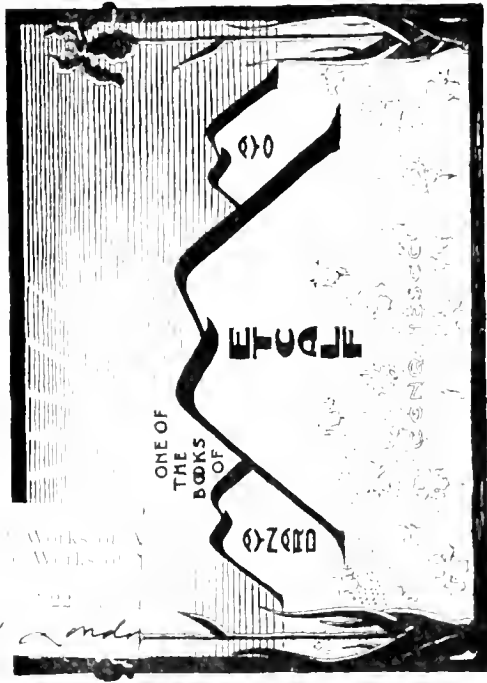
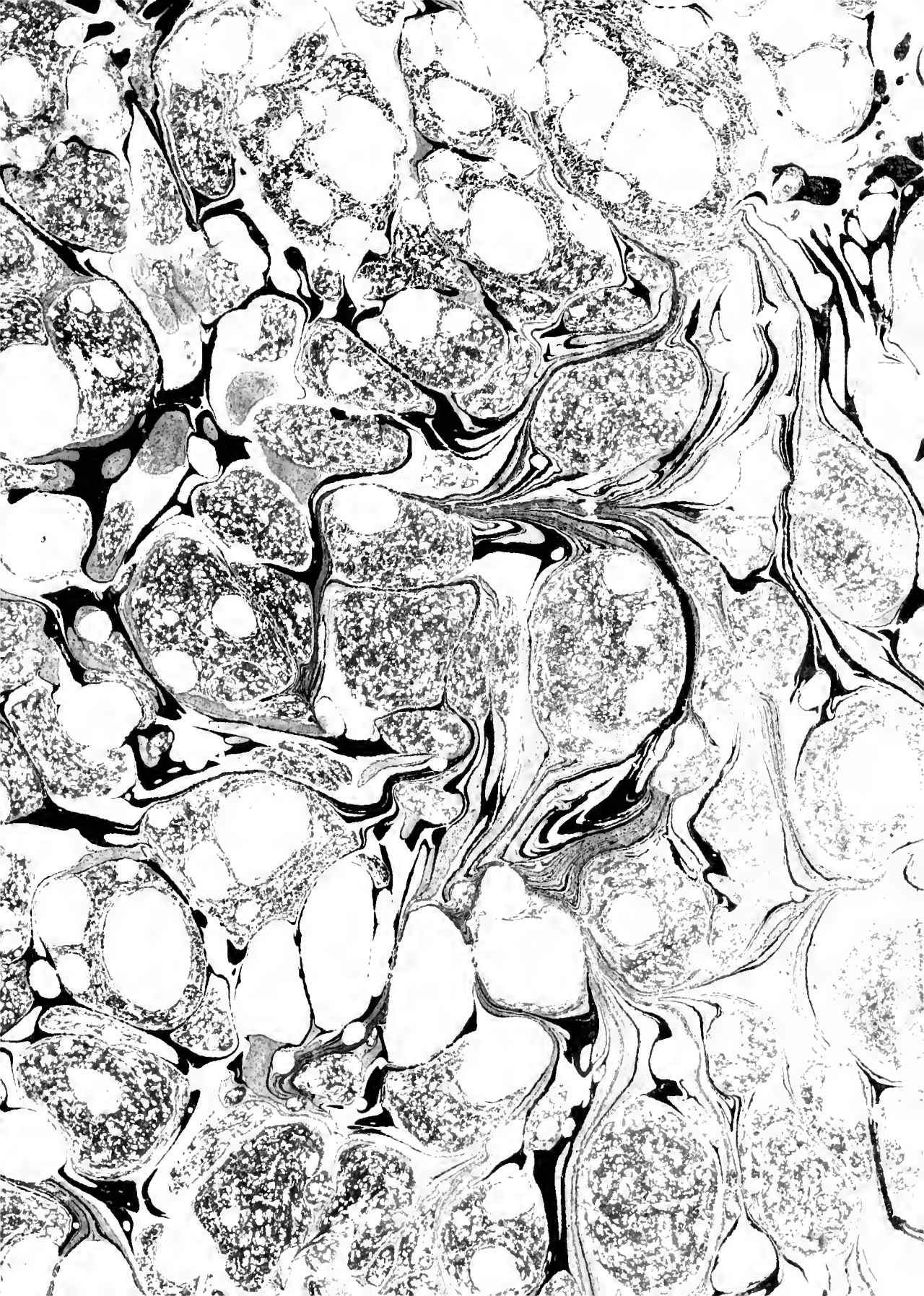




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THE
S E L E C T W O R K S

OF

ANTONY VAN LEEUWENHOEK,

CONTAINING HIS

MICROSCOPICAL DISCOVERIES

IN MANY OF THE WORKS OF NATURE.

TRANSLATED FROM THE DUTCH AND LATIN EDITIONS PUBLISHED BY THE AUTHOR,

BY SAMUEL HOOLE.

VOLUME THE SECOND. PART THE THIRD.

Hy sprack oock van de boomen, van den Ceder-boom aen die op den Libanon is, tot op den Blos die aen den wandt uyt wast: hy sprack oock van het Uee, ende van het Gevogelte, ende van de kruppemde Dieren, ende van de Afschen.

I. Koningen, iv. 33.

And he spake of trees, from the cedar-tree that is in Lebanon, even unto the hyssop that springeth out of the wall: he spake also of beasts and of fowl, and of creeping things, and of fishes.

I. Kings, iv. 33.

L O N D O N :

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M,DCCC,VII.

On the formation of the Elm, Beech, Willow, Alder, Ebony, Box, and Lime-tree, with an explanation of the manner in which Pipe-staves for making Casks are prepared from Oak-timber.

THE formation of the Elm is represented in Plate XI. *fig.* 1. A B C D, and the size of the piece of wood here magnified, when viewed by the naked eye, is shewn at letter E; this figure also contains as much as the tree encreased in size in the space of one year; A B and C D, denote the places where, in autumn, the growth ceased.

The small perpendicular or ascending vessels which lie in great numbers intermixed among the large ones, are less in this wood than in the Oak, and moreover, each of these small tubes or vessels is composed of tougher and thicker membranes than those in the Oak.

At A B and C D are shewn the horizontal vessels lying lengthwise.

Fig. 2. F F F F, exhibits the horizontal vessels cut transversely; such of them as appear collected in small parcels, I take to be those which are beginning to be formed from the perpendicular vessels; and, that those which are collected together in great numbers, are the produce of several years growth, and contain as great a number as ever would be formed in the future growth of the tree.

G G G G, shew the very small perpendicular vessels lengthwise. H H represents one of the larger perpendicular vessels cut lengthwise down the middle. Upon a more accurate examination of these, we shall perceive them to be composed of exceedingly thin membranes, covered with filaments twisted in a serpentine form, and having the appearance of dark spots and tubercles or risings, as at *fig.* 3. B.

Among this species of tree, we often see some, from which, at the

thick part of the stem the rising sap leaks or oozes out, and in this liquor I have often, in the summer time, observed various animalcules, but which animalcules I could by no means conceive to have issued from the wood; but rather, that the first rudiments of them had been deposited either by the rain or dew.

At *fig. 4*, A B C D, is pictured a small particle of Beech, to the naked eye appearing of the size represented at F; the length of this figure also shews the thickness acquired by the tree in one year's growth, the beginning and ceasing of which growth plainly appear at the letters A D or B C.

In this wood are two sorts of perpendicular vessels, large and small ones; and, I am inclined to think, that there are also two sorts of horizontal ones, very minute, one sort of which appears at E E E, lying in small numbers together, and when cut transversely they are shewn in *fig. 5*, at the letters H H H.

The other sort lie in detached parcels, and are shewn lengthwise at D C; these vessels are also very small in comparison with the perpendicular ones, and are composed of large clusters collected together, a representation of them when cut transversely may be seen in *fig. 5*, at I I.

K K K K, are the large perpendicular vessels cut longitudinally; and these I have almost always observed to be covered with particles which, viewed by a common magnifier, exhibited the appearance of globules.

Fig. 6, A B C D, is a small piece of Willow, to the naked eye appearing of the size shewn at F; this wood consists of two sorts of perpendicular vessels, small and large; the large ones are covered with particles bearing the appearance of globules, and in these I observed certain oblique streaks, which I long ago concluded to be valves*:

* The author's opinion respecting the probable use of valves, in the vessels of trees may be seen in his *Essay on the Oak*, vol. I. p. 1

one of these is shewn in *fig. 7*, where the perpendicular vessels are exhibited lengthwise, and it may be seen in the vessel marked G G. The small perpendicular vessels are composed of excessively thin membranes, and these it was not in my power to represent truly in a drawing.

In this wood I only observed one sort of horizontal vessels, which are shewn lengthwise in *fig. 6*, at E E E; the number of these is very small, compared with the horizontal vessels I have observed in other woods.

Fig. 7, H H, represents them cut transversely, and intermixed with the perpendicular vessels, which in the same figure appear lengthwise.

In one of the large perpendicular vessels represented in *fig. 7*, at G G, I have shewn the appearance they exhibit of being covered with globules, but those excessively minute.

Fig. 8, is a particle of Alder, about the thickness, when viewed with the naked eye, of an hog's bristle: this wood has two sorts of perpendicular vessels; the smaller ones consist of excessively thin membranes, the larger of membranes covered with particles of wonderful minuteness, and to which no other figure can be assigned than that of globules.

A B or C D denote the length of the tree's increase in the space of one year; E E E E are the horizontal vessels.

Fig. 9, F F F F, are the large perpendicular vessels cut longitudinally; H H are the small perpendicular vessels: G G, are the horizontal vessels, which here are seen cut transversely.

Fig. 10, A B C D E F, is a small particle of Ebony, the growth of the island Mauritius: this figure is drawn from a microscope, magnifying objects much more than those from which the drawings of the other woods were made, because the vessels in this wood could not be conveniently discovered by a common magnifier. The

particle of wood here shewn, in which are to be seen about eleven hundred perpendicular vessels, is not larger than can be covered by a common grain of sand. I did, at first, design to have made a drawing of a larger portion of this wood, with intent to shew the decrease of the growth in autumn, and its renewal in the spring; but on reflection I found that such an attempt would be fruitless, forasmuch as this wood grew in a climate where vegetation never ceases; the island Mauritius being situated a few degrees to the northward of the Tropick of Capricorn.

In this wood there are four sorts of perpendicular vessels; G G G are the largest of them; some of which seem to have had in them a kind of fluid substance, which in drying coagulated in several places, as may be seen in *fig.* 11, at K K, where one of these largest vessels is shewn cut longitudinally.

Fig. 12, is also one of those larger vessels (drawn from a microscope of still greater magnifying power), much more transparent than the former one, and likewise with inconceivably minute particles.

The second sort of perpendicular vessels, which generally are situated between the horizontal ones, are shewn lengthwise at A B C and H H; these, in many places, seem partly filled with a black substance.

The third sort of perpendicular vessels lie in the same direction with the circumference of the tree, and are represented at B E or C D.

The fourth sort of perpendicular vessels, which are shaped somewhat like a lozenge, and are placed among the larger perpendicular vessels, are composed of a much more solid wood or substance than the vessels in the other woods; for the small round cavity which appears in the face of each vessel, cut transversely, as appears in the figure, is the only aperture in each vessel, and the remainder of such vessel surrounding this cavity is solid wood, constituting the substance of the vessel; and this fourth sort of vessels are so closely

compacted and conjoined together, that they seem to be one single piece of wood; as if we were to figure in imagination a number of small holes, bored in the most exact order and proportion, in a piece of the hardest wood.

Fig. 11, II, are the horizontal vessels cut transversely: two of these may be seen lengthwise, in *fig. 10*, at the letters A B C and H H. *Fig. 11, LL*, are the smaller perpendicular vessels cut longitudinally.

Fig. 13, A B C D, is a very small piece of Box-wood, drawn from the same microscope as the piece of Ebony before-described; and, although I examined this wood very attentively, I could not perceive in it any appearance of alteration in its texture, as respecting or denoting the different seasons of the year in which it was produced; for which reason I made a drawing of no more than one minute particle, here represented. I found this wood to consist of two sorts of perpendicular or ascending vessels, namely, a larger sort and a smaller, intermixed with the larger.

The larger vessels are composed of fine membranes, full of particles wonderfully minute, which are represented in *fig. 14*, where the larger vessels are shewn, cut longitudinally, as denoted by the letters E E E E.

The cavities of the small vessels are very much like to those in the small vessels of the Ebony-wood, and these, when cut longitudinally, are shewn in *fig. 14*, at F F.

A B, or C D, are the horizontal vessels, as they appear viewed lengthwise, and cut transversely, they are shewn in *fig. 14*, at G G.

I have often examined the Lime-tree, as well that which is brought to us from Norway to be used in carving wooden figures, as that which grows in this country; and, in order to shew the wonderful make of this wood, I have caused a drawing to be made

of a small piece, as seen through the microscope; this is shewn at *fig.* 15, H I K L.

All the parts of which this wood consists, seem to me, to be formed with a cavity in each of them; and, if we reflect on the minuteness of the perpendicular vessels and tubes which are to be seen in this small piece of wood, we cannot but contemplate them with admiration, as I myself have often done; for the real size of this piece of wood, from K to L, is not more than five hairs breadth.

At H I, and also at K L, are shewn the horizontal vessels in the Lime-tree. I have observed that, in some places, they lie much closer together than here represented.

In *fig.* 16, A B C D, are shewn some of those vessels and woody tubes of the Lime-tree, which, in the former figure, appear cut transversely, and are here shewn longitudinally.

TO THE READER.

Among the different species of wood described by Mr. Leeuwenhoek, he makes no farther mention of the Ash than by laying down the same criterion to judge of its goodness as of the Oak. His words are as follows:

“The Ash timber brought from Norway, which is very slow in its growth, is of a very spongy texture, and consequently of a weak and perishable nature, and by no means to be compared to the Ash which grows in these parts, and in a good soil; by reason that this wood, as well as the Oak, at the first of its growth, every year forms very large vessels, and the rest of the year much smaller ones, consequently the quicker the growth, the fewer of the larger, and the more of the smaller vessels will be formed within the same space. I have examined many Norway Ash-trees, and have observed that, in the space of twenty, thirty, nay, forty years, they had not acquired an inch in their semidiameter; whereas those growing in the neighbourhood of our town would encrease an inch in their semidiameter for many years successively; and I consider

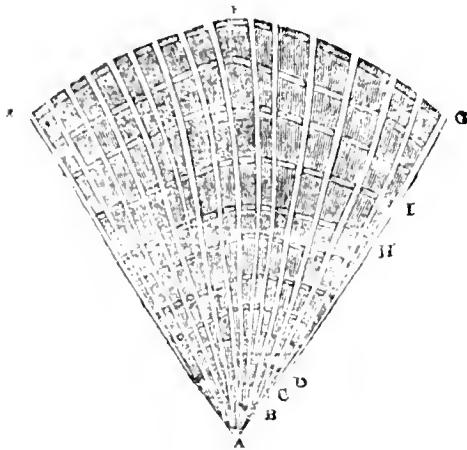
Ash-timber, growing in warm climates, to be, beyond comparison, preferable to such as is produced in cold regions."

The Translator in resuming this part of the subject, observed a passage in the Essay on the Oak, which had somehow escaped him; he therefore takes leave to introduce it here.

As many persons cannot conceive how casks made of Oak can contain liquors, when they consider that the wood is composed of nothing but numbers of small tubes united, I cannot refrain from giving an explanation of the manner in which Oak is prepared, and worked up in the making of tubs or casks, which is as follows: The tree is split or cleft lengthwise into two, four, or six pieces in proportion to its size, but so that the splitting or cleaving may extend from the circumference to the centre of the tree; though when a tree is cleft in two, as I have seen performed in much of the Riga timber, pieces are again cleft from the flat sides of each half, making a sort of four square staves; likewise, when large trees have been thus cleft down the middle, planks or staves are made from them, by cleaving pieces from that side which was the middle of the tree; and these pieces will be four square and not triangular; by all these methods of splitting or cleaving the wood, the perpendicular, and also the horizontal vessels will be divided lengthwise, or according to the same direction in which they were placed in the tree, and they will all lie lengthwise in the plank or staff, the perpendicular vessels from end to end, and the horizontal ones from side to side, by which means the liquor in a cask or vat, made of timber thus prepared, must press against the sides of the vessels or tubes of the wood; and this is the reason why casks made of Oak, though it is formed entirely of tubes, do yet retain the liquor;—to illustrate this by an example:

Let A E F G, represent a piece of Oak timber, split or cleft lengthwise into six parts, one end of which is sawed transversely, and planed or smoothed, as represented in the figure. Now if this wood be cleft in such manner that A E, A F, or A B C D H I G is made one side of the stave, it follows that all the perpendicular vessels, and also the horizontal ones which extend from the centre A to the circumference E F, and here are denoted by sixteen double lines, must be separated from each other longitudinally, whereas, on the contrary, if the wood was to be cleft from E towards G, then all the horizontal vessels would be cut transversely, and thus lying through the stave with one of their orifices opening in the cavity of the catk, and the other on the outside, the liquor would leak out. And this is the reason, why if entire trees are sawed into staves, all those, except what are sawed out of the middle of the tree, will suffer the water to pass through them, especially if the pressure of the water be very great.

And considering this, we are not to wonder that new built ships are subject to leaks, for if the horizontal vessels have their orifices both on the inside and outside of the planks, and these are not stopped by some kind of pitching or dressing, the water must leak through.



Various observations on the Herring.

I HAVE often reflected on the following circumstance attending river fish, namely, that where they have a plentiful supply of food, their intestines are always covered with fat; and on the contrary I have observed, that many sea fish, such as Cod-fish, Haddocks, Plaice, Flounders and the like, though they are plump and bulky, never have any fat on their intestines: Herrings, however, do not come under this description, for, not only their bowels are very fat, but their whole bodies are so much so, that sometimes when they are cut, the fat or oil follows the knife, especially at that time when the roes begin to swell, at which time they are called in this country *Maatgens-Haringen*.

After much turning this matter in my thoughts, I had a fancy to know what is the food of this fish, and for that purpose I enquired of many men, used to that fishery, what food they generally found in the stomachs of Herrings when first caught, but the constant answer I got from them was, that they never found any. At length I met with a merchant who fits out ships for the Herring fishery, and from him I learned, that in a certain tract of sea near the coast of Scotland, Herrings are caught, in the stomachs of which are found some kind of small fishes, but that those Herrings will not keep long.

Not content with this, I determined to wait for the season, when certain Herrings are brought to our town, which, as I have heard, are caught in great numbers not far from Amsterdam, in that bay or sea, which, among us, is called by the name of the *Zuyder Zee*.*

* The *Zuyder Zee* or *Zudder Zee*, a small Mediterranean sea between North Holland and Friesland, whence probably its name of Zuyder, or Southern sea, being so situated in respect of North Holland. It is very shallow, and by Sir William Temple in his remarks on the United Provinces, supposed to have been formed by some great inundation in the middle age, or about the ninth century.

These Herrings being brought to our town for sale about the middle of March, I dissected six of them, and I found in that substance which I took out of one of their stomachs, three living worms, all of the same shape, and somewhat thicker than an hair; but, about four hairs breadth distance from their heads, their bodies, which in that part were round, were about four times thicker.

This Herring had been dead at least twenty-four hours before I examined it, and perhaps these worms might have lived a long time, if they had remained in the chyle and within the stomach.

Moreover, in these worms or animalcules, I saw a vein, bent in a serpentine form, and of a blackish colour, lying the whole length of the animal, and near this vein certain small round particles driven to and fro with great swiftness, affording me a very pleasant spectacle.

Another of the Herrings had nothing except a certain red matter in its stomach, which, upon viewing it by the microscope, I thought I saw to consist of certain round particles, which were almost ground down by the action of the stomach; and I saw that they had been composed of many vessels: this was also the appearance of the white chyle which I took out of the stomachs of all these Herrings.

Seeing these things, I did not wonder that fishermen should imagine Herrings have no food in their stomachs, because Herrings do, in my opinion, feed on such small fishes, that they cannot take in sufficient quantities of them to distend their stomachs, as we see in other fish; and hence it is said, that Herrings have no food within their stomachs.

Now, since we find so small a portion of food in the stomachs of Herrings, and yet that they are so fat at the time, when, as I said before, their roes are yet very small, we must conclude that Herrings are continually feeding, and that their food consists of such minute fishes as escape the eyes of the fishermen, whereas other fishes are accustomed to cram their stomachs with larger fishes, and to such a degree, that sometimes they are distended to a fifth or sixth part of the fish's own bulk; and therefore it is, that the fishes

they so swallow, remain in their stomachs several days, until their bones are by the process of digestion so comminuted, as to be fit for admission into the intestines.

Since then we see, that Herrings have so little food at a time in their stomachs and intestines, and yet become so very fat, which I think is owing to their taking abundance of nourishment, we must conclude, that there are more animalcules or minute fishes in the sea, than has ever yet been thought of: and hence we are not to wonder that Herrings are sometimes caught in one, and sometimes in another part of the sea; sometimes in the shallows, and sometimes in the deep water, according to the places where the small fishes on which they feed do from time to time resort; so that, here I think, the words of scripture may be applied “Wheresoever the carcase is, there will the Eagles be gathered together.”

About the end of March, some more of the same sort of Herrings were brought to our town for sale; several of these I opened, and saw that their livers were as large in proportion to their size as those in other fish, which livers were also provided with a gall-bladder; these Herrings were in that state, that many of them were destitute of roes. In examining the chyle which I took out, both from their stomachs and intestines, I perceived no difference, except by observing in some of them a few round particles, which I deemed to be eggs of fish; and of this I was the more assured when, upon breaking the membrane in which these round particles were enclosed, and which was very tough, I saw the contents to be of a fluid nature, mixed with larger and smaller globules. Several of these round particles I separated, as well as I could, from the chyle, and placed beside them some single grains, or eggs, which I had taken out of the hard roes of other Herrings, and then I clearly found that those globules which I had taken out of the stomach and bowels, were Herring's eggs; whence, I concluded, that those Herrings which, about this season, are sometimes caught in the Zuyder Zee, in such quantities, that they are more than sufficient for consumption, will, for want of other food, devour their own eggs or roes.

In the next place, I took out of the cavity of the belly of two Herrings a white worm, about as thick as a horse-hair, whose body, at each end, especially near the tail, was very thin and pointed. This species of worm I have found in the stomach and bowels of several kinds of fish. These worms, I concluded, had forced their way through the stomach or bowels, and thus had got into the cavity of the belly, because their bodies were of a very hard texture.

The circumstance, that these worms should penetrate, through the intestines into the cavity of the belly, may seem wonderful to many persons, who cannot conceive how a wound can be made by a worm in any intestine, and yet the chyle not run out of that wound into the cavity of the belly. But it should be considered that the head of this worm (which is very sharp), piercing through the intestine, would cause little or no injury to the part, because neither the head or body make much of a cut or wound, and, therefore, the vessels in its passage would only be stretched asunder, and, immediately after it had passed, close again.

A gentleman of my acquaintance, hearing me argue in this manner, told me, that he kept, in an enclosed place at his house, a parcel of hens, in order to supply himself with eggs, and that his maid servants were accustomed to throw the sand and sweepings of the chambers into this place, without taking the trouble to pick out such pins as might be in them, and that the hens, which were kept thus shut up, finding no small stones or pebbles to swallow, as it is in their nature to do, will pick up the pins. These fowls are generally killed in the winter-time; and, my friend told me that, upon cutting them up, himself and all his family, not excepting his men and maid servants, had seen several pins sticking in their breasts; and that, also, a pin was found which had come out from one of their bodies through the breast-bone; and that another pin was found in the bottom of the breast, with its point downwards, sticking out of the skin, so that only the upper part of the pin remained between

the skin and flesh, and two pins stuck to the flesh in the middle of the stomach, which had penetrated through its coat; and, within the stomach, were many pins mixed with the food, which, probably, if the fowls had remained alive, would have found a passage out in some part or other. This being the case, it is no wonder that a worm should find a passage, not only into the cavity of the stomach, but into other parts of the body.

On the 14th of April more Herrings were brought to market; when I observed many eggs scattered among them, so that I concluded for certain, that, at this time, most Herrings deposite their eggs or roes.

