had been said or done in support of the doctrine, made a show of refuting it by means of counter experiments, of which the most formidable are the following:—Admitting the result of the experiment of the cutting off of the anthers before the ripening of the pollen to be what Linnæus and others affirm, the abortion of the seed; he will not allow that it authorizes any conclusion in favour of the sexes of plants, because he thinks it is to be expected that a wound in any essential part of the plant, together with consequent loss of juice issuing from it, will occasion abortion in the seeds: and in confirmation of the presumption he quotes an experiment of Malpighi, who found that the ripening of the seeds of a Tulip was prevented by means of the pulling off of the petals before their expansion. But the two experiments are not at all of the same kind. In the latter there was a material injury done to the flower, in consequence of its being prematurely stripped of the covering of the corolla; in the former there was no material injury done to the flower, because the anthers were not cut off till after the natural expansion of the petals; in which case it is very well known that if the pistil is impregnated even with the pollen of another flower the seeds will still ripen. But Alston does not even admit the fact that the stripping of a plant of its stamens will render the seed abortive; alleging in support of his opinion Geoffroy's experiments on Maze, in which it was found that some of the ears ripened a few seeds, even when the stamens were entirely cut off before the bursting of the anthers; together with a similar experiment of his own upon a solitary Tulip, by which the ovary suffered nothing, but increased and came to maturity quite full of seeds. Now the defect of the argument is that we are not told whether the seeds were put to the proper test; that is, whether they were sown and found capable of germination.

The next counter experiment was made upon *Diœceous* plants. Three plants of common Spinach, which were removed before it could be told whether they were to be fertile or barren to a distance of at least eighty yards from the bed in which they were raised, and from which also they were separated by several intervening hedges, proved in the end to be all fertile, and ripened plenty of seeds that germinated again when sown. A solitary plant of Hemp also that sprang up in Dr. Alston's garden, having no other plant of the species within a mile of it to his knowledge, grew luxuriantly, and produced seeds that germinated also when sown.

Answered. These experiments are contradictory, no doubt, to the experiments of Linnæus; but they afford no argument against the doctrine of the sexes: for in the first place it cannot be proved that some of the pollen from the Spinach bed, or from a neighbouring male plant of Hemp, might not have reached the insulated plants by means of a favourable combination of circumstances; and in the next place it is not certain that the plants in question were not furnished with some minute and latent male flowers, by which the impregnation might have been effected.

Adduced by Spallanzani. But the most truly formidable, as well as the most philosophical, opponent of the doctrine of the sexes was the celebrated Spallanzani, who, having an hypothesis to support which that doctrine stood directly in the way of, found it necessary by all means to overthrow it if possible. It is the less surprising, therefore, that his experiments gave generally the result he wished for; though I do not introduce the remark with a view to detract from the degree of accuracy and of credit that is due to him, as he is well known to have been a most able and masterly experimenter, and to have exhibited a degree of candour and ingenuousness that must have been sufficient to prevent him from any intentional misstatement, in his giving also those results which were the most unfavourable to his own hypothesis.

Spallanzani's first experiment was made upon the *Ocymum basilicum*, an hermaphrodite plant; the anthers of several flowers being all cut off before the pollen was ripe, and the stigmas carefully secured from the access of the pollen of other flowers: in which case it was found that most of the seeds produced were evidently imperfect; though there were also a few that seemed to be completely matured, by their exhibiting on dissection the same appearances as others that had been exposed to the action of the pollen. But when these apparently perfect seeds were put to the proper test, they were found to be in reality imperfect; they did not germinate when sown.*

This result was discouraging enough, but was not sufficient to deter the Signior from further experiment, who now directed his attention to plants of the class *Monœcia*, to try whether he could not obtain from them a result more favourable to his hypothesis. The subject of his experiment was the *Cucurbita Citrullus*, the male flowers of which were destroyed as soon as they made their appearance; and the female flowers, in order to prevent all suspicion of the access of pollen, were inclosed in bottles luted to the stem by the neck, so as to exclude even the external air. The seeds which were procured in this way germinated and produced plants.

This result was as favourable to his hypothesis as could be wished; but to give to the argument

* Dissert. vol. iii. chap. i. Eng. Trans.

against the sexes all the weight he could, he now directed his attention to the class *Diœcia*, selecting as the subject of experiment some plants of the *Cannabis sativa* and *Spinacia oleracia*, from which he obtained also results equally favourable to his views. For after taking every precaution to secure the female plants from the access of pollen, as in the above example I suppose, seeds were still procured that germinated when sown.

From the above experiments, which appear indeed to have been made with great accuracy, Spallanzani seems inclined to conclude that the pollen is not in any case essential to fecundation; and rails much against the Linnæans for drawing general conclusions from particular premises, insisting that they should never go beyond the extent of their own experiments. But if the philosopher is not allowed to infer a general conclusion from a fair and legitimate induction of particulars, then our knowledge of the works of nature must remain very limited indeed, and a great many of Spallanzani's conclusions would not be what they are; for although he utterly disclaims all such procedure in principle, as being wholly illogical, yet he is by no means ashamed to resort to it in practice. And yet after all the parade of argument and experiment which he produces, the doctrine of the sexes of plants has suffered but little from his attack; for, in spite of his most desperate efforts, still he is obliged to admit it in part, that is in the case of hermaphrodite plants, which according, to his own experiments, form an obstacle that cannot be surmounted. And even with regard to monœcious and diœcious plants perhaps his experiments are not altogether free from error, as we can oppose to them the experiments of Linnæus on the very same species; who will be allowed to have been as attentive, and accurate, and successful an observer, as Spallanzani. The necessity of aiding the impregnation of Melons and Cucumbers, as practised by gardeners, is against his conclusions in the one case; and the possibility of the existence of some latent and undetected male flower lurking among the females is against it in the other.

But although Spallanzani is extremely anxious to disprove the doctrine of the sexes of plants, and although his experiments turned out to be rather favourable to his views, he does not seem after all to lay a great deal of stress upon them; thinking that the doctrine may still be true, and that the ripening of the seeds that were perfected without the aid of the male flowers might have been effected by means of a power, inherent in the female flowers, of propagating to a certain number of generations without the assistance of the male; as in the case of the *Aphis* among insects, according to the observations of Bonnet; and as in the case of some plants which he had himself observed to be propagated in this way to three generations. But there does not seem to be any very good ground for this supposition, nor does it seem to be much supported by the observations of

Bonnet on the *Aphis;* from which it does not follow that it had the power of propagating without the male to ten generations and no more, but rather that his observations were not pursued farther.

Spallanzani suggests also the possibility of the fecundation of the ovary, by means of some seminal principle residing in the pistil, and capable of supplying the place of the pollen, as well as necessary in the case of *Monœcious* and *Diœcious* plants, to ensure the perfection of the seed. This conjecture is perhaps countenanced in some degree by Koëlreuter's account of the chemical properties of the moisture exuding from the stigma when ripe, which he represents as being precisely the same with the chemical properties of the pollen. But this is leaving the matter precisely as it was taken up; for, if the suggestion of Spallanzani is true, then there exists at least a virtual sexuality in vegetables, to all intents and purposes.

Reiterated by Smellie. The last and least formidable adversary of the doctrine of the sexes of vegetables whose opposition I shall take any particular notice of at present is Mr. Smellie, author of the Philosophy of Natural History. Violent in proportion to his want of argument, he pushes his opposition to a greater length than any of his predecessors, though with less effect. Spallanzani had admitted that fecundation cannot be effected in hermaphrodites without the aid of the pollen, and that it may possibly be so effected in monœcious and diœcious plants also; contending only in fact for the establishment of the principle, that nature in extraordinary cases may have recourse to extraordinary means. But this is a concession which Mr. Smellie is by no means inclined to make, not admitting the existence of sexes, or the efficacy of the pollen, in any case whatever; to countenance which opposition one would think he must have been able to produce a variety of the most delicate and decisive experiments that ever were made on the subject, and that they had all succeeded to his wish. But what must be the surprise and disappointment of the reader when he is informed that all Mr. Smellie's dogmatism and pertinacity rests only on the very slender and narrow foundation of one poor experiment made upon the *Lychnis dioica*, which, by the by, is not his own experiment after all.

But in order to account for the very sweeping and decided conclusion of Mr. Smellie, it is to be recollected that he began his reasonings on the subject with a wish that the doctrine of vegetable sexuality might prove to be false, as well as with the hope of showing some little ingenuity in refuting a doctrine that was supported by the great Linnæus, and thus avowedly contending for victory rather than for truth. Like Spallanzani he begins by complaining of the insufficiency of the arguments drawn from analogy, by which the doctrine of the sexes had been occasionally illustrated; and, like Spallanzani also, is guilty of committing the very identical sin he condemns, employing such arguments wherever it suits his purpose, in all cases excepting that of sex. Witness his very first chapter on the analogies between the plant and animal; in which he exhibits, no doubt, an example of the most meritorious self-denial in forbearing to pursue the analogy throughout the several sexual organs, to which he certainly had a strong temptation, though he affects to regard the doctrine as an absurdity.

But if plants, like animals, are found to produce a new individual arising from a germe, seed, or egg, why should it be thought strange if they are furnished with analogous organs of generation? The alleged impregnation, says Mr. Smellie, is impossible: because if the doctrine were even true, the seed could be impregnated by the pollen only in a gelatinous state; and yet in most hermaphrodites, it has acquired considerable solidity before the pollen is shed. But this assumption, which is founded on the already refuted arguments of Pontedera, is good for nothing; because Mr. Smellie cannot tell when impregnation is and is not practicable merely from the state of the seed; and because it is not necessary that a seed should be impregnated by the pollen of its own flower. The experiment of the Leipsic and Berlin Palms is regarded as defective, because it was not continued for several successive years with and without pollen; and because it is possible it might have produced fertile seeds, in the year of the experiment at any rate. The futility of this objection requires no reply, and proves only that the objector was much in want of argument. It is said to be ridiculous to suppose that the pollen should be wafted by the winds, or carried by insects to impregnate the germe; but if it can be proved that the pollen retains its fecundating property for some considerable length of time after it is shed, which Linnæus has actually done, there is no absurdity in supposing that it may in some cases be conveyed to the pistil by the wind. The wind must necessarily waft it along, and it may certainly fall upon the stigma of the female plant: and, if insects should occasionally be the carriers of it, still it is far less wonderful than the feats of Spallanzani, in a case which I need not specify.

Varieties which have been proved to proceed, at least occasionally, from the intermixture of the pollen of plants of different species or varieties, Mr. Smellie ascribes wholly to soil and culture; dismissing the experiments on the subject by saying that the same results might have happened if the conditions had been reversed, and finally contending that the doctrine of the sexes is disproved by the fact of the propagation of plants from slips and layers in which new individuals are formed without the intervention of sexual organs. But if this is at all an argument, it is one from which the sexualist has but little to fear; as in the case of slips and layers there is in fact no production of a new individual, but merely a prolongation of the old; or at best a multiplication by means of division, as in the case of the *Polypi*: and although plants are capable of being multiplied in this manner, it is no proof that they may not be propagated by means of sexual intercourse also.

Refuted.

Such is the futility of Mr. Smellie's reasoning on this interesting and important subject, in which the reader will perceive that every thing is hypothetical. But at length we come to the boasted and long expected experiment which is to outweigh the whole body of evidence for the doctrine of the sexes, whether as adduced by Linnæus or others. And what is this boasted experiment? It is that of a female plant of the *Lychnis dioica*, which was placed by Mr. Smellie in a spot so situated that no male plant of the same species was known to grow within a mile of it, and which yet produced seeds. It is to be recollected, however, that the experiment did not succeed with Mr. Smellie himself; but when the plant was removed to the garden of Dr. Rutherford at Edinburgh, it succeeded after a year or two of expectation. But where is the proof that there was no male plant within a mile of it during the whole time of experiment; which, if it could be produced, would be but of little avail, as we are not told after all whether the seed was capable of germination.

By the opponents of Cryptogamy.

Lastly, the doctrine of the sexes of plants has been objected to as altogether unfounded, upon the presumption that plants destitute of conspicuous flowers are destitute of flowers altogether, and consequently of sexual organs, which if not necessary in some cases are not necessary in any. Plants of the class *Cryptogamia* are, as their name imports, destitute of conspicuous flowers, and hence they have been regarded by many botanists as being destitute of flowers altogether; as may be seen from the title by which they are characterized in the method of Tournefort and others: but it is now very well known that plants may possess all that is necessary to constitute a flower, without being furnished with a gaudy and conspicuous corolla.

If the stamens and pistils are but present under whatever shape, they constitute to all intents and purposes an effective flower, because they are by themselves capable of producing perfect seed; as in the case of *Hippuris*, *Salicornia*, and *Lemna*, which are indeed destitute of petals, but are furnished with stamens and pistils that produce seeds. But in plants that are strictly cryptogamous many botanists have denied the existence of stamens and pistils under any form whatever, regarding it as absurd even to suppose their existence; and either contending that they are propagated without seeds, or inferring that seeds may be formed without the intervention of sexual organs, which if not necessary to the reproduction of what are called cryptogamous plants are not necessary to the reproduction of any, and consequently do not exist. But a contrary inference would have been the more logical, and might have been de duced thus:—The organs of fructification have been detected in most plants, and the peculiar functions of the several organs. There are, however, some plants producing fruit, in which the organs of fructification have not yet been detected, but we conclude, from analogy, that such organs exist in them, and discharge similar functions: and this inference has been accordingly confirmed by means of the actual discoveries to which the aid of the microscope has led. The first minute portion that ever was removed of the veil concealing the fructification of cryptogamous plants was removed by the hand of Michelli, in his detecting of the stamens and pistils of Mosses, though he does not seem to have entertained any correct notion of their respective functions:* and to the meritorious example of minute investigation which he thus exhibited we are perhaps indebted for the succeeding illustrations of Dillenius and Linnæus, who detected indeed the parts of the fructification of the Mosses, but unfortunately mistook the capsule for the anthers, and consequently the seeds for the pollen. At length, however, the task of investigation was undertaken by the illustrious Hedwig, who detecting the errors of his predecessors, and penetrating into the very recesses of the mystery, exhibited a view of the subject so correct and so well supported by fact as to leave but little doubtful, and to authorise a conclusion directly the reverse of that of Linnæus—the anthers of Linnæus being proved to be in fact the

Refuted by the discovery of the fructification of the mosses.

* Nova Plantarum Genera, 1729.

the fruit, and the cones and stars being presumed to be the male organs.

The legitimacy of this conclusion seems now to be almost universally admitted; and yet it has been lately suspected that the Mosses are hermaphrodites, containing in the urn both the germe and pollen. Such at least is the opinion of M. Palisot Beauvois, a French botanist of some considerable celebrity;* who regards the column as constituting an individual viscus, and containing a sort of granular and powdery substance as well as the urn—the powder of the latter, according to M. Beauvois, being the pollen; and that of the former being the seed. This opinion is certainly plausible, and may perhaps prove to be the truth. In the month of November, 1805, I examined some capsules of *Bryum argenteum* before the *operculum* had fallen, but not till it had become a little brownish with age, and found that the column actually contained within it a quantity of fine granules imbedded in a pulpy and viscid substance; the granules of the capsule being in nearly the same state at the same time. The granules of the column were easily distinguished from those of the capsule both in size and colour, the former being by much the smallest and almost perfectly transparent; and the latter being comparatively large, as well as opaque and green.

Such are the two sets of granules on which M. Beauvois founds his opinion of the sexual organs

* Prodrome des Mousses et des Lycopodes.

of the Mosses; and it claims at least a fair and impartial consideration, as the cones and stars of Hedwig are by no means universal. I begin, however, to suspect that M. Beauvois has, like Dillenius and Linnæus, misapprehended the true and natural character of the two containing organs respectively; and that the powder of the column is the pollen, and the powder of the urn the seed. My reasons are the following:—1st, Because the granules of the column are much smaller than the granules of the capsule, and therefore more analogous to the character of pollen in general. 2dly, Because like the pollen they are observable only at a certain period of the plant's growth; that is about the time of the fall of the *operculum*, or a little before it, so that if you look for them sooner you find only a gelatinous mass, and if later, they are gone. 3dly, Because the column after discharging its contained granules becomes, like the anthers of other plants, shrunk and shrivelled up long before the granules of the capsule are ejected.

Perhaps an objector may say that the stamens are on this supposition contained within the pistils, which is contrary to all analogy. But why may not the Mosses be allowed to form an exception, if no other plants do, as they are in almost all other respects confessedly anomalous. And if the urn contains the pollen, and the column the seed, then do the Mosses present an anomaly much more wonderful than the eccentric position of the fruit;

namely, that of ripening and discharging their seeds before the discharge of the pollen, which is an absurdity.

I know it is the opinion of Mr. Brown,* that M. Beauvois has been led into a mistake with regard to the source of the central grains, which he believes to have been pushed into the substance of the column by the pressure necessary to cut it up, or to have been carried over the surface of the section by the cutting instrument. This, it must be confessed, is giving M. Beauvois very little credit for the accuracy of his observations; although I am satisfied there are but few botanists better qualified than Mr. Brown to judge of that accuracy. It is not for me to attempt to decide any thing where such authorities have differed; nor am I possessed of a sufficient induction of particulars to form any decision; but I will venture to add that I am quite confident of the existence of two sets of granules in the individuals of the species I examined; as well as positive that the granules of the urn were not forced into the substance of the column by means of the dissecting instrument.

And presumptive discovery of that of the Ferns, Algæ, and Fungi.

But the Mosses are not the only tribe of plants that has been proved to be furnished with organs of fructification after having been supposed to be totally destitute of them. The Ferns, Algæ, and Fungi, according to the illustrations of Hedwig,

* Lin. Trans. vol. x. part ii.

As illustrated by Hedwig,

have been found to exhibit similar proofs of sexuality also, so that Cryptogamy can now be scarcely said to exist; and much less any thing like equivocal generation; whence it seems indeed to follow, according to the favourite maxim of that great naturalist, that all plants spring from seed.

At the same time it must be admitted that there still exists some considerable diversity of opinion on this subject, notwithstanding all that has been done by Hedwig and others to prove that all plants are furnished with distinct sexual organs, capable of producing perfect seeds. Gærtner, a most able and accurate observer of nature, controverts the opinion of Hedwig, and contends that many of the plants called Cryptogamous, instead of being thus completely furnished with stamens and pistils, are either defective in some part of their sexual apparatus, so as not to exhibit the male and female organs distinct; or are destitute of a sexual apparatus altogether, and propagated not by seeds, but by gems. In the former class he ranks the Ferns, Mosses, and *Fuci*, discarding the alleged stamens of Hedwig and others altogether, and contending that the ovary is also the organ of fecundation, absorbing and elaborating a mucous substance with which it is found to be surrounded, particularly in the *Fuci*, and thus effecting its fecundation, as in the Aphrodites of Adanson.* But

* Famil. des Plantes, vol. i. p. 264.

if the mucous substance is that by which impregnation is effected, it is to all intents and purposes a pollen, which in the case of the *Fuci* is necessarily mucous as being suited to the nature of the element in which the plant vegetates.* In the latter class he ranks the *Fungi, Confervæ,* and *Ulvæ,* contending that they are wholly without seed as without sex, and propagated merely by gems. But on the contrary, M. Correa De Serra contends that they are in all respects similar to the grains of the *Fuci,* and equally entitled to the appellation of seeds. So that if Gærtner has erred on the one hand in denying the universality of the sexes, and degrading the grains in question from the rank of seeds, Hedwig has erred on the other, in elevating a variety of substances, rather too hastily, to the rank of stamens and pistils.

And Gærtner.

But if Gærtner's theory should even be unfounded, it exhibits at least a view of the comparative perfection of plants as connected with the perfection of their sexual apparatus, which should not be omitted. When the species is propagated by gems only, without seed, as in the lowest orders of vegetable beings, no sexual organs are perceptible. When the seed is inconspicuous and seemingly nothing but an embryo, then the female organs are perceptible but not the male organs, and the plants are called Aphrodites. When the radicle

* M. Correa De Serra Fruct. of Submersed Algæ.

of the embryo, constituting a nucleus, is perceptible in the seed, then also the pollen appears, but the flower has no beauty. And when the embryo is found no longer constituting a mere nucleus, but surrounded with its cotyledons, then there is to be seen both the apparatus of flower and of sexual organs. The first class includes plants without sex, the *Confervæ*, *Ulvæ*, *Fungi*. The second class includes the Approdites, the *Filices*, *Musci*, *Fuci*. The third class includes what are called ambiguous plants, such as *Zostera*, *Zamia*, *Cycas*. And the fourth class includes all plants whatever with conspicuous flowers. This gradation, if not true, is at least beautiful; and will perhaps be admitted to be also useful; from which we may infer the truth of the observation—that even the very errors of a great mind are edifying.

CHAPTER VII.

OF THE IMPREGNATION OF THE SEED.

Admitting that the stamens and pistils are the male and female organs of vegetable generation, and that the pollen is the substance by which the impregnation of the seed is effected, how is it conveyed to the ovary? and what is the amount of its action?

SECTION I.

Access of the Pollen.

WHEN the stamens and pistils are situated near each other, as in the case whether of Hermaphrodite or Monœcious flowers, the elastic spring with which the anther flies open will generally be sufficient to disperse the pollen, so as that part of it must infallibly reach the stigma. The facilities tending to ensure the access of the pollen as resulting from the relative proportion, situation, and mutual sympathies of the stamens and pistils have been already noticed; as well as the possible action of winds wafting the pollen to a distance, and hence including the case of Diœcious plants also. But with all the above facilities the impregnation of the seed would still in many cases be impracticable even in Hermaphrodite flowers, without further aid; particularly in such as do not perfect their stamens and pistils at the same time. For although the action of the wind cannot but be efficacious in some such cases; yet it will, in some others, naturally give to the flower a direction calculated rather to prevent than to aid the access of the pollen, by causing the corolla to veer round like a vane according to the quarter from which it may happen to blow; or the very figure of the corolla may operate as a bar to the entrance of the pollen

By the agency of winds.

which must be surmounted by extraordinary means.

Or insects attracted by the nectar.

What then are the means instituted by nature for effecting the impregnation of Hermaphrodites so circumstanced? The true reply to this inquiry seems to have been first suggested by Koëlreuter, namely, the agency of insects; and has been since confirmed by the more leisurely observations of Spregnel, who found that the pollen in the above case is very generally conveyed from the anther to the stigma through the instrumentality of Bees, though sometimes through that of insects peculiar to a species. The object of the insect is the discovery of honey, in quest of which, whilst it roves from flower to flower and rummages the recesses of the corolla, it unintentionally covers its body with pollen, which it conveys to the next flower it visits, and brushes off as it acquired it by rummaging for honey; so that part of it is almost unavoidably deposited on the stigma, and impregnation thus effected. Nor is this altogether so much a work of random as it at first appears. For it has been observed that even insects, which do not upon the whole confine themselves to one species of flower, will yet very often remain during the whole day upon the species they happen first to alight on in the morning. And their agency is also completely secured, from the necessity they are under of procuring food; though nature in her care for the impregnation of the vegetable has not only lodged a honey in the flower to tempt the taste of insects, but seems to have furnished also the means of attracting even the eye. This is thought to be done by means of the coloured spots with which many flowers secreting a honied fluid are marked, which Spregnel calls *maculæ indicantes*, as indicating the treasure that is contained in the flower, and thus attracting the attention of the insect. But the very figure of the flower seems often intended to produce the same effect. Spregnel has enumerated several hundreds of flowers which in their figure as well as colour resemble insects, and hence attract the notice of the plunderers of their honied stores. The beautiful example of the Bee Orchis is known to almost every body.

Or colour,

Or figure of the flower.

Such then are the means by which the notice of the insect is attracted; and such also is the structure of the internal parts of the flower, that it must of necessity pass across the stamens and pistils in procuring the honey it is in quest of, which passage is often a work of considerable difficulty, particularly when the tubular part of the corolla is beset with hairs, as in many flowers of the class *Pentandria* and *Didynamia*. But one of the most difficult and singular cases of Hermaphrodite impregnation as aided by the agency of insects is that of the *Aristolochia Clematitis*. The corolla of this flower, which is tubular, but terminating upwards in a ligulate limb, is inflated into a globular figure at the base. The tubular part is internally beset with

In Hermaphrodites.

stiff hairs pointing downwards. The globular part contains the pistil, which consists merely of a germen and stigma together with the surrounding stamens. But the stamens being shorter than even the germen, cannot discharge the pollen so as to throw it upon the stigma, as the flower stands always upright, till after impregnation. And hence without some additional and peculiar aid the pollen must necessarily fall down to the bottom of the flower. Now the aid that nature has furnished in this case is that of the agency of the *Tipula pennicornis*, a small insect, which, entering the tube of the corolla in quest of honey, descends to the bottom and rummages about till it becomes quite covered with pollen; but not being able to force its way out again owing to the downward position of the hairs, which converge to a point like the wires of a mouse-trap, and being somewhat impatient of its confinement, it brushes backwards and forwards trying every corner till after repeatedly traversing the stigma it covers it with pollen sufficient for its impregnation; in consequence of which the flower soon begins to droop, and the hairs to shrink to the side of the tube, effecting an easy passage for the escape of the insect.*

In monœcious,

Monœcious plants are, according to Spregnel, mostly impregnated by insects also, excepting such as are destitute of nectaries. But many of them do not require that aid, in which case the male and

* Willdenow, p. 317.

female flowers stand close together, as in *Typha, Coix, Carex;* the females being lowest, and their petals being deeply or minutely laciniated so as not to interrupt the pollen in its fall, as in the genus *Pinus*.