Again, I took several Herrings, and, from the appearance of their intestines, I judged they had been longer out of the water than those I formerly examined; yet, in the intestines of one of these, I saw a living worm.

Moreover, I took, out of the bowel of a male Herring, the soft roe of which was grown to its full size, several Herring's eggs, which were in that part of the gut near its orifice; from whence it again appeared to me, that the shell or membrane of these eggs is so tough and hard, that it cannot be digested or dissolved, either in the stomach or intestines.

I also found, in this intestine, many particles not of animal, but vegetable substances, and, among them, a hollow piece of a rush, or some such vegetable; in which I could very plainly perceive, the pores or tubes of which it was composed, and also some exceedingly small flat particles, also consisting of tubes, joined together. I likewise found several thin oblong particles, the formation of which could not accurately be distinguished, they being impervious to the light; and, among other things, I saw the shell of a sea-snail, not larger than a grain of sand, whose circumference was divided into eight parts or joints. The shell of this snail was so very thin, that when the moisture it contained was evaporated, it entirely lost its handsome shape. And thus it was manifest to me,

that Herrings not only feed on small fishes, and even on their own eggs, but that, when urged by hunger, they will swallow any thing they meet with.

Since then we see, that the smaller fish, and especially Herrings, which take so little food at a time, that it is the vulgar opinion, because their stomachs are not distended like those of other fish, that they live entirely without food, do yet become very fat; when, on the contrary, cod and other fish, which sometimes have, at one time, as many as six shell-fish in their stomachs, have, nevertheless, no particles of fat, either between the fleshy parts, or in the intestines, except the liver: What, I ask, is the conclusion to be drawn from hence? Surely (with submission to better judgment), no other than this: that those kind of fish have no veins or vessels adapted to secrete the fat from the food which is digested in the stomach or intestines, and much less any vessels to secrete the fat between the fleshy parts.

But since cod, haddocks, and other fish of the like kind, are destitute of vessels to separate the particles of fat, and yet are always able to get a large supply of food, such a quantity of nourishment is conveyed to the fleshy parts of their bodies, that they become so hard and dry (I mean at the time when their roes first begin to swell, which is in the summer), that they are quite insipid to the palate, and, therefore are said to be out of season, whereas, it should more properly be said, that they are then too much fed, or have taken too much nourishment; but, when the roes in these fish begin to increase, so that in a few weeks, they must be deposited, so much of the nourishment passes into the roes that the hardness of the fleshy parts is diminished, and then those fish again become tasteful to the palate, and are, therefore, said to be in season.

Now, as the larger fish are fed and nourished by devouring the smaller ones, I have often thought, whether minute cod-fishes, or other fish, when first come forth from the eggs, may not feed on those exceeding minute fishes, or animalcules, which are found in

incredible quantities in all waters, and are so minute, that millions of them are scarcely equal to the size of a grain of sand; and, when we consider that the young fry of perch, trout, jack, and the like, when only a few days or weeks old, are always observed swimming slowly against the stream, we may conclude, that it is with intent the better to catch those very minute fishes, or animalcules, which are carried down the stream in the contrary direction, and to feed on them, till they are of sufficient growth to prey on larger fish; and, if we do but observe the very large eyes of those small fry, much bigger in proportion than other parts of their bodies, we may form a tolerable conjecture, that in the first creation, they were appointed to have such large eyes, for enabling them the better to discover and catch those minute fishes which serve them for prey.

I have formerly laid it down as a fact established*, at least in my opinion, that fishes living in rivers or very deep waters, or such waters as are always in motion, are not obnoxious to any decay or disease, and never die of old age; and now, to apply that idea to the subject under consideration. I have examined the scales of Herrings, and I uniformly drew this conclusion, that all the Herrings caught by our fishermen were, as far as I could observe, barely a year old; in confirmation of which opinion, upon inspecting Herrings caught about the beginning of the month of June (which were procured privately, or as it were by stealth, because, by our laws, Herrings are not then permitted to be taken), they being sometimes sent to me as presents, I always found them to be of a much smaller size than those caught a month or two later; and I have thought that perhaps all Herrings caught in the North Sea issued from the roes or spawn which had been deposited the preceding year. Now, if we lay it down as a certain fact, that all the kinds of fish to us known when they first begin to lay their eggs or spawn, are not a year old, and that the greater part of those Herrings, which are found on these coasts in any one year, deposite the eggs from whence issue all

* See vol. I. p. 71.

the Herrings caught in the succeeding year, (for if it were not so, there would certainly be found in the shoal some Herrings much larger than others, as we may observe in all kinds of fish which have scales); we may from thence draw this conclusion, that when Herrings have deposited their spawn, they do immediately, or in a very short time afterwards, quit these seas, and never return thither.

About twenty years ago, I heard a story of an uncommon large Herring, which was to be seen in our town, and, for the novelty of the thing, was given a present to a company of great personages at a public dinner. This Herring, I think, very probably had wandered from the shoal, or had by some means been detained in our sea.

If then, we observe that in the sea adjoining to us, no Herrings are found which are more than a year old, and that they then entirely quit our coasts, and none of them are caught the following year; who can assign any other reason for this, than that Herrings, when grown large, require more food for their growth and sustenance than they can get in our seas, and therefore they migrate, or resort to those places where they find a greater or more solid supply of food.

To conclude, the secrets of Nature, respecting fishes in the sea, as well in regard to their resorting to certain places, as in their departure from thence, cannot be investigated with any precision: for instance, what shall we say of the fish called the Shad, (in Dutch *Elft*), for those fish, when the time of their propagation is at hand, are found in our rivers; but when that time is past quit the rivers, nor are ever afterwards, as far as I have been able to learn, caught in the sea.



OF THE ANT*.

IN a Treatise, lately published in Germany, on Microscopical subjects, the author has given drawings of the Ant and its egg, in the latter of which, he has represented the young Ant within the egg, though not more than an eighth part the size of a full grown Ant: but as these figures appeared to me, to be very inaccurately taken, I determined to enter upon an investigation of the subject myself.

Upon digging up an Ant's nest, in my garden, and examining the eggs (as they are commonly called), I found them not only to be of different sizes, but also some of the young Ants within them, were of a white colour, and lay, as it were, motionless, until they arrived at their full growth. Hereupon I began to form very different ideas from the opinions hitherto entertained, respecting these creatures; and, I concluded, that the Ant, as well as the Weevil and other minute animals (in these cold regions), does, in the winter season, lie without motion, and does not take any nourishment; and that the collections of food which Ants are observed to make, and to heap together in their nests, during the summer season, is for no other purpose than to feed their young.

Their sentiments may appear new and strange to many; but how can we conceive the egg of any creature to be larger than the hind part or belly of the animal from which it proceeds, and,

* This Essay was written in the year 1687. As the Author herein combats some generally received opinions, respecting the Ant, the Translator has given his own words, as nearly as possible, and in the order in which they stand, that the Reader may form his own opinion upon the subject.

that in time, it can become as large as the whole body of the parent, before the young one comes forth, and that, when come to this growth, it shall not afterwards increase in size? This, however, if these are the real eggs, we must conceive of the Ant; but, whether the same takes place in any other creature I know not, nor have ever observed. In all probability these eggs, as they are called, when first laid, are very small, and continually increase in size; the conclusion is, that they must constantly receive some kind of nourishment to cause such increase; whence we may infer, that the collection of food which Ants, in the summer time carry to their nests, is, principally, to feed their young. This, my opinion, I doubt not will be greatly controverted, because the young Ants lie inclosed in a kind of skin or membrane, and, consequently, it is difficult to conceive how they can receive nourishment from the father or mother, when thus completely separated from them. And yet it is still more difficult to conceive, how an animal in the egg, and also the egg itself, can daily grow larger, without receiving any nourishment. I did indeed, at first, imagine that the young Ant was completely formed in this egg; but at length I rejected that opinion, determining to examine the truth of this matter, and, if possible, to discover by what means these eggs of the Ant do increase in size.

And, recollecting, that I could not be at a loss for Ants' nests in my garden, having been formerly much infested by them, I took a spade full of earth, mixed with Ants and their eggs, which I put on a clean sheet of paper, and sat myself down before it, to examine the Ants carefully, who are accustomed, when their nests are disturbed, to carry away their eggs. Many of these I took from them, and observed, that what most of them were carrying was no other than a young Ant, quite white, and without motion; though in others of these Ants the white colour was turned to a red. Others were white, and of an oblong shape; and these I took to be what are usually called the Ants' eggs. These last were smaller and smaller, and some of them no bigger than a common grain

of sand: but, at first, I had bestowed my attention only on those particles which I had taken from the Ants, conceiving them to be of use to me in my enquiry.

To this end I had brought with me several new glasses, in which I collected some of each of the before mentioned sorts, as far as I was able, and, as I had only my spectacles with me, but which magnified objects considerably, I examined, after the Ants had almost all run away, the earth (which consisted of clay and sand mixed together), and found in it several very small white particles, which, I concluded, had been left there by the Ants, mixed with some grains of sand.

Upon my return home, I examined all with the microscope, and found, that those which approached nearest to the likeness of Ants, had not assumed their perfect figure, though of the full size of an Ant; and the only difference consisted in this, that they were entirely white, and without any motion, and their claws and horns lay disposed in regular order, in like manner as we observe in caterpillars, previous to their change into flying insects, though these, of which I am now speaking, were not inclosed in any membrane. These are, doubtless, what are generally called Ants' eggs.

Others of these white particles, which were also of the size of an Ant, and approached nearer the shape of an egg (of which sort I found many), I perceived to be short thick maggots, having in their bodies a black spot, which, probably, most persons would conclude to be the young Ant within. Of these I carried many about with me in my pocket, and observed, that before their change approached, they purged off this black matter, which was their excrements. I saw some of them, within twenty four hours, and others within forty eight hours, put off their skin, and change into a white crystal.

Others of these particles, or eggs, were smaller and smaller, till some of them I found less than a grain of sand; and, among the smallest of the particles which I had taken from the Ants, or found

among the earth of their nest, I discovered several eggs so minute, that I could not, with my naked eye, discern what they were.

One of these eggs I placed before the microscope, and caused a drawing to be made of it, which is shewn in Plate XII. *fig.* 1. A B C. And, to give an idea of the minuteness of this egg, I measured it by a rule divided into aliquot parts; and, I must say, that ninety of the diameters, breadth or thickness of this egg, would not amount to an inch in length.

Another egg I placed before the microscope, in which the maggot was so far grown, that with its head, it advanced beyond the shell of the egg. This is to be seen at *fig.* 2. D E F; in which figure, E F denotes what had been a part of the yolk of the egg, but was now transparent, and only filled with air.

The next day after I had turned up this Ant's nest, as before mentioned, it rained very hard, and the day following, it was very fair weather; whereupon I went to visit the place, and found two holes newly made in the earth, and the Ants running in and out. I took up a spade full of the earth where these holes were made, and found in it many eggs of the same minuteness I have just mentioned: these I put into different glasses, some of which I carried in my pocket: and, in some of them, after the space of twenty four hours, I perceived the maggot to be perfectly formed in the egg; but, in the greater part of them, I found the maggot imperfect, being dead, and the egg dried, with the shell contracted. I caused a drawing to be made, from the microscope, of one of these last named eggs, because, in it, the limner could distinguish all the parts of the maggot's body. This is shewn at *fig.* 3. G H I.

I also placed before the microscope one of the maggots, which, at the same time, had crept out of the egg, and caused a drawing to be made of it as it then appeared. At *fig.* 4. K L M, is the head, and M N K, the body of this maggot.

Upon attentively observing this maggot, I was much gratified with

the sight of an incessant motion appearing within its head (and, if I may be allowed the expression, in its brain), for this motion was as regular as we can imagine, just as if we were to see the motion of the lungs of any animal in respiration.

Sometimes I saw one of these minute maggots open its mouth, as if in quest of food, at the same time producing a minute globule of air at its mouth, which, passing through the head, went into its throat.

Moreover, I caused one of these maggots to be drawn when so far grown, that its body was the tenth part of one full grown, and which is represented at *fig. 5, O P Q R*; in which figure, P Q is the head, and the rest the body. These maggots, when young, lie in a curved posture, but, when almost full grown, they assume a straight figure. This maggot was drawn from a microscope, magnifying objects much less than that from which the former figures were taken; from whence the minuteness of these eggs may very easily be conceived. I could have given a drawing of a full grown maggot from the microscope, but I think it unnecessary, because, from the last figure, we can easily form a judgment of a maggot, of the same shape, when ten times larger.

I must confess, that I formerly thought no otherwise than that those maggots to which, hitherto (for want of knowing better, we have given the name of Ants' eggs), as seen by the naked eye, were, in reality, the eggs of the Ant, and that the black spot which we see in their bodies was the minute young Ant, not yet brought into life. But I cannot sufficiently wonder, how any one can suffer his imagination so to deceive him, as to fancy, when using a microscope, that, through it, he sees in this egg (as we call it) a young unformed Ant. And not only so, but to make a drawing representing its body, head, claws, and eyes; when, on the contrary, no person whatever can see the least appearance of any such in this maggot which, hitherto, has been called the egg.

I have seen some of these maggots, while very small, to have the

dark spot before mentioned, and others much larger, and even perfectly formed, not to have the least appearance of it. And, to investigate the true nature of this dark spot, which some are so simple as to imagine is a young Ant, I opened the maggot, and saw most plainly that it was the animal's stomach, and that the darkish appearance was caused by the food in the stomach; and, with regard to those maggots which have not such a spot, this is owing to the different kind of food brought to it by the parents. In this dissection I saw not only the stomach, but the intestines adjoining to it, which were filled with globular particles of a darkish colour.

How is it possible for us to see in a maggot (which we call an egg) a very minute young creature? for I have before said, that the whole and entire maggot is transformed into an Ant, saving only its skin which is left behind; but this skin or cuticle is so thin, that it does not amount to an hundredth part of the maggot's substance.

This maggot has on its body, except the belly, many hairs, and is uncommonly sluggish in its motions, so that it is seldom seen even to stretch itself out, or to contract its body, but it often moves its head, and sometimes, though not often, opens its mouth. It is, in a word, entirely unable to seek its food, and, as I may say, lies immoveable in its nest, without ever changing its place, and therefore it is necessary that the parent Ants, or some others of the tribe, should continually provide food and bring it to the young ones.

This being the case, the commonly received opinion must be done away, respecting the industry of Ants in carrying provisions to their nests for the purpose only of laying up a stock of food for winter; whereas on the contrary, the greatest part, and who can tell whether or not the whole, is laid up for the maggots which proceed from their eggs (and who cannot possibly feed themselves), and to provide them with nourishment until they are of sufficient growth to put on the shape of Ants.

And, in like manner as I have said, that the Weevil does not lay many eggs at a time, like the Silkworm's moth, and the common

fly, so it is with the Ant, which is a long lived animal, and during the whole summer continues to lay its eggs (at least in my opinion), and if so, they must be continually employed in providing food for their young. And this is an innate impulse or instinct implanted in them by Nature, for otherwise their young (which are maggots) must be starved for want of food.

I am aware that what I have advanced, and these my observations will be disputed by many, and the rather, as so much has been written of the industry and foresight of Ants in laying up store for the winter, but this I do not at all regard. I freely speak my sentiments as they occur to me; and I think it is not at all improbable that Ants throughout the winter, and especially if the cold be very severe, can live without nourishment, lying motionless under ground as other small animals do in the open air, such as the flea, the weevil and others. And if we do but observe the Ant, when in autumn they devour our grapes, and take notice, if it be cold, how slowly they move, we may very well conclude that, when the cold increases they may be entirely inactive during the middle of winter.

Some persons would have us believe that Ants bite the human species, and that their bite is venomous: but this is what I could never observe; and it seems to me very plain, that the mouth of an Ant is not made for any such purpose, partly because its pincers, which are provided with points or teeth, and are situated in the fore part of the head at the opening of the mouth, cannot exert sufficient force to penetrate through the external skin of our bodies, and make any impression on the sensitive cuticle; for the pincers are too short, and the teeth in them too many in number, for so small an animal to have the power of reaching with them the sensitive cuticle; and in the next place, if we do but inspect all the parts of an Ant's mouth, when in motion, we shall see that they are not of a nature calculated to do hurt to any animal.

I have often seen an Ant produce out of its mouth four oblong particles; but they are provided with small joints like those in the

horns of the Ant, so that the pincers or teeth of this animal seem designed for no other use than to grind its food, and also to drag the provisions it collects for its young to the place where the young maggots lie. But I always observed, and without any one instance to the contrary, that these Ants have a sting which is placed, not in the head, but in the hind part of their bodies, and, if at any time, we molest or hurt the Ant, it immediately puts forth its sting (which at other times it keeps within its body), and continually endeavours to strike, for it does nothing but bend its tail downwards, and incessantly moves its sting in and out of its body.

This motion of the sting, is so natural to the Ant, that though the hind part of its body be cut off from the rest, the sting will continue to move for some time. And the Ant not only gives us pain merely by stinging, but with the sting it emits a transparent liquid which is transmitted to the end of the sting; and, from this liquid, I conceive the principal part of the pain proceeds, owing to the sharp saline particles it contains, and that from hence also proceed those humours or swellings upon the skin which we experience on being stung.

The swellings or pimples which I experienced from the Ants, in my preceding observations of their manner of generation, gave me more pain than in my life before I had experienced from them. For it is almost twenty years ago that, having been violently stung by Ants, I then, upon examination, discovered that they carried a sting in the hind parts of their bodies.

Although this liquid emitted by the Ant, is in very small quantity, I tried all means I could devise to discover, if possible, the kind of particles it might contain, but I could observe nothing, except that this liquor (though very transparent and fluid), consisted of parts so fixed that it would not evaporate; in like manner as if it had been an oil: in one place I fancied I saw some particles lying, of the nature of salts, with some others, so minute, that I could not ascribe to them any particular shape.

The sting of the Ant has not any aperture at its extremity, through which this noxious liquid is emitted (as I have heretofore said is the case with the Scorpion), but it is provided with a cavity, or groove, similar to what we see in those instruments used by sailors to wet their sails, in order to make them hold the more wind. Through this groove, or channel, the Ant is able to emit the liquid, and that as far as the extremity of the sting.

Upon my taking off this liquid from the sting, I found that the animal's supply was not exhausted; for, when it drew in the sting, a fresh quantity of liquid came again forth with it, and this, not once, but several different times. This gave me reason to guess the cause of an uncommonly large tumour I once had, in my arm, from the stinging of an Ant, which was, that the Ant, having crept up my shirt sleeve on the inside, and there begun to sting; before I could strip off my other clothes, as well as my shirt, it had stung me, several times, successively, from whence a large quantity of that noxious liquid had been injected, causing a larger swelling, and a greater degree of pain.

Fig. 6, B C D, represents the sting of the Ant. *A B D E* denote part of the animal's tail, which is covered with hairs.

What I have here noted is, however, only to be understood of the red coloured Ants, and these are the only sort of them which I ever saw in my garden; but, in other gardens, I observed another species of Ants, of a blackish colour, and somewhat less in size than the red Ants, and not nearly of so hardy a make; for I could easily compress or squeeze the hinder parts of these Ants, which part of the body, in the red ones, was very hard. And, whereas I have said, that I always found the former Ants provided with a sting, I could not discover any sting in these last mentioned ones, although I opened more than twenty five of them.

As to the maggots in the nests of these black Ants, they did not differ from those produced by the red Ants, except in being somewhat smaller than the red, when full grown. And, wherea-

the first mentioned maggots changed into the form of a crysalis (of which I saw great numbers, lying among the maggots), I could not, among these last maggots, discern a single crysalis; but I saw, among them, a great number of what are called Ants' eggs, which, at one end, had a black spot. A parcel of these I brought home with me; and, upon opening them, I found, in some of them, a perfect Ant, of a colour inclining to black, in others of them, an Ant quite perfect, but white.

The shell, or covering of this Ant, did not appear to me, to be either a membrane, or the skin of the maggot, which it had stripped off in undergoing its change, for it was quite smooth, and without any wrinkle. And, as I could not imagine how this covering was formed about the Ant, I put some of the most perfect maggots into a clean glass; and, when they had been in it for two days, I found them to differ much from the first mentioned ones. For these latter ones had begun to spin, as silk worms do, and, when the thread was about as long as two hairs breadth, they fastened it, with their mouths, to the threads they had already spun (doubtless, by means of some glutinous matter proceeding out of their mouths); and, in this spinning, I did not perceive them move more than their heads and the two joints of their body next the head: and, after two days, they had proceeded so far in their spinning that the maggot, within the case, was no longer to be discerned. When these had remained in the glass in my closet, the space of four days, I observed, that some of the Ants in them had gained so much strength as to break through the web or case in which they lay, and run about the glass.

Hence it also appeared to me, that these last maggots, in their change into a crysalis, or an Ant, not only at once assume all their limbs, and the complete shape of their bodies, but also that, after this change, they never grow any larger, unless they undergo a farther change, and become flying insects. So that we see the error of those who say, that, in what they call the eggs of Ants, they

can distinguish the young Ants when not perfectly formed, and which, in process of time, increase in size; when, on the contrary, no Ant, or rather its maggot, is inclosed in its web or case, until it has attained its full size.

This being so, it appears, that the young Ant, whether a maggot, a crysalis, or a maggot in its web or case, has, hitherto, been improperly called by the name of an Ant's egg; for, in like manner as we can, with no propriety, denominate the cone or case wherein a silk worm is inclosed, a silk worm's egg, it is equally improper to call the maggot, crysalis, or the web of the maggot, wherein a young Ant is inclosed, an Ant's egg. For, upon comparing the true size of an egg, laid by the Ant, with that of the web or case wherein the maggot, containing the young Ant, is inclosed (and which is commonly called the Ant's egg), I will venture to say, that nearly an hundred and seventy five Ants' eggs do not more than equal the size of the web or case spun by the maggot, and in which it is changed into an Ant.

I have before said, that, at one end of the web or case spun by the young Ant, there is a black spot, which, upon examination, I judged to be the excrement of the maggot, and which it purged off when it was about being changed into an Ant.

This web or case, spun by the maggot, is of so close a texture that one can seldom discern the Ant within it, unless when it is so far advanced in its growth that it begins to assume a black colour, or when it moves itself within the covering or case.

In all the Ants' nests that I have examined, I have observed some of the Ants to have wings; but in none of the nests have I been able to find the least traces of the provisions which they are said to store up; so that I am still more assured that, in the summer season, the Ants have enough to do to collect and carry into their nests a sufficient quantity of food for the support and nourishment of the multitudes of their maggots, both great and small, with which their nests are, in many places, quite filled.

Moreover, about the middle of the month of August, I examined several black Ants' nests on the Lands we call *Daynen* or the Downs, as well in the neighbourhood of Haerlem, as elsewhere, but I did not find in them any of the maggots from which these Ants were bred, nor any of the webs spun by them, but only some empty webs or cases, out of which the Ants had crept.

I have also seen, in some nests, the greatest part of the Ants changed into flying animals; and, among these winged Ants, some were eight times larger than the common black Ant.

I, afterwards, again examined the nests of the red Ants in my garden, and found in them Ants, with their eggs, the maggots, and crysals, in as great numbers as before.

I also placed several grains of wheat before the Ants' nests, thinking they would not have strength to carry them away, but I did not observe them at all disposed to touch the wheat, and they only carried into their nests a small barley-corn.

I have thus in my researches discovered two species of Ants; and doubtless there may be many other species of them in these parts, which have not come under my observation. As to what sorts of Ants are found in other countries, which are much larger than ours, and what may regard their collecting provisions, and the nature of their domestic œconomy, all these things are unknown to me.

These are my observations (such as they are), respecting the Ants, from which I have collected, and satisfied myself of, the following particulars: First, that Ants lay very minute eggs; secondly, that from those eggs maggots are produced, which are fed by the Ants; and that the food which Ants are continually carrying to their nests in the summer time, is for no other use than the nourishment of those maggots: and thirdly, that what people call Ants' eggs are either maggots, crysals, or the webs spun by the maggots, (with maggots in the inside).

To shew the true size of our Ants, I caused a drawing to be made of some of the red ones, which is expressed at *fig. 7.*

After this, being about two hours' journey from our town of Delit, and going into a plantation, belonging to a gentleman's house, a working man shewed me several Ants' nests, namely, some of the red Ants of the same size with those in my garden, in which I found a great number of eggs and maggots of different sizes, and many crysals among them; also, some nests of black Ants, which appeared to be propagated in the same manner as I have before described respecting them. But in these black Ants' nests, I found no eggs nor maggots, except some maggots employed in spinning their cases, and some crysals in the cases or webs. I also here found a third sort of Ants, of a bright yellow colour; these were as small as the black Ants, and in them I could not discover any sting. The maggots that came out of the eggs of these Ants, when brought to their full growth, changed into Ants without spinning a web, in like manner as I have said of the red Ants.

For my still farther satisfaction, I took out of my garden two several Ants' nests, and the earth which contained the most Ants, with their eggs, maggots, and crysals mixed; these I put into a glass about nine inches deep, and eight inches in circumference, on which I put a glass cover, and gave the Ants for food some sugar, a piece of a pear, and some cherries; placing the glass upon my desk, in order, if possible, to discover the manner in which the Ants fed their maggots; but these the Ants carried away, and concealed under the earth in the best manner they were able, so that I could get a sight of but few of them at a time. But, upon observing how motionless the maggots lay, I saw, that though shaken about or carried by the Ants from place to place, or whether lying in heaps or singly, and no Ants near them, they did not stir themselves in the least.

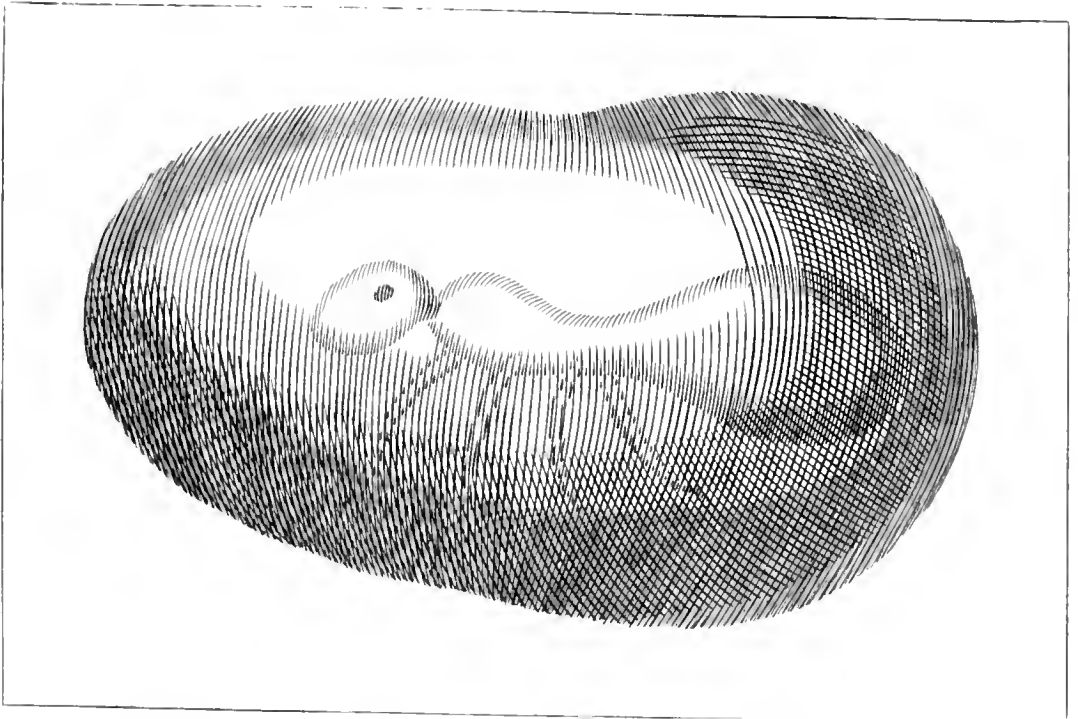
And as I frequently saw an Ant with its mouth close to the head or mouth of a maggot, and in that posture lie motionless for a time, I considered that probably one or more of the organs which the Ant has in its mouth, might be formed for no other end than to

put into the mouth of the maggots the food provided for them by the parents, which is what in beasts we name suckling.

From these my observations, I concluded that probably the food which we see the Ants in these countries carry to their nests, is what they, for the time, have more than they can take into their stomachs, and that they hide it in holes and corners in their nests, until they have fed their young with the nutriment prepared in their stomachs, and this being done, they resort to the provisions they have brought; which being prepared in their bodies to become nourishment for their young, they can immediately, without quitting their nests, supply the young maggots with more food.

ADDITION, BY THE TRANSLATOR.

Mr. Leeuwenhoek having in this Essay noticed particularly, the erroneous figure given by a German author, of what he calls the Ant's egg; that figure taken from Mr. Leeuwenhoek's copy of it, is here subjoined: the Translator not chusing to insert it in the Folio Plate, lest it should, at first sight, be mistaken for a drawing given by our Author, with the accuracy of whose figures this is altogether incongruous.



*Of a certain poisonous Reptile, common in the East Indies, called in Latin, Millepeda Indica, in Dutch Duyfent-been, and in English vulgarly named the thousand legs.**

I HAVE often heard mention made of the poisonous puncture or bite of a certain noxious animal in the East Indies, called a Millepeda, or thousand legs; this animal, as I have been told, creeps over the bodies of persons asleep, and being of a very cold nature, the person is induced by the unusual sensation to move; if the person does not stir, the animal passes away without doing him any injury, but if he moves, it bites him with the two fangs or teeth, placed in the fore part of its head; and although no blood follows the bite, but only a very small red or livid spot appears, yet an intolerable pain, with a swelling, ensues, which remains longer with some persons than others; and, to relieve this pain, the most approved remedy is, to drown the animal in oil of Olives, and to anoint the wounded part with the oil.

Being desirous to see one of these creatures, I gave in charge to some labourers employed in this town to unload the East India ships, to bring to me a living Millepeda, in order that I might if possible discover from what cause the poison in its bite proceeded; and thereupon they brought me one, about the size of my little finger, (though some of these are two or three fingers' length). I immediately took hold of it with a small pair of pincers, near one of its two fangs or piercers, and examined the fang by the microscope, and all the while, the animal was continually opening and shuttings its fangs, as endeavouring to bite. I saw that in each of

* We have in England an animal probably of this species, though not venomous, and of much smaller dimensions, to which the name of thousand legs is given, particularly by children. It is often found under stones or rubbish; it is seldom above two or three inches long, but in shape exactly similar to the Millepeda here described; and wonderfully nimble in its motions.

these fangs, there was a round hole placed in a kind of groove or furrow leading towards the point, the intent of such furrow being, as I conceive, to convey some kind of liquid issuing from the hole to the very point of the fang, while it is within the part wounded; and hence, I imagine, that the Millepeda by its bite, wounds and tears open some of the blood vessels, or other vessels within the skin, and at the same time infuses a liquid into the wounded part, which liquid, I suppose, contains some noxious and very pungent salt, and that the pain felt is not occasioned by the mere bite, but from that noxious or pungent liquid. I did intend to have pursued my observations still farther, and had desired the labourers to bring me more of those animals, but none were then to be had; for though several were seen in the ship while the goods were unloading, they had been all killed upon the spot.

The forceps, fang, or what may be called the sting of this Millepeda, I preserved, and caused a drawing to be made of it. *Fig. 8, A B C D E F*, represents a part of this forceps or sting, as seen through the microscope. At *C* is to be seen the furrow or channel, and also the hole in it, through which this noxious animal, in wounding a man, injects the poisonous liquor.

After this, I was presented by a certain gentleman who is fond of collecting all sorts of foreign animals, with a large Indian Millepeda, of which I have also caused a drawing to be made, because many persons know nothing of this poisonous creature: this is shewn at *fig. 9, G H I K L*, and in this figure, *I K*, are the two fangs or stings, one of which is exhibited in the former figure, as seen through the microscope.



OF THE FLEA.

BEING desirous to know how long time was requisite for the egg of a Flea to produce a perfect Flea, I sat about making experiments to ascertain that fact; but it was not till after many trials that I could make a regular series of successful observations.

In the month of July, I enclosed several Fleas in a glass, that they might lay their eggs; the worms or maggots hatched from their eggs, I nursed with all the care I was able, feeding them every day with flies, which I first killed; these they devoured with great avidity, and thereby were very speedily increased in size. My observations on this subject are as follows:

On the 6th of July the worm came out of the egg.

On the 17th of July the worm appeared all over white, from whence I concluded that it was near dying; I twice offered it fresh flies for food, but it would not eat, and appeared to me to be motionless, but viewing it with the microscope, I saw that it was employed in spinning round itself a web or covering.

The 21st of July, this worm was changed into an aurelia or chrysalis, which was of a transparent white.

The 25th of July, this chrysalis assumed somewhat of a red colour, which continually grew deeper and deeper.

The 30th of July in the morning, it was entirely red, and in the evening the Flea it contained was leaping about the glass.

Hereby we see that in the middle of the summer, in four days time after an egg is laid by the Flea, it produces a worm or maggot, which in eleven days attains to its full growth, in four days time it is changed into a chrysalis, and in nine days more it becomes a perfect Flea.

Now if we lay it down, that it is with these creatures, as with most small animals, which in like manner undergo a change, (at

leat) as far as I know) that their eggs are formed in their bodies before they come out of their aurelia state: it will follow, that Fleas immediately upon their coming forth into life out of their webs, can copulate, and in three or four days time lay eggs, so that in the space of twenty-eight days, from a newly laid egg, a Flea may be produced. This being the case, it is no wonder that in the summer we at some times perceive no Fleas in our houses, and at other times they appear in great numbers without being brought from elsewhere; and if we also suppose, as I am well assured is the case, that Fleas, after they have copulated and laid the eggs which they had within them, do then die, we may conclude that we are not tormented for a long time by one and the same Flea.

But whereas many flying insects do, within a few successive days, lay all their eggs, this is not the case with Fleas; for a Flea, which in one day or night laid 4, 5, 6, 7, 8, and even 12 eggs, being afterwards shut up in a glass laid no more, though upon opening it, I found in its body many eggs remaining, some of which were exceedingly small, whence I concluded that a Flea being so shut up, its laying no more eggs was occasioned only by want of food. To satisfy myself in this respect, I took several female Fleas, after they had laid their first parcel of eggs, and contrived to confine them on my hand, in order that they might thence get a supply of nourishment: one of these sucked the blood with great avidity, standing as it were upon its head, and lifting up its hinder and middle feet with a quivering motion in them, this Flea the following day laid two or three eggs, and two or three days successively afterwards, I placed it again on my hand, that it might get a further supply of food, but in vain, for it did nothing but leap about and endeavour to escape. The like happened to me in several other trials I made, but if the Fleas would have continually fed on my arm or hand, I would have done my best to ascertain what is the natural term of this creature's life.

On this occasion I cannot but observe that we meet with many

men who fet themselves in opposition to all new discoveries, being riveted to old opinions; yet are themselves too indolent to investigate the subject, and the rather, as these researches into Nature's works are often very troublesome, and bring no gain or profit. I should endeavour to confute such opponents, did I not think it would be lost labour; and, indeed, I know not who would bestow so much labour as I have done on that minute and despised creature the Flea.

During the space of two months and upwards, I put into the hands of my maid-servant four or more glasses, with cork stoppers, and directed her to confine in them as many as she could catch of the largest sort of Fleas, which are always females, with a caution to handle them gently, that none of the veins or vessels in their bodies might be injured; and these Fleas I frequently removed into fresh glasses, to preserve the eggs they laid from being soiled with the excrements they voided, in which, by the way I have plainly seen a great quantity of saline particles. Now, if, as has been observed above, in twenty-four days a perfect Flea is produced from an egg, we see that in a single summer, or rather from the month of March to November, Fleas may be propagated seven or eight times, and so often may we be infested by new generations of them.

Not content with the preceding observations, I at several times trained up or nursed the worms from the Fleas' eggs, till they grew to their full size, and I found that an egg in three days time produced a worm or maggot, and the worms or maggots in twelve or thirteen days, or little more, came to their full size. In particular, about the middle of August, I had two maggots, one of which came out of its egg about two hours before the other: the first of these I glued to the point of a small brass pin, and placing it before the microscope, I gave it into the limner's hand, directing him to make a drawing of it, as nearly as he was able, for the worm moved itself about violently.

Plate XII. *fig.* 10, A B C exhibit this worm or maggots as glued to the pin, and appears to the view as if it lay on its back. A is the head, on which are two prominent parts, like horns, out of which proceed two other sharp weapons, so small that the learner did not perceive them until they were pointed out to him : between these horns appear two shorter prominent parts, which are not placed on the head, but at the lower part of it, where the mouth is situated : these parts the maggot uses in moving from place to place, and also those parts at the hind part of its body, which are pictured at C.

After this drawing was made, the worm struggled so violently, that it got loose from the glue, and in fixing it again on the pin, I happened to touch its head with some of the glue, whereby it was much hurt, and after remaining fixed for six hours, it died, soon after which I perceived the moisture of its body so much evaporated, that it was contracted into many wrinkles, and at the end of twenty-four hours it was so dried as to lose all likeness of a maggot, whereas the other which was hatched two hours later, and had remained fixed before the microscope only two hours less, was not only alive, but its body as fully distended as if it had been newly hatched.

After this worm had been thirty hours before the microscope and was still very lively, I thought I saw its body not so completely filled out, and consequently that some of its moisture was evaporated. I frequently afterwards inspected it, and at the end of sixty-four hours, I saw it still in motion, but its body diminished in size : this day the sun shone very hot.

At the expiration of four days and nights this worm was still in motion. I did not examine it again till sixteen hours afterwards, and its body was then much contracted : the following morning the moisture of it was so evaporated, that it could not be known to be a worm.

This circumstance, namely, that in living creatures there is so

little waste of moisture, I have observed, not only in this maggot, but in many minute animals, and even in the eggs of small insects, (among which I reckon those of the flea); for if the fruitful and barren eggs are placed beside each other, we shall see the barren ones dry away, whereas the fruitful ones will retain their moisture, at least, so far as to afford the maggot in the egg sufficient for its nourishment.

But what shall we say, when we observe that the evaporation of the moisture in small living creatures, or their eggs, is so little, inso-much, that a Flea shall remain wrapt up in the web or case it spins for four entire months, and yet its moisture not evaporate so far as to cause its death; whereas the moisture exhales from the body of a dead flea in so short a space of time. In a word, we cannot but wonder at these appearances, and here our reflections must terminate.

Before this worm, I have last mentioned, came out of its egg, I placed the egg before the microscope, and caused a drawing to be made of it, because I could not only see the worm alive in the egg, and how its body was placed, but I could also discern through the shell, many of the joints in its body.

Fig. 11. D E represents the egg of the Flea with the worm or maggot in it, and which egg had been laid but three days. *Fig. 12,* F G, is the egg from which the maggot had crept out, and in which may be seen the manner how it had broke open the shell. These eggs of the Flea are no larger when viewed by the naked eye, than small grains of sand: and as objects do not appear of the same size to the eyes of every one, so it was with the limner who made this drawing; for the breadth of this egg appeared to me twice the size here represented, and the worm also in the same proportion. I could, however, if necessary, have made drawings of these objects from microscopes of greater magnifying powers.

After the preceding remarks, I so far succeeded in the treatment

of several worms or maggots, that I had two which acquired their compleat portions of nourishment, and began to spin their webs; but, because the bottom of the glass wherein they lay was stopped with a smooth piece of cork, and that pressed in so tight that they could not penetrate it, they could not completely cover themselves with the web. For in like manner as silk worms are placed in a paper of a round tapering form, called a cup, in order that it may fix its web on every side, and be completely covered by it; so the worm produced from the Flea's egg, when come to its full size, endeavours to creep into cracks, holes, or corners, in order to inclose itself on every side with its web.

At several times I contrived to prevent the maggots enclosing themselves completely in their webs, in order that I might the more easily discern their alteration into a chrysalis or aurelia. But how often soever I viewed them after they had ceased eating, and their change approached, I could only perceive that they placed themselves in the same position as they had lain in the egg. But examining them in the afternoon in this posture, I found upon looking at them three hours afterwards, that one of them was changed into a chrysalis. Upon viewing another of them through the microscope, I perceived a mite upon its body, where it remained some time, and another larger mite running about the glass, whereupon I concluded, that at the time those worms are in their state of change and unable to defend themselves, the mites will seize and feed upon them. After this worm had thus laid for some hours, I saw that its body was somewhat contracted, whereupon placing it before the microscope, I perceived three holes pierced in its body, part of its skin stripped off, and the body of it beginning to dry away.

Hereupon I began to consider, that this web or covering spun by the Flea's worm or maggot is necessary to it, and that without it, those creatures could not easily be propagated; for there are seldom any fragments of food, or the bodies of small animals lying

about our houses, but they are immediately found out by mites who come to feed upon them; and, in the present case, though I thought I had perfectly secured these maggots from the mites, because the glass tube wherein I had put them, which was almost an inch in diameter, and five inches long, was so closely stopped at each end with cork, that I should have thought it impossible for any mite to get in; yet now I found the contrary to be the case: and indeed, I have often found mites feeding on things in places where I wondered they could so soon discover their food. And now, upon seeing these mites about the maggots, it occurred to my thoughts, whether or no those small and very slender prominent parts in the maggot, which in *fig. 10*, are shewn at A, might not be weapons designed for its defence against the mite.

But the web spun by the maggot, and wherein it incloses itself, before its transformation, protects it in that state from all attacks of the mite; and the like we may conclude, to be the case with the silk worm, which while in its state of transmutation into a butterfly, is protected by its cone or case, not from mites, but from birds; and this I am well assured is the case with all caterpillars, which when of full growth, and the time of their transmutation approaches, do inclose themselves in some kind of web, or covering.

The maggot which I have said I saw changed into a chrysalis, I the next morning placed before a microscope, and delivered it to the limner, to make a drawing of it.

Fig. 13, A B C D E F, is this chrysalis; A K, are the horns of the yet unformed Flea; L G B, is one of its fore legs, which rise out of the head at L; and herein, at B, may be seen all the small joints which will be found in the legs and feet when perfectly formed: the like joints may also be seen in C and D.

H C is one of the middle feet, and at G may be seen that the fore feet partly cover the middle feet, and these in part cover the hind feet; I D and C H are the two hind feet, which are in part covered by the middle ones.

Fig. 14, M N O P Q, represent the pellicle put off by the maggot, when it changed into a chrysalis, leaving the wrinkles in it as they here appear; and at *M* are the places where the maggot's horns stood.

Three days after this drawing had been made, I saw that the chrysalis was very much altered, and many parts appeared in it which till then could not at all be distinguished; therefore I caused a drawing to be made of this also, to shew such alteration.

Fig. 15, A S T V, is this drawing, taken as exactly as the limner was able to represent this second appearance of the chrysalis.

Fig. 16, A B C D E F G H I K L M N, represents an entire Flea, glued by the back to the point of a needle, and so placed before the microscope, but being first killed, because it would be impossible to make a drawing of a Flea while alive. Nor, indeed, did I intend to have given a drawing of the whole Flea, but a certain learned gentleman strongly urged me to it, saying, that the world would thereby be more convinced, that Fleas are not produced from corruption, but in the ordinary way of generation, and I therefore complied with his request. In this animal a great number of blood vessels were to be seen, particularly in the belly, but the limner only drew a few of them, saying, that it was not in his power to imitate the remainder in his drawing.

This Flea appeared to me, through the same microscope, eight times larger than here shewn, though the limner declared that it did not seem any larger to him. Nor could I ever have believed that there was such a diversity in the sight of different people as I now find to be the case. But this limner was very short sighted.

At *D E F* are shewn the hind feet; *F G H* the middle feet; and *I K L* the fore feet; placed on the head: and between these lie the organs or weapons with which the Flea pierces or bites, and seeks its food. *L M N* are two horns on the Flea's head.

I had formerly figured to myself the manner in which Fleas must get their food out of the bodies of men and animals; but because

those parts in a Flea which I heretofore took to be the piercers or weapons by which it draws the blood, appeared now to be the sheath or case of the real piercers, which divide into two parts when the Flea thrusts out its piercers from between them. I will now describe how these parts appeared to me upon the examination of three several Fleas.

Fig. 17, A B C are the two horns on the head, which have been shewn in the last figure, each of them has three joints, besides the joint fixed to the head; D E is the sheath or case, with the stings or piercers, which in a manner fit or lock into each other, as I observed while I was endeavouring to separate them. F A D is part of the head.

Fig. 18, G H I K, represents also the piercers, and their case or sheath, placed in the head, between the two fore feet; which piercers and their sheath are very difficultly discovered, unless the fore feet be first cut off close to the head, and even thus they will seldom be seen; because the Flea, when not using its piercers, draws them within the sheath, and places them close to its body. But as a Flea, though its fore feet are cut off, will yet live a long time, my way always was, when I endeavoured to get a view of the piercers, to cut off its head, lest, when I had removed the piercers from their position close to the body, and separated them from each other, the Flea should draw them again out of sight.

G I, and G K, are two parts, which, having cavities in them, opposite to each other, constitute the sheath of the piercers; which piercers are placed in the cavity marked at G H. These two stings or piercers might indeed be taken for a single one; but if we view the figure narrowly, it will appear that the limner has pictured the second sting as it were within the first, and not reaching quite to the end at H.

Fig. 19, L M N O P, exhibits the sheath of the stings or piercers as I found them in a third Flea. Here I had better success than in the dissection of the two former Fleas. For at L N and L O are

represented those two parts which, when joined together as I have before said, constitute the sheath, and are covered with many hairs. At LM and LP are the stings or piercers; and in one of them, LM, may be seen the cavity in which the other of them, LP, is placed, when both are at rest: and when these two piercers are inclosed in the sheath, these four organs may be taken for one single sting.

While I was employed about these observations, my servants brought me a Flea, fixed to a small needle. Upon placing this before the microscope, I saw not much motion in its feet, and the stings or piercers were put forth from their case; and the sting, LP, which was inclosed in the cavity of the sting, LM, was moved up and down in a very pretty manner, with great swiftness, and as it were in play.

Moreover, farther to gratify the curious, I pulled off one of the Flea's hind legs; in doing which, part of the muscle belonging to it was separated from the body, this I fixed to the point of a brass pin, in order the better to place the whole leg before a microscope of greater magnifying power, and to cause a drawing to be made of its true shape.

Fig. 20, B C D E F G H I K L M N, represents this hind leg; A B is the muscle, separated from the animal's body at A. The first joint is shewn at C D M N, and B C N is the part by which it had been united to the body: in this first joint might very plainly be seen four distinct parts, lying longitudinally, and probably were tendons and blood vessels; which vessels, and their more minute ramifications, I could plainly see. This joint, and all the others seen through the microscope, exhibited a transparent yellow colour, like that of amber.

Between the letters D E L M may be seen the perfect articulation whereby this first was united to the second joint.

E F K L, represents the second joint, in the middle of which not only the nervous or muscular parts lying lengthways, were plainly

to be seen, but also a blood vessel, distinctly appearing to be composed of annular parts, in which manner the blood vessels of Fleas and other insects are formed, similar to the aspera arteria, or the wind-pipe in the lungs of animals. And at one time, having a Flea fixed by its hind part to the point of a needle, before the microscope, I saw throughout the whole length, on each side of this joint, as between E F and L K, and also, in the middle of it, a wonderfully swift motion or current of the juices, which appeared to me the plainer as there were globules mixed with those juices: but I did not notice this motion, except at the time the Flea was moving its leg or foot, and then this motion continued for some time after the leg and foot were at rest. Many persons viewing this would say, that they had seen the circulation of the blood in the Flea's foot.

Moreover, upon the motion of the leg, as well in this joint as in the others, there was so great an agitation of the component parts as is inconceivable. This agitation, I concluded, was only caused in the fleshy muscles of the leg, because it partly continued after the death of the Flea; but it was impossible for the limner to imitate, in his drawing, all the parts he saw: and he often, while making the drawing of this leg and the other parts of the animal's body, broke out into an exclamation, "Heavens! what wonders here are in so small a creature!"

FGIK is the third joint of the leg, in which may be seen many sharp-pointed particles, sticking out like thorns.

At GHI are shewn the five last or smallest joints of the Flea's leg, all formed in the same manner; and here plainly appears how all these joints mutually depend upon, and are subordinate to, each other.

At H are two perfectly formed claws: with these every one of the feet is furnished, and the Flea uses them with such wonderful swiftness and dexterity, that though when shut up in the glass he leaps about, and touches only a small particle of dirt or impurity,

though it were only a particle of his own excrement adhering to the glass, he can, by a touch only of one claw, cling to it.

Now if we reflect on this wonderful and complicated formation of joints in a Flea's leg, we shall cease wondering that it can leap to so great a height as we see; nor ask the question (which I have often had put to me) whether Fleas had wings to carry them so far and high?

Farther, I cut a maggot, produced from the Flea's egg, and was almost full grown, into two pieces, and placed each part before a microscope of the same magnifying power as that by which the maggot newly hatched was drawn: but if I had placed the maggot entire before the microscope it would have been impossible, by reason of its continual motion, for the limner to have given a true figure of it.