The impregnation of Diœcious plants is often effected by insects also, as has been already seen in the case of the Fig, and their flowers are said to be always furnished with nectaries; the male flowers being larger than the female flowers, that the insect, as it has been thought, may have the better opportunity of loading itself with pollen.* And diœcious plants.

From the fact of the agency of insects in conveying the pollen to the stigma it will follow that no plant requiring such aid can possibly perfect its seed unless the specific insect has access to it, or unless some such aid is given to it by the cultivator. And hence botanists attribute the imperfection of the seeds of hot-house plants to the want of the insect by which the species may be impregnated in its native climate. This conjecture is countenanced by the following experiment, as related by Willdenow:—A plant of *Abroma augusta* had flowered for many years in a hot-house at Berlin without producing any fruit; but when the gardener by means of a hair pencil placed a little of the pollen upon the stigma of several of the flowers, perfect fruit was produced from which new plants were raised.

* Willdenow, p. 320.

Passage of pollen through the style.

But admitting that the pollen is conveyed to the stigma by the means above stated, how is it thence conducted to the ovary? It was at one time generally supposed that the pollen is conducted from the stigma to the ovary by means of a longitudinal canal perforating the style. This canal is distinguishable in many of the liliaceous plants in which it seems indeed to constitute the passage of the pollen particularly from the phenomenon of the *Amaryllis formosissima*, the fluid exuding from the stigma of which returns again through the perforation of the style tinged with yellow, the colour of the pollen. But the existence of the canal in question, though distinguishable in the *Amaryllis formosissima*, and other liliaceous plants, cannot be admitted as a universal property of the style, at least it cannot be detected. And if it is so very fine as to escape all observation, then it could not admit the particles of pollen, which are in some cases comparatively large, as in Marvel of Peru; the pollen of which exceeds the style itself in diameter, and could not consequently be admitted by a central canal.

But in order to effect the impregnation of the seed it is not necessary that the particles of pollen should enter the style entire. The finer part of their contents is sufficient, and is indeed the only effective part in the act of fecundation: so that whether we regard it as a subtle and elastic vapour with Grew, and Adanson; or merely as an oily and gelatinous fluid exuding or exploding from the globule; still it will admit of being conducted through the channel of the tubes of the style, although no central canal should exist in it.

Quantity of pollen necessary.

But another question has also arisen out of the subject with regard to the quantity of pollen necessary to effect impregnation. Adanson was of opinion that the smallest possible particle, if conveyed to the ovary is sufficient. But this opinion is supported by no proof, and is even contradicted by later observation; the merit of having ascertained the fact seems due to Koëlreuter, whose experiments are decisive of the question. The globules of pollen contained in all the anthers of an individual flower of *Hibiscus syriacus*, were 4863, of which 50 or 60 at least were necessary to effect a complete impregnation. For when the attempt was made with a smaller number the seeds were not all ripened, though those that were ripened were perfect. Ten globules were the least by which the impregnation even of a single seed could be effected in this plant.* But in the *Mirabilis Jalappa* and *longiflora*, the flowers of which contained about 300 globules of pollen, two or three were found sufficient for impregnation, as the seed was not improved by the application of more. It was also found that the impregnation of flowers having two or more styles was completely effected, even when

* Willdenow, p. 323.

the pollen was applied but to one of them; which shows that there is a communication between all the styles, and consequently between all the germens.

SECTION II.

Agency of the Pollen.

ADMITTING that the pollen is conducted to the ovary through the channel of the tubes of the style, how after all is the ovary fecundated; or the seed rendered fertile? On this subject naturalists have been much divided; and according to their several opinions have been classed under the respective appellations of ovarists, animalculists, and epigenesists.

SUBSECTION I.

Theory of the Ovarist.—According to the opinion of the first class, the embryo pre-exists in the ovary, and is fecundated by the agency of the pollen as transmitted to it through the style. This seems to have been the opinion of Grew, who says expressly in his Anatomy of Plants, that when the summits of the stamens open, and the pollen is discharged upon the pistil, some subtile and vivifying effluvium escapes; which, descending through the medium of the style, impregnates the embryo. Bonnet and Haller seem to have been of the same

As maintained by Grew,

opinion also, as well as many other eminent naturalists. But the most convincing evidence in support of the opinion of the ovarists is that which has been produced by Spallanzani, as founded on a series of observations on the flowers of the *Spartium junceum*. This plant was chosen on account of its producing at the same time flowers in all the different stages of progress. His first observations were made upon flower buds not yet expanded: they seemed to form a compact and solid body; but upon being dexterously opened, the petals, which were yet green, were with some difficulty discovered, then the stamens, and then the pistil. The powder of the anthers was even perceived imbedded in a glutinous substance; when the pistil was freed from the surrounding integuments, and attentively viewed with a good glass, the pod was also discovered of about $1\frac{1}{10}$ line in length. Several protuberances were seen upon its sides; which, upon opening it longitudinally, were found to be occasioned by the seeds, which though but small globules were already discoverable, arranged in their natural order, and attached by filaments to the interior of the Pod. Upon dissection, they did not exhibit any appearance of the several parts and membranes into which the mature seed may be divided; but a spongy, homogenous mass. Flowers in the same state of forwardness were not fully expanded till twenty days after. On dissecting buds of a larger size the petals were found to be somewhat yellowish and

Spallanzani.

less compact; and the powder of the anthers was thrown out by the slightest agitation; but the lobes and plantlet were not yet perceptible in the seeds.

On the eleventh day after the flowers had fallen, that is, after impregnation had taken place, the seeds which were formerly globular began to assume the figure of an heart, attached to the pod by the basis, and exhibiting the appearance of a white point towards the apex. And when the heart was cut open longitudinally, the white point proved to be a small cavity enclosing a drop of liquid.

On the twenty-fifth day after the flowers had fallen, the cavity was much enlarged towards the base; but was still full of the liquid, in the midst of which there appeared a small and semi-transparent body, of a yellowish colour and gelatinous consistence, fixed by its two extremities to the opposite sides of the cavity.

In a month after the flower had fallen, the heart-shaped seeds became kidney-shaped.

In forty days after the flower had fallen, the cavity was quite filled up with the body that had been generated within it; and which was now found to consist of a thin and tender membrane enveloping the two seed-lobes, between which the plantlet attached to the lower extremity was also perceptible. And hence the seed was now visibly complete in all its parts.

From these and a variety of other observations on a variety of other species, all of which exhibited similar appearances in the generation of the seed, Spallanzani concludes that the seeds pre-exist in the ovary before the access of the pollen, by which they are merely rendered fertile; and contends that the embryo, though not previously perceptible, may yet previously exist.*

The theory of the ovarists is supported also by Gærtner, who describes the vegetable egg as pre-existing in the ovary, where, furnished with its proper integuments, it waits the fecundating influence of the pollen which is necessary to its complete developement; so that it requires in fact the exertion of two distinct energies to bring it to perfection, the vital principle, and the seminal; the former generating and organizing the different parts of which the egg consists in common with the other parts of the plant; and the latter communicating to the egg thus formed a distinct vegetable life.† Gærtner,

SUBSECTION II.

Theory of the Animalculist.—But the theory of the ovarists is not without its difficulties; for as the embryo is never found to make its appearance till after fecundation, it has been thought that it must necessarily pre-exist in the pollen of the anther; from which it is conveyed to the ovary through the

* Spal. Dissert. vol. iii. chap. i. Eng. Trans.
† Gært. Introd.

As originating in the speculations of Leuwenhoeck,

medium of the style, and afterwards matured. This theory was founded upon that of Leuwenhoeck, with regard to annual generation; which supposes the pre-existence of animalcula in the seminal principle of the male; the animalcula being conveyed *in coitu* to the ovary of the female where alone they are capable of developement.* Hence it has been denominated the theory of the animalculists, and transferred to the case of vegetables by Morland, Needham, Gleichen, and others, who regard the pollen as being a congeries of seminal plants, one of which at least must be conveyed to the ovary entire before it can become prolific.

And adopted by Morland.

But if the embryo pre-exists in the pollen may it not be detected by inspection before impregnation takes place? Spallanzani examined the pollen in its ripe and perfect state, with great care, and under glasses of the highest magnifying powers, but could distinguish nothing exhibiting the appearance of an embryo. It may be said, however, that the embryo must still be supposed to pre-exist in the pollen, though not visible, as Spallanzani has said of its pre-existence in the ovary; and that its invisibility is no proof of its non-existence. The animalculists have no doubt a right to offer this reply; but as the embryo is not visible whether in the ovary or pollen, till after fecundation has taken place, no conclusion can be drawn on either side from the circumstance of its invisibility.

* Phil. Trans. No. 145, p. 74.

But admitting that the invisibility of the embryo is no proof of its non-existence in the pollen, the total want of a passage, in most styles, fit to conduct the particles of pollen entire, exposes this theory to the most serious objections, if it does not rather render the alleged mode of impregnation altogether impracticable. And if a passage of sufficient width were found to exist even in all styles, still the probabilities of the two cases are in favour of the ovarist. For if the embryo is to pre-exist at all, is it not more likely that it should pre-exist in the ovary where it is to be brought to maturity; than that it should first be generated in one organ or plant, and then transferred to another to be developed? Is it not also most extraordinary that the embryo should so invariably assume the same position in the same species of seed, if it is merely conducted to the ovary from a different organ or plant, and introduced as it were at random? And is not the doctrine of the ovarist countenanced from the analogy of the process for which he contends to that of the generation of the animal egg, which is produced complete in all its integral and distinct parts even without the co-operation of the male, though still destitute of the principle of fertility? And finally is it not further countenanced from the fact of the apparent and numerical perfection of parts often observable in the fruit of insulated female plants, in which the embryo is not always wanting, but only not fecundated? For which reasons the theory of the ovarist seems to me to be much more consonant to truth than that of the animalculist.

SUBSECTION III.

Theory of the Epigenisist.—But the difficulties inseparable from both theories, together with the phenomenon of hybrid productions, have given rise also to a third; this is the theory of the epigenisists, who maintain that the embryo pre-exists neither in the ovary nor pollen, but is generated by the union of the fecundating principles of the male and female organs; the former being the fluid issuing from the pollen when it explodes; and the latter, the fluid that exudes from the surface of the stigma when mature. As applicable to the case of plants, this theory has been stoutly defended by Koelreuter, who adduces in support of it a variety of experiments instituted with a view to ascertain the fact by means of impregnating the ovary of one species with pollen taken from another, in which cases the plant obtained from the seed uniformly exhibited a combination of the characters of both species. The following is a most prominent example, being the result of his experiments on *Nicotiana rustica* and *paniculata;* the former having egg-shaped leaves, with a short and yellow corolla approaching to green; and the latter having roundish or cordate leaves, with a green corolla approaching to yellow, and a stem longer by one

As defended by Koelreuter.

half. A flower of the former species was accordingly deprived of all its stamens, and fecundated with pollen from a plant of the same species. The plant raised from the seed thus obtained was an hybrid, exhibiting in all its parts an intermediate character betwixt the two species from which it sprang. The stamens of this hybrid, as well as of all others he ever raised, were imperfect; but when its pistils were impregnated with pollen from the *paniculata* as before, the new hybrid obtained from the seeds now produced was more like a *paniculata* than formerly; and when the experiment was continued through several successive generations, it was at last converted into a perfect *paniculata*.*

This is thought to be an infallible demonstration of the truth of the doctrine of the epigenisists. But why may not the pollen of one species of plant be allowed to produce some particular change upon the developement of the embryo of another species, although that embryo should be supposed to have pre-existed in the ovary? The action of the pollen thus introduced must amount to something; and it is just as difficult to conceive how an individual whether proper or hybrid should be generated from the union of the seminal principles of two plants of the same or of a different species, as from the peculiar effect of the pollen of the same or of a different species, upon an embryo already existing.

* Willdenow, p. 328.

But the doctrine is yet liable to a much more serious objection; for if the seed is generated from the union of two fecundating principles which form an intermediate offspring, then female plants of the class *Diœcia* ought occasionally to produce seeds whose offspring shall be *Hermaphrodite*, or at least *Monœcious*, which was never yet known to happen.

SECTION III.

Hybrids.

As illustrated by the experiments of Bradley and Mr. Knight.

ALTHOUGH the arguments of the epigenisists are by no means satisfactory, yet it cannot be denied that hybrid productions partake of the properties both of the male and female from which they spring. This was long ago proved to be the fact by Bradley,* and more recently confirmed by the experiments of Mr. Knight; as well as happily converted to the advantage of the cultivator. Observing that farmers who rear cattle improve the progeny by means of crossing the breed, he presumed from analogy that the same improvement might be introduced into vegetables. His principal object was that of procuring new and improved varieties of the Apple and Pear to supply the place of such as had become diseased and improductive, by being cultivated beyond the period

* New Impr. of Plant. and Garden. Lond. 1717.

which nature seems to have assigned to their perfection. But as the necessary slowness of all experiments of the kind, with regard to the fruit in question, did not keep pace with the ardour of his desire to obtain information on the subject, he was induced to institute some tentative experiments upon the common Pea, a plant well suited to his purpose, both from its quickness of growth, and from the many varieties in form, size, and colour, which it afforded. In 1787, a degenerate sort of Pea was growing in his garden which had not recovered its former vigour even when removed to a better soil. Being thus a good subject of experiment, the male organs of a dozen of its immature blossoms were destroyed, and the female organs left entire. When the blossoms had attained their mature state, the pollen of a very large and luxuriant grey Pea was introduced into the one half of them, but not into the other. The pods of both grew equally; but the seeds of the half that were unimpregnated withered away, without having augmented beyond the size to which they had attained before the blossoms expanded. The seeds of the other half were augmented and matured as in the ordinary process of impregnation; and exhibited no perceptible difference from those of other plants of the same variety, perhaps, because the external covering of the seed was furnished entirely by the female. But when they were made to vegetate in the succeeding spring the effect of the experiment was obvious. The plants rose with great luxuriance, indicating in their stem, leaves, and fruit, the influence of this artificial impregnation; the seeds produced were of a dark grey. By impregnating the flowers of this variety with the pollen of others, the colour was again changed, and new varieties obtained superior in every respect to the original on which the experiment was first made, and attaining, in some cases, to a height of more than twelve feet. In these experiments it was observed that the plant had a stronger tendency to produce coloured blossoms and seeds than white ones. For when the pollen of a coloured blossom was introduced into a white one, the whole of the future seeds were coloured. But when the pollen of a white blossom was introduced into a coloured one, the whole of the future seeds were not white.*

Superfetation.

Mr. Knight thinks his experiments on this subject afford examples of superfetation, a phenomenon the existence of which has been admitted amongst animals, but of which the proof amongst vegetables is not yet quite satisfactory. Of one species of superfetation Mr. Knight has certainly produced examples; that is, when, by impregnating a white Pea blossom with the pollen both of a white and grey Pea, white and grey seeds were obtained. But of the other species of superfetation in which one seed is supposed to be the joint issue of two

* Phil. Trans. 1789.

males, the example is not quite satisfactory. Such a production is perhaps possible, and further experiments may probably ascertain the fact; but it seems to be a matter of mere curiosity, and not apparently connected with any views of utility. But the utility of the experiments, in as far as they show the practicability of improving the species, is very obvious. And the ameliorating effect is the same whether by the male or female; as was ascertained by impregnating the largest and most luxuriant plants with the pollen of the most diminutive and dwarfish, or the contrary. By which means any number of varieties may be obtained, according to the will of the experimenter, amongst which some will no doubt be suited to all soils and situations. Mr. Knight's experiments of this kind were extended also to wheat; but not with equal success. For though some very good varieties were obtained, yet they were found not to be permanent.

Improvement of fruits.

But the success of his experiments on the Apple-tree were equal to his hopes. This was indeed his principal object, and no means of obtaining a successful issue were left untried. The plants which were obtained in this case were found to possess the good qualities of both of the varieties employed, uniting the greatest health and luxuriance, with the finest and best flavoured fruit.*

* Phil. Trans. 1799.

Many experiments of a similar nature were tried on other plants also; from which it appeared that improved varieties of every fruit and esculent plant may be obtained by means of artificial impregnation, as they were obtained in the cases already stated. Whence Mr. Knight thinks that this promiscuous impregnation of species has been intended by nature to take place, and that it does in fact often take place, for the purpose of correcting such accidental varieties as arise from seed, and of confining them within narrower limits. All which is thought to be countenanced from the consideration of the variety of methods which nature employs to disperse the pollen, whether by the elastic spring of the anthers, the aid of the winds, or the instrumentality of insects.

But although he admits the existence of vegetable hybrids, that is, of varieties obtained from the intermixture of different species of the same genus, yet he does not admit the existence of vegetable mules, that is of varieties obtained from the intermixture of the species of different genera; in attempting to obtain which he could never succeed, in spite of all his efforts. Hence he suspects that where such varieties have been supposed to take place, the former must have been mistaken for the latter. It may be said, indeed, that if the case exists in the animal kingdom, why not in the vegetable kingdom? to which it is perhaps difficult to give a satisfactory reply. But from the narrow limits within which this intercourse is in all cases circumscribed, it scarcely seems to have been the intention of nature that it should succeed even among animals.

CHAPTER VIII.

OF THE CHANGES CONSEQUENT UPON IMPREGNATION.

WHATEVER may be thought of the different opinons of the ovarist, animalculist, and epigenesist, and whichsoever of them may be adopted by the phytological inquirer, it is at all events an object of the first importance to trace out the peculiar changes consequent upon impregnation, as effected, whether in the flowers or fruit.

SECTION I.

External Changes.

AT the period of the impregnation of the ovary the flower has attained to its ultimate stage of perfection, and displayed its utmost beauty of colouring and richness of perfume. But as it is now no longer wanted, so it is no longer provided for in the economy of vegetation. Its period of decline has

Decay of the flower.

commenced; as is indicated first by the decay of the stamens, then of the petals, and then of the calyx, which wither and shrink up, and finally detach themselves from the fruit altogether, except in some particular cases in which one or other of them becomes permanent and falls only with the fruit. The stigma exhibits also similar symptoms of decay and the style itself often perishes. The parts contiguous to the flower, such as the bractes and floral leaves, are sometimes also affected; and finally the whole plant, at least in the case of annuals, begins to exhibit indications of decay. But while the flower withers and falls, the ovary is advancing to perfection, swelling and augmenting in size, and receiving now all the nutriment by which the decayed parts were formerly supported. Its colour begins to assume a deeper and richer tinge; its figure is also often altered, and new parts are even occasionally added—wings, crests, prickles, hooks, bloom, down. The common receptacle of the fruit undergoes also similar changes, becoming sometimes large and succulent, as in the Fig and Strawberry; and sometimes juiceless and indurated, as in compound flowers. Such are the external changes consequent upon impregnation as effecting the flower and fruit.

Augmentation of the ovary.

SECTION II.

Internal Changes.

Ovary divisible into distinct organs.

If the ovary is cut open as soon as it is first discoverable in the flower, it presents to the eye merely a pulpy and homogeneous mass. But if it is allowed to remain till immediately before the period of its impregnation, it will now be found to be divisible into several distinct parts, exhibiting an apparatus of cells, valves, and membranes, constituting the pericarp, and sometimes the external coats of the seed. In this case the umbilical cord is also to be distinguished; but the embryo is not yet visible. These changes therefore are to be attributed merely to the operation of the ordinary laws of vegetable developement, and are not at all dependant upon impregnation.

Umbilical cord.

But impregnation has no sooner taken place than its influence begins to be visible; the umbilical cord, which was formerly short and distended, is now generally converted into a long and slender thread. Sometimes the position of the seed is altered. Before impregnation the seeds of *Caryophyllus aromaticus* and *Metrosideros gummifera* are horizontal; after impregnation they become vertical. Before impregnation the seeds of *Magnolia* are erect; after impregnation they become inverted and pendulous.*

* Gært. De Seminibus.

Position of the seed. The figure of the seed is often also altered in passing from its young to its mature state; changing from smooth to angular, from tapering to oval, from oval to round, and from round to kidney-shaped. But all seeds are not brought to maturity of which the rudiments may exist in the ovary. *Lagœcia* and *Hasselquestia* produce uniformly the rudiments of two seeds, of which they mature but one.*

But the principal changes resulting from impregnation are operated in the seed itself, which, though previously a homogeneous and gelatinous mass, is now converted into an organized body, composed of different membranes enveloping, or enveloped by, one another.

The testa. The *Testa*, which is the external coat of the seed, is formed from the original cuticle of the nucleus, and augmented by means of the juices conveyed to it through the umbilical cord. Hence it is sometimes formed, but never capable of being detached from the mass of the nucleus, previous to fecundation; after which it is easily, though not spontaneously separated, till the maturity of the fruit.

Subtesta. The *Subtesta*, which is the inner coat of the seed and lies immediately under the *testa*, originates in the interior vessels of the umbilical cord, which are prolonged in a multiplicity of ramifications throughout the whole membrane, is soft and pulpy till the embryo is matured; as the juices by which the embryo is nourished pass through it. It is seldom distin-

* Gært. De Seminibus.

guishable before impregnation; but when the seed is ripe it is easily detached from the interior parts, though not always easily from the *testa*.

With these two integuments the enclosed nucleus of the seed is in its young state almost always furnished, and is generally found to consist of the four following parts:—the *Chorion*, the *Amnios*, the *Sacculus colliquamenti*, and the *Embryo;* all, except the *chorion*, being the uniform product of fecundation, and all, except the embryo, often disappearing in the mature state of the seed.

The chorion. The *Chorion*, so named by Malpighi,* is the soft and pulpy substance of the primitive nucleus of the seed, forming at first its principal mass; but finally disappearing and leaving no trace of its existence behind, being gradually converted into the nourishment of the other parts.

The amnios. The *Amnios* is a clear and transparent fluid, the product of fecundation, sometimes thin and sometimes gelatinous, generated and contained within the *chorion*, but at last absorbed by the embryo, or converted into a solid substance called the albumen.

The sacculus colliquamenti. The *Sacculus colliquamenti* is a thin, white, and pellucid membrane, originating in the vessels of the internal umbilical cord, and being a proper integument with which the *amnios* is sometimes invested.

The embryo. Last of all the *Embryo* is produced, the principal object of fecundation, and end of all the genital apparatus occupying the centre of the nucleus, but

* Opera Omnia, p. 71.

appearing first in that region where the umbilical vessels perforate the internal membrane, and where the *sacculus colliquamenti* originates; not where the umbilical cord enters the *testa.* Its first formation eludes the search of the keenest eye aided by the best glasses. But, by and by, as it augments in size and solidity, it becomes at length visible, in some plants sooner and in others later, after impregnation. In *Helianthus* it is perceptible on the third day after impregnation, but in *Colchicum* not till after several months. Its figure is at first globular, its contexture pulpy, and its colour white. It swims in the *liquor amnii,* from which it derives its nourishment, seemingly unconnected with either the seed or plant; but immersing itself deeper and deeper every day, and always in such a position as to turn the radicle towards the exterior of the seed, and the opposite extremity towards the centre; which extremity divides itself into lobes called cotyledons, through which the nourishment of the plantlet passes, or in which it is elaborated. At length the chorion is exhausted, and the amnios absorbed or converted into albumen, and the embryo with its integuments transformed into a perfect seed.

Fecundation spurious or incomplete.

Such are the phenomena, according to the description of Gærtner,* accompanying or following the impregnation of all flowers producing seeds, except where the fecundation is spurious or incom-

* De Seminibus. Introd.

plete. The fecundation is spurious when the ovary swells but exhibits no traces of perfect seed within. This often happens in the case of plants producing berries, as in that of the Vine and *Tamus*. It is incomplete when barren and fertile seeds are intermingled together in the same ovary. This proceeds from some defect either in the quantity or quality of the pollen; but rather in the quality, as it is not always plants having the most pollen that produce the most seeds. The two stamens of the *Orchideæ* fecundate 8000 seeds, and the five stamens of Tobacco fecundate 900; while the 50 stamens of *Barringtonia*, the 230 of *Thea*, and the 80 of the *Caryophylli*, fecundate only two or three ovaries.

SECTION III.

Specific Examples.

As the exhibition of specific examples is always the most edifying, as well as generally the most pleasing, mode of instruction, I shall subjoin the following observations upon the changes succeeding impregnation in the seed and fruit of several of our most common plants; that the reader may have it in his power to refer to the individual case, if he is inclined to repeat the observation.

SUBSECTION I.

The Cherry.—On the 4th of May 1808, the first blossoms of a Cherry-tree had just expanded. The ovary was externally egg-shaped and smooth, but marked with a longitudinal ridge on the side to which the pistil bends; internally it was a pulpy and homogeneous mass, not yet exhibiting any traces of organization.

On the 8th, a cavity was discoverable in the centre of the ovary by means of a longitudinal section, occupied by two small vesicles, containing a transparent and jelly-like substance.

On the 12th, one of the vesicles had begun to exhibit symptoms of decay. The other had increased considerably, and formed now the *nucleus* of the seed.

On the 16th, the petals were now falling, and the stamens withering away after having discharged their pollen; but the pistil was yet pretty entire, and the ovary was about one fourth of an inch in length. The external part or pericarp, which was still green and pulpy, contained in its cavity the *nucleus* of the future seed, of a white and gelatinous appearance, considerably increased, together with the shrunk vesicle lying between the *nucleus* and pericarp. The tubes and spiral vessels, forming the umbilical cord and conducting the sap from the fruit-stalk to the *nucleus*, were seen by the assistance of a microscope upon the longitudinal section of the ovary. The *nucleus* was not easily separated from the pericarp without injuring it, but its surface was seen to consist of a net-work of fibres, being the ramifications, no doubt, of the umbilical cord.

On the 17th, the *nucleus* was found to be separable from the pericarp. Its form was egg-shaped, its colour was white, and its surface was marked with a scar towards the upper extremity where it had been detached from the umbilical cord. Upon dissection it was still found to consist of a net-like cuticle, and a pulpy, colourless, and transparent mass contained within it. The cuticle is no doubt the *testa* of Gærtner, or *secundina exterior* of Malpighi, and the contained substance the *chorion*.

On the 18th, there began to be formed on the inner surface of the pericarp a sort of fibrous substance, or rather a net-like plexus of fibres immediately investing the *nucleus*. This was the commencement of the formation of the *pyrena*. The cup, stamens, and pistil, had now fallen.

On the 19th, the *pyrena* had acquired a considerable degree of compactness, and was in some individuals distinguishable from the rest of the parts on the horizontal section, which now exhibited four distinct and concentric circles; the first and interior circle being the line bounding the gelatinous mass of the *chorion* which occupied the centre; the second being the coats of the *nucleus*, which were now easily separated from the *chorion;* the third being the *pyrena;* and the fourth being the pulpy pericarp.

On the 20th, the four concentric circles of the transverse section were still distinctly visible, but less easily separated.

On the 22d, the longitudinal section exhibited similar appearances, but no traces of the *amnios* or embryo were yet perceptible.

On the 24th, the parts were considerably enlarged, but still soft and pulpy. The two middle circles were the firmest; but it was now more difficult to detach the envelope of the *nucleus* from the interior mass.

On the 1st of June, the exterior pulp was considerably augmented in size, and the *pyrena,* in some individuals, pretty hard. The sap vessels of the foot-stalk were seen to enter the *pyrena* at the lower extremity, and afterwards to emerge from its interior surface rather above the middle, forming the external umbilical cord and *testa,* or *secundina exterior* of Malpighi, from the inner surface of which they were again seen to emerge at the lower extremity and to form the internal umbilical cord; which, entering the *chorion* at the base and passing on in the direction of its longitudinal axis, terminated ultimately in the *sacculus colliquamenti* and *amnios,* now visible for the first time in the apex of the *chorion;* the *sacculus* being apparently a prolongation of the umbilical cord into the figure of an inverted cone, as described by Malpighi.*

On the 2d, the induration of the *pyrena* was considerably augmented; and the *sacculus colliquamenti* and *amnios*, though not distinguishable on the longitudinal or transverse section of the *nucleus*, were yet separable from the *chorion* by means of a little manipulation. The *embryo* was not yet perceptible.

On the 4th, the umbilical cord, or *sacculus*, extending throughout the length of the *chorion*, was now visible on the longitudinal section for the first time; together with the *embryo* occupying the upper region of the *amnios*, and measuring about the $\frac{1}{100}$ part of an inch. The seed lobes were perfectly distinct, resembling a pear cleft half way down from the apex, but swelling a little where they unite, and then terminating in a small point directed to the circumference. This point was the radicle, which began to assume a yellowish tinge immediately upon being exposed to the air. The envelope of the *nucleus* seemed as if about to separate into two portions—the *testa* and *subtesta*, or exterior and interior integuments of Gærtner.

On the 5th, the *chorion* was somewhat shrunk, and the umbilical cord passing through it had assumed a curvilinear position, as described by Malpighi. The *embryo* measured about $\frac{1}{30}$ inch.

On the 9th, the *embryo* measured about $\frac{1}{10}$ inch, but was flatter in its appearance than before. The

* Opera Omnia. De Sem. Gener.

lobes had not yet separated at the top; but the *umbilicus* was less distinct.

On the 10th, the *umbilicus* was considerably shrunk, and the plantlet now separable from the cotyledons, measuring about $\frac{1}{15}$ of an inch in length. The *amnios* occupied about half the cavity of the *nucleus*.

On the 15th the *chorion* was much shrunk and the cotyledons proportionably enlarged. They were now pouch-shaped, circular, but individually plano-convex with the flat sides laid close together.

On the 20th, the fruit had begun to assume a reddish hue, the *embryo* measured about $\frac{1}{10}$ of an inch in length; the *amnios* being now much firmer than before, and the *chorion* almost entirely absorbed.

On the 26th, the fruit was ripe; and the *embryo*, having exhausted the whole of the *chorion* and *amnios*, was now complete, occupying the whole extent of the cavity of its envelopes.

SUBSECTION II.

The Pea.—On the 4th of June, 1808, when the legume of a flower fully expanded was opened, and the *nucleus* of a seed cut asunder, it was found to consist merely of a homogeneous mass of pulp, measuring about $\frac{1}{30}$ of an inch in diameter.

On the 8th, when a legume was opened of which the petals had just fallen, a small speck was apparent on the longitudinal section of the *nucleus*, which proved to be a cavity generated in the centre, the *nucleus* now measuring $\frac{1}{20}$ of an inch. In some individuals two cavities were perceptible, the one central, the other towards the circumference.

On the 10th, when the *nucleus* of a seed measuring about $\frac{1}{10}$ of an inch in diameter was cut open, the cavity, which was now considerably enlarged, was found to be filled with a thin and transparent fluid. This fluid was the *amnios*, but the *embryo* was not yet perceptible.

On the 13th, when a legume was opened which has acquired nearly its full length, and of which the *nucleus* measured one fifth of an inch in diameter, the *embryo* was perceptible floating in the upper region of the *amnios*, and measuring $\frac{1}{33}$ of an inch in length; the two lobes being expanded somewhat in the shape of a horseshoe, but without any apparent attachment to the sides of the cavity. The radicle was not yet distinguishable, nor the plumelet; but there was a protuberance in the cleft of the lobes.

On the 15th, when the *nucleus* measured one-fourth of an inch in diameter, the *embryo* measured one-eighth. It was still floating in the fluid, and still apparently without any point of attachment to the exterior portion of the *nucleus*. The lobes were much expanded at the top, resembling individually the segments of an egg or pear bisected longitudinally, and united by the small end. The additional cavity observed on the eighth proves to be the section of a perforation, which originating at the scar, and running a considerable way along the back of the seed, seems to be the passage through which the nutritious fluid is conveyed from the *umbilicus* into the interior of the seed. If the radicle is not attached to the *testa* at the extremity of this perforation, it is at least turned towards it.

On the 20th, when the *nucleus* was still about one fourth of an inch in diameter, and nearly globular, the *embryo* occupied about one half the cavity. The lobes were now approaching, and the radicle, with its integument, was seen projecting in the form of a conical protuberance from their point of union. The plumelet was not yet visible.

On the 22d, when the *nucleus* measured about one third of an inch, the cotyledons occupied almost the whole of the cavity. The lobes were nearly united; and the radicle, with its integument which projected about $\frac{1}{20}$ of an inch in the form of a conical protuberance, was curved in the line of the circumference of the lobes. The plumelet was now also discernible, split into several divisions at the top and lodged between the lobes, but forming a sort of cavity in one of them of about $\frac{1}{50}$ of an inch in length.

On the 25th, when the seed had attained to its full size, the lobes were completely united; and the *embryo* occupied the whole of the cavity of the envelope; the radicle measuring one eighth of an inch, and the plumelet $\frac{1}{20}$.

SUBSECTION III.

Wheat.—On the 10th of June, 1808, when the spike was yet wholly invested with the sheathing part of the leaf, the anthers, which were closely invested with a corolla, were yet green and pulpy, and measured about $\frac{1}{10}$ of an inch in length; the filaments were not quite so long. The nectaries measured about $\frac{1}{17}$ of an inch in length. The styles, which were not yet expanded, formed by their union a sort of cone upon the top of the ovary, and measured about $\frac{1}{30}$ of an inch in length. The ovary, which was besides crowned with a fine down, measured about $\frac{1}{20}$ of an inch in length. It had assumed something of its turban-shaped figure, and the convex and furrowed sides were readily distinguishable. When cut open with the knife it presented the appearance of a white and pulpy mass, seemingly homogeneous; but when inspected more minutely it was found to contain a central globule of a looser texture and darker colour, measuring about the $\frac{1}{150}$ part of an inch in diameter. This globule was the *chorion* of Malpighi.

On the 15th, when the spike was half extricated, the anthers exhibited nearly the same appearance as before; but the filaments were now equal to them in length. The nectaries were $\frac{1}{10}$ of an inch in length, plump and bulging at the base, and finely fringed with down. The styles were somewhat expanded. The ovary, which now measured about $\frac{1}{20}$ of an inch, was a white and fleshy mass, but slightly tinged with green where it invests the contained globule, which is now more viscid.

On the 20th, when the spike was completely extricated, the anthers were beginning to change to yellow. They were now about one sixth of an inch in length and the filaments twice as long. Some of them had shed their pollen, of which thousands of particles were found adhering to the expanding styles. The nectaries were somewhat shrunk at the base. The ovary exhibited more of green on the inner surface. The enclosed globule or *chorion* was as before.

On the 29th, when the pollen was shed and the styles beginning to fade, the nectary scales were much shrunk. The ovary, now $\frac{1}{10}$ of an inch in length, was thick and less fleshy in its contexture. The green substance is a film lining its interior surface. The enclosed globule somewhat enlarged.

On the 6th of July, when the ovary measured one eighth of an inch and had assumed an oblong figure, the green film was detachable from its inner surface, seeming to be formed of an expansion of the umbilical cord. The enclosed globule measured $\frac{1}{10}$ of an inch, and was now separable into an envelope and a thin and colourless fluid, into which the jelly-like substances of the former stages had been converted. Perhaps this should now be called the *amnios*.

On the 10th, when the nectaries were now shrunk into thin scales fringed with hairs, the ovary measued about one fifth of an inch, and was also much shrunk; being now instead of a thick and fleshy mass only a thin and fibrous envelope. The green film that formerly adhered to the inner surface of the ovary, and was detached from it with difficulty, is now perceptible even through the ovary, and begins to adhere to the *nucleus* which is still a clear fluid enclosed in its proper envelope, and not so much globular in its figure as oval, tapering to a small point at the base.

On the 14th, when the ovary measured about one fourth of an inch, the fluid of the *nucleus* was beginning to be converted into *albumen*, within its proper envelope, to which the green film adheres, but from which, as well as from the ovary, it may readily be detached though not always entire, being of a very slender texture. Its colour is less decidedly green, except at the furrow, where it originates in the umbilical cord. The *embryo* is not yet perceptible.

On the 20th, when the ovary had augmented considerably in width, the length being as before, and the *nucleus* had been converted almost entirely into a soft albumen, the embryo was at last discoverable in the base of the *nucleus*, but scarcely distinguishable into its several parts; the whole measuring only $\frac{1}{50}$ of an inch by $\frac{1}{100}$ at the broadest part.

On the 24th, when the seed was swollen to nearly its full size, the three envelopes were still very distinct. The outer envelope is what was the ovary; the middle envelope is the green film, now changing to brown; the inner envelope is the proper integument of the *albumen*. The *albumen* is now pretty firm; the *embryo* plainly distinguishable into its radicle, plumelet, and scale-like appendages, whether vitellus or cotyledon; the scale being $\frac{1}{15}$ of an inch in length, the plantlet $\frac{1}{20}$.

On the 28th, when the seed was now at its full size, being more than one fourth of an inch in length, the nectary scales were still adherent to the base of the ovary; but shrunk to a thin membrane, though retaining their fringed borders. The green film was now more closely united to the inner envelope. The *embryo* was also very distinctly seen by means of removing the envelopes, and easily extricated from the *albumen*, on the surface of which it is accumbent. It measured together with its scale-like vitellus $\frac{1}{14}$ of an inch in length by $\frac{1}{20}$ in breadth. The *vitellus* does not seem to be very correctly discribed by calling it a scale, or at least, not completely described; for at the base of the scale and continuous with its substance, there may now be seen projecting a sort of little bag or pocket with an upright flap in front, in which the radicle of the embryo is lodged, and out of which the plumelet protrudes itself accumbent on the upper part of the scale.

On the 1st of August, when the seed seemed nearly ripe, and the scale measured $\frac{1}{10}$ of an inch in length by $\frac{1}{15}$ in breadth, the plumelet had increased considerably in size, filling up almost the whole of the area of the upper portion of the scale. The upright flap of the pocket is distinctly visible, from which the true radicle may now be disengaged with a little dexterity of manipulation, though only by means of tearing the plantlet from the scale at the collar.

On the 4th, when the seed seemed to be quite ripe, the green film after changing to a pale brown had disappeared by insinuating itself into the inner envelope, or adhering to it so closely as to be separable from it only by scraping it off in small fragments. The plumelet filled up the area of the upper portion of the scale entirely. The rudiments of the lateral fibres were now visible in the form of small protuberances issuing from the collar, one on each side the flap. And when the plantlet was cut open at the collar, several small globules were discoverable, which are perhaps destined to become future stalks. The embryo and albumen were firm and compact; and in the course of a day or two the Wheat was cut down.

SUBSECTION IV.

The Hazel-nut.—On the 30th of June, 1808, the most advanced nuts were about one third of an inch in length. When the nut was cut in two, the *putamen*, which was of a greenish colour, but somewhat firm at the apex, was found to contain the *chorion*, a white and succulent mass, resembling the pith of the Elder; through which the umbilical cord was seen passing in the direction of its axis, and having attached to it, near the apex, a small gelatinous globule on each side of about $\frac{1}{30}$ of an inch in diameter. There were two incipient *nuclei* similar to the two small bags found in the young Cherry.

On the 14th of July appearances were much as before, with the exception of that of a small cavity generated in the centre of the gelatinous globules; and a sort of net-work of fibres overspreading their surface. In some cases three *nuclei* were visible.

On the 25th, the nut seemed nearly at its full size. The *chorion* was now more firm; but the umbilical cord was less distinctly visible. It was visible, however, on the horizontal section, as a small speck in the centre; and also on the longitudinal section near the apex where the *nuclei* were attached to it; of which, one only was in a thriving state, the other being somewhat shrunk. The healthy one, which was somewhat egg-shaped, measured $\frac{1}{10}$ of an inch in length, and was white and full of veins on the surface. When cut asunder, it was found to consist of a pretty thick coat enclosing a watery fluid. This fluid was no doubt the *amnios*, but the embryo was not yet visible.

On the 2d of August the progress of developement was as follows:—In a *nucleus* or kernel measuring $\frac{1}{10}$ of an inch, the embryo was just visible together with its two lobes, occupying the upper part of the cavity, and measuring about the $\frac{1}{100}$

part of an inch. It was surrounded with a clear and transparent fluid—the *amnios*. In a *nucleus* measuring one sixth of an inch, the embryo measured $\frac{1}{15}$. In a *nucleus* measuring one fifth, the embryo measured $\frac{1}{10}$. The surrounding substance was now gelatinous. In some cases both the *nuclei* seemed to be impregnated and were continuing to thrive; and it is known they do sometimes both ripen, as may be seen in the course of cracking a good many nuts. In the larger specimens the veins were very conspicuous.

On the 15th, many nuts had attained to nearly their full size. In one specimen in which the *nucleus* measured one fourth of an inch in length, the outer envelope now full of veins was not easily separated from the under envelope, though both were easily separated from the cotyledons, which measured about one fifth of an inch, having the remainder of the *amnios* lodged between them at the top, but still gelatinous and attached to the coats. The plantlet measured about $\frac{1}{30}$ of an inch.

In another specimen in which the *nucleus* measured about one third of an inch, the remainder of the *amnios*, which was still gelatinous, was now wholly within the cleft of the lobes, and lodged in a sort of hollow near the *apex*.

In a third specimen in which the kernel seemed nearly ripe, the *amnios* had totally disappeared, except a thin and membranaceous film, which was still separable from the cotyledons. The umbilical cord was now also very distinct, as well as the mark which it had occasioned on the surface of the kernel.

SUBSECTION V.

General Remarks.—In the above examples the process of the developement of the vegetable embryo will be found to be analogous to that of the developement of the animal embryo, according to the observation of Malpighi and Gærtner. In tracing the progress by means of the repeated inspection of the ovary, the *chorion* is found to be first developed; then the *umbilicus* pervading the centre of the *chorion*, and expanding at the extremity into the *sacculus colliquamenti;* then the *amnios*, which after being elaborated in the *chorion*, is conveyed by the *umbilicus* to the *sacculus*; and lastly the *embryo*, occupying the upper region of the *amnios*—at first a mere point incapable of being distinguished into its several parts, but by and by exhibiting the rudiments of root, stem, and leaf, together with its cotyledons or seed-lobes, between which the *amnios* penetrates, and hence reaches the embryo.

Malpighi is of opinion, however, that the *amnios* is augmented and the embryo nourished, not merely by means of the nutriment that passes through the *umbilicus;* but also by means of the juice of the *chorion* which penetrates through the *sacculus colliquamenti* and so reaches the embryo; because in some plants there is no visible *umbilicus* passing through the *chorion* and forming a *sacculus* for the *amnios,* an example of which we have seen in Wheat, and which Malpighi exemplifies in the Laurel;* though in this example I think there must have been some mistake.

On the 30th of June, 1808, the appearances of the ovary of the common Laurel of the gardens, *Laurus nobilis,* were as follows:—The *amnios* had just made its appearance in the upper region of the *chorion* which now resembled a mass of transparent jelly. There was no *umbilicus* visible upon the mere inspection of the *chorion* even with all its transparency. But when the *chorion* was laid open by means of careful dissection, in the direction of its longitudinal axis, the *umbilicus* was now discernible adhering to the lower extremity of the *amnios.* On a transverse section the *chorion* was also evidently perforated in the direction of its axis. This seemed at first to have been the perforation of its *umbilicus,* but was found at last to be merely the canal through which it passed. It seemed impractibable to extricate it entire; it was so very tender.

On the 6th of July, I succeeded at last in extricating it entire, together with the *amnios,* when the external umbilical cord was also equally visible passing from the ovary into the *nucleus.* As I effected this extrication in many specimens, I can

* In lauro iidem occurrunt humores concreti nullo tamen intercedente umbilico. Anat. Plant. p. 58.

have no doubt of the existence of an *umbilicus* in the *chorion* of the Laurel.

CHAPTER IX.

THE PROPAGATION OF THE SPECIES.

As the life of the vegetable, like that of the animal, is limited to a definite period, and as a continued supply of vegetables is always wanted for the support both of man and beast, nature has taken care to institute such means as shall secure the multiplying and perpetuating of the species in all possible cases; art has invented others for the use and accommodation of man, and fancy has imagined more.

SECTION I.

Equivocal Generation.

It was long a vulgar error, countenanced even by the philosophy of the times, that vegetables do often spring up from the accidental mixture of putrid water and earth, or other putrid substances, in the manner of what was called the equivocal generation of animals; or at the very least, that the earth contains the principle of vegetable life in itself, which in order to develope it is only necessary to expose to the action of the air. The former alternative of the error has been long ago refuted, and as I believe eradicated, no one now contending for the doctrine of vegetable generation from putrefaction: but the latter, though it has been also refuted, has not yet lost its hold of the minds of the unlearned. The farmer still believes, and will still tell you, that the earth throughout its whole mass teems with the rudiments of vegetables, or at least of all such as he is not under the necessity of cultivating, which it will develope without any seed if only exposed to the action of the air; alleging in support of his opinion that earth dug up from any depth and thrown in heaps on the surface, will immediately begin to send up young plants, though no seed has been sown upon it. But the fallacy of this argument is easily exposed, for in the first place the roots of such plants as are near to it will extend themselves around its edges, and make encroachments upon the lower part of it; and in the next place the seeds of plants, whether near or at a distance, will be conveyed to it by the winds, by cattle, or by birds, and so furnish the upper part of it, so that the argument is good for nothing. Let the experiment be made where the earth shall be perfectly insulated, except from light, air, and water, and let the result be marked. This was done long ago by Malpighi, who having procured some earth that had been dug up from a great depth, enclosed it in a glass vessel over the mouth of which he spread several folds of silk, so as to admit air and

A vulgar prejudice.

Refuted by Malpighi.

water; but to exclude all such small seeds as might be wafted on the winds: the result was that no plant came up.*

In the summer of 1811, I had an opportunity of making some similar experiments on earth obtained from a considerable depth by the digging of a well at Purleigh, near Maldon, in the county of Essex. On the 15th of April I exposed a lump of this earth, being chiefly a black clay taken from the depth of 100 feet, to the action of the air and weather, and to the operation of such other contingencies as might occur: it was placed upon a slate in one of the quarters of my garden. On the 10th of May I placed another lump taken from the depth of 150 feet upon a slate also, but under a hand-glass, which was removed only to give it an occasional watering. No symptoms of vegetation appeared in either the one or the other till the 3d of September following, when several plants were found in a state of protruding their seed leaves from the surface of the exposed clay, and one also from the surface of the insulated clay; the former proved afterwards to be plants of *Senecio vulgaris*, or Common Groundsel, which was now coming up from seed all over the garden, and hence easily accounted for: the latter proved to be a plant of *Ranunculus sceleratus*, the seed of which was undoubtedly brought to the clay along with the water it was watered with, which came from a pond at no great distance, round the edges of which

* Anat. Plant. Pars Altera, p. 92.

the plant was springing up in great plenty from seeds previously dispersed, which, as they float long on the surface of the water, could not but have mixed more or less with the portion conveyed to the clay. This presumption is grounded upon the fact that a number of other plants of the same species were coming up in other parts of the garden that were watered from the same pond; while in the exposed clay which never was watered except by rains or dews, no such plant was found: hence it follows equally as from the experiment of Malpighi, that the earth produces no plant without the intervention of a seed, or of some other species of vegetable germe deposited in it by nature or by art.

SECTION II.

Seeds.

WHEN the seed has reached maturity in the due and regular course of the developement of its several parts, it detaches itself sooner or later from the parent plant, either singly or along with its pericarp, and drops into the soil, where it again germinates and takes root, and springs up into a new individual. Such is the grand means instituted by nature for the replenishing and perpetuating of the vegetable kingdom; the wisdom and efficacy of which will equally appear whether we regard the great fertility of vegetables in general, and incalculable fertility of

Their wonderful profusion,

some species in particular, as has been already stated;* or the care with which nature has provided for the dispersion of the ripened seed.

And dispersion,

If seeds were to fall into the soil merely by dropping down from the plant, then the great mass of them instead of germinating and springing up into distinct plants, would tend only to putrefaction and decay; to prevent which consequence nature has adopted a variety of the most efficacious contrivances, all tending to the dispersion of the seed.

By the elasticity of the pericarp.

The first means I shall mention is that of the elasticity of the pericarp of many fruits, by which it opens when ripe with a sort of sudden spring, ejecting the seed with violence, and throwing it to some considerable distance from the plant. This may be exemplified in a variety of cases; the seeds of oats when ripe are projected from the calyx with such violence, that in a fine and dry day you may even hear them thrown out with a slight and sudden snap in passing through a field that is ripe. The pericarp of the *Dorsiferous Ferus* is furnished with a sort of peculiar elastic ring, intended as it would appear for the very purpose of projecting the seeds. The capsules of the Cucumber, *Geranium, Geum*, and *Fraxinella,* discharge their seeds also when ripe with an elastic jerk. But the pericarp of *Impatiens*, which consists of one cell with five valves, exhibits perhaps one of the best examples of this mode of dispersion. If it is accidentally touched when ripe

* Book i. chap iii.

it will immediately burst open, while the valves, coiling themselves up in a spiral form and springing from the stem, discharge the contained seeds and scatter them all around.

The bursting of the pericarp of some species of *Pines* is also worthy of notice. The pericarp, which is a cone, remains on the tree till the summer succeeding that on which it was produced, the scales being still closed. But when the hot weather has commenced and continued for some time, so as to dry the cone thoroughbly, the scales open of their own accord with a sudden jerk, ejecting the contained seeds: and if a number of them happens to burst together, which is often the case, the noise is such as to be heard at some considerable distance. This crackling noise was observed and traced to a fir-tree, namely *Pinus Pinea*, at Rendlesham Parsonage, on July 14th, 1808, by two young gentlemen, my pupils, who thought the tree was bewitched till the cause of the noise was pointed out to them.

The twisted awn of *Avena fatua*, or Wild Oat, as well as that particularly of *Geranium cicutarium*, and some others, seems to have been intended for the purpose of aiding the further dispersion of the seed, after being discharged from the plant or pericarp. This spiral awn or spring, which is beset with a multitude of fine and minute hairs, possesses the property of contracting by means of drought, and of expanding by means of moisture. Hence it remains of necessity in a perpetual state of contraction or By the awn.

dilatation, dependant upon change of weather; from which, as well as from the additional aid of the fine hairs, which act as so many fulcra, and cling to whatever object they meet, the seed to which it is attached is kept in continual motion till it either germinates or is destroyed.*

The awn of Barley, which is beset with a multitude of little teeth all pointing to its upper extremity, presents also similar phenomena. For when the seed with its awn falls from the ear and lies flat upon the ground, it is necessarily extended in its dimensions by the moisture of the night, and contracted by the drought of the day. But as the teeth prevent it from receding in the direction of the point, it is consequently made to advance in the direction of the base of the seed, which is thus often carried to the distance of many feet from the stalk on which it grew. If any one is yet sceptical with regard to the travelling capacity of the awn, let him only introduce an awn of Barley with the seed uppermost between his coat and shirt sleeve at the wrist, when he walks out in the morning, and by the time he returns to breakfast, if he has walked to any great distance he will find it up at his arm-pit. This journey has been effected by means of the continued motion of the arm, and consequently of the teeth of the awn acting as feet to carry it forward.

It is obvious, however, that the modes of dispersion now stated can never carry the seed to any

* Withering Arrang. vol. ii. p. 610.

great distance; but where distance of dispersion is required, nature is also furnished with a resource. One of the most common modes by which seeds are conveyed to a distance from their place of growth is that of the instrumentality of animals.