Fig. 21. A B C D, represents the head and three first joints of this maggot's body: I had placed it upright before the microscope, but before the limner had got it, the head was inclined downwards, as shewn at A B C; nevertheless this piece of a maggot, for the space of an hour, moved itself, and often lifted up the head.

Fig. 22. E F G represents the four last joints of the maggot, constituting its hind part, in which not only may plainly be seen, at F, the organs or limbs with which the tail of this maggot is provided, but the place may also be distinguished where the excrements are voided.

Now this maggot not being provided with any limbs or organs for moving from place to place, except what are seen in *fig. 21*, at D, in its head; and in *fig. 22*, as F, in its tail: the many hairs it has on its body are a great help to its motion; but, at the same time, are a very great hindrance to it, if it meets with any liquid matter. So that even the hundredth part of the liquid excrement voided by the Flea at one time, is sufficient not only to adhere to those hairs on the maggot, so as to stop its progress, but also to kill the maggot itself, by reason that it is not able to extricate itself from the moisture: therefore, as we see how little able these mag-

gots are to endure moisture, we may conclude that, if in places where Fleas abound, the floors or pavements be well wetted with water, the maggots may be destroyed, and consequently the places cleared of Fleas.

I have caused a drawing to be made of the glass wherein I inclosed the Fleas I had under my observation, for the information of any who may chuse to prosecute my experiments. *Fig. 23*, *NOPQ*, is this glass, the cavity or orifice of which, *NOQ*, is about the fourth part of an inch in diameter: the sphere or globular part, *OPQ*, is something larger, according as may happen in the blowing it. I did not put more than two or three Fleas at a time into one of these glasses, and as soon as they had laid any eggs, I took them out of the glass, that it might not be foiled by their excrements or the vapour of their bodies, otherwise the hairs on the bodies of the maggots would stick to those foulnesses, so that they could not be taken out of the glass, but would there expire.

I have now for several years observed Fleas to lay eggs, and from such of the eggs as were fruitful, maggots produced, which maggots were afterwards changed into Fleas as before described

Since we then see so plainly, that the Flea is endowed with as great perfection in its kind, as any large animal, all whose limbs may be seen with the naked eye, can any one give credit to the idle tales of old? one asserting that Fleas are produced from sand; another, from dust; and another from the dung of pigeons; and lastly, from urine: for that Fleas can be produced from dust and filth, I utterly deny, as appearing to me impossible: nevertheless, that out of the dust of a floor or pavement, Fleas will sometimes come forth, I readily grant; but this is brought about in quite a different manner.

If many children be kept in a room, in which the Fleas are not every day extirpated, and the floor or pavement is sometimes swept; or if a floor or pavement be swept near to a place where a dog is accustomed to lye, there may very easily be taken up among

the sweepings, not only young maggots from the eggs of Fleas, but full grown ones, and out of the chinks or cracks maggots may be swept, some of which are spinning their webs, and others, which have completed them, and are changed into cryfales; so that out of such sweepings Fleas may come forth day after day: and it may happen, that maggots newly hatched, may among such dust and rubbish find something to afford them nourishment, so as to grow up to be complete Fleas. And I am the more inclined to believe that this may be the case, because I could never get out of any dog, how much soever he was infested with Fleas, any of their eggs, nor see any about his body. But if we take a cushion covered with a green or any other dark colour, and let a dog lie on it who is infested with Fleas (which I have often done), we shall find that the Fleas do not lay their eggs upon or near the dog, but the eggs will be found in the corners and sewings of the cushion, where, by their whiteness, they will easily be discerned.

We have, indeed, moderns who favour those old opinions, of whom I will only say, that if they were provided with a good microscope, and would attentively bestow a few days, as I have done many, in the investigation of the subject, they would not broach such fables and childish tales, as they now write and publish to the world.





On the seeds of Trees, with the author's reasoning and observations on the possibility of intermixing two different species of tree, so as to produce a third, partaking of the nature of both. A singular peculiarity in the seed of Cotton.

UPON an accurate examination of every kind of seed, we shall discover in it the origin or first formation of the leaves and root of the future tree or plant, according to the respective species of such seed.

This is particularly observable in the seed of the Ash, which is represented of its natural size in plate XIII. *fig.* 1. In this seed, though no bigger than here pictured, I discovered not only two leaves, but also that part from which the root would grow, being all very large in proportion to the size of the seed.

Again, all seeds in their formation, on the parent plant, receive their nourishment from a small stalk, string, or ligament, consisting of many vessels through which the nutritive substance is conveyed, analogous to that which in animals is called the navel string. In some seeds this stalk or ligament is very short, in others as long, or longer than the seed itself. I will give some instances of this, beginning with the seed of the Ash.

Fig. 2. A B is the half of the shell, case, or covering, inclosing a seed of the Ash. A D shews where the small seed E F was placed, and from whence being taken out, as represented in the figure, there appears the string or ligament A F, through which the whole seed E F received its nourishment, being derived from the part A, which was joined to the tree, and through which alone the same can be conveyed. It is also to be observed, that the part in the seed whence the future root will proceed, is at the point F, where the ligament is joined to it; so that the leaves of the future tree when in the seed, while it is united to the parent tree, are placed,

as I may say, with their points downwards, and their roots and stalks upwards. But we must consider that the seeds of the Ash, which grow in * clusters on the tree, do at length, by their weight, hang downwards, and that the same is the case with apples and pears, which while they are very small, stand erect on their stalks, and consequently the origin of the future plants formed in them, is placed with its root upwards; but when the fruit grows to such a size, as by its gravity to hang downwards, then the young plant in the seed or kernel, has its root and leaves in the same position as it will have when growing.

Upon opening the seed of the Ash, I found in the middle of it two large leaves, and also the first rudiment, or beginning of that part which would become a root, and these leaves were larger than I have observed in the seed of any plant whatever. These leaves when examined by the microscope, appeared as in *fig. 3.* C D E F, and I found them to consist of an immense number of thin round prominent globules, which I endeavoured to imitate in the drawing, though they are not there represented nearly so small as they appeared to me. I also saw in those leaves a great number of fibres, or more properly vessels covered with wonderfully minute globules; which vessels, but without the globules, are represented in the figure: these vessels arise from the inside of that part whence the future stem or trunk of the tree, and also the root are produced; which is pointed out in the figure at A B C F G H. This last mentioned part which would produce the stem and root, besides those vessels from which the fibres of the leaves issue, and which may properly be considered as the marrow or sap of the wood, is moreover provided with many vessels; and in order to represent those vessels in a clearer light, I cut a piece transversely, at the place marked with the letters B G, and having placed the piece or slice before the microscope, I made as exact a drawing as I was able of it, with all the vessels in it, as

* In England, these clusters of seeds on the Ash, from the resemblance they bear to a bunch of keys, are called Ash-keys.

they appeared to me, a representation of which is given at *fig. 4.* I K L M. In this small particle, I not only judged that there were about a thousand vessels taking their course upwards, but it also gave me a representation of a complete branch of the tree cut transversely; for the external surface represented the bark; the part next it, in which are very small dark spots, denoted the wood; which dark colour was only caused by this, that in cutting the slice, the knife had squeezed together, or stopped, the small tubes or vessels of which it was composed; the middle part, the sap, or those vessels which would produce the stem or branch, and being cut transversely, appeared round bodies. But this figure was drawn from a microscope of greater magnifying power than that used in the former, in order to shew the nature of this stem and root more distinctly, so that the diameter of this figure at I L, is about twice the diameter of the part marked in the last figure by B G.

Moreover, in this small trunk or stem, I perceived in some places, some round particles, as if they were small round corpuscles in the tubes or vessels, which are represented between the letters A and B.

I have thought it right here to exhibit the size of the young leaves in the seeds of some other trees, to shew that it by no means follows of consequence, that the largest seeds contain the largest rudiments, or originals, of their future plants, for the young plants in the small seeds of the Ash are vastly superior in size to the very small leaves which the large seed of the Walnut contains, and which, as seen by the microscope, are shewn at *fig. 5* and *6*, the originals of which I took from two seeds of that species, in order to note if there might be any material difference between any two of them. In these figures I have only represented the leaves, because in the rudiments or originals of the future stems and roots, (which in this seed are somewhat longer than the leaves), I perceived little or no difference in the form from those in other seeds. In drawing these two figures, I found it quite impossible to repre-

sent the very minute globules of which these young and unformed leaves were composed.

I have in another place said, that there are no blood vessels intermixed among the small fibres or fleshy ligaments of which a fleshy fibre consists; but that the blood vessels which surround the fleshy fibres, are placed in or between membranes; and I have also laid down my idea, how far the fleshy fibres are nourished from the blood vessels; so in this case, I say, that all the leaves of trees and plants are formed of globules (besides the vessels or fibres of the leaves), all inclosed in a membrane constituting the surface of the leaf, and the manner in which, I imagine, that these globules, although they touch no vessels, are yet nourished by the vessels, I take to be this: we must first understand, that almost all the leaves of trees and plants, while united to the plant, consist of two third parts water or a watery substance, and that the globules, of which for the greatest part, the leaves consist, do not lie singly or separate, but many of them heaped together. Thus let us suppose *fig. 7*, A B C D E, to be the vessels in a small piece of a leaf, and that these vessels have a great quantity of globules, which in the nourishment of the leaf are supplied and supported by the vessels: then upon the globule F receiving a supply of nourishment, particularly water, from the vessels to which it is closely conjoined, it must necessarily impart this nourishment to G, and that the same nutritive substance will be conveyed from G to H, not only because these globules are closely conjoined together, but also because as before mentioned, the greatest part of the substance of the leaf, and consequently of each globule, consists of a watery matter or substance; and thus, those globules which are next the vessels, cannot receive any nourishment from them, but they must impart of it to those globules next to them, and those again to others. I have formerly compared this communication of nourishment to dry globules or balls of clay, which we will suppose lying in a glass, and only one of those balls to be made wet, and the moistening being continued,

the other balls of clay contiguous to this moist one, receive the moisture from it and become wet, and these again lying in contact with others, at length all the globules or balls of clay will, by means of the first globule, become wet; and this is the case in the nourishment of leaves and also of fruits.

Fig. 8, A B C represents the outside of the young leaves in an Almond, and upon removing them, I perceive several leaves inclosed in them. *Fig 9*, A B C, is the outside of the leaves in the kernel of a Cherry, and *fig. 10*, A B C, the leaves, as they appeared to me, in the kernel of an Apple.

Now, if in such small seeds as those which produce the Ash (one of the largest trees growing in this country), and which seed is so light, that six of them scarcely weigh four grains, if in these seeds, I say, we discover, not only the perfect leaves with their vessels, but also the stem and root of the future tree, and these much plainer and larger than in the Walnut or Hazle, we shall also easily perceive that wise and provident Nature in all her operations, and especially in the propagation of plants and animals, perfects her work, by ways and means similar or analogous.* For the seeds of trees and plants not only contain in them the origin of the future plant, but also a white substance which we denominate meal, in order to nourish and support the young plant, until it has struck its roots into the earth and can draw nourishment from thence. Besides which, many seeds are provided with an oily substance, whereby the young leaves and plants are kept from drying up, and † many seeds which have not this oil will not remain long good out of the ground.

* In the original it is "all seeds must necessarily be provided with a mealy substance," &c. but this is a mistake which the author corrects presently afterwards in his account of the seed of Cotton.

† This is particularly the case with the Acorn, which by nature is designed to vegetate immediately after it quits the tree.

Fig. 11. G H I is the half of the hard shell of the Filbert. K L M is the nut or kernel taken out of the shell. G is that part of the shell which was united to the tree, in which part there is a very loose or spongy passage through which the ligament of the nut or kernel passes, as G L, to convey the nutriment for the formation of the young plant, and also a sufficient quantity of aliment to support it, when the nut is sown or planted, until the root of that young plant in the nut, has extended itself so far out of the hard shell as to reach the earth. This figure also shews that the substance, which through the ligament is conveyed for the support of the nut, does not at all arise from the hard shell, but that the ligament having a passage through the shell, conveys the nourishment from the tree itself.

These strings or ligaments, whereby the young plant and the rest of the substance of seeds is nourished, have also coats or barks of their own, and within this coat or bark in the ligament of a Filbert, I think there are above an hundred small vessels, all which, as far as I could see, are formed of fibres in a twisted shape, in like manner as if one were to wind a piece of small copper wire round a pin, and then draw out the pin from the wire, leaving it in the form delineated at *fig. 12, A and B.*

It is worthy of consideration respecting these ligaments, that in almost all seeds the ligament is joined to that part of the nut or kernel, from which the young plant will shoot, as may be seen at *fig. 13, letter A,* (which is a Filbert drawn somewhat larger than the natural size, in order to shew more easily the course of the vessels proceeding from the ligament), in which figure the ligament takes its course from A to B, and in its passage spreads itself into divers branches, and these again into smaller ones; and thus spread through the whole nut, which ligament, or the vessels arising from it tend to the same point, and finish where they begun, that is, where the young plant will be produced.

I made a transverse section of the ligament by which the Almond is nourished, because it is somewhat thicker than that in the Filbert,

and made a drawing of it as seen through the microscope, which is to be seen at *fig. 14*, C D E F G. This ligament is divided into seven compartments, which compartments are of a reddish colour; the vessels they contain, I have represented in the figure as they are placed in one of the compartments at F G H; and from a view of the vessels in this single compartment, any one may easily figure to himself the great number of vessels in the ligament, by which the Almond, the Filbert, and most seeds are nourished; for upon examining the ligament of the Filbert, I found no difference in it, except that the ligament of the Almond was larger in every part.

In my remarks and contemplations respecting the propagation of trees and the nature of seeds, I turned my attention to the Willow, which is no otherwife propagated among us, than by cutting off a branch and planting it in the ground, where it grows to a tree. But because I had observed several Willows growing in fields and on the banks of streams, in places where I judged they were not planted by hand in the way I have described, but to have been produced from seeds; I turned my mind to discover what was the fruit or produce of the Willow, in order to discover the nature of its seed. The only fruit of the Willow is a kind of wool or cotton produced on it, about the beginning of the month of June, and at that season, upon examining this cotton, I saw lying in it many dark coloured particles, a little larger than grains of sand. Upon viewing these particles by the microscope, I found them to be the seed of the tree, and that the cotton and these seeds were formed in a kind of cells of a violet colour, and of these cells I counted seventy-five, placed near each other on a small branch, which seemed destined for no other purpose but to produce the seed, and in each of these cells, three, four, or five small seeds lying among the cotton; and I could perceive that the cotton was formed out of the seeds in the time of their growth. These seeds were of the size represented at *fig. 15*: the cotton of the seed was in two, three, four, five, and sometimes of six filaments, joined together by a kind of knot and united to the

seed: and when the seed and cotton were fully ripe, the cells in which they were contained, burst open, and the cotton loosed itself from the seed, and then each particle of cotton which before had laid in regular order beside each other, as at *fig.* 16, started afunder, as represented at *fig.* 17; by which means a small quantity of the filaments of the cotton being so widely dispersed, was, with the least wind carried away, bearing with it many of those minute seeds, and flying over walls and buildings, could deposit the seeds of the Willow in very distant grounds. Upon narrowly inspecting this seed, I saw that the part from whence the future root would arise, and which was about one third of the seed, was provided with many vessels which seemed to consist, for the most part, of oblong and round particles. The rest of the seed consisted of two parts joined close together like leaves, very solid in proportion to their size, of a dark green colour, and seeming to be formed of globules. Having separated these afunder, I saw two very minute protuberances, which I imagined were the beginnings of two leaves, and the origin of the future tree; hence I concluded, that the first two parts which I have compared to leaves, were only designed to afford nourishment to the young plant, until it should be provided with roots, by which it could extract its nourishment from the earth.

In order further to satisfy my curiosity respecting this very minute seed, from which so large a tree as the Willow is produced, I took some of the seeds, and, in the month of June, placed them in moist sand in my closet, in order to see how the beginning of vegetation would be performed in this very small seed. But before I put the seed into the sand, and while it was yet very dry, I viewed it with the same microscope from which the drawings of the preceding young formed plants are taken, and it appeared as represented at *fig.* 18, A B C D E F: but these seeds were of very different shapes, for in drying they became crooked, some more than others. A B E F, is that part of the seed from whence the root would arise.

When this seed had lain in the wet sand thirty-six hours, it appeared as at *fig.* 19, G H I K L. And here it appears how much of the seed was to form the root, which is marked by the letters G H K L; and it was not only grown longer, but in the very short space I have mentioned, six distinct roots had grown out, as represented at G L; and those parts, H I K, which I have described to be like leaves, and which, while the seed was dry, could not be separated without the greatest difficulty, now opened as it were of themselves, to make room for the plant contained between them. And when this seed had remained in the moist sand seventy-two hours, I found that the roots had spread themselves into divers branches, which were so strongly twisted among the particles of sand, that it was impossible to separate them without breaking the roots.

It is well worthy of remark, respecting the Willow, that the seeds are ripe before the leaves on the tree are grown to their proper size, whereas the fruits of most trees, and consequently the seeds contained in them, do not arrive at maturity till much later in the summer, or else in autumn; so that in the Willow, the seed being ripe in the spring, a new tree may be produced from it the same year. This I have also observed in the Elm: for, about the end of May, I took some seeds, which are of a very small size, from an Elm, when the leaves on the tree were about half grown: these seeds, which were dry, I put into wet sand, and after three days they began to grow. I have also found that the Poplar tree, which produces a cotton formed with two flat sides, like the Willow cotton and the Indian cotton, produces its seed about the end of May, or the beginning of June.

Here we see, that in such a small seed as that of the Willow, not only the young plant and the root of it, which is provided with vessels as if it was a complete tree, can be seen, but also that within six-and-thirty hours the seed will begin to grow, even in a close room, so that the young roots may be distinctly seen.

I have sometimes thought, that if it was possible to take out the young plant from any seed, and unite it to another seed, in that place where the young plant was formed in that seed, a tree or fruit would from thence be produced of a species unknown before: as, for example, if we could take out the young plant from the Walnut, and unite it to the substance of a Chestnut, a tree would be produced not like either of the trees from whence those fruits were respectively produced, and consequently a tree of a new species. And a certain eminent gentleman hearing me mention this, urged me strongly to make the experiment, saying, that though of those two seeds only one could vegetate, yet we might expect that some third plant of a new species would be the result. For my part, I considered the thing as impossible to be performed, and so I found it upon further investigation of the subject; but as there are not many men well informed in the first formation or growth of trees and plants, I will here describe the nature of the first formation of the young plant, both in a Chestnut and a Walnut.

Fig. 20 is a Chestnut, broken in half; *A* is the rudiment or first beginning of the plant, and though the Chestnut is a very large seed, yet the original or first formed leaves can very rarely be discovered in it; and indeed I must confess, that among many Chestnuts I found but one in which I could discover the two leaves of the young plant; the upper part of the young plant was somewhat of a round form, and furnished with a sort of cavity. This beginning of the plant, marked at *A*, is not only firmly united to the substance of the nut, but there is one remarkable particular in it, different from other plants, that it has not only two strings or ligaments to convey nourishment from the Chestnut to the young plant, until it is of size and strength to draw nourishment from the earth, but these ligaments are provided with a great number of vessels, having their rise in the Chestnut, to convey the nutritious juices; and these vessels produce others in the root and stem. These

vessels are of the same formation as I have already described the ligaments of the Almond and Filbert.

Fig. 21, A B C is a Chestnut, which I put into an earthen vessel filled with moist sand, and placed in the chimney, it being in the winter season, and watered it every second, third, or fourth day, according as the difference of heat seemed to require: C D and A E, are the two ligaments to which the young plant is united; F G, is that part which will spring up to a stem; G, indicates the leaves beginning to shoot; and E H D, the root. And here we see to what use the Chestnut is destined, namely, to support and nourish that small part contained in it, which is to constitute the future plant; and this is performed through the two ligaments, until the young plant and its root are grown to a sufficient size to draw their nourishment from the earth.

Fig. 22, is a Walnut, so divided or cut as to shew, at I K, the origin or first beginning of the young plant. And whereas I have said, that in a Chestnut the young leaves are not to be distinguished; on the contrary, they may easily be seen in a Walnut: for in every one of them that I examined, I could, with the microscope, as plainly see the leaves, as with the naked eye we can see young leaves in the spring. I, denotes that part which will grow into branches and a tree; K, the sharp-pointed part from which the root will issue.

I also treated several Walnuts in the same manner as I have mentioned respecting Chestnuts, in order to observe their vegetation. *Fig. 23*, L M N O P, is the Walnut; L and P, are the ligaments, to convey nourishment from the root to the young plant. These are provided with a great number of vessels, which, from the observations I made on two several seeds, I was well assured were spread through the whole substance of the nut, or seed, in order to convey its most nutritious juices to the young plant, until it should be sufficiently grown to draw its nourishment from the earth; R P, is the root.

From these observations, shewing the close connection of the young plant by the ligaments with their multitude of vessels, with that mealy substance which we call the feed, it plainly appears, that we cannot take out the young plant from such feeds, without breaking those ligaments and their vessels; and when they are broken the young plant is dead, and cannot be removed into any other feed; so that it seems to me impossible to remove the young plant from a Chestnut to a Walnut, and so to place it that the Chestnut shall grow in the Walnut.

And though we may be able to take out the young plant from the feeds of Ash, Lime-tree, Gooseberries, Currants, or the like, without breaking the ligaments, or rather without observing them, yet we must consider that were there are no ligaments with vessels in them, yet, in stead of them, the globules composing the mealy substance of the feed are placed in such order, and so closely united to the beginning young plant, that they either supply the place of vessels, or in reality are vessels, the true structure of which is to us inscrutable. Moreover, there will be always a considerable difference between the size of the young plants in different feeds, and consequently the place whence the plant is taken in one feed, will be too small or too large to receive the young plant from another. Add to which, that we cannot take out the plant without breaking the feed, which, by such breaking, will become useless. So that it is plain there is no possibility of taking the young plant out of one feed, and uniting it with the farina or mealy substance in another.

I have said, that feeds contain in them not only the first rudiments or origin of the future plant, but also a mealy substance, and some of them an oil; which mealy substance is designed by Nature to nourish and support the young plant, until its roots are so far grown out of the shell that they can draw nourishment from the earth. But, upon examining the feeds of the Cotton tree which

grows in Persia and Bengal,* and which seeds are found among the cotton brought to us from thence; I was astonished to see the variety of Nature's operation in the formation of this seed. For, upon opening these seeds, of which seven, eight, or nine are contained in one shell or hulk with the cotton, I did not find any thing of a mealy nature or substance, but the oblong round figure of the seed is only caused by four distinct leaves, with wonderful exactness doubly folded together, and as it were embracing and defending that part from which the stem or root will proceed. I caused a drawing of this to be made, somewhat magnified, in order to shew the leaves, with their fibres or vessels, and also certain dark round globules which are to be seen on the leaves: these are shewn at *fig. 24, DEFG*. When I cut these globules open as exactly as I was able, the pieces cut off exhibited the brightest grass-green colour that can be imagined; a particle cut thicker was of a darker green; and the globule viewed entire was of a deep green, verging to a black colour. *a b c*, Are three pins, with which the leaves are spread open. The substance of these leaves was composed of exceeding small globules, of a kind of grass-green colour. I enquired of several persons who had been in India, what was the form and nature of the leaves of the Cotton tree, but among all to whom I addressed myself, I only found one gentleman and lady, who told me that the leaves of those trees were marked with spots. Here we see the wonderful regularity of Nature's works, namely, that in the seed of this tree there is not only formed a perfect young plant, but the leaves in that young plant are dotted with points or spots, in the same manner as the leaves growing on the tree. In the figure, *DE* is that part from which the stem and root will be produced, and, upon cutting it open, I found in it a very few grass-green globules.

* The Cotton tree is now cultivated in the West Indies, where it forms a considerable branch of trade.

These observations recalled to my mind (for I compare these feeds with those eggs which I have heretofore taken out of the bodies of some insects) that in some of the eggs so extracted, I did not find any substance destined to the nourishment of an animal, but, instead of it, perfect and living animals. For in like manner as in those eggs, while yet in the parent's body, a perfect young one is formed, so the Cotton tree produces in its feeds not only the leaves of the young plant, even while the feeds are yet on the tree, but that part in the feed from whence the root and stem are to be produced, is in this feed uncommonly large.

And in like manner as with regard to the eggs of the insects, I have mentioned, that as soon as the young one has broken the egg, it immediately comes forth and runs, creeps, or swims about; so the leaves in this feed having received the moisture requisite for their growth, do swell and enlarge themselves until the thin skin wherein they are inclosed bursts or cracks, and the leaves then expanding, that part whence the root and stem will proceed grow to a sufficient length to draw its nourishment from the earth. In a word, some insects are perfect in their eggs before they are voided by the parent, and the plant in the feed of the Cotton tree is perfect, and requires no nourishment to be provided for it when it is of sufficient maturity to quit the tree.

I cut a slice transversely from that part whence the root and stem derive their origin, and caused a drawing of it to be made from the microscope, for no other reason than because the internal part containing the vessels, which in others is of a round or oval figure, is in this a figure of eight sides, four of which are somewhat curved, as at *fig. 25*, A B C D, which was full of pores: the part surrounding it, which appears at E F G H, was very white, and in it I could not discern any pores. The outward part or compartment, I K L M, both in pores and in its white colour was similar to the internal one, with this difference only, that the pores in the inner one were somewhat larger. And though the greatest

part of the feeds which I examined were very old, yet I found that this young plant contained in it a considerable quantity of oil. Some other feeds I opened, in which the young plant was so fresh that I thought I could make it vegetate, but I have yet met with no Cotton feeds which I could by any means cause to grow.

I afterwards saw, in a curious gentleman's cabinet in this town, specimens of two other Indian feeds, in which the leaves and root of the future young plant were extraordinary large, but no oil or mealy substance, so that we find there are some feeds which contain only the young future plant or tree.

If any one is desirous of seeing the young plant in great perfection, as it lies in the mealy substance of the feed, let him examine the feed of the Lime tree * when ripe; for in this feed are to be seen two leaves not flattened nor folded up, but of a perfect handsome shape, the same as a young leaf on the tree; and through the microscope the vessels or fibres in the leaves are as plainly to be seen, as with the naked eye we can see them in a full grown leaf: the origin or first rudiment of the young plant, which in this feed is extraordinary large, is placed in the contrary direction from the parent tree, whereas in other feeds it points towards the tree: this young plant, before it comes to its full growth in the feed, is not of a green colour, but when full grown it becomes of a bright yellowish green.

* A figure of this is given in Baker's Employment for the Microscope.



ON THE GENERATION OF EELS.

IT is the common opinion in this country, that Eels are produced without the ordinary process of generation; a notion which I could never conceive, as I have often declared; and I will say, that if this were true, there is no reason why Eels should not be produced in such quantities, as in a manner to fill all our canals.

Nevertheless, this opinion is not only entertained among the vulgar, but I have found some respectable and learned men inclined to favour it; and they have gone so far with me, as to assert, that they knew in what manner Eels were generated, which they imagine to be as follows:

If in the month of May, two turfs of grafs be taken and laid on each other with the graffy sides together, and before sun-set be placed in the water, so that the grafs of those turfs be even with the surface of the water, and if the dew fall copiously that evening, upon taking up the turfs, the following morning, several minute Eels will be found in the grafs, which they suppose proceed from the dew; in confirmation of which they add, that if no dew has fallen, there will be no Eels found.

But upon examining this matter, we must consider, that in very windy weather no dew falls, and small Eels in cloudy weather, get to the bottom of the water: but the warmer and calmer the weather, the more is the dew that falls: moreover, these minute Eels in warm weather, swim or creep among the leaves and greens on the surface of the water, and do most probably, at the same time, creep among the blades of grafs near the surface; and this, I suppose, has given rise to the notion of their being generated in this manner.

In farther reflecting on this subject, it occurred to me, that I had often in summer time, seen boys by the sides of the ditches round about this town, with small twigs, which they dipped among

the leaves swimming on the surface of the water, and, twisting the twigs round, so as to lay hold of those leaves and draw them out of the water, they pulled out with them many minute Eels; and among those, I have seen many young Eels of different sizes, some of them remarkably minute and smaller than we ever observe the young fry of fishes which are produced from eggs; and this confirmed me in my opinion, that Eels are not always bred in the same month or season of the year.

In order farther to investigate this subject, I, at several times, dissected some large Eels, examining particularly that part of the belly where one expects to find some appearance of young, (and I am well assured, that Eels of every kind, without distinction, do breed young ones), in the intestines of which I found many different kinds of animalcules; and afterwards I examined some Eels without dissecting them, by compressing the belly near the aperture into the intestines: from one of these there issued a small quantity of thick liquor, which being diluted and placed before the microscope, I plainly saw that it contained animalcules, which in shape, length, and thickness, exactly represented Eels, but so minute, that in my opinion, they were not a fiftieth part the thickness of a hair.

After this, upon reflecting that Eels might probably breed in the winter time, I directed a fisherman to bring me some every week, beginning in the month of February.

The first of these I dissected, and upon examining the parts where I supposed the young would be deposited, I found nothing but some very minute globules, somewhat less than the globules of blood, and of these some were of an oblong shape: proceeding in my observations, I found these kind of globules more and more extended in length, till I saw them of the perfect shape of Eels, and at last I extracted a great number of minute Eels, lying together in a transparent liquid, which, through the microscope, appeared as perfectly formed as minute Eels viewed with the naked eye.

This spectacle gave me great pleasure, partly because, after taking so much pains, I had now discovered the manner of the propagation of Eels; and partly, because this was a complete answer to those who said behind my back, " Since Mr. Leeuwenhoek is endeavouring to establish the regular generation of all animals, let him shew us in what manner Eels are bred."

The sight of these minute and perfect, though unborn Eels, which were, as I have before said, not a fiftieth part the size of an hair, I exhibited to several of my acquaintance, who wondered to see such minute creatures so completely formed.

I pursued my observations on this subject till the months of August and October, till I could barely see these minute Eels with the naked eye, and I judged that they might at that time be brought forth into life by the parent.

It is by some asserted, that from the skins of Eels thrown into the water small Eels will be produced. This may perhaps be true, though not, as they imagine, from the corruption or putrefaction of those skins, but in the following manner. Let us suppose that among a score or more Eels which have been skinned, one of them may have some minute young ones in its belly, which, by the pressure in the operation of skinning, might be squeezed out and remain adhering to the skin; those minute Eels, when thrown into the water with the skin, may there find food and grow to their full size; and by this means, if Eel-skins are thrown into a newly made canal, Eels may thereby be bred therein.



*On the multitude of eyes or optical organs in the eye of a Beetle.
The optic nerves of each of the optical organs in the eye of a
large Fly, particularly described: also the brain of a Gnat; and
the nature and probable use of the hairs on the feet of Flies
and Crab-fish.*

I HAVE formerly made mention of the great number of optical organs, or eyes, with which small flying animals, such as the dragon-fly,* or libella, the common fly, and others, are furnished; and I have often shewn those eyes to some curious gentlemen, when they came to visit me, to their great delight, particularly when they found that objects might, with the greatest clearness, be discerned through each of those optical organs, to the number of several hundreds at a time.

Among other instances, I, upon a certain occasion, exhibited to several English gentlemen of rank, the great number of optical organs which are found on the tunica cornea, or horny part of a Beetle's eye; at which sight those gentlemen were astonished, and the rather, as it is a kind of proverbial expression in England, when they would reproach any person with blindness or stupidity, to say, "He is as blind as a Beetle," under a notion that that animal is void of sight.

With regard to this creature, I began by separating from the head that part which is commonly called its eye, and, having cleared away the blood vessels, and other matter adhering to it, I placed it before the microscope, and then I saw that the protuberance, or rising of this eye, was not a perfect hemisphere, being rather more extended in length than in breadth.

* This creature is vulgarly called, particularly by children, an horse-flinger; but very erroneously, for it has not a sting, nor any weapon of offence, that is discoverable by us.

I also made a computation, as accurately as I was able, of the number of optical organs which were to be seen, in a row, in the largest segment, or longest side of the eye, and I found the number amount to sixty; and if we suppose that, in the smallest segment, or narrowest part of a Beetle's eye, there are forty such optical organs in a row, these sixty and forty, added together, make one hundred; the half of which, being fifty, is what we must take to be the number of eyes in the hemisphere which composes a Beetle's eye, if we consider such eye to be of a spherical shape.

Now, if we suppose that the two eyes of a Beetle do, together, make up a perfect sphere, and that in a great circle* on such sphere, one hundred optical organs are placed, we shall find, according to this rule laid down by Metius, "As 22 is to 7, so is the quadrature of the great circle to the contents of the superficies," and the rule being applied as follows:

22 — 7	10000	100 the great circle.
	7	100
	22) 70000 (3181	10000 Quadrature.
	·40	
	180	
	·40	
	18	

the result is, that if each eye in a Beetle's head is taken as an hemisphere, the two will make up a sphere containing, on its surface, 3181 optical organs or eyes.

I have caused a drawing to be made of the eye, or rather of the

* It has been mentioned in another place, but is here repeated to save the trouble of turning back, that by the great circle is meant, the largest which can be drawn upon a globe, or sphere, or, in other words, a circle, whose diameter is a line passing through the centre of such sphere to the surface.

multitude of optical organs composing the eye of the Beetle, as far as the limner was able to discover their structure by the help of the microscope, with intent, partly to exhibit the great number of those optical organs, and partly to shew that each of these organs is of a convex shape: not that I would be understood to mean, that every one of these optical organs is a portion of a sphere;* for, if so, the Beetle would not be able to discern objects at any distance, but that each of them is more flattened than spherical.

Upon exhibiting this object to the limner, he compared these convexities, or protuberances, to those round buttons which are at present used in men's clothes, and made of that metal commonly called Prince's metal.

Plate XIV. *fig.* 1, A B C D E F, represents a part of the tunica cornea, or horny part of the Beetle's eye. A B C denote that part which was contiguous to the head. D E F A represent the greatest part of the longest row of eyes on the superficies of the tunica cornea; in which, from D E F to A, I counted more than sixty optical organs; and, between the same letters, may be seen how each of these optical organs rises with a kind of protuberance.

It is well known, that when any object is placed before the microscope, it must be so adjusted that it may neither be too far removed from, nor brought too near to the focus of the glass, for, in either of those cases, the object cannot be seen distinctly. For the same reason, if any person is desirous to contemplate objects through those optical organs which are in the tunica cornea of the Beetle, he will find it necessary to allow a somewhat greater distance between the tunica cornea and the focus of the magnifier, so that the focus of each may (if I am allowed the expression), become united in one point, as we do when two convex lenses are placed one before the other in a frame: and, by this means, he will see the object multiplied several hundred times, by reason of the great

* I address myself here to those who have some knowledge in optics.

number of optical organs in the tunica cornea, but all wonderfully minute ; for the steeple of the new church in our town, the size of which, and its distance from my house, I have mentioned elsewhere,* when viewed through these optical organs, appeared no larger than the point of a very small needle.

Hence, it appears, how greatly those persons are mistaken who say, that a Beetle is blind ; and how much perfection is to be found in the organs of sight of so small and so despised an animal, to say nothing of the other parts of its body ; but which creature, whoever sees, immediately crushes with his foot, as loathing the sight of that black creeping thing.

In the month of August I saw, sitting on a glass, at the backside of my house, a Fly almost as large as a bee, which species of Fly, though not very numerous, I observe every year in the same place.

I dissected the tunica cornea of both eyes of this Fly, and, on examination, I found them to be covered with a great number of wonderfully minute hairs, which did not cover the organs of sight, but were placed in the intermediate spaces between them.

Moreover, I took out of the inside of the tunica cornea, that matter or substance with which it was filled, in order to examine it by the microscope, because, till that time, I never could clearly satisfy myself to what end this substance was created, and the rather so, as, upon viewing it, I judged that it consisted of a collection of threads or fibres. Upon spreading this a little asunder, to examine it more accurately than I had before done, I saw that all those particles which I had before considered as a collection of threads or fibres, were nearly of the same length, but one of the ends of each somewhat thicker than the other, and the thicker end rounding at the extremity.

Upon repeatedly, and more carefully, examining this spectacle, I was, to a certainty, assured that every one of that great quantity of

* See vol. I. Essay on the Silk-worm, page 62.

particles like threads which presented themselves to my sight, were no other than optic nerves; and that the larger and rounding extremity of each of these nerves was destined to occupy the small cavity of each optical organ on the inside of the tunica cornea; so that in a word, I may venture to pronounce, that every optical organ seen in the tunica cornea is provided with its particular optic nerve.

As to the other extremities of the optic nerves, which, being situated towards the inner part of the head, are smaller in size: this must, I think, necessarily be the case, partly because the tunica cornea dilates itself in a spherical form, whence it necessarily follows, that the optic nerves as they tend towards the internal part, must by degrees grow smaller, and partly because the place where they terminate must be smaller than the space within the tunica cornea. And who knows, whether that part in which the optic nerves so terminate, may not be the brain itself, not yet discovered?

In order the better to satisfy the curiosity of others on this subject, I placed some of the optic nerves of this fly upon a glass, before the microscope, which I delivered to the limner, in order that he might make as accurate a drawing of them as he was able.

Fig. 2, G H shews two of these optic nerves, the part H was placed near to, or rather within the cavity of the optical organ; the other end G, towards the internal part of the fly's head.

In *fig. 3*, at I K L are seven of those optic nerves, the larger ends of which were also placed next the tunica cornea.

Fig. 4, M N O P Q, shews a great number of optic nerves heaped together, the upper ends of which, as seen at the letters N O P, were placed next the tunica cornea. When these nerves are seen in such heaps as here pictured, their true length cannot be distinguished, but when lying together in a small number, and a moderate sized parcel, the light may easily be seen through them, they being somewhat transparent. All this is shewn in the figure.

I have in the course of my observations discovered, that all the

minutest particles in the fibres of fishes, when at rest in their natural state, are, as it were, contracted in various annular folds or wrinkles; but when put in motion or extended for any purpose, then all those wrinkles and annular foldings are opened or smoothed, and the fibre becomes longer.

The same kind of annular wrinkles I also discovered in the optic nerves of the fly now under consideration; whence we may conclude, that every one of these optic nerves is endued with the power of alternate extension and contraction equally with the optic nerves in a human eye. For it must needs be, that when we move our eyes from one side to the other, their optic nerves will be more extended than when we look straight forwards.

Having made this observation, I asked the limner whether he could follow with his eye those circular marks or lines in the optic nerves, and, upon his answering, that he could very plainly see them, I desired him to imitate them as nearly as he was able in his drawings, and this is to be seen in *fig. 2*, and *3*.

Now if this fly had been dead sometime before I had dissected the eye, and taken out the optic nerves, I am very sure, that in that case, I should not have discerned any of those wrinkles or contractions. In like manner we daily observe in fishes, that after they have been dead sometime, their fleshy parts, if cut asunder, do not become shorter, which our people call *krimpen**, and consequently those parts of the fish are neither so firm, nor so grateful to the palate, as when they are cut asunder while the fish is alive.

Upon discovery of so many wonders, and of such perfection in the eye of a fly, we are again compelled to cry out; How little do we know? And if this is seen in the eye of so large a fly, it must

* *Krimpen* in Dutch signifies to contract or gather up in folds, whence the phrase to crimp fish is plainly derived; which cruel practice seems also to have been imported from Holland. And it must be confessed, that with all Mr. Leeuwenhoek's abilities as a philosopher, he seems not to have had much sensibility of compassion for the creatures which he subjected to his examination.

be understood, that the same obtains in the smallest, and which I have seen in the dissection of common flies.

At the time the limner was making these drawings, I took a small Gnat of that sort which is not troublesome to us, having no sting: the head of this Gnat I cut off, in order to extract its optic nerves, but I could not, after several trials, see them distinctly. While I was busied in this attempt, I frequently saw the brain in the Gnat's head surrounded with a great many vessels, which I judged to be blood vessels: and though it should seem scarcely possible to extract the brain from this creature's head, without destroying the texture of the brain and its vessels, yet, I at length succeeded to my wish, and, without their being much injured, placed the whole before the microscope; this I delivered to the limner, that he might make a drawing of it, and the rather, as I had been told by a gentleman of eminence, that a certain person, whenever my discoveries were the subject of conversation, was accustomed to say, that the experiments stated by me could not possibly be performed; because (as that same person pretended), the instruments used by me, however exquisitely constructed, could not be at all capable of performing the dissections I described. But I little regard these malevolent insinuations of mankind, and perhaps this very person is one of those who would be glad, if he was able to perform the like operations.

Fig. 5, R S T, represents the brain taken out of the head of this Gnat, and also the vessels, which in part surround it, and in part pervade the substance of it, as nearly as the limner could follow them in his drawing; for while he was employed about it, he frequently declared that he could by no means represent in the drawing all the vessels which he saw.

The flies before mentioned, have the extremities of their feet covered with an incredible number of hairy parts, by the help of which they are better able than other flies to climb up a glass though it be ever so free from impurities or irregularities, of which they might take hold. I have therefore often placed the feet of those flies before

the microscope, in order to view the means by which they can fasten themselves to the glafs and run up it; and I have, for some years past, thought that I could discover that these hairs, were each of them provided with crooked parts like hooks, by the help of which they can take the firmer hold on glafs, but which parts have never, to my knowledge, been described by any person, though the figures of those hairs may be seen in many authors.

In further prosecution of this enquiry, and revolving the subject in my mind, a thought occurred to me respecting those large crabs, which are sometimes brought to this town for sale, and are, as I am informed, caught among the rocks in Norway; but not that I would at all compare these crabs to flies, or that kind of animalcule, otherwise than by analogy, for the feet of those crabs are covered with many hairs. I therefore determined to inspect them, and examine the hairs, particularly those on the hind feet; for those feet are not furnished with claws, or other weapons, to grasp hold of any thing like the fore feet, but only with one straight claw, and with many short hairs.

Upon placing these hairs before the microscope, I saw, not without admiration, that many of them were provided with a double row of parts like teeth, placed in very exact order beside each other, in like manner as if we were to imagine the back of a knife cut into a double row of teeth or notches. I have caused a drawing to be made of one of these hairs, in order to shew its wonderful formation, which I am persuaded is intended for this purpose, that when the crab is climbing up the rocks, he may be enabled by this assistance to fix his feet firmly on the rocks or stones.

Fig. 6, M N O P Q R, represents this hair, in which those tooth-like parts are very plainly to be seen, but only one side of the hair is here visible, and these teeth in the middle of the hair are longer, as from *Q* to *P*, and from *P* to *R* they grow gradually shorter.

Fig. 7, S T, represents a part only of the hair, but in such a position that the two rows of teeth in it are conspicuous.

Upon this subject of crabs, although I have sufficiently exhibited to the learned world, the circulation of the blood in various animals, and have demonstrated that it is uniformly and regularly performed after the same manner in all, yet, as I have met with persons who did not hesitate to deny this to be the fact, I will here relate my observations on two small live crabs, about an inch in diameter; and, judging that the extremities of their two smallest, or hind feet, which were very broad and thin, would be proper objects to discover the circulation, I placed one of them before the microscope, and immediately saw an incredible number of particles of blood, which appeared to be globules; these, though not red, were of a darker colour than the liquor they floated in, and they were running along a blood vessel which might be called a vein, with such swiftness, and so great was the number of particles, that it could not easily be conceived but by an eye-witness. In fact, I cannot compare the appearance of these round particles of blood to any thing better than by supposing we were to look through a large opening, or a window, at a fall of snow, violently agitated by the wind; nor do I remember that I ever saw the blood driven through the vessels with such swiftness. It was also a most delightful object to behold this large blood vessel, on each side of it, crossed by smaller vessels, in which the blood was driven forward with equal swiftness.

Upon changing the point, or place of view, I there saw the blood in equal agitation, and in no less a number of vessels.

Moreover, I happened to see a vessel in which the blood could not proceed in its regular course, but yet was in continual motion to and fro, and all within a space not larger than the thickness of an hair; whence I concluded, that, in the smallest branches, where this vessel arose, and in which the circulation was completed, the blood was stagnated, and thence its regular course in this vessel impeded.

The pleasure I derived from this spectacle was so much in my mind, that I was induced to a farther examination of the subject, and

to bestow still more attention on it, by examining the hind feet of many small live crabs. In one of these I saw, by the microscope, not only a large artery, about the size of an hair, which, before it came to the extremity of the foot, divided itself into several small branches; but I also saw a great number, both of arteries and veins, crossing each other; and, on turning my eye a little towards the sides of the foot, I there saw the blood poured into so many minute vessels, that the substance which we should call the fleshy part of the animal, lying between the skin or shell of the foot, seemed to consist of nothing but blood vessels; so that when I shewed this circulation of the blood to a certain learned physician, he frequently exclaimed, "O how wonderful is this!"

The pleasure which this object afforded me was the greater, because the globules of blood, driven through all the vessels, being fewer in number, were much wider asunder than the globules in the blood of terrestrial or aquatic animals which are of a red colour; so that, I am well assured, the globules in red blood are twenty-five times more in number than those, in the same space, in the blood of a crab.

After I had several times repeated my observations on these minute crabs, and my sight was in a manner wearied with the spectacle, however pleasing, I had a mind to see the blood when drawn out of the vessels, and the rather because the serum or fluid in which the globules were swimming, was so very transparent, that it would be impossible to discern the circulation without the assistance of those globules.

The artery in this crab's foot was too small to be opened in the ordinary way; I therefore determined to cut off a small part at the extremity of the hind foot with a pair of scissors, and then to collect the blood as it first issued forth, and place it before the microscope. This being done, I saw in the small portion of blood which issued from the artery, the globules of blood for some time continue their circular motion.

Moreover, I examined the artery which I had thus cut afunder, in that part of it which was near to the animal's body, and where it was also divided into many smaller branches, in order to see whether the circulation was there continued, and with great pleasure I saw, that the course of the blood was not interrupted, only it proceeded with a somewhat slower motion.

Hence I plainly perceived, that when any part of a limb is cut off, the circulation of the blood will continue in the vessels that are not wounded, especially if the effusion of blood from the vessels which are cut afunder can be stopped. And we may also, from this experiment, be well assured, that there is no artery, which is not throughout the whole of its course divided into smaller branches, whereby it by degrees becomes smaller, until the very smallest ramifications return back towards the heart, and constitute what we call veins.

A portion of the blood taken from this crab's foot I examined by the microscope, to discover, as far as I was able, of how many parts each globule of blood was composed; and I concluded that each globule was composed of six distinct globules, as in the human blood: moreover, I saw that the globules of crab's blood, when brought into contact, coagulated in the same manner as the blood in an human body, though, till this time, I had been of opinion that the blood of fishes would not be disposed to coagulate, because we do not observe any heat in it.

I exposed a portion of the thinnest part of this blood to evaporate in the open air, in order to see whether, in that thin part called the serum, I should discover any particles; and, in doing this, I saw so many particles, which, to my eye, appeared spherical, that the whole substance seemed composed of them and no other, and, upon stirring the substance with the point of a needle, those particles came still more distinctly into view.

Moreover, I discovered a great number of saline particles, which, upon the least application of heat, coagulated in irregular parts.

But in several places I saw a great number of saline particles lying near together, of a more regular shape, and having six sides: but, with all the attention I could bestow, I did not discover a single particle in crab's blood resembling the figure of our common salt; so that it is plain, to me, that the sea salt does not enter into the substance of the bodies of fishes.

I have caused a drawing to be made of this crab, in order to shew the species wherein I saw the circulation performed in so many vessels, and which is well worthy the attention of the curious.

Fig. 7, A B C D E F, represents this crab, and at A B and E F are shewn the two external joints of each hind foot, in which I have said the circulation can be seen in so great a number of small vessels.

I then examined the parts which to the naked eye seem hairy, and with which hairs the joints A B and E F are covered, in order if possible to discover the circulation there; but the vessels composing these hairy parts were so minute and slender, that not a single globule of blood could pass through them, and consequently it was impossible for me to see the circulation in those parts.



Of certain animalcules bred on the leaves of the Willow, and which produce the knobs or excrescences frequently seen on those leaves.

THERE is a sort of Willow in this country, the leaves of which are of a deeper green, and of a larger size than others of that tree; these Willows are for the most part planted for the sake of their young shoots, which are used in husbandry, and gardening for binding hedges, fences, and branches of trees together, by reason that they are very flexible, and at the same time very tough; they are therefore called by us *Zeem-teenen*, which may be expressed in English, by the words leathery twigs.

Upon the leaves of these Willows, I have often observed certain knobs or risings, and having gathered some of those leaves, and opened the knobs, I at several times found that they contained more than one kind of worm or maggot; but none of those I at first saw, appearing to be full grown, I opened some others of the knobs very gently, and, where I saw a maggot inclosed, I stopped up the opening I had made, and put the leaf into a glass tube, that the animal inclosed might there perfect its growth, and undergo its change of state: this however I did not see any of them arrive at, though at the same time, I had observed many of the knobs vacant, and with a small hole in them, through which the maggot had issued, leaving the cavity within, partly filled with the excrements it had voided.

Plate XIV. *fig.* 8, A B C D E, represents one of these Willow leaves, on which are seven of the knobs or excrescences I have mentioned: in some of these a hole is to be seen, as shewn at the letters

F G H ; at the letter K appears a knob, wherein, upon opening it I found a maggot.