Many seeds are thus carried to a distance from the place of growth merely by their attaching themselves to the bodies of such animals as may happen accidentally to come in contact with the plant in their search after food; the hooks or hairs with which one part or other of the fructification is often furnished serving as the medium of attachment, and the seed being thus carried about with the animal till it is again detached by some accidental cause, and at last committed to the soil. This may be exemplified in the case of the *Bidens* and *Myosotis*, in which the hooks or prickles are attached to the seed itself; or in the case of *Galium aparine* and others, in which they are attached to the pericarp; or in the case of the Thistle and Burdock, in which they are attached to the general calyx.

By the instrumentality of animals.

Many seeds are dispersed by animals in consequence of their pericarps being used as an animal food. This is often the case with the seeds of the Drupe, as Cherries, Sloes, and Haws, which birds often carry away till they meet with some convenient place for devouring the pulpy pericarp and then drop the stone into the soil. And so also fruit is dispersed that has been hoarded up for the winter, though even with the view of feeding on the seed itself, as in the case of nuts hoarded up by Squirrels, which are often dispossessed by some other animal, that not caring for the hoard scatters and disperses it. Sometimes the hoard is deposited in the ground itself, in which case part of it is generally found to take root and spring up into plants. But it has been observed that the Ground Squirrel often deprives the kernel of its germe before it deposits the fruit it collects; which it has been supposed to do from some peculiar instinct as the means of preventing the germination of the seed. It has been suggested, however, that the preference thus given to the embryo arises perhaps from its possessing some specific flavour peculiarly agreeable to the animal's taste; and this is perhaps the true solution of the question.* Crows have been also observed to lay up acorns and other seeds in the holes of fence-posts, which being either forgot or accidentally thrust out, fall ultimately into the earth and germinate.

But sometimes the seed is even taken into the stomach of the animal, and afterwards deposited in the soil, having passed through it unhurt. This is often the case with the seed of many species of berry, such as the Miseltoe, which the thrush swallows and afterwards deposits upon the boughs of such trees as it may happen to alight upon. The seeds of the *Loranthus americanus*, another parasitical plant, are said to be deposited in like manner on

* Barton's Elem. of Bot. p. 233.

the branches of the *Cocoloba grandiflora*, and other lofty trees;* as also the seeds of *Phytolacca decandria*, the berries of which are eaten by the robin, thrush, and wild pigeon.† And so also the seeds of Currants or Roans are sometimes deposited after having been swallowed by blackbirds or other birds, as may be seen by observing a Currant-bush or young Roan-tree growing out of the cleft of another tree, where the seed has been left, and where there may happen to have been a little dust collected by way of soil; or where a natural graft may have been effected by the insinuation of the radicle into some chink or cleft. It seems indeed surprising that any seeds should be able to resist the heat and digestive action of the stomach of animals; but it is undoubtedly the fact. Some seeds seem even to require it. The seeds of *Magnolia glauca* which have been brought to this country are said to have generally refused to vegetate till after undergoing this process, and it is known that some seeds will bear a still greater degree of heat without any injury. Spallanzani mentions some seeds that germinated after having been boiled in water: and Du Hamel gives an account of some others that germinated even after having been exposed to a degree of heat measuring 235° of Fahrenheit.

In addition to the instrumentality of brute animals in the dispersion of the seed I might add also that of man, who for purposes of utility or of orna-

* Barton's Elem. of Bot. p. 232. † Ibid. p. 234.

ment, not only transfers to his native soil seeds indigenous to the most distant regions, but sows and cultivates them with care. But this view of the subject I will leave to the reader's own reflections, and hasten to the other modes of dispersion instituted by nature; one of the most effective of which is that of the agency of winds.

By the agency of winds.

Some seeds are fitted for this mode of dispersion from their extreme minuteness, such as those of the Mosses, Lichens, and Fungi, which float invisibly on the air and vegetate wherever they happen to meet with a suitable soil. Others are fitted for it by means of an attached wing, as in the case of the Fir-tree and *Liriodendron tulipifera*, so that the seed in falling from the cone or capsule is immediately caught by the wind and carried to a distance.

Others are peculiarly fitted for it by means of their being furnished with an aigrette or down, as in the case of the Dandelion, Goat's-beard, and Thistle, as well as most plants of the class Syngenesia; the down of which is so large and light in proportion to the seed it supports, that it is wafted on the most gentle breeze, and often seen floating through the atmosphere in great abundance at the time the seed is ripe.

Others are fitted for this mode of dispersion by means of the structure of their pericarp, which is also wafted along with them, as in the case of *Staphylea trifolia*, the inflated capsule of which seems as if obviously intended thus to aid the dispersion of the contained seed by its exposing to the wind a large and distended surface with but little weight. And so also in the case of the Maple, Elm, and Ash, the capsules of which are furnished, like some seeds, with a membranous wing, which when they separate from the plant the wind immediately lays hold of and drives before it.

Rivers and currents of the ocean.

Finally, a further means adopted by nature for the dispersion of the seeds of vegetables is that of the instrumentality of streams, rivers, and currents of the ocean. The mountain-stream or torrent washes down to the valley the seeds which may accidentally fall into it, or which it may happen to sweep from its banks when it suddenly overflows them. The broad and majestic river, winding along the extensive plain, and traversing the continents of the world, conveys to the distance of many hundreds of miles the seeds that may have vegetated at its source. Thus the southern shores of the Baltic are visited by seeds which grew in the interior of Germany, and the western shores of the Atlantic by seeds that have been generated in the interior of America. But fruits indigenous to America and the West Indies have sometimes been found to be swept along by the currents of the ocean to the western shores of Europe. The fruit of *Mimosa scandens*, *Dolichos pruriens*, *Guilindina bonduc*, and *Anacardium occidentale*, or Cashew-nut, have been thus known to be driven across the Atlantic to a distance of upwards of 2000 miles: and although the fruits now adduced as examples are not such as could vegetate on the coast on which they were thrown, owing to soil or climate; yet it is to be believed that fruits may have been often thus transported to climates or countries favourable to their vegetation.

SECTION III.

Gems.

Gems distinguished from seeds.

THOUGH plants are for the most part propagated by means of seeds, yet many of them are propagated also by means of gems; which have been already defined, in as far as their definition could be given without a direct reference to the mode of their generation, as being distinct from that of the seed, but which, till after the discussion of the subject of vegetable sexuality, it was pehaps premature to introduce. What then are the essential marks by which gems are to be distinguished from seeds? The following are the discriminations of Gærtner:*--

First Mark.

The first and most essential marks by which the gem is to be distinguished from the seed is that of its being formed without the intervention of a sexual apparatus; and merely by the agency of the vital and organizing principle of the plant. Gærtner describes it as originating in what he calls the flesh of the plant, which he does not, however, accurately

* Introd. de Seminibus.

define, calling it the pith, loosely speaking; though in the case of woody plants it is plain that he means by it the alburnum, as he says all buds are rooted in that substance which is under the inner bark, and form with it one body.

Secondly, the gem is distinguished from the seed as consisting chiefly of a pith, and having no divisions internally into distinct parts, similar to the albumen vitellus and cotyledons of the seed; nor any proper integuments externally, similar to the *Testa* and *Subtesta* of the seed; but merely a bark modifying the medullary substance, and giving it its external and proper form: which bark consists either of the indurated substance of the granule itself; or of divisions of the cellular tissue adhering to it, as in the *Propago;* or of the bark of the parent plant by which the granule has been enveloped, as in the *Gongylus;* or of the inner bark forming a permanent part, or outer bark forming only a temporary appendage, as in the case of the bud and bulb. **Second mark.**

Thirdly, the gem is also often distinguished from the seed by means of its appendages, the theca and involucrum, or sheath and involucre; which originate in the bark of the parent plant. The sheath is peculiar to some species of *Propago,* and is a vessel containing a number of individuals, which it retains for a certain time, and then disperses; as may be exemplified in the genus *Marchantia.* The involucre is peculiar to compound gems, it consists of **Third mark.**

one or more cortical scales, which defend the tender gem and are attached to it only by the base. In buds it is deciduous, in bulbs it is permanent: but it does not form a complete envelope, being always open at that part where the gem is to burst out; or if shut, it is shut only by the over-lapping of its parts.

Fourth mark.

Lastly, the gem is distinguished from the seed in its mode of developement. The integuments of the seed perish after germination, but the covering or appendages of the gem do not. They are incorporated into the substance of the new plant, as in the *Propago* and *Gongylus*; or at least they continue to vegetate along with it, as in the case of the scales of the bulb. The gem sends out a number of small roots formed from the bark, and but seldom one; while the seed sends out one main root only from the pre-existing radicle, and but seldom more than one. In the gem, the interior part is first formed and then the appendages or covering; in the seed the integuments are first formed, and then the embryo appears.

Definition of the gem.

If the scope of the above distinctions is taken into the account the definition of the gem will then be as follows:—The gem is an organized substance bursting from the surface of the plant without the aid of sexual apparatus, or previous fecundation; and developing its parts either by forming a continued extension of the parent plant, or by detaching itself from the parent plant altogether, and forming a new individual.

This definition is obviously applicable, in one alternative or other, both to the bud and bulb; by which last it is well known that the species is often propagated, as in the very common case of bulbous rooted plants. If the bulb of a Snow-drop or Lily is taken up when the season of flowering is past and deprived of its external coats or scales, the rudiments of young bulbs will be discovered lurking at the base of the scales in the form of small buds, though some may perhaps be found farther advanced and ready to burst their integuments; which after they have ultimately done, they then detach themselves from the parent bulb altogether and form new individuals. Such is the mode of the propagation of the radical bulb. The bulb.

But the species is also often propagated by means of the caulinary bulb. This bulb generally appears in the axil of the leaves, as in *Dentaria bulbifera* (*Pl.* III. *Vol.* I.), and *Lilium bulbiferum*. At first it seems a sort of knob or tubercle; but by and by it is a bulb, often separating spontaneously from the parent plant and taking root in the soil. In some of the alliaceous plants the caulinary bulb is very common, and is produced at the origin and between the spokes of their umbels. Among gardeners they are known by the name of *Cloves*.

Some plants produce a sort of bulb even in the midst of their spike of flowers, which detaching itself from the parent plant strikes root and forms also a new individual. Such are *Polygonum vivi-*

parum and *Poa alpina;* and as plants of this kind are mostly alpine, it has been thought to be an institution or resource of nature to secure the propagation of the species in situations where the seed may fail to ripen.

The bud.

The bud though it does not spontaneously detach itself from the plant and form a new individual, will yet sometimes strike root and develope its parts if carefully separated by art and planted in the earth: but this is to be understood of the leaf-bud only, for the flower-bud if so treated always perishes.*

But the species may sometimes be propagated even by means of the leaves; as is, I believe, the case with the leaves of the Orange, Aloe, Sea-onion, and some species of Arum, which if carefully deposited in the soil will grow up into new plants,—by virtue, no doubt, of some latent gem contained in them; in which case, as well as in all of the preceding cases, the propagation of the species is obviously effected by means of a principle different from the seed, which botanists generally designate by the appellation of the gem.

The propago and gongylus.

But this is not so obviously true in the case of the *Propago* and *Gongylus*—the simple gems of Gærtner; because it has been contended that they are still but seeds. Gærtner, however, excludes them entirely from the rank of seeds upon the foregoing grounds, and maintains, in opposition to the

* Mirbel Phys. Veg. vol. i. p. 220.

opinions Hedwig and others, that several tribes of what are usually denominated cryptogamous plants are propagated solely by gems.

The Lichens, according to Gærtner, are of this description; that which is usually regarded as their seed being merely a powdery propago bursting from the surface of the plant, and vegetating without changing its form. Hedwig, after Adanson, contends indeed that the granules immersed in the *scutellæ* of the Lichens are true seeds. But it is to be recollected that all *Lichens* are not furnished with *scutellæ*, nor all *scutellæ* with granules; and much less, consequently, with a sexual apparatus.

The *Fungi* also, according to Gærtner, are all gemmiferous, having no sexual organs, and no pollen impregnating a germe. In the genus *Lycoperdon* the gelatinous substance that pervades the cellular tissue is converted into a proliferous powder; in *Clavaria* the fluid contained in the cavities of the plant is converted into a proliferous powder also: and in the *Agarics*, *Hydnum*, and *Boletus*, vesicles containing soboliferous granules are found within the *lamina*, pores, or tubes. Hedwig, on the contrary, ascribes to the *Fungi* a sexual apparatus, and maintains that the pollen is lodged in the volva. But here it is to be recollected as in the cases of the scutellæ of the Lichens, that all Fungi are not furnished with a volva, and consequently not furnished with pollen.

The *Confervæ* and *Ulva*, together with the genera *Blasia* and *Riccia,* are also, according to Gærtner, propagated only by gems; while *Marchantia, Anthoceros, Jungermannia,* and *Lycoperdon* are said to be propagated both by gems and seeds.

Challenge of Gært-ner. Such is the sum of the theory of Gærtner, who adds—If it be said that these granules are not gems but seeds, let it be proved that they have the usual integuments of seeds: and if it be said that they are exceptions to the general rule, and are seeds though destitute of the usual integuments, let it be proved that the plants producing them are furnished with sexual organs.

Accepted by Corrêa. The challenge thus held out to the Cryptogamist has been accepted by M. Corrêa de Serra,* at least with regard to the proscribed genera of submersed *Algæ;* which he has endeavoured to restore to the rank of seed-bearing plants. The true *Fuci* were admitted by Gærtner to produce perfect seed, though only upon the Adansonian notion of vegetable *Aphroditism;* but the *Ceramiums,* together with the *Ulvæ* and *Confervæ* were regarded as producing gems merely, and on the following grounds:—

1. Because in the *Ceramiums* and *Ulvæ* the grains are solitary, are not contained in a proper uterus, and are consequently without a placentation.

2. Because in germinating they leave no coat behind.

* Phil. Trans. 1796.

3. Because in the *Confervæ* two or more of them often coalesce, and yet form but one individual.

In reply to the first argument, Mr. Corrêa maintains that the grains of the *Ceramiums* and *Ulvæ* are precisely similar to those of the true *Fuci*, enclosed in a *uterus* enveloped with a soft and juicy substance, affixed no doubt by some placentation, furnished with a proper skin, and disengaging themselves from the parent plant at the period of their maturity, though Gærtner says only by the plant's decay.

In reply to the second argument Mr. Corrêa does not pretend directly to controvert the fact which he seems to think no observation can accurately ascertain, but merely the principle by which it has been supposed that no substance can possibly be a seed unless it has a coat to leave behind it in germinating, and that no substance can be a gem if it has one;—a principle arising, as he thinks, out of the supposed analogy between the seed of vegetables and eggs of animals, or between the gem and the living fœtus: but gems, as he asserts, do sometimes leave a coat behind them, as in the scales of the bud; and eggs have sometimes no coat to leave, as in the spawn of Frogs.

In reply to the third argument he contends that its scope is precisely the reverse of that alleged by Gærtner, because it is known that in the case of the coherence of other acknowledged gems, the one always proves abortive and falls, tending only to nourish the other; but in the case of the adherence of other acknowledged seeds, as in that of *Dalea, Lagœcia, Hasselquestia*, and others, the abortive seed does not fall, but still continues adherent.

Having thus pulled to pieces the hypothesis of Gærtner, the next object of Mr. Corrêa is to establish his own—namely, that the mucous substance surrounding the grains of the plants in question is a true pollen. If this, he adds, is found to be contrary to the character of the pollen of terrestrial plants, so, it should be remembered, is the medium through which it has to pass; and if it should be said that the pollen of some aquatics, such as *Potamogeton* and *Vallisneria* is still powdery, it is to be recollected that their flowers emerge above water at the season of fecundation; or if there are any aquatics which do not emerge and have yet a powdery pollen, it should be recollected that the process takes place wholly under cover, as in the case of *Zostera*, in which the flower is situated in the cavity of the stem, and does not open till fecundation is over: and even in plants vegetating in the open air, nature employs various expedients to preserve the pollen from wet.

But it is not absolutely necessary that the pollen should be farinaceous even in terrestrial plants, or rather it is known and acknowledged that this is not always so. In the *Orchideæ* it consists of a mass of solid particles, assuming in the aggregate a sort of waxy appearance; in some of the *Contortæ* it is viscid; and in most of the *Apocyneæ* it is almost altogether a fluid. But if this is the case even with some terrestrial plants, much more with aquatics vegetating in a medium so ill adapted to the transmission of a farinaceous pollen. It follows, therefore, that the mucilaginous vesicles of the submersed *Algæ* surrounding the *uterus* are anthers furnished with pollen, and that the grains by which they are propagated are perfect seed, the flowers being thus *Hermaphrodites*: which conclusion is further countenanced by the fact that the parts here alluded to are merely temporary, the grains after fecundation increasing and finally disengaging themselves, and the mucous substance totally disappearing, as in plants with conspicuous flowers.

Its issue. Such is the view of the subject offered by Mr. Corrêa, extremely perspicuous indeed, and almost convincing. But it must at the same time be observed that in his reply to the first of Gærtner's arguments he adduces no examples in proof of his assertions; and does not even pretend to have discovered the placentation of the grains; but merely concludes that they must be so affixed. In his reply to the second argument of Gærtner I am not at all satisfied that the case is correctly stated. Because although the scales of the bud are indeed left behind, yet they do not at all come under the notion of proper integuments as understood by Gærtner; but merely of an exterior covering or appendage not necessarily included in the notion of the gem. And if the account of zoologists is true, the eggs of frogs are covered with a proper integument independant of what Mr. Corrêa calls their mucous albumen, which they unquestionably leave behind. In consequence of which I cannot think that Mr. Corrêa has established the point for which he contends.

SECTION IV.

Runners.

Runners are young shoots issuing from the collar or summit of the root, and creeping along the surface of the soil; but producing a new root and leaves at the extremity, and forming a new individual, by the decay of the connecting link. This takes place in a great variety of herbs, but particularly the Strawberry which is a good example, and from the root of which a number of creeping shoots are protruded in the course of the summer, extending like the radii of a circle to the distance of eight, ten, or twelve inches or more; and then striking root towards the extremity and producing a new individual which in the following year becomes wholly separated from the parent, by the decay of the connecting link, and sends out also new runners in its turn.

Exemplified in the Strawberry.

SECTION V.

Slips.

As the process of raising perennials from seed is very slow, gardeners have discovered or invented several ways of expediting the propagation of the species by means of artificial aid. For it has been found that if a young shoot or branch is cut off with the knife, and then planted in the soil, it will in many cases still continue to vegetate, sending out roots below and branches above, and forming a new individual. But this mode of propagation should perhaps be regarded after all as an extension of the old plant, rather than as the generation of a new one; though it serves the purpose of the cultivator equally well as a plant raised from seed, with the additional advantage of bearing fruit much sooner. It will not succeed, however, in all plants indiscriminately; but it succeeds extremely well in the case of Currants, Gooseberries, and Vines; as also in that of the Willow and Poplar, of which you can scarcely knock a stake into the ground that will not strike root. The shoot thus detached from the plant, and placed in the soil is denominated a slip.

An extension of the parent plant.

But how is the root generated which the slip thus produces? If the trunk of a tree is lopped, and all its existing buds destroyed, then there will

The root how generated.

be protruded from between the wood and bark a sort of protuberant lip or ring formed from the proper juice, and from which there will spring a number of young shoots. And if a root is taken and lopped it will in like manner send out new roots. But the formation of the root in the case of the slip is effected in the same manner, the moisture of the soil encouraging the protrusion of buds at and near the section; the bud that would have been converted into a branch above ground being converted into a root below.

SECTION VI.

Layers.

Artificial. INSTEAD of cutting off a portion from the parent plant altogether, in the manner of a slip, gardeners frequently select a branch and bend it down to the ground, till a part of it can be laid in the soil; the summit being still exposed to the air, and the whole being yet connected to the stem by the inferior part of the branch. When the branch is thus treated, the portion that is laid in the soil strikes root and elevates a new stem from the original summit of the branch, which is now denominated a layer, and converted into a new individual by detaching the branch wholly from the original stem. This mode of propagation is practised upon trees that are delicate and which cannot readily be propagated by means of slips; in which case the root is generated nearly as in the former, the soil stimulating the protrusion of buds which are converted into roots. But in many plants, such as the Currant and Laurel, this is altogether a natural process effected by the spontaneous bending down of a branch to the surface of the soil.

Natural.

SECTION VII.

Suckers.

Many plants protrude annually from the collar a number of young shoots, encircling the principal stem and depriving it of a portion of its nourishment, as in the case of most fruit-trees. Others send out a horizontal root, from which there at last issues a bud that ascends above the soil and is converted into a little stem, as in the case of the Elm-tree and *Syringa*. Others send out a horizontal shoot from the collar or its neighbourhood; or a shoot that ultimately bends down by its own weight till it reaches the ground, in which it strikes root and again sends up a stem, as in the abovementioned case of the Currant Bush and Laurel. The two former are called suckers or offsets, though the term offset should perhaps be restricted to the young bulbs that issue and detach themselves annually from bulbous roots. The latter is not designated by any particular name, but may be regarded

Exemplified in the Elm and Syringa.

as a sort of natural layer, resembling also, in some respect, the runner; from which, however, it is distinguished in that it never detaches itself spontaneously from the parent plant, as is the case also with the two former. But if either of them is artificially detached together with a portion of root, or a slice of the collar adhering to it, it will now bear transplanting, and will constitute a distinct plant.

SECTION VIII.

Grafts.

Artificial. THE species is also often propagated, or at least the variety is multiplied, by means of grafting, which has been already shown to be an artificial application of a portion of the shoot of one tree to the stem or branch of another, so as that the two shall coalesce together and form but one plant. The shoot which is to form the summit of the new individual is called the graft; and the stem to which it is affixed is called the stock.

As the graft is merely an extension of the parent plant from which it came, and not properly speaking a new individual, so it is found to be the best method of propagating approved varieties of fruit-trees without any danger of altering the quality of the fruit, which is always apt to be incurred in propagating from seed, but never in propagating from the graft. Some gardeners will indeed tell you that a Rose grafted on a black Currrant will produce black Roses; but this is a vulgar error. The graft will also bear fruit much sooner than the tree that is raised from seed; and, if effected on a proper stock, will be much more hardy and vigorous than if left on the parent plant. And hence the great utility of grafting in the practice of gardening.

CHAPTER X.

CAUSES LIMITING THE PROPAGATION OF THE SPECIES.

FROM the various sources of vegetable reproduction, but particularly from the fertility and dispersion of the seed, the earth would soon be overrun with plants of the most prolific species, and converted again into a desert, if it were not that nature has set bounds to their propagation by subjecting them to the control of man, and to the depredations of the great mass of animals; as well as in confining the germination of their seeds to certain and peculiar habitats. The operation of the two former causes it is not necessary for me to illustrate at present. My remarks shall therefore be directed merely to the illustration of the latter; namely, that of the circumscription of the habitats and propagation of plants as dependant on soil, climate, and altitude.

SECTION I.

Soils.

All plants will not vegetate in all soils, many of them even affect a peculiar soil; and where that soil is not to be found, they will not grow. It should be observed, however, that in this view of the subject the term soil is used in a very extensive acceptation, as signifying not only the various sorts of mould which constitute the surface of the earth, but every substance whatever on which plants are found to vegetate, or from which they derive their nourishment. The most general division of soils in this acceptation of the term is that of aquatic, terrestrial, and vegetable soils; corresponding to the division of aquatic, terrestrial, and parasitical plants.

SUBSECTION I.

Aquatic Soils.—Aquatic soils are such as are either wholly or partially inundated with water, and are fitted to produce such plants only as are denominated aquatics. Of aquatics there are several subdivisions according to the particular situations they affect, or the degree of immersion they require.

Producing marine plants. One of the principal subdivisions of aquatics is that of marine plants such as the *Fuci* and many of the *Ulvæ*, which are very plentiful in the seas that wash the coasts of Great Britain, and are generally attached to stones and rocks near the shore. Some of them are always immersed; and others which are situated above low water mark are immersed and exposed to the action of the atmosphere alternately. But none of them can be made to vegetate except in the waters of the sea.

River plants.

Another subdivision of aquatics is that of river plants, such as *Chara, Potamogeton*, and *Nymphæa*, which occupy the bed of fresh water rivers, and vegetate in the midst of the running stream; being for the most part wholly immersed, as well as found only in such situations.

Marsh plants.

A third subdivision of aquatics is that of paludal or fen plants, being such as are peculiar to lakes, marshes, and stagnant or nearly stagnant waters, but of which the bottom is often tolerably clear. In such situations you find the *Isoetis lacustris*, Flowering Rush, Water Ranunculus, Water Lily, and a variety of others which uniformly affect such situations, some of them being wholly immersed and others immersed only in part.

SUBSECTION II.

Earthy Soils.—Earthy soils are such as emerge above the water and constitute the surface of the habitable, globe that is every where covered with vegetable productions. Plants affecting such soils, which comprise by far the greater part of the vegetable kingdom, are denominated terrestrial, being such as vegetate upon the surface of the earth without having any portion immersed in water or requiring any further moisture for their support beyond that which they derive from the earth and atmosphere. This division is, like the aquatics, distributed into several subdivisions according to the peculiar situations which different tribes affect.

Producing maritime, sylvatic, alpine, and other denominations of plants.

Some of them are maritime, that is, growing only on the sea-coast, or at no great distance from it, such as *Statice*, *Glaux*, *Samolus*, Samphire, Sea Pea. Some are fluviatic, that is, affecting the banks of rivers, such as *Lythrum*, *Lycopus*, *Eupatorium*. Some are champaign, that is, affecting chiefly the plains, meadows, and cultivated fields, such as *Cardamine*, *Tragopogon*, *Agrostemma*. Some are dumose, that is, growing in the hedges, such as the Bramble. Some are ruderate, that is, growing on rubbish, such as *Senecio viscosus*. Some are sylvatic, that is, growing in woods or forests, such as *Stachys sylvatica*, *Angelica sylvestris*. And finally, some are alpine, that is, growing on the summits of mountains, such as *Poa alpina*, *Epilobium alpinum*, and many of the Mosses and Lichens.