I also observed many dead maggots lying among the knobs, and upon searching after the cause of this appearance, I found at length a small maggot sticking close to one of a larger size, which smaller maggot seemed to have no power of moving itself from place to place, each extremity of its body, particularly the hind part, terminating in a kind of point. This maggot, with its mouth, had pierced into, and was fixed on the body of the larger one ; and from this and many other instances which I saw of the like kind, it appeared to me, that this small maggot was supported by preying upon the larger one ; and hence I considered that were it not for food drawn from those or the like kind of maggots, these smaller ones would be destitute of subsistence ; for they are not provided with any limbs or organs by which they can fasten themselves to the leaves of trees.

But how these last mentioned maggots could find their way into the internal parts of the knobs or excrescences was strange, and to me entirely undiscoverable, otherwise than by conjecture. I suppose, first, that the larger maggot was produced from an egg laid by some fly on the leaf of the Willow, and upon its biting or gnawing the vessels in the leaf, a liquid substance issued forth, surrounding the maggot on all sides, and in hardening formed the knob or excrescence we observe on the leaf : sometime afterwards another, and a smaller fly might place itself on the knob, and by means of some weapon or piercer, bore a hole in the knob and lay therein an egg, from which egg, the smaller maggot might be produced.

About the middle of July I repeated my observations on these maggots, and found many of them almost arrived to their full growth : and some actually changed into aurelias or crysals* : I also found

* The translator has taken the liberty to express the plural number of the word crysalis by this termination, instead of adding another s to it, with an apostrophe between the letters, as is sometimes done.

some of those minute voracious maggots, (voracious I may justly call them, since they prey upon a maggot not less than fifty times their size), which I judged were so far grown, that without taking any more food, they would soon be changed into flying animals.

I had, at first, no doubt that these aurelias or cryfales were of the same nature as those of common flies, namely, that when the maggot is about to be changed into a cryfalis, its skin contracts, and by such contraction becomes more firm and solid, and serves the animal for a shell or case during the progress to its change, and then we call it an aurelia or cryfalis, though it be not wrapped in such a web or spun case, as we observe many other flying insects to be; but upon opening these cryfales or aurelias, I found the maggot within them in its original shape, although it had lain in the aurelia state a fortnight. And, upon viewing these things by the microscope, I found I had been mistaken in my former opinion, for the shell or case which I have called a cryfalis, was neither composed of a web nor of the skin of the maggot itself, but I was obliged to conclude, that it was formed by a concreted substance issuing from the leaf of the tree, for it had the appearance of fibrous or branched parts, from which there issued smaller ramifications. And I did not find a cryfalis in any of the knobs I opened, excepting in those which I had kept in glasses: one of these cryfales, as it lay in the knob, is shewn at L.

After some weeks I observed, that from these cryfales certain blackish flies were produced, which at their tails had some oblong parts formed in the nature of stings, and another sort of flies, rather of a smaller size, but without any such appearance of stings.

Fig. 9, represents one of these flies the same size it appeared to the naked eye, and at the letters CD is shewn that part which had the appearance of a sting, and was two thirds as long as the animal's body. But, upon placing this before the microscope, it did not at all appear like a sting, for it was covered with a great number of small hairs, as shewn in *fig. 10* at the letters AB, and no aperture was to be discovered in it. Whereupon I began to consider that

perhaps this was only the case of the real sting, and, upon opening it, I found a sting, notched at the point like a saw, of which I have given a drawing in *fig.* 11, E F.

When I had attentively viewed this for a considerable time, I thought it seemed to contain another sting within it; I, therefore, endeavoured to open it, wherein I succeeded to my wish, and out of it, at the place marked E F, in *fig.* 11, I took two stings, both of the same make; a small piece of one of them is shewn at *fig.* 12, G H: each of these was jagged like a saw.

Hence it appeared, that those indentings or teeth, represented in *fig.* 11, at E F, did not properly belong to that part, but to the stings enclosed in it; and, when those stings were taken out, it appeared necessary to conclude, that the part which I had taken for the real sting was, in fact, only a second case for the two inner ones: in this second sheath, or case, as drawn in *fig.* 13, is shewn at I K, the cavity wherein the two stings lay; and farther, in *fig.* 12, another cavity is to be seen: all which things, put together, give reason to conjecture that, in each of these cavities, there may be contained some acrid juice of a poisonous nature.

From the foregoing observations, we may easily conceive, that these minute flies do not merely lay their eggs upon the surface of the leaves, but that, with their stings, they perforate the membranes of the leaf, and deposit an egg in the cavity; the maggot issuing from which, in biting the vessels, causes the juices of the leaf to issue forth, which form around it those knobs, or excrescences, I have described.

I observed, among the dead maggots in these knobs, two white oblong particles, but so very minute as to escape the view of the naked eye; these I concluded to be eggs, for there was, in no part of them, any resemblance of a maggot, and, the next day, two maggots issued from them, exactly of the same make with those which I have said preyed on the larger maggots.

After this I took one of those voracious maggots from the body

of a dead one, on which it had been feeding, and placed it on a living maggot, into which it immediately plunged its mouth, and, notwithstanding all the efforts the other made, by extending, contracting, and twisting its body, to shake it off, it still remained fixed to it. One of these voracious maggots, when fully grown, appeared no larger, to the naked eye, than is shewn at *fig. 14.*

I caused a drawing to be made, from the microscope, of one of the maggots I have been describing, when changed into an aurelia or crysalis. *Fig. 15, A B C D E F,* represents this crysalis, which, in the evening was a maggot, and, the next morning, was changed to the figure here represented, and, in its change, had put off an exceeding thin pellicle; and in like manner as the body of it, when a maggot, was composed of many parts like rings, so, when a crysalis, it retained the same annular or infected form. In this creature, not only the feet, but every single joint in them, might plainly be seen.

At the letters *G C* and *D G,* are shewn the two horns, and though these were inclosed in the same thin membrane with the body, yet every joint in them could very plainly be seen, and they were fully formed, and distinct from the body.

This maggot before, and for some time after, its change, was of a perfect white, but, after some days, the eyes, which may be seen at *B* and *E,* and each of them containing many optical organs, assumed a dark colour, inclining to a blue.

I had many of these maggots, and I watched them very narrowly, to discover in what manner their transformation from a maggot to a crysalis was performed; but the change, when it took place, was made in so short a time that I never could get a sight of it.

A description of some species of minute Insects, found in fresh water, in a Letter to Signor Anthonio Magliabechi, at Florence.*

To the illustrious, learned, and famous Signor Anthonio
Magliabechi.

THE very obliging letter with which you have honoured me, (to which were added what has lately been published in Italy by the learned, containing the curious invention of inclosing gunpowder in a bag, and computing the force which such bag would resist before it burst) is come to my hands; and I beg you to be assured that it was most acceptable to me: and for the favour of your letter, as well as the communication therein contained, I desire to return you my grateful thanks.

In reliance on your accustomed friendship, I have been induced to impart to you my observations and reflections respecting some very minute animalcules, hitherto very little noticed, which are found in the waters of this country.

Being employed in searching after certain animalcules, which I expected to find in those ditches or canals which divide our fields, I saw various species of creatures, but none of those which I was then in search of. Among these I observed certain animalcules, within whose bodies I saw so quick a motion as to exceed belief; they were about the size of a large grain of sand, and their bodies being transparent, that internal motion could plainly be seen: Among other things, I saw in the body of one of these animalcules a bright and round corpuscle, placed near the head, and in which a very wonderful swift motion was to be seen, consisting of an alternate extension and contraction. This particle I concluded to be the

* Magliabechi was at this time Librarian to the Grand Duke of Tuscany: he was not less famous for his learning than his peculiarities, of which several anecdotes are related in the *Adventurer*.

heart, and that the rapid motion of the parts round about it, which I observed in this animalcule, and the motion which I had seen in the others, proceeded from the action of the heart.

Upon observing in one of these animalcules eight or nine greenish particles I began to think that those were unborn young ones; to ascertain which I put one of the animalcules into a little water, being the quantity of five or six drops, that I might thereby see the young when newly issued from the parent and swimming in the water: but the next day, finding the animal dead, I opened its body while lying in the water, and then I not only very plainly saw most of the unborn young ones, but I could also discern the organs or limbs provided for their use in swimming.

This animalcule was of such a pretty shape, that I often viewed it with great admiration: indeed, I was so much pleased with its formation, that I thought larger animals viewed with the naked eye would, in comparison with this, appear rudely made.* The death of this creature seemed to me to be caused by want of food, because I had seen it frequently void its excrements, whence I concluded that it required a large supply of nutriment in proportion to its size.

After this I discovered another species of animalcule, almost of the same size with the former ones, but of entirely different make. This animalcule had a long forked tail, and each of the forked parts again divided into four parts, which last parts were provided with various organs.

But observing one of these animalcules, (though of the same species), to appear as if the hind part of its body was formed in a very different manner, I put two of them into a small portion of water, in order to examine them by a microscope of greater magnifying

* This being the case, it seems strange that Mr. Leeuwenhoek has not given a figure of this animalcule: from the account here given of it, and particularly the motion of its heart, it should seem to be of the species called *pulex aquaticus*, or water flea, of which a figure, as seen through the microscope, with a description of the creature, will be found in Baker's *Employment for the Microscope*.

power. By the help of this, I perceived that those parts near the tail, and which appeared like two bunches of grapes, were in fact, the animal's eggs, so placed together, that there were about three or four eggs in breadth, and nine or ten in length, joined to each other, but of a round shape, at each extremity, and the whole bearing the appearance of a bunch of grapes.

The eggs which adhered to the tail of these two animalcules, exhibited a small spot in the middle of them, and round about that spot a pellucid substance; whereas, on the contrary, the eggs of the other animalcule appeared to me of a greenish colour.

The following day some of those eggs, which in the middle had that spot, were missing, and after some hours had elapsed, all the eggs were separated from the tail; at the same time I saw a number of the shells of the eggs swimming in the water and also many minute animalcules of such a size as might be expected to issue from those eggs. But as all these animalcules, though of the same size, did not exactly resemble the parent in shape, I could not thoroughly satisfy myself in regard to them.

Another animalcule I had placed in a glass tube by itself; and the third day after it being so put apart, its eggs began to change, each of them being marked with a spot, and the external parts becoming transparent. The following day, pursuing my observations, I perceived that all the eggs were separated from the tail, and a great number of animalcules of the same size and make as those I had at first discovered swimming in the water, and moving themselves slowly from place to place by striking on the water with certain organs they were furnished with.

At sight of all these things, I was convinced that those animalcules which I had before observed, proceeded from the eggs; but, to be more certain in this respect, I put the parent, with all the young ones newly brought forth, into a clean glass, into which I had poured about the quantity of a cubic inch of water, in order to see whether in that water they would increase in size, and undergo any change of figure: but, upon viewing them again the following day,

I saw but a very few of them, and after about thirty hours from the time of their being first excluded from the eggs, I only saw one of these young animalcules, which was adhering to the glass, and seemed a little increased in size. The next day I could not perceive one of these young animalcules, upon which I began to think that perhaps the parent, for want of other food, might have devoured its own offspring.

Not content with the preceding observations, I afterwards, at three several times, caught some of these animalcules which had eggs fastened to their bodies, and placed each of them in distinct glasses, that so I might be better satisfied as to the nature of these creatures.

Among these I had one animalcule much larger than the rest, and an extraordinary number of eggs adhering to it. As soon as this animalcule had let go the eggs, and the young ones excluded from them were swimming in the water, I killed the parent; that by this means I might the more certainly determine in what manner these creatures acquired their growth and increase; but my glass happening to be leaky, and the water escaping out of it, I was disappointed.

Moreover, I had taken and placed apart another animalcule, half the size of the preceding one, which nevertheless had eggs fixed to its body, though not a fourth part so many in number as those on the former one. After two or three days, these eggs were so far altered, that I judged the young ones would soon issue from them; but the next day the animalcule was dead, and all the shells of the eggs were adhering to its body, though the young ones produced from them were swimming about in the water.

It is well known, that all the fish in our sea or rivers which lay eggs or spawn, and are therefore called oviparous, are, when a year old, able to propagate their kind, being then provided with roes or eggs; and that these eggs are, singly taken, as large in the smallest fish as in the largest of the same species; so that the difference in the size of the roes proceeds only from the greater num-

ber of eggs which they contain. The same is the case with respect to the minute animalcules of which I am now speaking: for the eggs of the smaller animalcules were of the same size with those of the larger ones, though the eggs of one were four times more in number than those of the other.

These last animalcules I placed in a glass filled with water, in which were swimming some other animalcules of the same kind, which were about seven or eight days old; at which time I judged these animalcules to be four times the size they were when first excluded from the egg.

On the 13th of July I had some few animalcules in a beer glass full of water, none of which had any eggs adhering to them; but the next morning I saw a number of eggs, of a greenish colour, on the body of one of them; whereupon I took this animalcule from the others, and put it into a glass tube, about the thickness of a common goose quill, with intent to discover in what space of time young animalcules would be produced from these eggs.

In the morning of the 16th of July, I perceived some animalcules which had issued from the eggs, and others still lying within the eggs: but whereas these eggs had before been joined together in very regular order, now, on the contrary, the entire eggs and the shells of those from which young ones had issued, lay dispersed in a very irregular manner.

In the mean time I had, on the 15th of July, observed another animalcule, having the lower part of its body filled with eggs, though the day before I had not seen a single egg on it: this, therefore, I placed by itself in a glass, and, on the 18th of July I saw all the young animalcules come forth from the eggs, and swimming about in the water.

From these observations I was certain, that all these animalcules brought forth their eggs in the space of a day or a night, and placed them on the hind parts of their bodies, in the exact and re-

gular order I have mentioned, and that, after three days more, the young within the eggs were perfectly formed.

These last-named animalcules did not live above three or four days, whence I conjectured that they died for want of food.

I determined to give a drawing of one of these animalcules, with the eggs adhering to its body, as nearly as the limner was able to represent it; for when I took one of them out of the water, none or a very few parts of it could be plainly distinguished, so that it was necessary to make the drawing while it lay in the water.

Plate XIV. *fig.* 16, A B C D E F G, exhibits this animalcule, as it lay with its back next the limner. The letters C H I and E K L represent four horns with which the head was furnished.

At the letters B M and F N are to be seen the eggs which the animalcule, having brought forth, had placed on the outside of its body about this part.

A O and G P, are the forked parts of the tail, which round about their extremities, shewn at the letters O P, were each again subdivided into four parts, which might have been taken merely for hairs; but when accurately examined, and with a good magnifier, then each of these parts (marked in the figure O Q and P R) appeared to be covered with a great number of wonderfully minute particles.

When the animalcule lay in this position, or even with its belly towards the limner, those limbs or organs with which it moves itself in the water, could not be distinguished. I, therefore, placed it so that its side might come into view, and that thus the limner might copy them to his best in the drawing.

Fig. 17, exhibits, at the letters *a b c d*, those organs or limbs, each of which consists of eight parts, though they could not all be here shewn; and each of these is composed of various organs, part of which may be seen at *fig.* 18, at the letters *e* and *f*.

This animalcule did not use these limbs in the same manner as

terrestrial animals which have many feet, nor as those in the water do for the most part, that is, by moving the feet successively one after another ; but this creature, with great swiftness, moved all its limbs at the same instant, so that with a single stroke it moved very quickly and made much way in the water.

Here we find another convincing proof, with how many perfections so minute an animalcule is endowed, as well in regard to the structure of its body, as to its innate disposition, or the instinct whereby it is prompted, when the eggs are fully grown within its body, to place them on the outside of it, and to the intent (as seems to me) to protect the eggs from being injured or devoured by other animalcules ; and that the young ones, when in a perfect state, are able to disengage themselves from the eggs. And who can discover all the farther perfections with which so minute and (to us) insignificant a creature may be endowed ?

When, therefore, we see these wonderful properties in so small and, to us, contemptible a creature ; and, moreover, figure to ourselves in imagination what farther we can suppose concealed in it, can any one with reason suppose that so many perfections are produced spontaneously, or by chance ? But we shall be a thousand times nearer the truth, if we lay it down as an axiom or indisputable fact, that all the living creatures we behold at this day, however minute, derive their origin from those which were formed at the Creation.

I have frequently declared this to be my opinion, and should not now have touched upon the subject, were it not that I find, by experience, most men still adhering to the old opinion, and the follies of antiquity, that many minute animalcules are produced spontaneously.

But if we see so much perfection, as well in the make of the body as in the aptitude to propagate its kind, in the animalcule of which I have been treating, we may, by parity of reasoning, conclude, that the same perfection must necessarily exist in the minutest fishes

or animalcules, even in those whose whole bodies are slenderer than those very slender particles with which each of the four parts (marked in the figure by the letters O Q and P R) are covered. For, in a word, the animalcule I have just been describing may be considered as an elephant, if compared with the most minute of those fishes or animalcules which may be discovered in all waters.

If it be then asked, to what end such exceedingly minute animalcules were created, no answer can readily be given which seems more agreeable to the truth, than that, in like manner as we see constantly, that the larger kinds of fish feed on the smaller; as, for example, that the cod fish preys on the haddock and other smaller kinds of fish; the haddock again on the whiting; these on still smaller fishes, and among the rest on shrimps; and shrimps on still more minute fishes; and that this gradually prevails among all the kinds of fish: so that, in a word, the smaller are created to be food for the larger. Again, if we consider the nature of our sea, abounding with fish, yet having nothing at the bottom of it save barren sand: stored with various shell-fish, yet destitute of every green herb; and if we, moreover, lay it down for a truth, that no fish can be supported on water alone, there will not remain a doubt, that the smaller fishes are destined, by Nature, to be the subsistence of the larger.

You will excuse, illustrious Signor, my boldness, in taking up your time with my remarks, of little value, since you can employ yourself in much more useful studies than to read the trifles I commit to paper.

I remain, &c.

Delft, in Holland,
the 16th October, 1699.

A. van Leeuwenhoek.

ON THE MAGNET OR LOADSTONE.

HAVING at several times examined the Loadstone, without committing my observations thereon to writing, I at length determined to make and note down a series of regular observations on the subject.

I took two Loadstones, each of them weighing some pounds, and suspended them to a balance, in order to see what tendency they might have to iron: I then brought a piece of iron very near to the Loadstone, but I did not perceive that the balance to which it was suspended, did at all move from its position.

After this I took one of the before-mentioned Loadstones, in order to see how much weight of iron it was capable of lifting. The first trial I made, was with the key of my house door; this the Magnet would not lift, but after I had filed off a little of the key from that part where the Magnet was to be applied, that by this means the fatty substance, left by repeated handling on the key, might be cleaned away, then the key adhered to the magnet. Upon observing this, I no longer wondered that the Loadstone, when it was before applied to the iron, had not shewn any attractive power, as if it had not much virtue, because I considered that this balance had been put to all sorts of uses in weighing.

I put a piece of this Loadstone, about the size of a filbert, into a glass tube, and closed the orifice of the glass by the help of fire. I then brought the Loadstone near to a sea-compass, and I found that the needle of the compass was as much attracted by the Loadstone, as if it had been on the outside of the glass.

I then broke the glass, and taking that part in which the Magnet was placed, I closed both ends with a blow-pipe, leaving so much cavity or portion of air within, as, in my judgment, would be sufficient to keep the glass and the Magnet in it from sinking in

water ; and by this proceeding I hoped to discover, whether the Northern part or pole (as it is called) of the Magnet would point towards the North.

Plate XIV. *fig.* 19, A B C D, represents this glass ; at E F is the piece of Loadstone which I placed in the middle of the glass, that neither of the extremities might be depressed in the water more than the other. I then put the glass into an earthen vessel filled with rain water, and I presently saw the North pole of the Magnet move towards the Northern side of the vessel ; yet sometimes the glass and the Magnet in it took a contrary direction, but which, I think, proceeded from hence, that as I often took the glass out of the water, I might, in replacing it, put the North Pole of the Magnet higher or lower than it should be, and therefore that it might not uniformly move the glass in the same direction.

I also bespoke some small steel needles, such as are used for sea-compasses, and both extremities of these, namely, the North and South, were rubbed with a Loadstone by a man who made it his business to prepare sea-compasses. These needles, I put into glass tubes, closed at the ends, and of a size just to float, and putting them into the water, I presently saw the extremities of the needles tend towards the North and South ; but the least particle of dust, or breath of air meeting the tubes did, I thought, impede their motion.

But finding, that by reason of the length of these tubes, the Magnet very difficultly and slowly exerted its influence on the tubes to move them in the water, I broke the glass again, and prepared another of such a shape, that the Magnet might put it in motion more speedily : the ends of this glass I also closed with a blow-pipe.

Fig. 20, G H I K L M N O, represents this glass ; the letter P indicates the place where the Loadstone was put, and by heating one end of this glass, I bent it in the direction L M, that its dis-

ferent motions in the water might be more easily distinguished. This glass I put into a very large and broad beer-cup, in which it sunk in the water as deep as the place marked K: the point M immediately veering about to the North-east part of the heavens, and as often as I altered its position, it returned to it again.

I had also several fragments of the before-mentioned Magnet, one of which weighed no more than five grains; others of them, somewhat larger. I took three of these small pieces or particles, and prepared for them three small glasses of the shape represented at *fig.* 21, Q R S T, in which, at V, the particle of Loadstone is to be seen. The part Q R was added for no other purpose than to keep the glass upright in the water. Two of these small glasses were so made, that no more than the part S T appeared above the surface of the water; the third was somewhat more elevated.

Upon putting these glasses into the water, the extremity or part, T, of one of them turned itself towards the West; and as often as I changed its position either way, it rested not till it had returned to its first position; and even when, with a quicker motion, I turned the cup round from the South towards the West, so that the part, T, almost pointed towards the North, that part immediately turned back to the West, and there rested. The second glass pointed its part, T, towards the East; and in whatever direction I moved it, it would not remain at rest, except in its first position.

I was greatly surpris'd to find, in these experiments, that such minute particles of Loadstone, closely shut up in glasses, and moreover immersed under water, would yet point to that part of the Heavens whereto they were inclined.*

* The uniformity of Nature's operations, in cases analogous, will be seen exemplified, or comparing these appearances in the Loadstone with what has been observed by naturalists in

The third glass being put into the same cup with the two former, I turned it in various directions, but it did not uniformly settle in the same position; for its part, T, sometimes pointed towards the South, sometimes towards the West, and then would incline a little from the West towards the South. When I first saw this, I began to conclude that the fragment, though broken off from a Magnet, might yet be destitute of North and South poles, and, therefore, had no power to put the glass wherein it was placed in motion. But soon afterwards rejecting that idea, I thought that perhaps the North and South poles in this fragment might lie so as to point one of them directly upwards and the other downwards; and that if so, they could not by any means put the glass in motion. I, therefore, several times took this small glass out of the water, and shook it up and down, that by such shaking the poles or corners, if they lay up and down, might assume on horizontal position. And, after frequent trials, I found that at length this glass took a certain direction, and kept that position, in like manner as I have related of the others.

Moreover, I took an iron key, and brought it near to the glass, while swimming in the water (but so as not to let the key touch the water) in order to see whether this small piece of Loadstone, thus inclosed and under the surface of the water, would be put in motion by the approach of iron: but I could not observe the least motion. Nevertheless, upon bringing a thick and long piece of iron very close to the water and also to the glass, the glass inclined a little to one side: the same I also observed in the larger glass tube, which is pictured at *fig. 19.*

that minute aquatic animal or insect, the Polype; which, if it be divided into many pieces, each piece will become a perfect animal, possessing all the properties of the original Polype, before divided. Mr. Baker, in his attempt towards a Natural History of the Polype, has a curious essay respecting the divisibility of the conscious principle in that living creature, which seems applicable to the inanimate properties multiplied in this subdivision of the Loadstone.—TRANSLATOR.

But the motion produced in the glasses by these pieces of Loadstone was so feeble, that I judged if a power not equal to the thousandth part of a grain, had been opposed to the small glasses, it would have given them a different direction; for these particles of Loadstone had very little virtue in them, if compared with what I have heard boasted respecting other Loadstones.

Moreover, I made another glass vessel, to contain a piece of Loadstone; the shape of which vessel is shewn at *fig. 22*, A B C D E F G H I K, and in the cavity of this, at A B C I K, a fragment of Loadstone was placed, as marked at L. The length of this glass, from C to D, was almost nine inches; the cavity in which part was about the thickness of an horse hair. I purposed to make the part, D E M, with a cavity of just such a size, that by means thereof a small part only of the glass might rise above the surface of the water: and, after three several trials, I made the glass so to my wish, that the upper part of the cavity, D E F, barely appeared above the surface. The reason why I made this glass of such a length was, partly that I might see whether the particle of Magnet, when so deep under the water, would yet have power to turn its North pole towards the North, and so to cause the glass vessel to change its position; and partly to observe whether, when sunk so deep, it would be at all affected by iron brought close to the surface of the water. This being done, I turned round the part, F G, several times; sometimes with a quicker and sometimes with a slower motion; and I always found that it would not remain at rest until its extremity, G, pointed towards the East.

I could not make this experiment in a beer-cup, by reason of the length of the glass; therefore I took a wooden vessel of that sort which are used in this country to measure charges of powder for guns, and called by us, *cardoes-doofen*, which may be rendered in English, cartridge-boxes.

After this I made another glass, the thinnest part of which, similar to what is pictured in *fig. 22*, at C D, was thirteen inches long:

into this I put a piece of Loadstone, which was half as long again as thick, but before inclosing it I carefully examined, by the help of a sea-compass, in what part its North pole was situated; and finding it to be, not at the extremity but rather towards the middle, I placed the Loadstone upright on its end in the glass. The top of this glass, when it was put into the water with the Loadstone in it, scarcely appeared above the surface, and, after moving it in several directions, I found that it would not remain at rest until the North pole of the Loadstone in it pointed to the North.

When this glass with the Magnet in it floated in a state of rest on the water, I took a piece of iron, almost eighteen inches long, one extremity of which I approached first to the surface of the water, and then to the outside of the wooden vessel near the bottom, about the part where the Magnet lay, in order to see whether by this the glass containing the Magnet would be put into any new motion, but I did not at that time perceive any.

After this I applied the same iron, not by its end but the whole length of it, as closely as possible to the wooden vessel, and immediately I perceived that the glass inclined somewhat towards it, whereupon I slowly moved the iron round the box, and saw that the Loadstone in the glass, and which was fourteen inches under water, followed the motion of the iron, so that in nearly a minute's time it performed a complete revolution. By this experiment the tendency of the Magnet was proved to be greater towards the iron than towards the North part of the heavens, as appeared by its following the motion of the iron. Then, after some interval, I placed the iron lengthwise, close to the wooden box or vessel, in order to see whether the Loadstone would be attracted by the iron, towards the side next to it, which however did not happen, for it kept its position unmoved, excepting only that its North pole altered its position a little from the North.

Afterwards I made another glass, wherein I put a fragment of Loadstone, which glass was of the same shape as that represented at

fig. 21, QRS, this I put into a common beer-cup filled with water, and when the Magnet had turned to the North, and was at rest, I took a round piece of iron, about an inch in diameter, and placed it lengthwise close to the beer-cup, when I immediately perceived that the glass, with the Loadstone in it, inclined somewhat to the iron; whereupon I moved the iron, as gently as I could, round about the cup, and the glass, with the Loadstone in it, immediately followed the motion, but without approaching nearer to the side of the cup, and in about a minute's space performed a complete revolution.

After this I placed the iron as close as possible to the cup, in order to discover whether the attraction between it and the Magnet would be sufficient to move the latter from the middle to the side of the cup next the iron, but this did not take place though they were left a whole night. From these things I conclude, that the attraction between the Magnet and the iron will shew itself upon the least application, so far as to cause the circular rotation I have mentioned, but no farther, for very little force is required to effect this, in comparison of that which would be necessary to draw the glass, containing the Loadstone, through the water to the side of the cup. Nevertheless, I do believe that there might be some tendency in the Loadstone towards the iron, and I have no doubt that if this Loadstone had been larger, or of stronger power, it would have moved to the side of the cup next the iron.

Once more, I made a much smaller glass, of the same shape as last mentioned, in which I put a small piece of Loadstone, not much more than the weight of a grain, and I found that small particle of Loadstone to produce the same effect as I had before observed in the other pieces, and the only difference in my observations was, that the larger the Loadstone the quicker were its motions.



On the Substance of the Brain in a Turkey, a Sheep, and a Sparrow.

IN my examination of a Turkey's Brain, I began with that part which is called the corticle;* this part, besides the small blood-vessels and globules found in it, is composed of a very pellucid crystalline, and (as it appears to the eye) oily substance, which, from its clearness and transparency, should rather be called the vitreous or glassy part, than the corticle or shell, of the Brain. When I spread this into small particles, I perceived to issue from it a small quantity of a thin fluid, composed of very minute globules, thirty-six of which would not be equal in size to a globule of the human blood; this liquor, though it did not extend farther from the brain than about the tenth part of the diameter of an hair, was yet mixed with those globules. I observed this fluid matter principally in the Brains of those Turkeys which had been killed some time, whence I concluded that it had partly issued from the small vessels of the Brain, and perhaps that some of those vessels themselves might have been dissolved into a fluid matter. Besides those small globules, there were some larger ones, of which I judged six would be equal in size to one globule of human blood; these two sorts of globules I judged might issue from the minute vessels which I might have

* That is, the shell or case, from the Latin word cortex, which signifies the bark of a tree.

broken, and, that, what in the animal, when alive, was a thin fluid, might become solid in the vessels when cold, and exhibit the appearance of those globules. I also saw, dispersed among them, some other bright irregular globules of different sizes, and some as large as a globule of human blood, and others larger. Among, or in the midst of this watery matter and globules, there were dispersed a great number of wonderfully minute blood-vessels, and these in no greater a portion of Brain than the size of a large grain of sand. Many of these blood-vessels were so minute that (to judge by the eye) if one of the red globules in the blood of a Turkey, or other bird, were to be divided into five hundred parts, not one of these parts could be contained in the cavity of those vessels. For I judged, that if the size of the cavity of those vessels should be considered as 1, the axis of one of those globules must be as 8, and consequently, if the axis of any sphere is 1, and the axis of another is 8, then the proportion will be as 1 to 512; and though these blood-vessels were so minute, yet their colour plainly shewed that the substance they contained was what gives the blood its red colour. And, if I had not taken notice of the faint colour which a globule of blood, singly taken, exhibits, it would have been impossible for me to know these to be blood-vessels, and thus it appears that the reddish liquid imparts to them some kind of colour; this I was the more certain of, by observing some of those vessels somewhat larger which assumed a redder cast. These very minute blood-vessels appeared of a deeper colour where three or four of them lay one on another, without any interjacent substance. From these appearances, I was more firmly of opinion than before, that the globules of blood, whence its redness proceeds, are divided into smaller parts, when they come to such minute vessels as they cannot enter without being divided; and I was now of opinion, that the dark colour of that part of the Brain, called the corticle, in which it differs from the white part, called the medul-

lary or marrowy substance of the Brain, proceeds from this, that the greater part of it consists of pellucid particles, which are so closely joined together, that they exhibit a glassy or watery transparency, and that the dark colour is increased by the multitude of blood-vessels passing through it. I also observed many very minute particles, which I judged to be globules, about the sixth part of the size of a globule of human blood: these were not transparent, and gave the Brain a darkish colour, and respecting them I thought that, perhaps, they might have issued from some of the blood-vessels which I had cut asunder.

We may also perceive that this corticle of the Brain is, in many places, deeply mixed among the medullary parts, but when we come to separate them gently, we find, between the two, so great a number of blood-vessels, that they seem to constitute an entire membrane; and we shall find that this internal part has as many blood-vessels as the corticle, or external part, of the Brain. Besides these small blood-vessels, I observed in the Brain other vessels so slender that, in my judgment, none of the before-mentioned globules, though divided into a thousand parts, could pass through them. I do not here take any notice of those blood-vessels as large or larger than a hair of one's head, which in great numbers run among the medullary parts of the Brain, and are every where again divided into branches,

When I came to what is called the medullary substance of the Brain, I observed several irregular globules of different sizes, some of which were equal to the size of a globule of human blood, and some larger, and which, as well as I could judge by my eye, for the most part consisted of a thin, transparent, and oily substance. These globules are formed in the medullary part of the Brain (and principally in that part where the spinal marrow begins), in such quantities that they seem to constitute the greatest part of the Brain. This great multitude of transparent globules causes the white colour,

because all transparent particles, which are not so connected together that the light can pass through them in a straight line, and do not constitute as it were an uniform body, must exhibit a white appearance to the eye, as I have shewn in * another place. These irregular globules were so closely joined, that when I endeavoured to separate them, some of them stretched out, and became twice as long as broad, and they appeared to me joined in the manner of net-work: upon observing which I began to consider whether the smallest blood-vessels, issuing out of the corticle, might not be again divided into smaller branches, and many of them surround these globules, or give way to them, as we see the small † horizontal vessels in some kind of wood give way to the perpendicular vessels, and take a circuit round the half of their circumference. This opinion of mine, that many of these irregular globules are surrounded by small vessels, was confirmed, when I saw many of the said vessels bright in the middle, and bearing something of a dark colour at the sides; and still more, when, upon breaking the globules, I found many small fibrous parts which seemed to be vessels lying among them. I also often saw parts of the medullary substance of the Brain of the same figure, as if we were to behold a fisherman's net, the threads of which can easily be extended any way, and that between each space of the net (which fishermen call a mesh), a very flexible body, in shape of a ball, was placed, which would assume either a round or an oblong form, as the net was drawn one way or the other. And to such a net I compare the multitude of minute vessels in the medullary substance of the Brain, and the balls to the irregular globules I have mentioned. The other parts of the medullary substance consisted of an almost infinite number of excessively minute globules, and a clear thin matter, which last I conjectured had issued from the wounded vessels, and also that some of the vessels themselves might have been dissolved into it.

* See Vol. I. p. 184, Addition by the Translator.

* This may be seen in Vol. I. Plate I. *fig. 1*, at the Letters E E E. *Translator.*

Proceeding farther in my inquiries, I took the head of a Sheep, and upon examining its Brain, I also found in the corticle, a great number of wonderfully minute blood-vessels, in which I could discover the substance which gives the blood its red colour; these vessels also gave the corticle a darkish complexion. I often contemplated this incomprehensible multitude of exquisitely minute blood-vessels, merely for the pleasure I took in viewing them. For I saw almost all of them again divide into branches; and I moreover took notice of a great number of globules, each about a sixth part the size of a globule of human blood, and which, I judged, issued from the vessels which were broken; and I concluded, that six of those globules would join to make up one globule of blood when they passed into larger vessels, for they were very inferior in transparency to the other globules adjoining. I also imagined that these globules, of which six would make up an ordinary sized globule, when they came to vessels so small as not to admit them, must be again divided into still smaller parts, and then these vessels would become colourless. And, in order more clearly to give the reader some idea of the incomprehensible slenderness of the blood vessels which are formed in the brain, I have made a computation of their size, judging by the eye, and by comparison with a large grain of sand; I take it then for a certain position that those globules which tinge our blood with a red colour are, when perfect, of such a size, that

100	one hundred of them laid side by side, do not more than
100	equal the thickness or axis of a grain of sand, and
100	consequently, that a million of globules of blood are
1,000,000	equal to a large grain of sand. I next observe the blood-
64	vessels in the Brain, respecting which I am certain, that
64,000,000	if one globule of blood was divided into sixty-four parts,
	none of such parts could pass through one of those ves-

sels; for I state the diameter of one of those vessels to be as 1, and.

4 the axis of a globule of blood to be as 4; the result is, that if
 4 a grain of sand was divided into sixty-four millions of parts, not
 16 one of those parts could enter the orifices of the smallest ves-
 4 sels in the Brain, and especially, if the small particles of the
 64 blood were not flexible; but I think that the small particles
 — of the blood are equally flexible with those globules which are
 of full size. I also found the corticle of the Brain in the Sheep to
 consist of a clear vitreous substance, the same as I have described in
 the Brain of a Turkey; with this only difference, that I saw in this
 some thin white streaks, which could not easily be discovered by the
 naked eye, some thinner than a hair of one's head: these streaks
 I judged to be caused by a greater collection than usual, of those
 large transparent globules which cause the medullary parts to appear
 white. I also observed in the medullary parts some dark streaks of
 the colour of the corticle, which dark streaks, I judged proceeded
 only from this, that there were few or none of those pellucid glo-
 bules in that part.

After this, I examined the white or medullary parts of the
 Brain, and principally those which are the beginning of the spinal
 marrow; and here I think I saw, sometimes very clearly, what I
 had not been able to distinguish in the Brains of Turkeys, namely,
 that those large and translucent (as they appeared to the sight) oily
 globules, were in a manner surrounded, or lay in the midst of, an
 inexpressible number of wonderfully minute vessels or streaks in the
 form of net-work, mixed with some larger ones, which were in a
 straight direction and very transparent, causing the Brain where
 they were collected in any quantity, to appear uncommonly white
 to the eye: these, by several observations, appeared to me to be
 about the twenty-fifth part the thickness of an hair, and to be of a
 dark colour. The remainder of the medullary substance consisted
 of the same kind of parts, as I have said I observed in the Brains
 of Turkeys. And yet I sometimes had my doubts, whether all I
 deemed to be vessels were so in reality.

After this, I took the Brain of an Ox immediately after the head was cut off, and forthwith proceeded to examine the corticle of the Brain with all the care and attention I was able. In several examinations of which, I thought I was assured that the vitreous and very pellucid substance (which constitutes the greatest part of the corticle), consisted of nothing else than of excessively slender streaks or vessels closely joined together; though at another time, I could not satisfy myself so fully in that respect as I wished to have done. With regard to the component parts of the corticle, I could not discern any difference from what I have before related of the other subjects I examined, except that I did not here perceive so much fluid matter, as where the animals had been killed some time. The white, or medullary substance of the brain, I found to be of the same nature as I have described in a Sheep. For, when I observed the very white lines or streaks in the medullary part (from whence the spinal marrow takes its origin), I found the reason of its great whiteness, to be a number of very transparent vessels adjoining to each other, which seemed to me, formed for conveying from the substance to nourish and support the spinal marrow, and in part the nerves: the largest of these vessels destined for conveying such nourishment, seemed to me, as nearly as I could judge by my eye, to be about the hundredth part of the thickness of a hair of my beard, though at another time, I saw them much larger; but in this instance, I think I happened on that part of the Brain where those transparent vessels were the smallest of all.

In all these my observations, it must be understood, that I take no notice of the multitudes of blood vessels dispersed throughout the Brain, which, upon dissecting it, may be seen with the naked eye. For these are, as it were, entire rivers, when compared with the small vessels I discovered, and have been now describing, which may be deemed as the smallest brooks, channels, and ditches.

I also examined the corticle of the Brain in Sparrows, immediately after having killed them. In these, I not only observed with

equal distinctness, the small vessels of every description, as I had seen them in the Brain of a Turkey; but also as clearly and distinctly as I had done in the Brain of an Ox and a Sheep, and in all other respects, there was no difference to be noted than between the magnitude of a Sparrow's Brain and that of an Ox; so that as before observed, there is no difference between them (except in the large blood vessels), save that the size in the larger animal is owing to its being composed of a greater number of parts. And when, with my utmost attention, I examined the vitreous substance (otherwise called the corticle) of the Brain, it seems to me wholly to consist of no other parts, than an incomprehensible multitude of the minutest vessels, which adjoin so closely to each other, that by their transparency they represent a glassy substance, when viewed by the microscope, and which appeared more plainly to me in the small Brain of the Sparrow, than in the large one of the Ox.

Now, though I am certain of the truth of my opinion, nor have any scruple or doubt respecting the existence of the minute vessels I have been describing, I had rather it were doubted of by others, if perhaps my sight has deceived me, but which I see no reason to believe. For we shall cease to doubt or wonder at the minuteness of these vessels, if we recollect and duly consider the minuteness of those which the smallest animalcules we view must be furnished with; or what will seem less wonderful, if we consider what I am going to relate, respecting the smallest vessels in our own bodies. For being lately busied in dissecting a human eye; in the black membrane which covers the cavity of the eye, and by which the rays of light are reflected, I saw streaks or vessels the smallest that I had ever discovered; and being desirous to judge of their slenderness, I took a large grain of sand, the axis of which was almost the thirtieth part of an inch: viewing this grain of sand by the microscope, I judged that its axis might be divided at least into 330 equal parts; and finding that eight of those small vessels, placed

2,640
2,640
105,600
1,584,0
5,280
6,969,600
2,640
278,784,000
4,181,760,0
13,939,200
18,399,744,000
18,399,744,000

side by side, would not equal $\frac{1}{33^{\circ}}$ th part of the axis of the grain of sand, it follows that the axis of such a grain is 2,640 times larger than the breadth of one of those small vessels. This number 2,640 being multiplied into itself, to find the solid contents of a body whose axis is 2,640, the result is more than eighteen thousand millions: so that a large grain of sand, as before-mentioned, must be divided into so many millions of parts before any one of these parts can pass into the cavity of those minute vessels. And the minute vessels, which in part constitute the vitreous substance of the Brain, I take to be so small, that no particle or

grain of large sand could pass through them, unless it were first divided into many millions of parts.

With regard to the Brain of a Sparrow, I found, by many observations, that those parts which are called the medullary substance do, for the most part, consist of wonderfully minute fibres or vessels: and in this part I met with as many small blood-vessels as I have before mentioned to have seen in the corticle. The remainder of the white substance in the Sparrow's Brain did not differ from that in the Ox, Sheep, and Turkey, as I have before mentioned, except that the transparent oily globules, which were enclosed by some of the vessels, were not so large in the Ox. But in the Brain of a Sparrow which had been killed twenty-four hours, I found those oily globules, as far as my eye could judge, to be of the same size as in the brain of an Ox or other animal. From these last observations, and from my former ones, I began to consider whether that great number of lucid globules (which may be considered to be of an oily substance) were not a fluid in the vessels, or perhaps the vessels themselves, which, while the animal was living, continually conveyed a substance for the nutriment of the spinal mar-

row and the nerves; and that, upon the animal's death, and the motion of the fluids in the vessels ceasing, and the parts growing cold, they collected or coagulated into irregular globules of different sizes, in like manner as we see that two or three globules of blood, lying so near as to touch each other, do, upon the approach of cold, concreate together and form an irregular figure. And as often as I revolve in my mind the situation and figure of the medullary substance of the Brain, it seems to be designed by Nature to convey the blood, which in these innumerable smallest vessels, passing through the vitreous part of the Brain (called the corticle), is prepared and elaborated, to the spinal marrow: so that most of the minute arteries which are found in the Brain, seem made only to convey the blood to the Brain, without its being returned from thence into the veins; and for this intent only, namely, that the Brain may be continually supplied with a fresh addition of blood, and may provide a constant fresh supply of substance, for the support and nourishment of the spinal marrow and the nerves. And as to the objection which some may make, that if the blood did really pass through the Brain, it would tinge the medullary substance of a red colour; such objection will be found of no force, if we consider that a * globule of blood, singly taken, exhibits very little of a red appearance; and, therefore, when divided into its most minute component particles, it may appear wholly colourless.

ADDITION, BY THE TRANSLATOR.

The Translator begs leave to subjoin a few words to this Essay, for the information of such of his readers as are not conversant in the doctrine of solids; and the rather, as what the author states respecting the minuteness of the vessels in the retina of the human eye may appear, at first sight, utterly incredible. But it must be remembered, that it is the solid contents of two spheres or globes of the sizes mentioned by the author, and not their respective diameters, that are the measure of his computation.

* See this explained in Vol. I. page 94.

To illustrate this, we must understand, that if a grain of sand were divided into parts (supposing all to be of a spherical shape) each of which was $\frac{1}{2848}$ th part of the diameter of that grain, each of such parts would be capable of entering the cavities of the small vessels he is treating of: though, at the same time, the rules of arithmetic teach us, that the solid contents of that grain would be to the solid contents of one of those component parts, as 18,392,744,000 to 1. Again, he tell us, that a globule of blood (of which a million are no more than equal to a grain of sand) must be divided into 64 parts before it could be received into some of the blood-vessels of the Brain; and that a grain of sand would be equal to 64 millions of such parts. But by the same rule we shall find, that the diameter of each such part is no more than $\frac{1}{400}$ th part of the diameter of a grain of sand. After all, it must be confessed, that these reduced subdivisions are not to be comprehended in idea, though not so inconceivably minute as the other proportions seem to intimate.—See a note on this subject, taken from the Spectator, in the Introduction to the First Volume.



On the Seeds found in the Fig and on Strawberries, with the Author's remarks on the great fecundity of Nature in the propagation of plants and animals.

I HAVE, at times, bestowed much labour in searching after the young plant in the seed of the Fig, but always without success; the reason of which I took to be, that when Figs are packed up at the places of their growth, in order to be exported, they are gathered before they are ripe. At length, however, while I was in search after foreign mites (that little pernicious insect which finds its way into all kinds of dried provisions, such as cheese, dried fish and bacon, and also dried fruits, as Figs and Raisins) I found a Fig with several hundreds of these animals in it, but otherwise of good taste and full grown, seeming to me as if it had been ripe when gathered. I was, therefore, induced to dissect some of the small seeds it contained; and, breaking open the hard outside shells of several of them, I took out the entire kernels they contained, then stripping this kernel of the membrane surrounding it, and clearing away some moisture which covered the plant, I saw a complete young plant, consisting of two leaves, and of the part from which a young tree would in due time have proceeded.

I have caused a drawing to be made, from the microscope, of the young plant in one of these seeds (of which seeds we know that every Fig contains great numbers), because I have often heard it said, that eating Figs in great quantities, will breed lice in the stomach; and that some persons (and among them a man of some eminence) have not scrupled to assert the same in writing; which idle tales certainly do no other than excite derision, for I am well assured that this notion took its rise from no other cause than the multitude of small grains or seeds with which all Figs abound, but which not one man in a thousand knows to be seeds, and still less,

that in every one of those feeds there is as much perfection as in an entire Fig-tree; and, therefore, it is very probable, that people comparing these feeds or grains in Figs, by reason of their smallness, with lice, first broached the idea that lice can be bred from Figs.

Plate XIV, *fig.* 23, A B C, represents the kernel or internal part, taken out of the hard shell of one of those feeds, which we observe in great numbers in every Fig. A B is that part of the kernel to which the string or ligament by which the feed received its nourishment was fixed. That part from which the root and trunk of the tree would proceed, is situated from A towards C; but the leaves of the young plant lie from C towards B. Every one of these grains or feeds in a Fig is distinct from the rest, being as it were surrounded with its own proper membrane; so that we may be certain that every feed, while in the Fig, is provided with two strings or ligaments, destined for its nourishment, namely, one that nourishes the hard shell, and the other that brings nourishment to the kernel within the shell.

At *fig.* 24, D E F G, is seen the young plant taken out of the feed of the Fig; and in this figure, E F G, denote the two leaves; and G D E, that part which would grow to the root and stem or trunk. But it must be observed, that this young plant does not constitute the whole kernel of this feed, but that the young plant (besides the membrane which surrounds it) is partly enveloped with a certain kind of substance, from which it receives support and growth, until the root is grown long enough to extract nourishment from the earth. All the space in the figure, between F D G, was filled with this substance.

Now, if we attentively consider and observe the membrane which surrounds this kernel, we shall not only see a great number of excessively minute vessels, but an incredible quantity of dots or cavities in it, so that, in a word, if it were possible to dive into all the hidden parts of this feed, nothing would be seen but wonders and

perfections, for no doubt this minute plant possesses as many perfections as an entire tree.

When I had steeped some of these seeds in water for a few hours, and then opened them, I could distinguish, as well in that part which would produce a root as in the young leaves, a great number of vessels with their divisions or cells, but as soon as the liquor evaporated, all those vessels disappeared.

Fig. 25, H I K exhibits the same plant as is shewn in *fig. 24*, with this difference only, that whereas the former was drawn in a position to shew only the sides of the leaves, in this figure the plant was so placed before the microscope as to shew the breadth of the leaves.

I am at times called upon to defend the system I have advanced respecting the propagation of animals (which is often the case with many eminent persons who come to visit me), and they continually object to what I have delivered respecting the abundant provision made by Nature in the first rudiments of the young of each species, of which few come to perfection, whereas (say they) Nature does nothing in vain. The answer I generally make to this objection is, by observing what multitudes of seeds are produced by trees, of which the present is a strong instance. For, when we see that a common Fig contains in it between four and five hundred seeds or grains, and that each Fig-tree may every year produce many Figs, and from each perfect seed a whole tree may spring; and consequently that a space of ground planted with Fig-trees may, in one year, produce so many seeds as would suffice to sow, not only an entire kingdom, but the whole surface of the earth, and that this is not only the case with Figs, but in every kind of tree whatsoever: when, I say, we continually see these things, I cannot but give it as my opinion (with due deference to better judgment) that we ought not to call in question for what purpose such a superabundant provision is made, but that it would become us better with reverence to acknowledge the Omniscience of the Supreme Being, who has

thought fit to make such a provision for the propagation of animals and vegetables, the reasons for which order of things are to us inscrutable, and respecting which we can only form conjectures.