SUBSECTION III.

Vegetable Soils.—Vegetable soils are such as are formed of vegetating or decayed plants themselves, to some of which the seeds of certain other plants are found to adhere, as being the only soil fitted to their germination and developement. The plants springing from them are denominated *Parasitical*, as being plants that will vegetate neither in the water nor earth, but on certain other plants, to which they attach themselves by means of roots that penetrate the bark, and from the juices of which they do often, though not always derive their support. This last circumstance constitutes the ground of a subdivision of parasitical plants, into such as merely adhere to other plants but do not feed on them, and such as do not merely adhere to other plants but do also feed on them.

Producing parasitical plants, such as Mosses.

In the first subdivision we may place parasitical Mosses, Lichens, and Fungi, which are found as often and in as great perfection on the stumps of rotten trees, and on rotten pales and stakes, as on trees that are yet vegetating; whence it is also plain that they do not derive their nourishment from the plants on which they grow, but from the atmosphere by which they are surrounded; the plant to which they cling serving merely as a basis of support.

In the second subdivision we may place all such plants as are strictly parasitical, that is, all such as do actually abstract from the juices of the plant to which they cling the nourishment necessary to the developement of their parts; and of which the most common, at least as being indigenous to Britain, are the Missletoe, Dodder, Broom-rape, and a sort of tuber that grows on the root of Saffron, and destroys it if allowed to spread.

Missletoe. The Missletoe, *Viscum album*, is found for the most part on the Apple-tree; but sometimes also on the Oak. The fruit of it when ripe is a soft, white, and shining berry, filled with a glutinous and sweetish juice, and about as large as a Pea. If this berry, whether by accident or design, is made to adhere to the trunk or branch of either of the foregoing trees, which from its glutinous nature it may readily be made to do, it germinates by sending out a small globular body attached to a pedicle, which after it acquires a certain length bends towards the bark, whether above it or below it, into which it insinuates itself by means of a number of small fibres which it now protrudes, and by which it abstracts from the plant the nourishment necessary to its future developement. When the root has thus fixed itself in the bark of the supporting tree, the stem of the parasite begins to ascend, at first smooth and tapering, and of a pale green colour, but finally protruding a multiplicity of branches by continually dividing into jointed forks. The leaves are of the colour of the stem, tongue-shaped entire, smooth. The plant is an evergreen; not readily distinguished in the summer, when the leaves of the tree on which it grows are fully expanded; but becoming very conspicuous in the winter, from the green and bushy appearance of its leaves, or from the white appearance of its ripened berries.

It seems to have been thought by some botanists that the roots of the Missletoe penetrate even into the wood, as well as through the bark.* But the observations of Du Hamel show that this opinion is not well founded. The roots are indeed often found within the wood, which they thus seem to have penetrated by their own vegetating power. But the fact is, that they are merely covered by the additional layers of wood that have been formed since the fibres first insinuated themselves into the bark.†

Among the Druids, the Missletoe of the Oak-tree was revered as sacred; and its medical virtues were held in the highest estimation. But it forms no prominent article in the Materia Medica of present times; except that it is still regarded by farmers and cow doctors as being of peculiar efficacy in some diseases incident to cattle; and by the lower orders of people in general as possessing some peculiar medical properties, in which they seem to think it operates as a sort of charm, but particularly in its capacity of affording a preventative to sterility; which accounts for the institution of the ancient and still prevailing custom with the inhabitants of the cottage of gathering boughs of it

* With. Arrang. vol. ii. p. 203.

† Phys. des Arb. liv. v. chap. i.

and suspending them from the ceiling of their apartments, about the season of Christmas when the fruit is ripe.

Dodder. *Cuscuta europæa,* or Dodder, though it is to be accounted a truly parasitical plant in the issue, is not yet originally so. For the seed of this plant when it has fallen to the ground takes root originally by sending down its radicle into the soil and elevating its stem into the air. It is not yet, therefore, a parasitical plant. But the stem which is now elevated above the surface lays hold of the first plant it meets with, though it is particularly partial to Hops and Nettles, and twines itself around it, attaching itself by means of little parasitical roots at the points of contact, and finally detaching itself from the soil altogether by the decay of the original root, and becoming a truly parasitical plant. Withering describes the plant in his arrangements as being originally parasitical; but this is certainly not the fact.

The *Orobanche,* or Broom-rape, which attaches itself by the root to the roots of other plants, is also to be regarded as being truly parasitical, though it sometimes sends out fibres which seem to draw nourishment from the earth. It is found most frequently on the roots of common Broom; but I have found it also on the roots of *Scabiosa arvensis;* and even upon the root of *Samolus Valerandi.* This last case I met with in the garden of the Rev. Dr. Dawson, of Burgh, in Suffolk, in the month of September, 1808. I think the *Samolus Valerandi* was raised from seed by the Doctor, who cultivated British plants with as much industry as others often cultivate exotics.

Epidendron flos æris.

The *Epidendron flos aeris*, a native of India beyond the Ganges, is regarded also by botanists as a parasitical plant, because it is generally found growing on other trees.* But there is a circumstance related concerning it which seems to excite a suspicion that it cannot be truly a parasitical plant. Mr. Loureiro says it will continue to vegetate for years even when suspended from the ceiling of a room, producing blossoms that exhale the richest fragrance; from which I think it may be inferred that it derives its nourishment wholly from the atmosphere, and not from the plant to which it adheres.

SECTION II.

Climate.

MOST plants are affected by climate, and many are confined to a particular hemisphere or latitude which they are seldom found to pass. Such is the case with the *Proteaceæ* of Jussieu, which are confined almost entirely to the southern hemisphere, and abound chiefly in the latitude of the Cape of Good Hope.† Hence it is that habitats and cli-

* Willdenow, Princ. Bot. p. 263.

† Lin. Trans. vol. x. p. 20.

mates are often the same; and hence also plants that are natives of the equatorial regions cannot be made to vegetate in high latitudes, except by putting them into a hot-house and keeping up an artificial heat. This is known to every body who is the least conversant in gardening, and forms one of the most difficult branches of the art. Hence it is impossible to naturalize the equatorial plants in this climate such as the Palms, Pine-apple, and others; because the degree of cold naturally subsisting in it would infallibly kill them. In like manner plants that are indigenous to the more temperate regions, cannot be made to vegetate in the equatorial regions, because the excessive heat of such regions would destroy them. The Wheat and Barley of Europe will not grow within the tropics; the same remark applies to plants of still higher latitudes, such as those within the polar circles which cannot be made to vegetate in more southern latitudes, nor can the plants of more southern latitudes be made to vegetate there.

Equatorial,

Tropical and polar plants,

Such is the case with plants in general, and such are the boundaries which they cannot pass, confining them to the peculiar habitat destined by nature. But some plants may be inured to climates of which they are not indigenous; and this seems to be most easily done in going from a hot to a cold climate, particularly with herbaceous plants. Because it often happens that the frosts of winter are accompanied with snow which shelters

Inured by cultivation to opposite climates, the plant from the inclemency of the atmosphere till the return of spring. Trees and shrubs, on the contrary, are naturalized with more difficulty, because they cannot be so easily sheltered from the colds, owing to the greater length of their stem and branches. But nature, always provident for the preservation of all her works, and always fertile in resources for the accomplishment of her object, has also furnished some plants with the capacity of vegetating in almost all climates, or of naturalizing themselves in almost any. This is particularly the case with greens and eatable roots, such as Cabbages, Carrots, Potatoes, that is, the common culinary plants most useful to man. And hence they have followed man into all climates and quarters of the globe. Some aquatic plants are found capable of vegetating also in almost all climates, perhaps because the water modifies in some measure the temperature. *Lemna minor* has been found throughout almost the whole of Europe, North America, and even Asia;* and *Fucus natans*, both under the equator and within the polar circles. Plants which grow in the depths of the ocean are not at all affected by climate, because they are beyond the reach of the influence of the sun's rays, and air; so that habitats in this case must be fixed by the greater or less degree of salts held in solution by the water. As the habitats dependant on climate are, like the climates themselves, bounded by certain

Or adapting themselves by nature.

* Willdenow, p. 395.

parallels of latitude as they recede from or approach to the equator; they are consequently the same in all longitudes, and nearly so in corresponding latitudes, on either side of the equator. But the warmer climates are more favourable upon the whole to vegetation than the colder, and that nearly in proportion to their distance from the equator. In Spitzbergen botanists have hitherto found only 30 indigenous plants, in Lapland 534, in Iceland 553, in Sweden 1299, in Brandenburg 2000, in Piemont 2800, in Jamaica, Madagascar, and the coast of Coromandel, from 4000 to 5000.* The same plants, however, will grow in the same degree of latitude, throughout all degrees of longitude, and also in correspondent latitudes on different sides of the equator; the same species of plants, as some of the Palms and others, being found in Japan, India, Arabia, the West Indies, and part of South America, which are all in nearly the same latitudes; and the same species being also found in Kamschatka, Germany, Great Britain, and the coast of Labrador, which are all also in nearly the same latitudes.

* Willdenow, p. 374.

SECTION III.

Altitude.

ALTHOUGH the above rule with regard to climates is of pretty general application, yet it is at the same time liable to a good many exceptions, owing chiefly to the difference of altitude that may and often does occur in countries of the same latitude; as well as to a variety of other causes affecting the vegetable. This must be obvious from the consideration that the temperature of any place is affected as much from its altitude as from its latitude. The summit of the mountains of the Andes, even where situated almost directly under the equator, are yet covered with eternal snow. As effecting temperature,

Hence it follows that all variety of climates may exist even in the same latitude merely by means of the altitude of the place, and consequently all varieties of vegetable habitat. And this was found by Tournefort to be literally the case during his travels in Asia. At the foot of mount Ararat he met with plants peculiar to Armenia; above these he met with plants which are found also in France; at a still greater height he found himself surrounded with such as grow in Sweden, and at the summit with such as vegetate in the polar regions. And consequently vegetable habitat.

This accounts for the great variety of plants which are often found in a *Flora* of no great ex- And natural floræ.

tent: and it may be laid down as a botanical axiom, that the more diversified the surface of the country the richer will its *Flora* be, at least in the same latitudes. It accounts also for the want of correspondence between plants of different countries though placed in the same latitudes; because the mountains or ridges of mountains, which may be found in the one and not in the other, will produce the greatest possible difference in the character of their *Floras*. And to this cause we may ascribe the diversity that often actually exists between plants growing in the same latitudes, as between those of the north-west and north-east coast of North America, as also of the south-west and south-east coast; the former being more mountainous, the other more flat. Sometimes the same sort of difference takes place between the plants of an island and those of the neighbouring continent; that is, if the one is mountainous and the other flat; but if they are alike in their geographical delimation, then they are generally alike in their vegetable productions.

Cold and lofty situations are the favourite habitat of most cryptogamic plants of the terrestrial class, especially the *Fungi*, *Algæ*, and Mosses; as also of plants of the class *Tetradynamia*, and of the *Umbellate* and *Syngenesial* tribes. Whereas trees and shrubs, Ferns, Parasitic plants, Lilies, and Aromatic plants, are most abundant in warm climates; only this is not to be understood merely of geographical climates, because, as we have seen, the physical climate depends upon altitude. In consequence of which, combined with the ridges and direction of the mountains, America and Asia are much colder in the same degrees of northern latitude than Europe. American plants vegetating at 42° of northern latitude will vegetate very well at 52° in Europe. The same, or nearly so, may be said of Asia, which in the former case is perhaps owing to the immense tracts of woods and marshes covering the surface, and in the latter to the more elevated and mountainous situation of the country affecting the degree of temperature. So also Africa is much hotter under the tropics than America; because in the latter the temperature is lowered by immense chains of mountains traversing the equatorial regions, while in the former it is increased by means of the hot and burning sands that cover the greater part of its surface.

The effects of altitude are observable also even in the case of aquatics, as modifying the habitats; thus some acquatics float always on the surface of the water, as *Lemna,* while others are either partially or wholly immersed. Such as grow in the depths of the sea are not influenced by climate; but such as are near the surface are influenced by climate, and have their habitats affected by it.

SECTION IV.

General Remarks.

Habitat influencing habit,

THE habit of vegetables is sometimes affected by the habitat, so as to give to plants of different countries, though of the same genus, a sort of characteristic feature by which their country may often be discovered, in the same manner as the national distinctions which are observable in the looks and colour of mankind, and which are effected chiefly by climate. On this subject botanists have made the following remarks :—Asiatic plants are remarkable for their superior beauty; African plants for their thick and succulent leaves, as in the case of the *Cacti;* and American plants for the length and smoothness of their leaves, and for a sort of singularity in the shape of the flower and fruit. The flowers of European plants are but rarely beautiful, a great proportion of them being amentaceous. Plants indigenous to polar and mountainous regions are generally low, with small compressed leaves; but with flowers large in proportion. Plants indigenous to New Holland are distinguishable for small and dry leaves that have often a shrivelled appearance. In Arabia they are low and dwarfish; in the Archipelago they are generally shrubby and furnished with prickles; while in the Canary Islands many plants, which in other countries are merely herbs, assume the port of shrubs and trees.

As exemplified in Asiatic, European, African, and American plants.

The shrubby plants of the Cape of Good Hope and New Holland exhibit a striking similarity, as also the shrubs and trees of the northern parts of Asia and America, which may be exemplified in the *Platanus orientalis* of the former, and in *Platanus occidentalis* of the latter, as well as in *Fagus sylvatica* and *Fagus latifolia*, or *Acer cappadocicum*, and *Acer saccharinum ;* and yet the herbs and undershrubs of the two countries do not in the least correspond.*

A change of habitat will often alter the habit of a plant so much that the species can scarcely be recognized ; particularly if you remove it from its natural and uncultivated state into a state of cultivation. Hence the colour of the flower is sometimes changed and frequently the figure of the leaves, as in common Colewort, and Celery : and hence the Crab-tree and others divest themselves of their thorns, and flowers are often rendered double.

Influencing colour and figure,

But plants will often thrive very well though transported from their native habitats by the art and industry of man even into countries where they would not naturally have disseminated themselves. Most of the culinary plants of Europe have been brought from the east, through the Greeks and Romans. And several useful vegetables, but particularly the Potatoe, have been brought from Ame-

Though not the health of the plant.

* Willdenow, Prin. Bot. p. 390.

rica.* *Phaseolus vulgaris,* and *Impatiens Balsamina* where brought originally from India; and *Datura Stramonium,* which is now naturalized in Europe was brought originally from India or Abyssinia. Buckwheat and most species of Corn and Peas came also from the east, and along with them several plants found among Corn only, such as *Centaurea Cyanus, Agrostemma Githago, Raphanus Raphanistrum,* and *Myagrum sativum.*

CHAPTER XI.

EVIDENCE AND CHARACTER OF VEGETABLE VITALITY.

Criterion of a vital principle.

The best and most satisfactory evidence of the presence and agency of a vital principle as inherent in any subject is perhaps that of its rendering the subject in which it inheres capable of counteracting the laws of chemical affinity. This rule, which seems to have been first instituted by Humboldt, is obviously applicable to the case of animals, as is proved by the process of the digestion of the food, and its conversion into chyle and blood; as well as from the various secretions and excretions effected by the several organs, and effecting the growth and developement of the individual, in direct opposition

* This most useful plant was first brought into Europe by Sir W. Ralegh, in 1623.

to the acknowledged laws of chemical affinity, which, as soon as the vital principle is extinct, begin immediately to give indication of their action in the incipient symptoms of the putrefaction of the dead body.

Applied to vegetables.

But the rule is also applicable to the case of vegetables, as is proved by the intro-susception, digestion, and assimilation of the food necessary to their developement; all indicating the agency of a principle capable of counteracting the laws of chemical affinity; which, at the period of what is usually called the death of the plant, begin also immediately to act, and to give evidence of their action in the incipient symptoms of the putrefaction of the vegetable. Vegetables are therefore obviously endowed with a species of vitality. But admitting the presence and agency of a vital principle inherent in the vegetable subject, what are the peculiar properties by which this principle is characterized?

SECTION I.

Excitability.

One of the most distinguishable properties of the vital principle of vegetables is that of its excitability, or capacity of being acted upon by the application of natural *stimuli*, impelling it to the exertion of its vegetative powers; the natural stimuli thus impelling it being light and heat.

SUBSECTION I.

Action of Light.—The stimulating influence of light upon the vital principle of the plant is discoverable, whether in the stem, leaf, or flower. The direction of the stem is influenced by the action of light.

Influencing the direction of the stem,

If a plant is placed in a room or cave in which there is only one small aperture for the admission of light, the stem will gradually bend towards that aperture. Bonnet sowed some French Beans in a dark cave, with a view to ascertain the effect of the small portion of light transmitted to them through the entrance: the stem was a little inclined towards the entrance during the day, but it regained its erect position partially at least during the night.

The vigour and colour of the plant,

The vigour and colour of the stem are also affected by the presence or absence of light. If a cutting of Potatoe is left to vegetate in a cellar, where there is but little access to light and air, the stem will shoot out to a great length in the direction of the light; but pale, and limber, and trailing on the floor. Bonnet planted three Beans for the purpose of comparative experiment, one in the open air, another in a tube of glass covered at the top; and a third in a tube of wood covered at the top also. The first plant was strong and luxuriant; the second was also strong, and inclined towards the sun; but the third, though tall, was pale and sickly. Hence it is upon the principle of the exclusion of light that plants are blanched, as in the case of the blanching of Celery, which is sometimes termed also etiolation.

The direction of the branches,

The direction and luxuriance of the branches depend also on the presence and action of light, as is particularly observable in the case of hot-house plants, the branches of which are not so conspicuously directed, either to the flue in quest of heat, or to the door or open sash in quest of air, as to the sun in quest of light. Hence also the branches of plants are often more luxuriant on the south than on the north side; or at least on the side that is best exposed to light.

The position of the leaf.

The position of the leaf is also strongly affected by the action of light to which it uniformly turns its upper surface. This may be readily perceived in the case of trees trained to a wall, from which the upper surface of the leaf is by consequence always turned; being on a south wall turned to the south, and on a north wall turned to the north. And if the upper surface of the leaf is forcibly turned towards the wall and confined in that position for a length of time, it will soon resume its primitive position upon regaining its liberty, but particularly if the atmosphere is clear. Bonnet tried to retain a leaf in its inverted position by means of twisting the leaf-stalk; but it was always found to untwist itself again in the course of a short time, and again to present its upper surface to the sun or light. This it was sometimes found to do even in the night; but always the most expeditiously in young subjects. If the experiment is often repeated, the leaf resumes its original position with more difficulty, and exhibits evident marks of being injured by the exertion, in the appearance of several black spots about the veins of the under surface, and in the scaling off of the cuticle.

But all leaves are not equally susceptible to the action of the *stimulus* of light. The leaves of the Mallow are said to exhibit but slight indications of this susceptibility, as also sword shaped leaves; the leaves of the Missletoe, which have never been known to resume a former position in consequence of any change in the position of the branch, because perhaps they are equally susceptible on both sides.* But succulent leaves are said to be particularly susceptible, notwithstanding their thick and firm texture; and if the leaf of a Vine is even separated from the branch and suspended by a fine thread, so as that the upper surface shall be turned from the light, it will yet gradually alter its position till it comes round again to it.† This experiment requires to be made with great care and delicacy lest the leaf should be made to turn by means of the effect of the atmosphere upon the thread; though in this case it may perhaps be said that the

* Smith's Introduction, p. 208. † Ibid.

change is not effected by the stimulus of the light acting on the vital principle, but rather on the fibres of the leaf. But the reply is that the leaf is not yet entirely deprived of the vital principle; as it is not to be supposed that the experiment would succeed upon a leaf that is withered and decayed.

Light the sole agent.

Such are the effects produced. Is light the sole agent? It had been conjectured that the effect is partly attributable to the agency of heat; and to try the value of the conjecture Bonnet placed some plants of the *Atriplex* in a stove heated to 25° of Reaumur. Yet the stems were not inclined to the side from which the greatest degree of heat came; but to a small opening in the stove. Heat then does not seem to exert any perceptible influence in the production of the above effects. Does moisture? Bonnet found that the leaves of the Vine exhibited the same phenomenon when immersed in water as when left in the open air. Whence it seems probable that light is the sole agent in the production of the effects in question.

Counter effects of darkness.

But as light produces such effects upon the leaves, so darkness or the absence of light produces an effect quite the contrary; for it is known that the leaves of many plants assume a very different position in the night from what they have in the day. This is particularly the case with winged leaves, which, though fully expanded during the day, begin to droop and bend down about sun-set and during the fall of the evening dew, till they meet together on the inferior side of the leaf-stalk, the terminal lobe, if the leaf is furnished with one, folding itself back till it reaches the first pair; or the two side lobes, if the leaf is trifoliate, as in the case of common Clover, which seems to have been first observed by the daughter of Linnæus. So also the leaflets of the False Acacia and Liquorice hang down during the night, on each side of the mid-rib, but do not meet beneath it. The leaves of *Mimosa pudica* fold themselves up along the common foot-stalk so as to overlap one another. But, perhaps, this effect is produced partly by the agency of moisture as it is accelerated by dews and rains, and may even be occasioned by artificial watering: or perhaps such leaves as fold themselves up in the above manner may require an interval of rest, which they thus obtain, after having been exposed throughout the day to the *stimulus* of light. And if so, then Linnæus has not without propriety designated the above phenomenon by the appellation of *The Sleep of Plants.*

Influence of light on the flower.

The expansion of the flower is also effected by the action of light. Many plants do not fully expand their petals except when the sun shines; and hence alternately open them during the day and shut them up during the night. This may be exemplified in the case of papilionaceous flowers in general, which spread out their wings in fine weather to admit the rays of the sun, and again fold them up as the night approaches. It may be exemplified also in the case of compound flowers, as in that of the Dandelion and Hawkweed. But the most singular case of this kind is perhaps that of the *Lotus* of the Euphrates as described by Theophrastus, which he represents as rearing and expanding its blossom by day, closing and sinking down beneath the surface of the water by night, so as be beyond the grasp of the hand, and again rising up in the morning to present its expanded blossom to the sun.* The same phenomenon is related also by Pliny.†

But although many plants open their flowers in the morning and shut them again in the evening, yet all flowers do not open and shut at the same time. Plants of the same species are, however, pretty regular to an hour, other circumstances being the same; and hence the daily opening and shutting of the flower has been denominated by botanists *The Horologium Floræ*. Flowers requiring but a slight application of stimulus open early in the morning, while others requiring more open somewhat later. Some do not open till noon, and some, whose extreme delicacy cannot bear the action of light at all, open only at night, such as the *Cactus grandiflora*, or Night-blowing *Cereus*. Horologium floræ.

But it seems somewhat doubtful whether or not light is the sole agent in the present case; for it has been observed that equatorial flowers open always at the same hour, and that tropical flowers change their hour of opening according to the length of Is light the sole agent?

* Περι φυτων ἱστος. το. Δ. † Lib. xiii. 18.

the day. It has been observed also, that the flowers of plants that are removed from a warmer to a colder climate expand at a later hour in the latter. A flower that opens at six o'clock in the morning at Senegal, will not open in France or England till eight or nine; nor in Sweden till ten. A flower that opens at ten o'clock at Senegal will not open in France or England till noon or later, and in Sweden it will not open at all. And a flower that does not open till noon or later at Senegal, will not open at all in France or England. This seems as if heat or its absence were also an agent in the opening and shutting of flowers; though the opening of such as blow only in the night cannot be attributed either to light or heat.

Vegetable weather-glass. But the opening or shutting of some flowers depends not so much on the action of the stimulus of light as on the existing state of the atmosphere, and hence their opening or shutting betokens change. If the Siberian Sowthistle shuts at night, the ensuing day will be fine; and if it opens, it will be cloudy and rainy. If the African Marigold contines shut after seven o'clock in the morning, rain is near at hand. And if the *Convolvulus arvensis*, *Calendula fluvialis*, or *Anagallis arvensis*, are even already open, they will shut upon the approach of rain, the last of which from its peculiar susceptibility has obtained the name of the Poor Man's Weather-glass.

Nutation. But some flowers not only expand during the light of day ; they incline also towards the sun, and follow his course, looking towards the east in the morning, towards the south at noon, and towards the west in the evening; and again returning in the night to their former position in the morning. Such flowers are designated by the appellation of *Heliotropes*, on account of their following the course of the sun ; and the movement they thus exhibit is denominated their *nutation*. This phenomenon had been observed by the ancients long before they had made any considerable progress in botany, and had even been intervoven into their mythology, having originated, according to the records of fabulous history, in one of the metamorphoses of early times. Clytie, inconsolable for the loss of the affections of Sol, by whom she had been formerly beloved, and of whom she was still enamoured, is represented as brooding over her griefs in silence and solitude; where refusing all sustenance, and seated upon the cold ground, with her eyes invariably fixed on the sun during the day, and watching for his return during the night, she is at length transformed into a flower, retaining, as much as a flower can retain it, the same unaltered attachment to the sun. This is the flower which is denominated *Heliotropium* by the ancients, and described by Ovid as *Flos qui ad solem vertitur*.* But it is to be observed that the flower alluded to by Ovid cannot be the *Heliotropium* of the moderns, because Ovid describes it

Exemplified in Heliotropium.

* Metamorph. lib. iv. l. 250.

as resembling the Violet: much less can it be the Sun-flower of the moderns, which is a native of America, and could not consequently have been known to Ovid; so that the true *Heliotropium* of the ancients is perhaps not yet ascertained.

Bonnet has further remarked that the ripe ears of Corn, which bend down with weight of grain, scarcely ever incline to the north, but always less or more to the south; of the accuracy of which remark any one may easily satisfy himself by looking at a field of Wheat ready for the sickle; he will find the whole mass of ears nodding, as if with one consent, to the south.

And in ripened corn.

The cause of the phenomenon has been supposed to be a contraction of the fibres of the stem or flower-stalk on the side exposed to the sun; and this contraction has been thought by M. De La Hire and Dr. Hales to be occasioned by an excess of transpiration on the sunny side; which is probably the fact, though there seems upon this principle to be some difficulty in accounting for its returning at night; because if you say that the contracted side expands and relaxes by moisture, what is it that contracts the side that was relaxed in the day? The moisture, of which it is no doubt still full, would counteract the contraction of its fibres, and prevent it from resuming its former position in the morning.

SUBSECTION II.

Action of Heat.—Heat as well as light acts also as a powerful stimulus to the exertion of the vital principle. This has been already shown in treating of the process of germination, in which it was found that seeds will not germinate at a very low temperature, even though placed in a proper soil, so that such as sow themselves do not generally come up till the spring when the temperature has been raised to some considerable height by the rays of the returning sun. But the same thing is observable with regard to the developement and maturation of the leaves, flower, and fruit; for although all plants produce their leaves, flower, and fruit, annually, yet they do not all produce them at the same period or season. This forms the foundation of what Linnæus has called the *Calendarium Floræ,* including a view of the several periods of the Frondescence and Efflorescence of Plants, together with that of the Maturation of the Fruit.

As influencing the protrusion of leaf, flower, and fruit.

Art. 1. *Frondescence.*—It must be plain to every observer that all plants do not protrude their leaves at the same season, and that even of such as do protrude them in the same season, some are earlier and some later. The Honeysuckle protrudes them in the month of January; the Gooseberry, Currant, and Elder, in the end of February or beginning of March; the Willow, Elm, and Lime-tree, in April;

Seasons peculiar to different plants.

and the Oak and Ash, which are always the latest among trees, in the beginning or towards the middle of May. Many annuals do not come up till after the summer solstice; and many Mosses not till after commencement of winter. This gradual and successive unfolding of the leaves of different plants seems to arise from the peculiar susceptibility of the species to the action of heat, as requiring a greater or less degree of it to give the proper stimulus to the vital principle. But a great many circumstances will always concur to render the time of the unfolding of the leaves somewhat irregular; because the mildness of the season is by no means uniform at the same period of advancement; and because the leafing of the plant depends upon the peculiar degree of temperature, and not upon the return of a particular day of the year. Hence it has been thought that no rule could be so good for directing the husbandman in the sowing of his several sorts of grain as the leafing of such species of trees as might be found by observation to correspond best to each sort of grain respectively, in the degree of temperature required.

A signal to the husbandman.

Linnæus, who instituted some observations on the subject about the year 1750, with a view chiefly to ascertain the time proper for the sowing of Barley in Sweden, regarded the leafing of the Birch-tree as being the best indication for that grain, and recommended the institution of similar observations with regard to other sorts of grain, upon the ground of its great importance to the husbandman. But however plausible the rule thus suggested may be in appearance, and however pleasing it may be in contemplation, it is not likely that it will ever be much attended to by the husbandman; because nature has furnished him with indications that are still more obvious in the very evidence of his own feelings, as well as perhaps more correct; as all trees of the same species do not come into leaf precisely at the same time, and as the weather may yet alter even after the most promising indications.

Seasons proper to different flowers.

ART. 2. *Efflorescence.*—The flowering of the plant, like the leafing, seems to depend upon the degree of temperature induced by the returning spring, as the flowers are also protruded pretty regularly at the same successive periods of the season. The Mezereon and Snow-drop protrude their flowers in February; the Primrose in the month of March; the Cowslip in April; the great mass of plants in May and June; many in July, August, and September; some not till the month of October, as the Meadow Saffron; and some not till the approach or middle of winter, as the *Laurustinus* and *Arbutus*. Such at least is the period of their flowering in this country; but in warmer climates they are earlier, and in colder climates they are later.

Between the tropics, where the degree of heat is always high, it often happens that plants will flower more than once in the year; because they do not there require to wait till the temperature is raised to a certain height, but merely till the developement of their parts can be effected in the regular operation of nature, under a temperature already sufficient. For the greater part, however, they flower during our summer, though plants in opposite hemispheres flower in opposite seasons. But in all climates the time of flowering depends also much on the altitude of the place as well as on other causes affecting the degree of heat. Hence plants occupying the polar regions, and plants occupying the tops of the high mountains of southern latitudes are in flower at the same season; and hence the same flowers are later in opening in North America than in the same latitudes in Europe, because the surface of the earth is higher, or the winters more severe.

Not dependant on the time of flowering.

ART. 3. *Maturation of the Fruit.*—Plants exhibit as much of diversity in the warmth and length of time necessary to mature their fruit as in their frondescence and flowering; but the plant that flowers the soonest does not always ripen its fruit the soonest. The Hazle-tree, which blows in February, does not ripen its fruit till autumn; while the Cherry, that does not blow till May, ripens its fruit in June. It may be regarded, however, as the general rule that if a plant blows in spring it ripens its fruit in summer, as in the case of the Currant and Gooseberry; if it blows in summer it ripens its fruit in autumn, as in the case of the Vine; and if it blows in autumn it ripens its fruit in the winter. But the Meadow Saffron, which blows in the autumn, does not ripen its fruit till the succeeding spring.

Calendarium floræ.

Such are the primary facts on which a *Calendarium Floræ* should be founded. They have not hitherto been very minutely attended to by botanists; and perhaps their importance is not quite so much as has been generally supposed: but they are at any rate sufficiently striking to have attracted the notice even of savages. Some tribes of American Indians act upon the very principle suggested by Linnæus, and plant their corn when the wild Plum blooms, or when the leaves of the Oak are about as large as a squirrel's ears. The names of some of their months are also designated from the state of vegetation. One is called the budding month, and another the flowering month; one the Strawberry month, and another the Mulberry month: and the autumn is designated by a term signifying the fall of the leaf.* So that the French revolutionists were anticipated even by the Indians, in their new names for months and seasons.

Miscellaneous phenomenon.

But there are several other ways in which the agency of heat may be observed as exciting the energies of the vital power. The leafits of some of the leguminous plants, when exposed to the action of an ardent sun, are often erected into a vertical position on each side the leaf-stalk, which they sometimes even pass so as to close together.

* Barton's Elem. p. 248.

Under similar circumstances the leaves of the Indian Mallow become concave; and it seems as if the effect were produced merely, or at least, chiefly by means of heat; because the same effect may be produced even by means of the application of a hot iron; and yet the leafits of many such plants fold themselves back at night so as to meet under the leaf-stalk, a phenomenon equally wonderful with that of nutation, and not attributable to heat. But several species of Mimosa exhibit a singular phenomenon even in the common foot-stalk, which is found to have a sort of natural movement dependant upon temperature also, so that it is elevated in the course of the day, and depressed in the course of the night, according to the observation of Du Hamel. At nine o'clock in the morning of a day in the month of September, the weather being moderately fine, the foot-stalk of a leaf of the *Mimosa pudica* formed by its position an angle of 100° with the lower part of the stem: at noon it formed an angle of 112°: at three o'clock in the afternoon it had fallen to an angle of 100°: and during the night it fell to an angle of 90°;* thus indicating an evident susceptibility to the *stimulus* of the action of heat.

The vital principle exerts its energies even in winter.

As the elevation of temperature induced by the heat of summer is essential to the full exertion of the energies of the vital principle, so the depression of temperature consequent upon the colds of winter has been thought to suspend the exertion of the

* Phys. des Arb. liv. iv. chap. vi.

vital energies altogether. But this opinion is evidently founded on a mistake, as is proved by the example of such plants as protrude their leaves and flowers in the winter season only, such as many of the Mosses; as well as by the dissection of the yet unfolded buds at different periods of the winter, even in the case of such plants as protrude their leaves and blossoms in the spring and summer, and in which it has been already shown there is a regular, and gradual, and incipient developement of parts, from the time of the bud's first appearance till its ultimate opening in the spring. The sap, it is true, flows much less freely, but is not wholly stopped. Hales lopped off some branches from plants of the Hazle, Vine, and Jessamine respectively, in the course of the winter, and covered the section of the separated branches with mastic, which in a few days were found to have lost considerably in weight; whence he inferred the motion of the sap, because it seems but reasonable to suppose that the dissipation of sap thus lost would have been repaired if the branches had not been cut off. Du Hamel planted some young trees in the autumn, cutting off all the smaller fibres of the root, with a view to watch the progress of the formation of new ones. At the end of every fortnight he had the plants taken up and examined with all possible care to prevent injuring them, and found that, when it did not actually freeze, new roots were always uniformly developed.

Though it is roused into increased activity in the spring.

Hence it follows that even during the period of winter, when vegetation seems totally at a stand, the tree being stripped of its foliage, and the herb apparently withering in the frozen blast, still the energies of vegetable life are exerted; and still the vital principle is at work, carrying on in the interior of the plant, concealed from human view, and sheltered from the piercing frosts, operations necessary to the preservation of vegetable life, or protrusion of future parts; though it requires the returning warmth of spring to give that degree of velocity to the juices which shall render their motion cognizable to man, as well as that expression to the whole plant which is the most evident token of life: in the same manner as the processes of respiration, digestion, and the circulation of the blood are carried on in the animal subject even while asleep; though the most obvious indications of animal life are those of the motions of the animal when awake.

By the stimulus of heat.

Heat then acts as a powerful stimulus to the operations of the vital principle, accelerating the motion of the sap, and consequent developement of parts; as is evident from the sap's beginning to flow much more copiously as the warmth of spring advances, as well as from the possibility of anticipating the natural period of their developement by forcing them in a hot-house. But it is known that excessive heat impedes the progress of vegetation as well as excessive cold; both extremes being equally prejudicial. And hence the sap flows more copiously in the spring and autumn, than in either the summer or winter; as may readily be seen by watching the progress of the growth of the annual shoot, which after having been rapidly protruded in the spring, remains for a while stationary during the great heat of summer, but is again elongated during the more moderate temperature of autumn.

There are also several substances which have been found to operate as stimulants to the agency of the vital principle when artificially, dissolved in water and applied to the root or branch. The germination of Peas is accelerated by means of moistening them in water impregnated with oxygenated muriatic acid, as was first ascertained by Humboldt: and the vegetation of the bulbs of the Hyacinth and Narcissus is accelerated by means of the application of a solution of nitre.* Dr. Barton, of Philadelphia, found that a decaying branch of *Liriodendron tulipifera* and a faded flower of the yellow Iris recovered and continued long fresh when put into water impregnated with camphor; though flowers and branches, in all respects similar, did not recover when put into common water.

* Willdenow, p. 295.

SECTION II.

Irritability.

PLANTS are not only susceptible to the action of the natural *stimuli* of light and heat, exciting them gradually to the exercise of the functions of their different organs in the regular progress of vegetation; they are susceptible also to the action of a variety of accidental or artificial stimuli, from the application of which they are found to give indications of being endowed also with a property similar to what we call irritability in the animal system. This property is well exemplified in the genus *Mimosa*; but particularly in that species known by the name of the *Sensitive Plant*.

Exemplified in the Sensitive Plant,

If a leafit of this plant is but touched, however slightly, by any extraneous body, it immediately shrinks into itself, and communicates the impulse, if strong, perhaps to the whole wing, each leafit shrinking, or each pair of leafits collapsing in succession, and the leaf-stalk itself sinking downwards as if by a joint, at its point of union with the stem. The following experiments were made by Duhamel with a view to ascertain the extent of its susceptibility:*—At eight o'clock in the morning of a day in September a leaf-stalk of a Sensitive Plant formed with the lower part of the stem an angle of 135°, which upon being touched fell to an angle of 80°;

* Phys. des Arb. liv. iv. chap. vi.

an hour afterwards it rose again to 135°, and upon being touched a second time fell again also to 80°; an hour and a half afterwards it rose to 145°, and upon being touched fell to 135°, where it remained till five o'clock in the evening, when upon being touched it fell to 110°. Hence it follows that the susceptibility is greatest in the morning, or during the heat of the day; but the leaf recovers itself sooner or later according to the vigour of the plant, the season of the year, and temperature of the atmosphere, as well as the hour of the day at which the experiment is made; though it does not always recover itself in the same way: for sometimes the common foot-stalk recovers first, sometimes the lateral foot-stalk, and sometimes the leafits themselves.

The leaves of *Dionæa Muscipula*, or Venus' Fly-trap, are also extremely susceptible to the action of accidental *stimuli*. They are all radical and approaching to battledore-shaped, with a sort of circular process at the apex, which is bisected by a midrib and ciliated with fine hairs like an eye-lash: this circular process is the seat of irritability, which, if it is touched with any sharp-pointed instrument, or if an insect but alights upon it, the segments immediately collapse and adhere so closely, that the insect is generally squeezed to death in its grasp; or at the least detained a prisoner. Dionæa Muscipula,

A similar susceptibility to the action of accidental stimuli has been observed in the leaves of the seve- Drosera, ral British species of *Drosera*, or Sun-dew, of which a very full and satisfactory account is given in the second volume of Withering's Arrangements, under the head of this genus.

Berberis communis,

But sometimes the irritability resides in the flower, and has its seat either in the stamens or style. The former case is exemplied in the flower of the Berberry, the stamens of which when undisturbed lie reclined upon the petals, and shelter the anthers under their concave tips. But if the inner side of the filament is accidentally or intentionally touched with any fine instrument or other pointed substance, the stamen immediately bends itself inwards till its anther strikes against the stigma. This fact had been long known to botanists, but it remained to be ascertained whether the susceptibility in question was confined to the inner side of the filament merely, or whether it pervaded the whole stamen. With this object in view, Sir J. E. Smith, having procured some flowers fully blown, on the 25th of May, 1786, examined them with great care, and after applying the point of a quill or fine bristle with all possible delicacy to every part of the surface of the stamen, he found that it no where exhibited any indications of susceptibility except on the inner side of the filament and towards the base. It had been thought that the stamens possessed this property only at the time of shedding the pollen; but Sir J. E. Smith found that they possess it at all ages, and even when the petal with its annexed filament has fallen to the ground, gradually recovering their original situation, and capable of being again stimulated as before.*

Cactus Tuna,

The stamens of *Cactus Tuna*, a sort of Indian Fig, are said to be endowed with a similar irritability. If a quill or feather is drawn across its long and slender filaments, which surround the germen in great numbers, they will immediately begin to bend to the one side, and will by and by sink down to the bottom of the flower.†

And Stylidium glandulosum.

The latter case, or that in which the seat of irritability is confined to the style, is exemplified in *Stylidium glandulosum*, a native of New Holland. The style of this flower, which is about an inch in length, is bent backwards a little above the base, in the manner of the piece of iron that is fixed to the end of a shepherd's crook, or to the end of the pole of a chaise; so that the style forms a sort of hook with the flower-stalk, the stigma being reflected so as in many cases to touch it. But if the stigma is itself touched with the point of the finger, or other suitable instrument, the style is immediately put into motion, and flies back till it bends itself as much in a contrary direction, and on the other side of the flower, as it did in its first direction. This experiment I had an opportunity of making on a plant in Kew Gardens, on the 31st of May, 1810.

* Smith's Tracts. † Ibid.

SECTION III.

Sensation.

FROM the facts adduced in the preceding sections it is evident that plants are endowed with a capacity of being acted upon by the application of *stimuli*, whether natural or artificial, indicating the existence of a vital principle, and forming one of the most prominent features of its character. But besides this obvious and acknowledged property, it has been thought by some phytologists that plants are endowed also with a species of sensation.

As ascribed to plants by Dr. Percival and Sir J. E. Smith,

The detail of the arguments adduced in support of this opinion is to be met with in a paper written by Dr. Percival, and published, as I believe, in the second volume of the Manchester Transactions, though I have never had an opportunity of consulting it; but as the opinion has been also adopted by Sir J. E. Smith and advocated with some degree of zeal, it is to be presumed he has selected and exhibited the most substantial arguments which the case affords, either in his Lectures or Introduction. And yet it cannot be said that he advances his arguments with any great degree of confidence, as he seems rather to hope that the doctrine may be true, than to think he has proved it to be so. But he regards the irritability of the Sensitive Plant and others, the phenomenon of the fecundation of the *Valisneria*, together with that of the sleep of plants, as

On the ground of phenomena of the Sensitive observed in the *Papilionaceæ*, Water-lily, and others, as affording at least a strong presumption that plants are endowed with the faculty of sensation: because he thinks it difficult to account for the phenomena on any other supposition; and because the supposition is besides the most consonant to our notions of the Divine goodness, as there are but few plants in comparison that suffer from the attacks of men or animals.*

Plant and others.

The opposers of this doctrine argue thus:—If the mere communication of a sentient principle were sufficient to secure the happiness of the sentient being, however situated and in whatever degree conferred, then it might be consonant to our notions of the Divine Goodness to suppose its existence in plants. But as plants have no means whatever of self-preservation or defence, and are exposed indiscriminately to the perpetual attacks both of men and animals, sensation could hardly be regarded as a blessing if it were even conferred on them. We detract nothing therefore from the Divine Goodness by supposing them devoid of sensation; we rather add to it.

And perhaps it is less difficult to account for the phenomena in question than has been imagined. Is not the susceptibility of the *Mimosa*, *Stylidium*, and others to the action of irritating stimuli, something similar to that of the muscular fibre of animals when exposed to the action of the Galvanic

That may be accounted for otherwise.

* Introduction, chap. i.

fluid, after the sentient principle is gone? Is not the submersion of the Water Lily during the night, if such is the fact, the result merely of the shrinking of the stem, in the absence of light and warmth? or of an alteration in the specific gravity of the flower, in consequence of the folding in of the petals? Is not the emerging of the male flowers of the *Valisneria* at the period of impregnation, as well as the subsequent sinking down of the female flower, to be attributed to the same cause? And is not the expansion of the petals during the day, and their shutting up during the night, as well as also the mutation of the plant, to be attributed merely to the chemical action of light and heat operating upon the fibres, or vital principle of the plant, as was supposed by Hales? If these causes are sufficient to account for the effects in question, then it would be altogether unphilosophical to allege the agency of a higher cause.

Hedysarum gyrans.

But one of the strongest indications of the existence of a species of sensitive principle in the plant is perhaps that which is exhibited in the case of *Hedysarum gyrans*. This plant is a native of India, and grows on the banks of the Ganges, its leaves are ternate, the middle leafit being larger, and the lateral leafits smaller. All of them are in perpetual motion up and down, sometimes equably and sometimes by jerks, but without any unison between each other; the motion being always the most distinct and most rapid in the lateral leafits.

If their motion is temporarily suspended by grasping them in the hand, they quicken it when the hand is removed, as if to make up for lost time, and by and by resume their original velocity. This movement does not depend upon the application of any external stimulus, because it takes place alike by night and by day, in the dark and in the light, and requires only a very warm and fine day to be effected in the best style; the leaves exhibiting then a sort of tremulous motion in addition to that already described. Such is a phenomenon that puzzles and astonishes every beholder, and still remains inexplicable; but which participates more of the character of animal spontaneity than any other movement hitherto observed in vegetables.

SECTION IV.

Instinct.

Difficulty of giving a good definition.

THERE is also a variety of phenomena exhibited throughout the extent of the vegetable kingdom, some of which are common to plants in general, and some peculiar to certain species, that have been thought by several botanical writers to exhibit indications, not merely of sensation, but of instinct. The tendency of plants to incline their stem and to turn the upper surface of the leaves to the light, the direction which the extreme fibres of the root will often take to reach the best nourishment, the folding up of the flower on the approach of rain, the rising and falling of the Water Lily, and the peculiar and invariable direction assumed by the twining stem in ascending its prop, are among the phenomena that have been attributed to instinct.* I have myself endeavoured to establish the doctrine of the existence and agency of an instinctive principle in the plant, upon the ground of the direction invariably assumed by the radicle and plumelet respectively in the germination of the seed; and to my paper on this subject I must for the present be content to refer my reader.†

SECTION V.

Definition of the Plant.

But if vegetables are living beings endowed with sensation and instinct, or any thing approaching to it, so as to give them a resemblance to animals, how are we certainly to distinguish the plant from the animal? At the extremes of the two kingdoms the distinction is easy; the more perfect animals can never be mistaken for plants, nor the more perfect plants for animals, but at the mean, where the two kingdoms may be supposed to unite, the shades of discrimination are so very faint or evanescent that of some individual productions it is almost impossible to say to which of the kingdoms

* Tupper's Probability of Sensation in Vegetables.

† Lin. Trans. vol. xi. part ii.

they belong. Hence it is that substances which have at one time been classed among plants, have at another time been classed among animals; and there are substances to be met with whose place has not yet been satisfactorily determined. Of these I may exemplify the genus *Corollina*, which Linnæus placed among animals, but which Gærtner places among plants; and between authorities so great who shall attempt to decide? To the unexperienced naturalist perhaps the undertaking may appear easy; but the great diversity of rules which have been devised for the purpose of fixing the limits of the two kingdoms shows but too plainly the difficulty of the task.

Definitions of the earlier botanists. The definitions of the earlier botanists were very inaccurate. One of the ancients defined a plant to be an animal fixed by means of a root. But this definition is good for nothing, for it requires the assistance of at least two others to make it intelligible —one for the term animal, and another for the term root; and if when you come to the term animal you proceed upon the same principle, you must then say that it is a wandering plant that has no root to fix it: so that thus you define your terms in a circle, and explain nothing.

Of Jungius. Jungius, a botanist who flourished about the beginning of the 17th century, defined a plant to be a body possessing vitality, but without sensation, and fixed to a certain spot from which it derives the nourishment necessary to the developement of its parts, and reproduction of the species. This definition is no doubt a great improvement upon the former, but it cannot be said to be by any means correct; for as it has not yet been proved that plants are endowed with sensation, so neither has it been proved that they are totally devoid of it. And it is very well known that all vegetables are not confined to a particular spot, and that such as are so confined do not always derive their nourishment from that spot; many of the aquatics even in their vegetating state are wafted on the surface of the water by means of the winds, or impelled by the action of the waves, and many of the Lichens and Algæ are attached even to the solid rock.

Of Linnæus.

Linnæus, the great reformer of natural history and chief of all botanists, undertook, as well became him, to fix and define the boundaries of the mineral, vegetable, and animal kingdom; his definition is as follows:—"Stones grow; plants grow and live; animals grow, live, and feel." This definition is extremely plausible, and bears upon the face of it the genuine stamp of the bold and masterly manner of Linnæus. But with all due deference to that great and illustrious naturalist, still his definition must be regarded as defective, at least as relative to the distinction between the animal and the plant. For in the first place, as it is not quite certain that some plants do not also feel as well as live, it is liable to the same objection with the definition of Jungius, on which indeed it seems to be founded; and in the next place, as we are possessed of no criterion by which we may infallibly judge of the existence of the faculty of sensation, the difficulty of decision remains the same as before. For if I should happen to meet with an animal which does not exhibit what I might be inclined to regard as a satisfactory evidence of sensation, I must of necessity arrange it in the class of vegetables, while at the same time it still remains an animal.

Of Bonnet.

M. Bonnet, of Geneva, defined the plant to be an organized body nourished by means of roots placed externally; the animal being just the converse—that is, an organized body nourished by means of roots placed internally, namely, the lacteals of the animal system. This definition is sufficiently applicable to the generality of cases, but it fails just where the foregoing definitions have been found to fail—that is, in cases which are really doubtful. And if this criterion is the only true test of distinction between the animal and vegetable, then all animals whatever before they are protruded from the egg or womb are to be regarded as plants; because they are then nourished by means of an umbilicus, which we cannot but regard as an external root.

Of Hedwig.

Dissatisfied with all previous distinctions, and qualified from the depth of his knowledge and extent of his views to mark and select the most decisive characters of discrimination, the acute and indefatigable Hedwig suggested the following rule, founded as he thought on a universal law of vegetable nature, and affording the only incontrovertible test by which the plant is to be discriminated from the animal; namely, that the reproductive organs after having discharged their peculiar functions, uniformly decay and drop off before the fruit has reached maturity, while those of the animal remain permanent, and perish only with the individual itself.* But if it is true, as Gærtner maintains, that some genera, perhaps even some tribes of plants are destitute of sexual organs altogether, and propagated not by seeds but by gems; or if there are either plants or animals whose sexual organs have not yet been detected, as in the case of the Polypi, what after all is the value of the rule?

Of Mirbel. Finally, M. Mirbel, a botanist of some considerable celebrity on the continent, has introduced a criterion founded on the character of the substances on which plants and animals respectively feed. Plants feed upon unorganized substances, that is, upon earths, salts, water, or gases: animals feed upon substances already organized, that is either upon vegetables, or animals, or their products; but never wholly upon substances in an unorganized state. Such is obviously the fact, at least in the case of the more perfect animals and vegetables, which M. Mirbel was accordingly not the first to remark; for the remark had been made, essentially at least, by Lord Bacon, though the division of material substances into organized and unorganized

* Tracts relative to Bot. London, 1805.

was not yet introduced:* but it was made in the very terms of the division by M. Bonnet, in his Considerations sur les Corps Organisés; and by Fourcroy, in his Systême des Conaissances Chimiques,† who regards it as affording the best criterion for distinguishing the mineral from the plant. But M. Mirbel seems to have been the first who has adopted it as affording a universal criterion for distinguishing the plant from the animal, and it seems to have already obtained a preference even with some of the best judges, though it does not yet appear to have been very scrupulously put to the test.‡ Is it true that the *Lumbricus terrestris* feeds only on earth, as has been generally supposed, or does it feed also on roots? What is the food of leeches and minnows, which have been known to live for years merely in pure water? And what is the food of zoophytes in general? Till these questions are satisfactorily answered the criterion cannot be deemed infallible. Regarding it, however, as the best ground of distinction that has hitherto been suggested, I deduce from it the following definition of the vegetable and animal:— A vegetable is an organized and living substance springing from a seed or gem, which it again produces; and effecting the developement of its parts by means of the intro-susception and assimilation of

* Videmus enim herbas et plantas, ex terra et aqua nutriri; animalia vero, ex herbis et fructibus. De Aug. Scien. l. iv.

† Smith's Introduction, chap. i.

‡ Tome vii. p. 34.

unorganized substances, which it derives from the atmosphere or the soil in which it grows. The definition of the animal is the counter-part: an animal is an organized and living being proceeding from an egg or embryo, which it again produces; and effecting the developement of its parts by means of the intro-susception of organized substances, or their products.

If no one of the foregoing rules or definitions is altogether without exception, neither is there any one of them without its utility. They are all founded on some leading feature observable in at least the greater part of the subjects meant to be characterized; and if the naturalist does not succeed in the attainment of his object by means of the adoption of any one rule, he will probably succeed by means of the aid of another; for if all of them should even prove to be defective, they will not all be defective in the same respect; and at the most it is only in a few cases that difficulties are likely to occur, and in which it is to be feared that difficulties will always remain. For if nature has not assigned to the animal and vegetable kingdoms respectively any definite and specific limits, but has blended them as it were both together, it is in vain for man to institute his distinctions. It is extremely desirable, however, that some criterion should be established, as general in its extent and as easy in its application as possible; and for all practical purposes, perhaps plants may be distinguished from animals with sufficient accuracy by means of the trial of burning; as animal substances in a state of ignition exhale a strong and phosphoric odour, which vegetable substances do not.

CHAPTER XII.

CASUALTIES AFFECTING THE LIFE OF VEGETABLES.

As plants are, like animals, organized and living beings, they are, like animals also, liable to such accidental injuries and disorders as may affect the health and vigour, or occasion the death of the individual; which is at any rate eventually effected by means of the natural decay and final extinction of the vital principle. Hence the subject of vegetable casualties divides itself into the three following heads—Wounds, Diseases, Natural Decay.

SECTION I.

Wounds.

A WOUND is a forcible separation of the solid parts of the plant effected by means of some external cause. It may be intentional, as in the case of incision, boring, girdling, grafting, pruning, felling, and such like operations; or it may be accidental, as in the case of injuries sustained by the rubbing or browsing of cattle; by the bite and depredation of insects, Intentional and accidental.

hares, rabbits; by lightning; by weight of fruit; or by violent storms of wind, hail, snow.

SUBSECTION I.

Some-times necessary to the health of the plant.

Incision.—Incisions are sometimes necessary to the health of the tree, in the same manner perhaps as bleeding is necessary to the health of the animal. The trunk of the Plum and Cherry-tree seldom expand freely till a longitudinal incision has been made in the bark; and hence this operation is often practised by gardeners. If the incision affects the epidermis only it heals up without leaving any scar; if it penetrates into the interior of the bark it heals up only by means of leaving a scar; but if it penetrates into the wood, the wound in the wood itself never heals up completely; but new wood and bark are formed above it as before.

SUBSECTION II.

Employed to extract the sap.

Boring.—Boring is an operation by which trees are often wounded for the purpose of making them part with their sap in the season of their bleeding, particularly the Birch-tree and American Maple. A horizontal or rather slanting hole is bored in them with a wimble, so as to penetrate an inch or two into the wood, from this the sap flows copiously; and though a number of holes is often bored in the same trunk, the health of the tree is not materially if at all affected. For trees will continue to thrive though subjected to this operation for many successive years; and the hole, if not very large, will close up again like the deep incision, not by the union of the broken fibres of the wood, but by the formation of new bark and wood projecting beyond the edge of the orifice, and finally shutting it up altogether.

SUBSECTION III.

Girdling.—Girdling is an operation to which trees in North America are often subjected when the farmer wishes to clear his land of timber. It consists in making parallel and horizontal incisions with an axe into the trunk of a tree, and carrying them quite round the stem so as to penetrate through the *alburnum,* and then to scoop out the intervening portion. If this operation is performed early in the spring and before the commencement of the bleeding season, the tree rarely survives it; though some trees that are peculiarly tenacious of life, such as *Acer saccharinum* and *Nyssa integrifolia,* have been known to survive it a considerable length of time.*

Employed to kill the plant.

SUBSECTION IV.

Fractures.—If a tree is bent so as to break part only of the cortical and woody fibres, and the stem or branch but small, the parts will again unite by

* Barton's Elem. of Bot. Part ii.

being put back into their natural position, and well propped up. Especially the cure may be expected to succeed if the fracture happens in the spring; but it will not succeed if the fracture is accompanied with contusion, or if the stem or branch is large; and even where it succeeds the woody fibres do not contribute to the union, but the granular and herbaceous substance only which exudes from between the wood and liber, insinuating itself into all interstices and finally becoming indurated into wood.

SUBSECTION V.

Pruning.—Wounds are necessarily inflicted by the gardener or forester in the pruning or lopping off of superfluous branches, but this is seldom attended with any bad effects to the health of the tree, if done by a skilful practitioner; indeed no further art is required merely for the protection of the tree beyond that of cutting the branch through in a sloping direction so as to prevent the rain from lodging. In this case the wound soon closes up by the induration of the exposed surface of the section, and by the protrusion of a granular substance, forming a sort of circular lip between the wood and bark; and hence the branch is never elongated by the growth of the same vessels that have been cut, but by the protrusion of new buds near the point of section.

SUBSECTION VI.

Grafting.—In this operation there is a wound both of the stock and graft; which are united, as has been shown in a former chapter, not by the immediate adhesion of the surfaces of the two sections, but by means of a granular and herbaceous substance exuding from between the wood and bark, and insinuating itself as a sort of cement into all open spaces: new wood is finally formed within it, and the union is complete.

SUBSECTION VII.

Felling.—Felling is the operation of cutting down trees close to the ground which many of them will yet survive, if the stump is protected from the injuries of animals, and the root fresh and vigorous. In this case the fibres of the wood are never again regenerated, but a lip is formed as in the case of pruning; and buds, that spring up into new shoots, are protruded near the section: so that from the old shoot, ten, twelve, or even twenty new stems may issue according to its size and vigour. The stools of the Oak and Ash-tree will furnish good examples; but there are some trees, such as the Fir, that never send out any shoots after the operation of felling.

SUBSECTION VIII.

Destruction of Buds.—It has been already shown that the buds which expand in the spring are generated in the preceding summer, and augmented and prepared for developement in the intervening winter. But if the buds are destroyed in the course of the winter, or in the early part of the spring, many plants will again generate new buds that will develope their parts as the others would have done, except that they never contain blossom or fruit. By what means are the buds regenerated?

Du Hamel thought they sprang from pre-organized germes which he conceived to be dispersed throughout the whole of the plant. His proofs are founded on the following experiments:—Having taken some cuttings of the Willow, he stuck them in the earth, and made them at the same time pass through a barrel filled with earth, so as to have a portion exposed to the air between the earth and the barrel, and another portion projecting above the barrel. The part inserted in the ground produced roots, and the part passing through the earth contained in the barrel produced also roots, but the other two portions produced branches. It was of little consequence whether they were inserted in the earth by the upper or under extremity; and they vegetated even when made to pass through the barrel horizontally. But a cutting which was stuck into the under surface of the earth contained in the barrel, with the top pointing downwards, did not vegetate. Hence he concluded that germes both of the root and branch are dispersed throughout the whole of the plant, and are developed as the exigency of the case requires.*

Others less prodigal of germes think that the buds are regenerated only from the plexus of the vessels of the inner bark; perhaps, because it is from the inner bark that the union of the graft and stock is effected. But Mr. Knight thinks he has discovered the true source of the regeneration of buds in the proper juice that is lodged in the alburnum. This conjecture is supported by the following facts:

If the stalk of *Crambe maritima* is cut off near the ground in the spring, the pith within that part of the stalk which remains still attached to the root rots, and a cup is formed that collects water in the succeeding winter. The sides of the cup consist of a woody substance which resembles the alburnum of trees, and new buds are often seen in the following spring to be protruded from within the cup.

Buds were also observed to be generated on the sections formed by the knife in separating Potatoes into cuttings, and were in many instances elongated into runners, which gave origin to other tubers. Now the Potatoe, says Mr. Knight, consists of four distinct parts; the epidermis, true skin, bark, and internal mass, which he denominates an alburnum,

* Phys. des Arbres, liv. iv. chap. v.

though it may well be doubted whether this denomination is correct.

Mr. Knight's experiments were now extended to woody plants, a number of which he raised in the spring of 1802, from seeds of the Apple, Pear, and Plum-tree, and cut down in the autumn to the collar, exposing at the same time part of the root. In the beginning of the following spring, a number of small protuberances were observed on the bark of the exposed roots, which were found to be occasioned by small processes issuing from the alburnum. They were incipient buds, and were developed as the spring advanced, forming shoots similar in every respect to those which might have been expected from the stem that was cut down. Experiments that were made upon the stem and root of aged trees gave the same result, establishing, as Mr. Knight thinks, the position that the *alburnum* possesses the power of organizing and regenerating buds.

But this after all is not much different from the doctrine of the pre-organized germes of Du Hamel; and certainly not quite so convenient. For the germes of Du Hamel are always ready against the occurrence of any accident; whereas those of Mr. Knight are manufactured out of the alburnum only after the accident has occurred.

But is it not singular that buds thus regenerated never contain or produce either flower or fruit? Perhaps it is because the fruit bud requires more time to develope its parts, or a peculiar and higher degree of elaboration; and that this hasty production is only the effect of a great effort of the vital principle for the preservation of the individual, and one of those wonderful resources to which nature always knows how to resort when the vital principle is in danger.

SUBSECTION IX.

Destruction of Leaves.—Sometimes the leaves of a tree are destroyed partially or totally as soon as they are protruded from the bud, whether by the depredations of caterpillars or other insects, or by the browsing of cattle. But if the injury is done early in the spring, new leaves will be again protruded with subsequent shoots. This I observed in the case of a small Roan-tree that had been totally stripped of its leaves by the browsing of a cow; but new leaves were soon afterwards produced, as well as new shoots, though the tree had been transplanted both early in that spring, and in the spring preceding. The shoots were but short, and the leaves were protruded from buds not so forward as those that were first developed, and which would, perhaps, not have been developed that season except for this accidental defoliation. Some trees will bear to be stripped even more than once in a season, as is the case with the Mulberry-tree which they cultivate in the south of France and Italy for the purpose of feeding the silk-worm. But if it is stripped more than once in the season it requires now and then a year's rest.

SUBSECTION X.

Destruction of Bark.—The decortication of a tree, or the stripping it of its bark, may be either intentional or accidental, partial or total. If it is partial and affects the epidermis only, then it is again regenerated, as in the case of slight incision, without leaving any scar. But if the epidermis of the petal, leaf, or fruit, is destroyed, it is not again regenerated, nor is the wound healed up except by means of a scar. Such is the case also with all decortications that penetrate deeper than the epidermis, particularly if the wound is not protected from the action of the air. And if the decortication reaches to the wood, then the wound will not heal up in the foregoing manner at all. This Du Hamel proved by means of experiment.* Having stripped a trunk in the spring of a portion of its bark to the extent of a few square inches, he left the decorticated part exposed to the air. In the course of a few days after there appeared issuing from the lip of the wound, as if from between the wood and bark, a ring of new bark, which became broader and more solid during the summer, lessening the area of the original wound. At the end of the summer it was found that a new layer of wood was formed under

* Phys. des Arb. liv. iv. chap. iii.

this bark ; and in the following year a new ring of bark was generated concentric to the former, and also a new layer of wood beneath it; and so on successively, approaching the centre of the wound, till at last the whole area was covered, but without any actual union of the old and new wood. Such then is the process of nature in healing up wounds of this kind when left exposed to the air.

But the result is not the same when the wound is covered from the air. In the season of the flowing of the sap Du Hamel detached a ring of bark, of three or four inches in breadth, from the trunks of several young Elm-trees, taking care to defend the decorticated part from the action of the air, by surrounding it with a tube of glass cemented above and below to the trunk. After a few days the tubes became cloudy within, particularly when it was hot; but when the air became cool, the cloud condensed and fell in drops to the bottom. At last there began to appear as if exuding from between the bark and wood of the upper part of the wound, a sort of rough scurfy substance; and on the surface of the wood, as if exuding from between the longitudinal fibres of the alburnum, a number of gelatinous drops. They were not connected with the scurfy substance at the top, but seemed to arise from small slips of the liber that had not been completely detached. Their first appearance was that of small reddish spots changing by degrees into white, and finally into a sort of grey, and extending in size till they at last united and formed a cicatrice, which was a new bark, not indeed covering the whole wound uniformly, for some parts of it still remained uncovered, and not altogether like the other bark.

Hence, says Du Hamel, it is proved that the wood can produce bark. But the legitimacy of this conclusion is, I think, somewhat questionable. For in the first place the liber was not wholly stripped off, and in the second place the cicatrice was not complete; and in the third place the bark was not perfect.

If the decortication is total, the tree dies. Of sixty trees which Du Hamel barked in the spring, no one survived the experiments above three or four years, though many of them generated a portion both of wood and bark, originating at the summit, and descending sometimes to the extent of a foot.*

SECTION II.

Diseases.

Diseases are corrupt affections of the vegetable body, arising from a vitiated state of its juices, and tending to injure the habitual health either of the whole or part. The diseases that occur the most frequently among vegetables are the following:—Blight, smut, mildew, honey-dew, dropsy, flux of

* Phys. des Arb. liv. v. chap. ii.

juices, gangrene, etiolation, suffocation, contortion, consumption.

SUBSECTION I.

Blight.—Much has been written on the nature of blight; and in proportion as words have been multiplied on the subject, the difficulties attending its elucidation have increased. This disease was well known to the ancient Greeks, who, were however totally ignorant of its cause, regarding it merely as a blast from heaven, indicating the wrath of their offended deities, and utterly incapable of prevention or cure. It was known also to the Romans under the denomination of *rubigo*, who regarded is in the same light as the Greeks, and even believed it to be under the direction of a particular detiy, Rubigus, whom they solemnly invoked that blight might be kept from corn and trees. It is still well known from its effects to every one having the least knowledge of husbandry or gardening; but it has been very differently accounted for. And, perhaps, there is no one cause that will account for all the different cases of blight, or disease going by the name of blight; though they have been supposed to have all the same origin. If we take the term in its most general acceptation I think it will include at least three distinct species—blight originating in cold and frosty winds, blight originating in a sort of sultry and pestilential vapour, and blight originat-

Divisible into three species.

ing in the immoderate propagation of a sort of small and parasitical fungus.

First species.

The first species is often occasioned by the cold and easterly winds of spring, which nip and destroy the tender shoots of the plant, by stopping the current of the juices. The leaves which are thus deprived of their due nourishment wither and fall, and the juices that are now stopped in their passage swell and burst the vessels, and become the food of innumerable little insects that soon after make their appearance. Hence they are often mistaken for the cause of the disease itself; the farmer supposing they are wafted to him on the east wind, while they are only generated in the extravasated juices as forming a proper nidus for their eggs. Their multiplication will no doubt contribute to the spreading of the disorder, as they always breed fast where they find plenty of food.

But a similar disease is often occasioned by the early frost of spring. If the weather is prematurely mild, the blossom is prematurely protruded, which though it is viewed by the unexperienced with delight, yet it is viewed by the judicious with fear. For it very often happens that this premature blossom is totally destroyed by subsequent frosts, as well as both the leaves and shoots, which consequently wither and fall, and injure if they do not actually kill the plant. This evil is also often augmented by the unskilful gardener even in attempting to prevent it—that is, by matting up his trees too closely, or by keeping them covered in the course of the day, and thus rendering the shoots so tender that they can scarcely fail to be destroyed by the next frost.

The second species generally happens in the summer when the grain has attained to its full growth, and when there are no cold winds or frosts to occasion it. Such was the blight that used to damage the vineyards of ancient Italy, and which is yet found to damage our Hop plantations and Wheat crops. The Romans had observed that it generally happened after short but heavy showers occurring about noon, and followed by clear sunshine, about the season of the ripening of the Grapes, and that the middle of the vineyard suffered the most. This corresponds pretty nearly to what is in this country called the fire-blast among Hops, which has been observed to take place most commonly about the end of July, when there has been rain with a hot gleam of sunshine immediately after; the middle of the Hop-ground is also the most affected whether the blight is general or partial, and is almost always the point in which it originates. In a particular case that was minutely observed, the damage happened a little before noon, and the blight ran in a line forming a right angle with the sunbeams at that time of the day. There was but little wind, which was however in the line of the blight.*

Second species.

Wheat is also affected with a similar sort of blight,

* Hale's Body of Husbandry.

and about the same season of the year, which totally destroys the crop. In the summer of 1809, I had watched the progress of the growth of a field of wheat on rather a light and sandy soil, merely from having had occasion to pass through it every Sunday in going to serve a church. It came up with every appearance of health, and also into ear, with a fair prospect of ripening well. I had taken particular notice of it on a Sunday about the beginning of July, as exceeding any thing I should have expected on such a soil. But on the following Sunday I was surprised to find a portion of the crop, on the east side of the field, to the extent of several acres totally destroyed; being shrunk and shrivelled up to less than one-half the size of what it had formerly been, with an appearance so withered and blasted that I for some time imagined I had got into the wrong field. The rest of the field produced a fair crop.

Third species.

The third species attack the leaves or stem both of herbaceous and woody plants, such as *Euphorbia Cyparissias, Berberis vulgaris,* and *Rhamnus catharticus,* but more generally Grasses; and particularly our most useful grains, Wheat, Barley, and Oats. It generally assumes the appearance of a rusty-looking powder that soils the finger when touched. On the 25th of March, 1807, I examined some blades of Wheat that were attacked with this species of blight; the appearance was that of a number of rusty-looking spots or patches dispersed over the surface of the leaf, exactly like that of the seeds of Dorsiferous Ferns bursting their *indusium*. Upon more minute inspection these patches were found to consist of thousands of small globules collected into groups beneath the epidermis, which they raised up in a sort of blister and at last burst. Some of the globules seemed as if imbedded even in the longitudinal vessels of the blade. They were of a yellowish or rusty brown, and somewhat transparent. But these groups of globules have been ascertained by Sir J. Banks to be patches of a minute *Fungus*, the seeds of which, as they float in the air, enter the pores of the epidermis of the leaf, particularly if the plant is sickly; or they exist in the manure or soil, and enter by the pores of the root.* This *Fungus* has been figured by Mr. Sowerby and by Mr. F. Bauer and Grew. It is known among farmers by the name of *Red Rust*, and as it affects the stalk and leaves only it does not materially injure the crop.

But there is another species of *Fungus*, known to the farmer by the name of *Red Gum*, which attacks the ear only, and is extremely prejudicial. In the aggregate it consists of groups of minute globules interspersed with transparent fibres. The globules are filled with a fine powder which explodes when they are put into water. It is very generally accompanied with a maggot of a yellow colour, that preys

* Sir J. Banks on Blight, 1805.

also upon the grain, and increases the amount of injury. It has been well figured by Mr. Bauer.

SUBSECTION II.

Smut.—Smut is a disease incidental to cultivated Corn by which the *farina* of the grain, together with its proper integuments and even part of the husk, is converted into a black soot-like powder. If the injured ear is struck with the finger, the powder will be dispersed like a cloud of black smoke; and if a portion of the powder is wetted by a drop of water and put under the microscope, it will be found to consist of millions of minute and transparent globules, which seem to be composed of a clear and glary fluid encompassed by a thin and skinny membrane.

This disease does not affect the whole body of the crop, but the smutted ears are sometimes very numerously dispersed throughout it. Some have attributed it to the soil in which the grain is sown, and others have attributed it to the seed itself, alleging that smutted seed will produce a smutted crop. But in all this there seems to be a great deal of doubt. Willdenow regards it as originating in a small *Fungus*, which multiplies and extends till it occupies the whole ear.* But Mr. F. Bauer, of Kew, seems to have ascertained it to be merely a

* Princip. of Bot. p. 356.

morbid swelling of the ear, and not at all connected with the growth of a *Fungus*.*

It is said to be effectually prevented by steeping the grain before sowing in a weak solution of arsenic.

But besides the disease called smut there is also a disease analogous to it, or a different stage of the same disease, known to the farmer by the name of Bags or Smut-balls, in which the *nucleus* of the seed only is converted into a black powder, whilst the ovary, as well as the husk, remains sound. The ear is not much altered in its external appearance, and the diseased grain contained in it will even bear the operation of threshing, and consequently mingle with the bulk. But it is always readily detected by the experienced buyer, and fatal to the character of the sample. It is prevented as in the case of smut.

SUBSECTION III.

Mildew.—Mildew is a thin and whitish coating with which the leaves of vegetables are sometimes covered, occasioning their decay and death, and injuring the health of the plant. It is frequently found on the leaves of *Tussilago Farfara, Humulus Lupulus, Corylus Avellana*, and the *white and yellow Dead-nettle.* It is found also on Wheat in the shape of a glutinous exudation, particularly when the days are hot and the nights without dew.

Willdenow says it is occasioned by the growth of

* Smith's Introd. p. 348.

a *Fungus* of great minuteness, the *Mucor Erisyphe* of Linnæus; or by a sort of whitish slime which some species of *aphides* deposit upon the leaves.* In cultivated crops it is said to be prevented by manuring with soot.

SUBSECTION IV.

Honey-dew.—Honey-dew is a sweet and clammy substance which coagulates on the surface of the leaves during hot weather, particularly on the leaves of the Oak-tree and Beech, and is regarded by Mr. Curtis, who wrote a paper on the subject, as being merely the dung of some species of *aphides*.† This seems to be the opinion of Willdenow also,‡ and it is no doubt possible that it may be the case in some instances or species of the disease. But Sir J. E. Smith contends that it is not always so, or that there are more species of honey-dew than one, regarding it particularly as being an exudation, at least in the case of the Beech, whose leaves are, in consequence of an unfavourable wind, apt to become covered with a sweet sort of glutinous coating, similar in flavour to the fluid obtained from the trunk.§

It is certain, however, that saccharine exudations are found on the leaves of many plants, though not

* Princip. of Bot. p. 343. † Lin. Trans. vol. vi.
‡ Princip. of Bot. p. 343. § Introd. p. 189.

always distinguished by the name of honey-dew; which should not perhaps be applied except when the exudation occasions disease. But if it is to be applied to all saccharine exudations whatever, then we must include under the appellation of honey-dew the saccharine exudation observed on the Orange-tree by M. De la Hire,* together with that of the Lime-tree which is more glutinous, and of the Poplar which is more resinous; as also that of the *Cistus creticus*, from which the resin called *Labdanum* is collected, by means of beating the shrub with leathern thongs, and of the manna which exudes from the Ash-tree of Italy and Larch of France. It is also possible that the exudation of excrement constituting honey-dew may occasionally occur without producing disease; for if it should happen to be washed off soon after by rains or heavy dews, then the leaves will not suffer.

SUBSECTION V.

Occasioned by too much moisture.

Dropsy.—Plants are also liable to a disease which affects them in a manner similar to that of the dropsy in animals, arising from long continued rain or too abundant watering. Willdenow describes it as occasioning a preternatural swelling of particular parts, and inducing putrefaction. It is said to take place chiefly in bulbous and tuberous

* Phys. des Arb. tom. i. p. 150.

roots, which are often found much swelled after rain. It affects fruits also which it renders watery and insipid. It prevents the ripening of seeds, and occasions an immoderate production of roots from the stem. Succulent plants in particular are apt to suffer from too profuse waterings, and the disease thus occasioned is generally incurable.* The leaves drop, even though plump and green; and the fruit rots before reaching maturity. In this case the absorption seems to be too great in proportion to the transpiration; but the soil when too much manured produces similar effects. Du Hamel planted some Elms in a soil that was particularly well manured, and accordingly they pushed with great vigour for some time; but at the end of five or six years they all died suddenly. The bark was found to be detached from the wood, and the cavity filled up with a reddish-coloured water.

Incurable.

SUBSECTION VI.

Not always prejudicial.

Flux of Juices.—Some trees, but particularly the Oak and Birch, are liable to a great loss of sap, either bursting out spontaneously, owing to a superabundance of sap, or issuing from accidental wounds. Sometimes it is injurious to the health of the plant, and sometimes not. There is a spontaneous extravasation of the sap of the Vine, known by the name of the Tears of the Vine, which is not injuri-

* Princip. of Bot. p. 249.

ous. As it often happens that the root imbibes sap, which the leaves are not yet prepared to throw off because not yet sufficiently expanded, owing to an inclement season, the sap which is first carried up, being propelled by that which follows, ultimately forces its way through all obstructions, and exudes from the bud. But this is observed only in cold climates; for in hot climates, where the developement of the leaves is not obstructed by cold, they are ready to elaborate the sap as soon as it reaches them. There is also a spontaneous extravasation of proper juice in some trees, which does not seem in general to be injurious to the individual. Thus the gum which exudes from Chery, Plum, Peach, and Almond trees, is seldom detrimental to their health, except when it insinuates itself into the other vessels of the plant and occasions obstructions.

In what cases prejudical.

But when the sap ascends more copiously than it can be carried off, it sometimes occasions a fissure of the solid parts, inducing disease or deformity by encouraging the extravasation and corruption of the ascending or descending juices. Sometimes the fisssure is occasioned by means of frost, forming what is called a double alburnum; that is, first a layer that has been injured by the frost, and then a layer that passes into wood. Sometimes a layer is partially affected, and that is generally owing to a sudden and partial thaw on the south side of the trunk, which may be followed again by a sudden frost. In this case the alburnum is split into clefts, or chinks, by means of the expansion of the frozen sap. But a cleft thus occasioned often degenerates into a chilblain that discharges a blackish and acrid fluid to the great detriment of the plant, particularly if the sore is so situated that rain or snow will readily lodge in it, and become putrid. The same injury may be occasioned by the bite or puncture of insects while the shoot is yet tender; and as no vegetable ulcer heals up of its own accord, the sooner a remedy is applied to it the better, as it will, if left to itself, ultimately corrode and destroy the whole plant, bark, wood, and pith. The only remedy is the excision of the part affected, and the application of a coat of grafting wax.*

SUBSECTION VII.

Dry gangrene.

Gangrene.—Of this disorder there are two varieties, the dry and the wet. The former is occasioned by means of excessive heat or excessive cold. If by means of cold, it attacks the leaves or young shoots, and causes them to shrink up, converting them from green to black; as also the inner bark, which it blackens in the same manner, so that it is impossible to save the plant except by cutting it to the ground. If by means of heat, the effects are nearly similar, as may oftentimes be seen in gardens, or even in forests, where the foresters are allowed to clear away the Moss and withered leaves from the roots.†

* Willdenow, p. 354. † Ibid. p. 355.

Sometimes the disease is occasioned by the too rapid growth of a particular branch, depriving the one that is next it of its due nourishment, and hence inducing its decay. Sometimes it is occasioned by means of parasitical plants, as in the case of the bulbs of the Saffron, which a species of *Lycoperdon* often attaches itself to and totally corrupts. The harmattan winds of the coast of Africa kill many plants, by means of inducing a sort of gangrene that withers and blackens the leaves, and finally destroys the whole plant. The Nopal of Mexico is also subject to a sort of gangrene that begins with a black spot, and extends till the whole leaf or branch rots off or the plant dies.

Wet gangrene. But plants are sometimes affected with a gangrene by which a part becomes first soft and moist, and then dissolves into foul ichor. This is confined chiefly to the leaves, flowers, and fruit. Sometimes it attacks the roots also, but rarely the stem. It seems to be owing in many cases to too wet or too rich a soil; but it may originate in contusion, and may be caught by infection.

Dissolution of Thiery. But the Nopal is subject also to a disease called by Thiery *la dissolution,* and considered by Sir J. E. Smith as distinct from gangrene. I cannot however perceive the difference; I think it is Willdenow's dry gangrene. A joint of the Nopal, or a whole branch, and sometimes an entire plant, changes in the space of a single hour from a state of apparent health to a state of putrefaction or dissolution.

Now its surface is verdant and shining, and in an itstant it changes to a yellow, and its brillancy is gone. If the substance is cut into, the parts are found to have lost all cohesion, and are quite rotten; the only remedy is speedy amputation below the diseased part. Sometimes the vital principle collecting and exerting all its energies, makes a stand as it were against the encroaching disease, and throws off the infected part.*

SUBSECTION VIII.

Occasioned by want of light.

Etiolation.—Plants are sometimes affected by a disease which entirely destroys their verdure, and renders them pale and sickly. This is called *etiolation*, and may arise merely from want of the agency of light, by which the extrication of oxygene is effected, and the leaf rendered green. And hence it is that plants placed in dark rooms, or between great masses of stone, or in the clefts of rocks, or under the shade of other trees, look always peculiarly pale. But if they are removed from such situations and exposed to the action of light, they will again recover their green colour.

Rationale.

Etiolation may also ensue from the depredation of insects, nestling in the radicle, and consuming the food of the plant, and thus debilitating the vessels of the leaf so as to render them insusceptible to the action of light. This is said to be often

* Smith's Introduction, p. 340.

the case with the radicles of *Secale cereale*, and the same result may also arise from poverty of soil.

SUBSECTION IX.

Suffocation.—Sometimes it happens that the pores of the epidermis are closed up and transpiration consequently obstructed, by means of some extraneous substance that attaches itself to and covers the bark. This obstruction induces disease, and the disease is called suffocation. Sometimes it is occasioned by the immoderate growth of *Lichens* upon the bark covering the whole of the plant, as may be often seen in fruit trees, which it is necessary to keep clean by means of scraping off the *Lichens*, at least from the smaller branches. For if the young branches are thus coated, so as that the bark can not perform its proper functions, the tree will soon begin to languish, and will finally become covered with *Fungi* inducing or resulting from decay, till it is at last wholly choaked up.

But a similar effect is also occasionally produced by insects, in feeding upon the sap or shoot. This may be exemplified in the case of the aphides which sometimes breed or settle upon the tender shoot in such multitudes as to cover it from the action of the external air altogether. It may be exemplified also in the case of *Coccus Hesperidum* and *Acarus tellarius*, insects that infest hot-house plants, the latter by spinning a fine and delicate web over the leaf, and thus preventing the access of atmospheric air.*

Examples. Sometimes the disease is occasioned by an extravasation of juices which coagulate on the surface of the stalk so as to form a sort of crust, investing it as a sheath, and preventing its further expansion. On the 7th of July, 1816, I observed some stalks of a Grass partly enveloped with a crust, not unlike a piece of dried orange-peel, particularly when viewed through the microscope; the part thus enveloped proved to be that in which the spike was yet contained within its sheathing leaves. The crust which thus totally locked up and suffocated the spike extended from about 1½ to two inches in length, surmounted by the terminating leaf whose base it also invested, and thus giving to the Grass the appearance of a *Typha* in miniature.

On examining the crust more minutely it seemed to consist of thousands of yellowish globules imbedded in a sort of ground resembling mortar. But in some species the crust was much paler, and not unlike the *Boletus Medullapanis* in a recent state. It not only invested the outer leaf, but also the inner leaf though sheathed by the outer, and the spike though sheathed by the inner leaf. The ear was so totally consumed or so imperfectly formed that I could not yet ascertain what Grass it was. But it had the habit of *Holcus lanatus*, which, by

* Willdenow, p. 350.

finding in one specimen a part of the ear uninjured, I afterwards ascertained it to be.

If this crust is not originally occasioned by the puncture of insects, it is at least selected as affording a fit nidus for depositing their eggs. For in looking at some specimens about a week after, I found several in which the surface of the crust was disfigured with a sort of protuberant blister, which when opened up was found to contain a maggot. And even in unsheathing an ear which was thus locked up and apparently inaccessible to insects, I yet found a small black fly rummaging about in it.

Sometimes the disease is occasioned from want of an adequate supply of nourishment as derived from the soil, in which the lower part of the plant is the best supplied, while the upper part of it is starved. Hence the top shoots decrease in size every succeeding year, because a sufficient supply of sap cannot be obtained to give them their proper developement This is analogous to the phenomena of animal life when the action of the heart is too feeble to propel the blood through the whole of the system. For then the extremities are always the first to suffer. And perhaps it may account also for the fact, that in bad soils and unfavourable seasons, when the ear of Barley is not wholly perfected, yet a few of the lower grains are always completely developed ;* which not only shows the

* Smith's Introduction, p. 344.

great care of Providence for the preservation of the species, but points out also the efficient cause.

SUBSECTION X.

Caused by the puncture of insects,

Contortion.—The leaves of plants are often injured by means of the puncture of insects, so as to induce a sort of disease that discovers itself in the contortion or convolution of the margin, or wrinkled appearance of the surface.

As occuring in the leaves of the Peach and Nectarine,

The leaves of the Apricot, Peach, and Nectarine, are extremely liable to be thus affected in the months of June and July. The leaf that has been punctured soon begins to assume a rough and wrinkled figure, and a reddish and scrophulous appearance, particularly on the upper surface. The margins roll inwards on the under side, and enclose the eggs which are scattered irregularly on the surface, giving it a blackish and granular appearance, but without materially injuring its health.

Of the Vine,

In the Vine the substance deposited on the leaf is whitish, giving the under surface a sort of a frosted appearance, but not occasioning the red and scrophulous aspect of the upper surface of the leaf of the Nectarine.

Poplar,

In the Poplar the eggs when first deposited resemble a number of small and hoary vescicles containing a sort of clear and colourless fluid. The leaf then becomes reflected and conduplicate, enclosing the eggs with a few reddish protuberances on the upper surface. The embryo is nourished by this fluid; and the hoariness is converted into a fine cottony down, which for some time envelopes the young fly.

The leaf of the Lime-tree in particular is liable to attacks from insects when fully expanded; and hence the gnawed appearance it so often exhibits. The injury seems to be occasioned by some species of puceron depositing its eggs in the parenchyma, generally about the angles that branch off from the midrib. A sort of down is produced, at first green, and afterwards hoary; sometimes in patches, and sometimes pervading the whole leaf, as in the case of the Vine. Under this covering the egg is hatched; and then the young insect gnaws and injures the leaf, leaving a hole, or scar of a burnt or singed appearance. And Lime-tree.

Sometimes the upper surface of the leaf is covered with clusters of wart-like substances somewhat subulate and acute. They seem to be occasioned by means of a puncture made on the under surface, on which a number of openings are discoverable, penetrating into the warts which are hollow and villous within.

SUBSECTION XI.

Consumption.—From barren or improper soil, unfavourable climate, careless planting, or too fre- Causes of.

quent flowering exhausting the strength of the plant, it often happens that disease is induced which terminates in a gradual decline and wasting away of the plant till at length it is wholly dried up. Sometimes it is also occasioned by excessive drought, or by dust lodging on the leaves, or by fumes issuing from manufactories which may happen to be situated in the neighbourhood; or by the attacks of insects.

Teredo Pinorum.

There is a consumptive affection that frequently attacks the Pine-tree called *Teredo Pinorum,** which affects the alburnum and inner bark chiefly, and seems to proceed from long continued drought, or from frost suddenly succeeding mild or warm weather or heavy winds. The leaves assume a tinge of yellow bordering upon red. A great number of small drops of resin exude from the middle of the boughs of a putrid odour. The bark exfoliates, and the alburnum presents a livid appearance. The tree swarms with insects, and the disease is incurable, inducing inevitably the total decay and death of the individual.

* Willdenow, Princ. Bot. p. 351.

SECTION III.

Natural Decay.

IN the preceding section I have stated the chief of the diseases to which plants are liable, whether from external injuries, or from internal derangement. But although a plant should not suffer from the influence of accidental injury, or from disease, still there will come a time when its several organs will begin to experience the approaches of a natural decline insensibly stealing upon it, and at last inducing death. For in the vegetable as well as in the animal kingdom there is a term or limit set, beyond which the individual cannot pass, though the duration of vegetable existence is very different in different species. Inducing death.

Some plants are annuals and last for one season only, springing up suddenly from seed, attaining rapidly to maturity, producing and again sowing their seeds, and afterwards immediately perishing. Such is the character of the various species of Corn, as exemplified in Oats, Wheat, and Barley. Some plants continue to live for a period of two years, and are therefore called biennials, springing up the first year from seed, and producing root and leaves, but no fruit; and in the second year producing both flower and fruit, as exemplified in the Carrot, Parsnip, and Caraway. Other plants are perennials, that is, lasting for many years; of Whether in the case of annuals, biennials, or perennials.

which some are called under-shrubs, and die down to the root every year; others are called shrubs, and are permanent both by the root and stem, but do not attain to a great height or great age; others are called trees, and are not only permanent by both root and stem, but attain to a great size and live to a great age. The Oak-tree in particular is remarkable both for its longevity and size, being at least 100 years before it attains to its utmost perfection, continuing vigorous for perhaps 100 years more; and then beginning to decay. The immense Oak at Colethorpe, near Wetherby, is said to have exhibited symptoms of decay even in the reign of Queen Elizabeth.

But even of plants that are woody and perennial, there are parts which perish annually, or which are, at least annually separated from the individual; namely, the leaves, flowers, and fruit, leaving nothing behind but the bare caudex which submits in its turn to the ravages of time, and ultimately to death. Hence the ground of a division of the subject exhibiting, first, the phenomena of the decay of the temporary organs, and secondly, the phenomena of the decay of the permanent organs, and consequent death of the plant.

SUBSECTION I.

Decay of the temporary Organs.—The decay of the temporary organs which takes place annually is a phenomenon familiar to every body, and comprehends the fall of the leaf, the fall of the flower, and the fall of the fruit.

ARTICLE 1. *The Fall of the Leaf.*—The fall of the leaf, or annual defoliation of the plant, commences for the most part with the colds of autumn, and is accelerated by the frosts of winter, that strip the forest of its foliage, and the landscape of its verdure. But there are some trees that retain their leaves throughout the whole of the winter, though changed to a dull and dusky brown, as those of the Beech-tree; and there are others that retain them even in verdure till the succeeding spring, when they ultimately fall. Such plants are denominated Evergreens.

It was at one time indeed a vulgar error, and perhaps it continues to be so still, that Evergreens never shed their leaves at all. This error may be traced back even to the period of the fabulous history of the Greeks, with whose mythology it was closely interwoven, at least in one particular example as related by Theophrastus; who says that in the country of Cortynia, in Crete, it was reported there was a Plane-tree growing by a fountain which never shed its leaves, being the tree under the shade of which Jupiter was said to have had his interview with Europa.*

* Εν Κρήτη δὲ, λεγεται, πλάτανόν τινᾶ εἶναι εν τῆ γορτυναια προς κρηνη τινὶ, ἡ ου φυλλοβόλεῖ μυθολογουσι, δε ὡς επι ταυτη ἐμιγη τη Ευρωπη ὁ Ζευς. Περι φυτων. το. Α.

But Theophrastus was himself acquainted with the fact of the fall of the leaves of Evergreens, as every accurate observer of nature must be, though they do not actually fall till the young leaves have begun to appear, so that trees of this sort are never left wholly without leaves, which it was hence supposed they never shed. In warm climates it is said that many plants retain their leaves for several years; but in temperate and polar climates there are no such plants to be found.

Such is the fact of the annual fall of the leaves. What is the cause of their fall? The solution of this question seems to have totally baffled the attempts of phytologists, and to have been a puzzle that no one could make out. Du Hamel, one of the most sagacious and industrious of all phytologists, laboured hard to explicate the phenomenon, but without success. He observed that leaves which fall the soonest transpire the most, and are consequently the soonest exhausted and rendered unfit for the discharge of their functions; so that the period of the fall of the leaves of different species is probably in proportion to their capacity for transpiration. Their fall is accelerated by frost, or by excessive heat, followed by rain. It is also accelerated, if not actually induced, by the structure of the pedicle which is very different from that of the branch, having no prolongation of pith, and nothing analogous in its mode of insertion, nor in its external figure, which is divisible into an upper and under surface resembling the figure of the leaf. He compares the union of the leaf and stem to that of the joints of the Vine-twig, which at a certain period of its growth are stronger than the internodia, but which readily give way after a frost. The comparison, however, throws but little light on the subject, as the illustration is itself to the full as dark as the thing to be illustrated. But he offers an additional conjecture which is considerably more luminous; when the sap begins to flow less plentifully, the leaves, to whose vigour a great supply is necessary, soon become dry and consequently less fit to convey it. But it is known that the branches grow in thickness after they have ceased to grow in length, which must necessarily occasion, in some degree, a disruption of the fibres of the footstalk and stem, or branch, at the point of articulation; and hence the leaf loses its hold, and falls.* This is certainly a very plausible conjecture; though it may be doubted whether the explication will apply to the case of Evergreens, or of plants in warm climates, that retain their leaves for several years. It is not therefore, altogether satisfactory; and hence other explications have accordingly been offered.

The first of these explications of which I shall now take any notice is that of Willdenow; it is as follows:—As the sap is conveyed to the leaves in greater abundance during the summer, the vessels

* Phys. des Arbres, liv. ii. chap. ii.

of the petiole become gradually more woody, as well as the whole of the leaf. The sap by consequence stagnates, and at last the bond of union between the leaf and stem is dried up, and cracks. The wound that the stem thus receives cicatrizes before the petiole separates; and the petiole separates at last in consequence of the interrupted connexion between the leaf and stem which the crack has occasioned.*

This, it must be confessed, does not make up for the deficiencies of the hypothesis of Du Hamel; for in the first place there is no proof that the bond of union between the leaf and stem cracks in the manner here supposed. And even upon the supposition of its being the fact, it is, in the second place, extremely improbable that the petiole should after the cracking of this bond of union still continue attached to the stem, till the wound thus occasioned has cicatrized; because when the original bond of union cracks there remains no other bond of union by which the petiole is to retain its hold.

Another explication is that of Vorlick, as quoted by Willdenow; the leaf which possesses a peculiar vitality within itself, though dependant upon the vitality of the plant, and generally of shorter duration, dies when it reaches maturity; and the plant, being able to exist for a time without leaves, throws off the dead leaf as the animal throws off the dead part from the sound part.

* Princip. of Bot. p. 305.

But the peculiar vitality which the leaf is here supposed to possess seems to me to be altogether a groundless assumption, and an unphilosophical multiplication of causes without any apparent necessity. Is it not rather the individual vitality of the plant extended to a perishable organ, and again withdrawn when that organ has discharged its destined functions, or become by disease or decay unfit for the purposes of vegetation? This, I presume, is a better founded supposition than the foregoing; though the reference to the phenomenon of the throwing off of the dead part from the sound part in the animal subject is sufficiently well adapted to the purposes of illustration; and the analogy sufficiently striking, at least under some of its aspects, to warrant its introduction. For which, or for similar reasons, our learned president Sir J. E. Smith gives his sanction to the opinion of Vorlick, which he had himself indeed been previously led to adopt, though he was anticipated in the publication. The notion was first suggested to him by some remarks of Mr. Fairbairn, of Chelsea, who had observed that in the transplanting of trees, if the injury extends suddenly beyond the leaf, then the leaf remains firmly attached to the twig even though dead; but when the leaves alone are affected, and the vital energy acting with full force in the branch, the leaves are thrown off or fall upon the slightest touch. Hence Sir J. E. Smith concludes that leaves are thrown off by a process similar to that of the sloughing of diseased parts in the animal economy.

It does not, however, seem quite evident to me that the idea of sloughing is comprehended in the opinion of Vorlick, at least as represented by Willdenow; but if so, I do not think that the analogy is very well made out. Sloughing, in the animal economy, is that power or the exertion of that power by which the vital principle is capable of throwing off a part that has accidentally become diseased and unfit for discharging the functions to which it was originally destined; but not that power by which it is capable of throwing off a distinct organ intended by nature to be finally separated from the individual. Now in the case of the defoliation of the plant, there is, for the most part no disease, but merely a gradual and natural decay which reduces the leaf to a state, indeed, no longer fit for the purposes of vegetation, but to which it was intended by nature to be reduced for the purpose of facilitating its separation from the plant: and hence it always separates in a determinate manner, and at a determinate point, namely, at the base of the foot-stalk, which forms as it were a sort of natural joint or articulation, to which there is nothing analogous in the case of sloughing. If this were not the fact, it might be expected that a part of a leaf, or even the whole of it, should occasionally become permanent, as well as the branches, though no such thing has ever yet happened.

And in the sloughing of the diseased part there is yet another circumstance clashing with the analogy that is here instituted. The part supplying the place of the slough, or throwing it off, is formed or exists already formed immediately beneath it, and is precisely of the same character with what the slough originally was; which slough it pushes off as it comes itself to maturity, or acquires strength sufficient for the effort. But the leaves fall off when they have reached maturity of their own accord, without being at all pushed off by the new ones, which are yet merely in embryo, and do not even occupy the place of the old leaves, but are only formed contiguous to them, except in the case of the Plane-tree, the new leaf of which is formed precisely under the base of the foot-stalk of the old leaf: and yet I would not call the fall of that leaf sloughing, because the new leaf does not after all push off the old one; and because there is here, as in other cases, the same natural articulation uniting the leaf to the branch or stem, and rendering it a distinct organ that is ultimately and spontaneously to detach itself from the plant. Not that there exists no example whatever of vegetable sloughing, which the same tree will also furnish in the annual or rather continual exfoliation of its bark, but that the fall of the leaf does not seem to me to afford that example.

I can foresee an objection that may be urged against the above argument from the fact of the sloughing of the entire skin of the snake, and other species of serpents, which may be regarded as a distinct organ. But although the skin of the snake or of any other animal may be regarded as a distinct organ, yet it must be in a light very different from that of an organ attached to the body of a plant or animal by a natural joint or articulation that comes asunder of its own accord; for the skin of the animal in question is forced off in the manner of a slough merely by means of the formation of a new skin beneath it, which has already taken the place of the old skin in the living system, and to which it has just been shown that there exists nothing whatever analogous in the fall of the leaf. So that, after all, the best reason we can give is, perhaps, that the leaves fall in consequence of their being worn out, and no longer necessary to the immediate process of vegetation; which is evidently divisible into animal stages commencing with the approach of spring, and terminating with the return of winter, which is to the vital principle, apparently, a period of rest.

If it is necessary, however, to attempt an explication of the process by which the leaf is made ultimately to detach itself from the plant, it may be observed that it consists wholly in the change that is effected in the articulation uniting the foot-stalk to the branch, as is evident from the remarks of Mr. Fairburn; for in the case in which the injury extends suddenly beyond the leaf, the leaf may wither and decay, but will not fall off, because the articulation has not been duly prepared, and because the vital principle can now no longer act upon it from the intervention of the dead or diseased portion of the plant beyond which it has withdrawn itself. But in the natural process of vegetation the necessary change is effected by the leaf on the one hand, in its yielding to the influence of physical or chemical agencies, and withering and shrinking into narrower compass, when the usual supply of sap is no longer transmitted to it; and by the vital principle on the other, in its controlling and directing of chemical agencies so as to facilitate the final detachment of the foot-stalk, and form the scar necessary to its own protection. And this effect is operated by the converting of the substance that cements the respective fibres of the leaf-stalk and branch together from a soft and glutinous to a dry and brittle consistence, analogous to the change that takes place in the seams of the valves of ripening capsules or pericarps, so that the leaf falls at last merely by force of its own weight, or of the slightest breath of wind, but without the intervention of any previous chink or crack.

And if it is necessary to illustrate the fall of the leaf by any analogous process in the animal economy, it may be compared to that of the shedding of the antlers of the stag, or of the hair or feathers of other beasts or birds, which being like the leaves of plants, distinct and peculiar organs, fall off and are regenerated annually, but do not slough.

ART. 2. *The Fall of the Flower.*—The flowers, which, like the leaves, are only temporary organs, are for the most part very short-lived; for as the object of their production is merely that of effecting the impregnation of the germe, that object is no sooner obtained than they begin again to give indications of decay, and speedily fall from the plant; so that the most beautiful part of the vegetable is also the most transient. The flower of the Night-blowing Cereus, the most magnificent of all flowers, no sooner expands than it begins to decay, and before the sun has risen upon it its beauty is gone. The flowers of the Poppy and Tulip, though very gaudy, are very short-lived; and the beautiful blossom of our fruit-trees soon begins to fade. The scene often continues blooming indeed, both in the landscape of nature and of art, but that is more owing to the succession of blossoms on the same or on different plants, than to the permanency of individual blossoms. And so also of the flowers that adorn the field or meadow; they spring up in perpetual succession, but are individually of very short duration.

Art. 3. *The Fall of the Fruit.*—The fruit, which begins to appear conspicuous when the flower falls, expands and increases in volume, and, assuming a peculiar hue as it ripens, ultimately detaches itself from the parent plant and drops into the soil. But it does not in all cases detach itself in the same manner: thus in the Bean and Pea the seed-vessel opens and lets the seeds fall out; while in the Apple, Pear, and Cherry, the fruit falls entire, enclosing the seed, which escapes when the pericarp decays. Most fruits fall soon after ripening, as the Cherry and Apricot, if not gathered; but some remain long attached to the parent plant after being fully ripe, as in the case of the fruit of *Cratægus* and *Evonymus*, which may be seen in the hedges in the midst of winter, and of *Mespilus*, which continues till the succeeding spring. But these, though tenacious of their hold, detach themselves at last, as well as all others, and bury themselves in the soil, about to give birth to a new individual in the germination of the seed.

The fall of the flower and fruit is accounted for in the same manner as that of the leaf.

SUBSECTION II.

Decay of the Permanent Organs.—Such then is the process and presumptive rationale of the decay and detachment of the temporary organs of the plant. But there is also a period beyond which even the permanent organs themselves can no longer carry on the process of vegetation. Plants are affected by the infirmities of old age as well as animals, and are found to exhibit also similar symptoms of approaching dissolution. The root refuses to imbibe the nourishment afforded by the soil, or if it does imbibe a portion, it is but feebly propelled, and partially distributed, through the tubes of the alburnum; the elaboration of the sap is now effected with difficulty, as well as the assimilation of the proper juice, the descent of which is almost totally obstructed; the bark becomes thick and woody, and covered with Moss or Lichens; the shoot becomes stunted and diminutive; and the fruits palpably degenerate, both in quantity and quality. The smaller or terminal branches fade and decay the first, and then the larger branches also, together with the trunk and root; the vital principle gradually declines without any chance of recovery, and is at last totally extinguished; while the solid mass of the plant exposed to the chemical action of surrounding substances, to which it now yields, withers and dies away, presenting to the eye a decayed and rotten appearance, and crumbling into dust from which it originally sprang. Such is the transient duration of the vegetable, and counter-part of animal life.

END OF VOL. II.

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