In eating Strawberries, my attention was taken by those little knobs or risings we see on their surface, and I had no doubt that each of them was a seed: to satisfy myself respecting this, I took one of the largest and ripest Strawberries, and picked out several of the seeds; then stripping them of their outward covering, I found each of them provided with a ligament by which it had received its growth, I also broke open the hard shell, and found in each seed that substance we may call a kernel; from this kernel I took off the membrane, and placing the plant before the microscope, I caused a drawing to be made of it, in order to shew what numbers of seeds we swallow in eating only a spoonful of Strawberries; for, cutting one of the largest Strawberries into four pieces, I counted in one quarter of it, fifty seeds, and consequently, that one Strawberry was covered with two hundred seeds; and one of the smaller ones I judged to have an hundred and twenty.

Fig. 26, A B C D E, represents the plant taken out of the seed of a Strawberry; *A B C* is that part which would grow to a root; and *C D E A*, are the two leaves which would first appear above the ground.

These two leaves are always most exactly joined to each other; but as, while I was taking out the plant from the seed, the leaves parted a little asunder, I directed the limner to draw them in that position, that each leaf might be the easier distinguished.

Now, if we recollect that a young Strawberry plant, (for I never heard of Strawberries being propagated by sowing), does in one year send forth various branches (commonly called runners), which in a proper soil constantly strike good roots, and grow up to per-

fect plants, so that in the same year, many plants are produced from a single one, all which in the following year bear fruit, and each of them produces many Strawberries, each of which Strawberries has as many seeds as we have seen: if, I say, we duly consider these things, we must needs be astonished, and wonder in silence, at the multitudes of seeds which are produced by plants.



On the nature of the minute Fibres which compose the fleshy parts of several Animals, and also of Fishes and Insects.

IN my examination of the component Fibres of the flesh of oxen and cows, I judged that each single Fibre was so minute, that fifty of them, placed together, were no more than equal in diameter to one twenty-second part of an inch; but, if we take the twentieth part of an inch, and allow the other two parts for the membranes in which the fleshy Fibres are inclosed, it will amount to one thousand

1,000	fleshy Fibres in the length of an inch, and, conse-
1,000	quently, a million of such Fibres, wrapped in their
<hr/>	membranes, will be comprised in the compass of a
1,000,000	square inch. In some of my observations, it appeared

to me, that about an hundred of these fleshy Fibres, lying close together with a membrane surrounding them, composed one small fleshy muscle: at another time I saw, in an ox's tongue, three small fleshy muscles, each of them enveloped in a membrane, lying close together; and, having cut them transversely, I found that the space they occupied was not so much as could be covered by a grain of sand (one hundred of which grains, placed side by side, make up the length of an inch). Now, if we suppose that two hundred of these fleshy Fibres, inclosed in their membranes, compose a small fleshy muscle, five thousand of those small muscles will go to make up the dimensions of a square inch. I compared the thickness of these fleshy Fibres with the size of a hair taken from my wig, and I judged that at least four Fibres of an ox's flesh, taken near the ribs, were contained within the size of one of those hairs, and that a hair of my beard was nine times as large as one of those Fibres. We must not imagine that these fleshy Fibres are round, but each of

them assumes a different figure, so as to lie together and adjoin as closely as can be imagined. This my computation of the fleshy Fibres (as they are denominated by a certain medical gentleman in this country), I have here set down in order to shew their minuteness, and the rather as the same medical gentleman supposes, that these fleshy Fibres are inserted into the orifices of the veins and end in the arteries, and that the circulation of the blood is performed in these fleshy Fibres, and he says that he has seen valves in them; but these his observations are not taken from the microscope, but from the judgment of his naked eye, so that, doubtless, he mistook an entire fleshy muscle for a fleshy Fibre.

Plate XV. *Fig. 1*, is a fleshy Fibre, in which I have often observed some foldings or inequalities, like wrinkles, as A B C D, and others lying near them, as at E F G H, and again as at I K L M, and in this manner, viewed by a common microscope, they had the appearance of being composed of globules; but a fleshy Fibre also appeared as at N O P Q, and this last appearance, I think, represents truly the internal original filaments of which a fleshy Fibre is composed.

Upon seeing this, I was able to satisfy myself as to the reason why our fingers, arms, legs, and our whole bodies, while at rest, do not lie in a right line, but somewhat bent, and, as it were, tending to the position they were placed in before we were born; and, moreover, I think I can assign the reasons of the motions of our limbs, or rather the extensions and contractions of our muscles, namely, that when a muscle is extended the fleshy Fibres composing it have no wrinkles or folds, but when a muscle is not extended, but contracted or thicker in figure, then each fleshy Fibre is full of wrinkles or folds.

Fig. 2, A B C D E F G H is a fleshy Fibre which, by expanding and compressing it, in order more plainly to discover the number of filaments of which so small a Fibre consists, I caused to open very much in its component filaments, and it gave the representation of

a vein with its ramifications: this, to the best of my ability, I represented in the drawing. Hence it plainly appears, even more clearly than I can express, that the fleshy Fibres are again composed of a great number of filaments. And at one time I saw a fleshy Fibre, so accurately expanded and divided, that, in a space a little broader than from B to H in the figure, I saw seventy of these component filaments lying together; whence I concluded, that a fleshy Fibre no thicker (as I have before-mentioned) than the ninth part of a hair of my beard, contained in it an hundred filaments. I often imagined that I could discern the Fibres, or, more properly speaking, the vessels, of which the membranes in which these fleshy Fibres are inclosed consist: hence I gave farther scope to my imagination, and I reasoned thus: since we see that a large muscle consists of so many thousand smaller ones, or Fibres, each inclosed within its proper membrane, and that every one of those small fleshy Fibres is again composed of still smaller filaments, of which its internal structure consists, each of these filaments (one hundred of which go to making up a single Fibre) must be also a fleshy muscle, and may contain within it many still smaller filaments, inclosed in a distinct membrane. For we see that the power and wisdom of the Almighty Creator of the Universe, displayed in the formation and association of the different parts which compose his creatures, are so wonderful and incomprehensible, that the deeper we dive into the secrets of his created works, the more we are confounded and lost: especially when we see living creatures moving in the water of the similitude of minute eels, as pictured at *fig. 3*, which yet, with all their component parts, are smaller than one of the filaments of which a fleshy Fibre is composed; and, nevertheless, so minute an animalcule must necessarily be furnished with a skin, nerves, muscles, and other parts, all equally perfect as those in a large animal.

If any admirers of the secrets of Nature should desire to follow me in these my observations, I would advise them not to chuse for that purpose a hot and dry season, but rather a time when the air is

cool and moist; for when these small Fibres are divested of their membranes, and separated or split open, to discover their component filaments, this operation must be performed as expeditiously as possible (and even then it will often fail of success); for the moisture of these fleshy Fibres will, by reason of their thinness, quickly evaporate; and if that happens, the filaments of which the Fibre consists will dry up and cohere so closely, as to exhibit the appearance of a single, uniform, and solid bright substance.

In my latest observations respecting the muscle of a Hare, I plainly saw how some of the fleshy Fibres, which were very sharp at the extremities, terminated in the membrane of a muscle; some again, at the extremities, terminated in a muscular tendon. This muscle I placed before a good microscope, in order to shew the same to a gentleman who was visiting me; but the muscle being very slender, the moisture of it soon evaporated, whence, in a very short time, the fleshy Fibres became so closely joined, that I could not distinctly see any one of them separately, much less could I discover the place where they were united to the tendon, or indeed the tendon itself.

These discoveries induced me to repeat my former inquiries into the nature of those Fibres of which Fishes are composed; and, upon examining several parts of the cod-fish, I judged that the most solid fishy Fibres were in that part of the belly near the gills. These Fibres being divested of their membranes, I saw in them the same turnings, foldings, and wrinkles, as I have before noted respecting the Fibres of flesh; but they had not always the same figure; for sometimes they appeared as *fig. 4*, B E C D; at another time, as at F G; and also as at H I. Upon cutting these Fibres transversely, I very plainly saw the ends of those multitudes of filaments of which the inside of a fishy Fibre is composed. I also, but very rarely, saw, on the transverse cutting of these fishy Fibres, certain very bright lines or streaks, as at A. I debated in my mind, whether these were vessels destined for the nourishment of those

internal filaments or membranes, to inclose them; but because I very rarely had a sight of this appearance, I cannot say much respecting it. These fishy Fibres are of very different sizes, so that I have seen some four times as large as others; and in one half the circumference of such a fishy Fibre, I was well assured, I could count almost an hundred filaments; and some of these filaments appeared to my eyes to be divided in their longitudinal position; as is in some manner represented between K and L; so that in the circumference of one fishy Fibre there were two hundred filaments. And because this number is great, even to admiration, I cannot omit to make a computation of it.

$$\begin{array}{r}
 88 - 7 = 200 \\
 \underline{\quad\quad\quad} \\
 \quad\quad 200 \\
 \quad\quad \underline{\quad\quad} \\
 \quad\quad 40,000 \\
 \quad\quad \quad\quad 7 \\
 \quad\quad \underline{\quad\quad\quad} \\
 88 \overline{) 280,000} (3,181
 \end{array}$$

To find the contents of a circle from its known circumference, the rule of Archimedes is, as 88 is to 7, so is the square of the circumference of the circle to its contents. And, following this rule, we shall find the contents to be 3,181 filaments in a single fishy Fibre.

Now, who can in imagination conceive such an incomprehensible number of filaments in one of those Fibres composing the substance of a fish? And who can tell whether each one of these filaments may not be inclosed in its proper membrane, and contain within it an incredible number of still smaller filaments?

I have also examined the formation of the Fibres, as well in the bodies as in the claws of crabs, and I found them like those in the cod-fish, composed of many smaller filaments, and with the like folds or wrinkles as I before noted. Afterwards I examined the Fibres of a shrimp, and found their formation to be similar.

Upon dissecting the hind leg of a frog, I found that the flesh of it was, in like manner, composed of threads or Fibres, which Fibres being divided asunder, I perceived that each of them was composed of a great number of small filaments. But because the circular folds or wrinkles in each of these Fibres were larger than

those I had seen in the component fleshy Fibres of other animals, I caused a portion of one of them to be drawn, as seen through the microscope: this is represented at *fig. 5*, A B C D. The like folds or wrinkles I afterwards met with in the Fibres composing the small muscles in a lamb, taken out of the belly, near the hind leg.

These folds or wrinkles in the fleshy Fibres, not only convince me what is the reason why our limbs assume a bent posture when the muscles are at rest, but we may from hence certainly learn the reason why we can remain longer sitting than in a standing posture, and why, when sitting, we do not keep our arms and hands fully extended or hanging down, but are always moving them about; for if they remained long in a state of rest, one muscle would be too much extended, and another too much contracted, which would be repugnant to the natural disposition of the muscles. For the same reasons, when we stand for any length of time, we do not remain supported on both feet, but first raise up one foot and then the other, so that the fore-part of one foot at a time touches the ground, and then the muscles of that foot are at rest.

I was at first at a loss to conceive the reason why some of the circular folds or wrinkles in the Fibres of flesh and fish, as in the figure E F G H and I K L M, they are represented; but when we recollect, that these Fibres are not round, because they lie closely compressed together, then if we suppose one of those Fibres separated from the adjoining ones to be round, or in a short time to assume a round figure, we may easily conceive how this comes to pass: because a fleshy Fibre is composed almost one third part of a watery substance, which being soon evaporated, the internal part may be wrinkled or contracted in a bent or irregular form. Let us suppose *fig. 6*, A·B C D E, to represent a fleshy Fibre, which separated from other Fibres, will be of a round figure, which by the evaporation of its moisture may lose its round shape and be

indented and bent inwards, as is shewn at F; by which means the folds or wrinkles which before were in straight lines will now become crooked, as appears between B and D.

To pursue my observations on the Fibres which compose the substance of animals, I endeavoured to investigate the nature of those in a flea, considering, that if I could discover it in the fleshy Fibres and their filaments, we may be assured, that the same kind of formation prevails in all living creatures. And in this inquiry, I not once or twice, but very often, most clearly could see the fleshy Fibres in the breast of a flea, in that part where the feet are joined to it; and I saw also in them the same circular folds or wrinkles as I have described to be in fleshy and fishy Fibres. Some of these Fibres were thicker in the middle than at the extremities, as is represented at *fig. 7*, A B C D E F G H, which is a fragment of a fleshy Fibre taken out of the breast of a flea. Seeing this, I thought that the fleshy Fibres in this minute animal, terminated in a point at the extremities, and were there united to the membrane or tendon of the muscle, as I have mentioned respecting the fleshy Fibres in an ox: and I also saw some of the folds or wrinkles like C F, but for the most part they were as at A B G H, and, at various times, I thought I saw that these Fibres were composed of many smaller Fibres or filaments.

I continued my observations, by examining the flesh taken out of the feet of a flea, and I saw no difference between the formation and figure of the Fibres taken out of the breast and the feet, and I saw more than twelve of such Fibres in the foot of a flea joining to each other, and also many smaller Fibres in which I could not distinguish the folds or wrinkles: these last I took to be exceeding small blood-vessels and nerves.

I also took the flesh out of the feet of small flies and saw the fleshy Fibres in them to be formed in the same manner as before-mentioned.

The Fibres which compose the substance of a whale, I also

found to be each inclosed in a membrane, and to be composed of still smaller filaments; and with regard to the size of these fishy Fibres, each single Fibre was no larger than in the smaller fish; and indeed I have seen the Fibres in some cod-fish, eight times the size of those in a whale.

I also examined the component Fibres in the flesh of a mouse, a calf, and a hog, and found their formation to be the same as before described, namely, each surrounded with a particular membrane, and composed of smaller filaments: the Fibres in the flesh of all these animals was nearly of the size I have before laid down, so that I may say, the fleshy Fibres composing the body of an ox, are not, singly taken, larger than those which go to the substance of a mouse, though, as I have computed, the one animal is thirty thousand times the size of the other.



On the Scales which cover the surface of the skin of the human body, and on the formation of the Bones.

THE external surface of our bodies is covered with minute particles, which may properly be denominated Scales, placed in regular order beside each other, but so minute that two hundred, or two hundred and fifty of them may be covered by a common grain of sand, and, viewed by an ordinary microscope, they appear as in Plate XV. *fig.* 8, H. Many may wonder at the denomination of Scales, and consider that word as only applicable to the covering of fishes. But I see no reason why we should not assign that name to those I am now describing, as well as to the external covering of a trout or a carp, or even of a smelt, although these last are more than an hundred times less than those of a trout or carp; in like manner those particles covering our own bodies, though some thousand times less than a smelt's, may properly be denominated Scales, since they are such in fact, and answer the same purpose on our bodies as Scales do on those of fishes. These Scales on our bodies, as I have before said, lie in exact order beside each other, in like manner as in fishes; and I not only could see that their shape was a figure of five sides, but in many I could perceive parallel borders or ridges, which I imagined denoted the growth or increase of each Scale, as by a microscope we observe in fishes. A drawing of one of these Scales, as viewed by the microscope, is given at *fig.* 9, K; and this I judged to be a perfect Scale, which had been fixed in the skin at the end O P, and this part I always found to be not so broad as

the upper end. Many other Scales were not so long as this, nor their sides so smooth, and these I judged I had pulled or broken off the skin before they had come to their perfect size and growth. These Scales are wonderfully thin, for I judged them to be twenty-five times broader than thick; and I also perceived, that they were placed in triple rows on each other; or, in other words, that our skin is covered with three layers of them, for only one third part of each Scale is visible to the eye, and under it are, at least, two other Scales, which are covered by it, as appears at *fig.* 11, M, the part of which marked 1, 2, 3, 4, is only visible, the remainder being covered by other Scales. That part of the Scale marked 3, I seldom perceived so sharp pointed as here pictured; but as I considered this to be one of the most perfectly formed Scales I had met with, I copied its figure in the drawing. In fishes we also see, that while their Scales are fixed to their bodies, only a part of them is visible to us. But whereas, fishes never cast their Scales, at least as I could ever observe, this is not the case with our bodies, for I myself have seen on my own body a thousand Scales and upwards separated from the skin and adhering together; but when I pulled off any Scales sticking to my skin, and which I conceived were newly formed, either an effusion of blood followed, or a red spot appeared. I also took some Scales from the skin of my arm, in a place where a scar had been left on my being let blood about twenty-five years ago, and I saw many of them shaped as at *fig.* 10, L.

I observed on many of these Scales certain transparent irregular streaks, which I have represented in *fig.* 11, M, and these covered here and there with round globules about a sixth part the size of a globule of blood, and these are also represented in the figure; and these, I supposed to be occasioned by exsudations from the body left upon the Scales. From this construction of our external skin, I think, we may conclude that fleas, lice, and other insects, do not

need much force to insert their stings, because they can introduce them between the rows of Scales, and not strike through the solid substance of the Scales.

These my observations, seemed to me to prove, that there are no particular pores in the external surface of our skins, but that the moisture which proceeds from our bodies (particularly when we perspire), may, at the same time, issue in many places between each Scale, though the Scales appear firmly united to each other; and there may be between each of the Scales small canals or ducts through which the moisture may find a passage. Now, if we recollect how many places there are which produce the Scales, and that in a space covered by one Scale, there may be an hundred places through which the perspirable matter issues; and again, that two hundred Scales, regularly disposed, may be covered by a common grain of sand, we may conclude, that there may be twenty thousand passages in our bodies for perspiration, in a space covered by a grain of sand, excepting only the places wherein the Scales are rooted, or from whence they grow. Hence it appears, that our bodies may be, as it were, one single pore throughout, while physicians are always talking of the pores of the body, as if there were in it particular holes or orifices for that purpose, and especially where they see a small round drop of sweat, not considering the effect of the air, and that a small portion of moisture, issuing from many thousand apertures, when pressed by the air, must form itself into a globule.

I have seen a drawing, made by a gentleman of some eminence, representing certain furrows in the inside of the fingers and the hand, and published to the world as indicating the pores of the skin; but, for my part, I never considered these furrows, either in the hands or feet, as containing particular pores or orifices in the skin, and I always perceived a less propulsion of particles from these furrows than from the places round about them; and, upon examination, we

shall find the scales which cover the skin placed more closely together in those furrows than on the more elevated parts. And, for some time past, it has been my firm opinion, that the furrows or wrinkles on the insides of our hands and feet, are only those places where the external skin, by means of some straight vessels in it, is uncommonly close and compact, as we see in some trees which have furrows or wrinkles in their bark, proceeding from the vessels of the wood, which furrows are not made to emit any moisture from the wood, but only, as I imagine, to unite the bark more closely to the wood. For, as our external skin is only made to defend the parts within it, so the bark of a tree is appointed for the defence of the wood; and as our external skin, when a new one is formed under it, peels off by pieces, so the bark of a tree, when the new bark is formed, every year breaks off, piece by piece, unless the old and new bark cohere too strongly: and, in like manner as in our hands and feet there is a strong cohesion of the old and newly formed skins, it necessarily follows, that, in the place where the cohesion is strongest, there must be wrinkles or furrows in the skin.

I examined the Scales on the inside of my hand, and taken from that thick skin we call a callus, and I found the Scales to be of the same dimensions with those on the other parts of my body, but whereas those are thin and transparent, these were covered with so many globules and streaks, that they appeared composed of globules. But, as I have always found that not only the insides of the fingers but all the inside of the hand emits much moisture, I conclude, that as the Scales on our arms and other parts of our bodies, when they have no nourishment fall off, so, on the contrary, though the Scales on the inside of our hands and feet may want nourishment, they collect together in great numbers, and, by the moisture of the parts, are so closely glued or coagulated together, and such small spaces left for the transpiration, that what appears a thick skin is formed; though, in reality it is, for the most part, no more than a collection of Scales;

and this is still more increased by hard manual labour, which increases the collection and cohesion of particles.

After this I examined the outward skin of my mouth, particularly my under-lip, and found this to be also covered with Scales, but somewhat larger, broader, and thinner than those of the rest of our bodies. *Fig. 12*, is a Scale of this description, being one of the most perfect I met with; these Scales were also covered with a greater number of streaks; produced, as I suppose, in the manner I have mentioned above; and likewise with many very transparent globules, which are pictured in the figure. Another of these Scales, adjoining to the former, was of the shape represented at *fig. 13*. But these Scales were not placed as they are on other parts of our bodies, that is, three at least on each other, which is the reason that our skins appear white, (for all transparent particles, if laid loose on each other, exhibit that colour; such as paper, froth, spittle, pounded glass, snow, and the like); but, on the contrary, the Scales on the lips, as far as I have been able to discover, only cover each other lightly on the edges, so that the flesh and the redness of the blood are seen through the greatest part of each Scale, and for this reason the inside of the lips and mouth are red.

After this, being desirous to investigate the manner of the formation of the Scales on our bodies, I judged that these last mentioned Scales, taken from the lips, would be the fittest for that purpose, because they are more easily separated from the skin before they become dried, than on the other parts of the body. And, after repeated observations, I saw that almost all of them had in the middle a bright spot, rising above the rest of the Scale; which spot I had formerly thought was caused by accident; and from hence I now concluded for a certainty, that not only the Scales on our lips, but also on our whole bodies, are formed, in like manner as the Scales of fishes, of vessels, by which they are increased in

size; and if this is so, what shall we say of the perspirable parts of our bodies? I have already mentioned, that perspiration may be performed through the interstices between the Scales, which must be in great numbers; but the vessels composing the Scales will much exceed that number; for if these Scales are in constant growth, by the means of such vessels, then, by the continual motion of the body and of the thin juices, the perspirable matter may be expelled in great quantities through these vessels.

And, to make a computation of the almost infinite number of these minute vessels, I placed some of these Scales, taken from the surface of the skin, near some grains of sand, and I judged that the diameter of these grains was from ten to twenty times the diameter of those Scales; and the Scales are, as before mentioned, placed in a triple row one over another. I will take, therefore, the smallest number, and say, as I have before done, that

250	250 Scales can be covered by a grain of sand. And
<u>500</u>	supposing each Scale to be composed of 500 vessels,
<u>125,000</u>	then the perspirable matter may be expelled through
	125,000 passages, within the compass of a grain of sand,

besides the orifices or outlets it may find between the Scales.

With a sharp-pointed instrument I took from the arm of a Negro girl, about thirteen years of age, a small portion of the upper skin, and found it to consist of nothing but Scales, joined together as I have said of my own skin; but these Scales were not so large, by reason, as I concluded, that the girl was not come to her full growth; for an infant has as many Scales on its body as a grown person, and they increase with it in size. And I am certain, that though a perch, jack, or cod-fish, be no bigger than a joint of one's finger, their bodies will have as many Scales on them as when they are grown larger. On placing these Scales, taken from the Negro girl, before the microscope, I found them not so transparent as those on my own body; and in the place on her arm whence I

had taken them, I saw there was left a black spot. These Scales I conceived to be such as were almost ready to fall off; for when I attempted to take others, which were firmly fixed in the skin, an effusion of blood followed. When I placed two of these Scales one on another, they assumed a blackish colour; and we must not wonder, that when viewed singly through the microscope they should be transparent, and yet when on the skin appear black, for if ink, though very black, is placed very thin before the microscope, it will only seem a little darker than common water; and the same is the case with respect to black silks, for if a filament of such silk, as thin as what is spun by the worm, is viewed through the microscope, it will appear, not black, but of a dark colour and also somewhat transparent. But many particles, laid one on another, although not wholly black, will appear so to our eyes. And hereupon, finding that the blackness of Negroes is only caused by the Scales being of that colour, I was enabled to form a better judgment than I had before done respecting the appearance of children newly born, for they are almost always of a red colour, by reason that the Scales on their bodies, or only the first rudiments of them, are then beginning to appear; so that the blood is seen through the skin, giving it a red colour: and the same is the case with Negro children newly born, with this difference only, that they are of a deeper crimson than our children. And I am certain, that as our children, by the growth of transparent Scales on their bodies, become white, so the children of Negroes, by the growth of the Scales on their bodies become of a black colour. The only thing that puzzled me about this Negro girl's skin was, that on the inside of her hands, and the bottom of her feet, the Scales forming that part called a callus were quite white; but an old lady in this town, who formerly lived in Brazil, and to whom the girl's grandmother had been a servant, told me, that this girl's parents were from Angola; and that though all Negroes are born red, and

by degrees acquire a black complexion, yet the insides of their hands and the soles of their feet are always white; whence I concluded, that the Scales on their hands and feet are transparent.

I know that there are many men among us who think (and I have heard some say so), that the blackness of the Negroes proceed from no other cause than their bodies being rubbed with some kind of oil. For, say they, when born they are of a red colour like our children. But in like manner as it is impossible to dye the wool or hair on the body of a sheep, horse, or other animal, so that it shall retain the colour, because the hairs or wool are continually falling off; so it is impossible to dye those Scales that cover the skin, so as to remain black, because they very soon drop off. And perhaps also the small vessels which compose the Scales on the bodies of Negroes may assume a black colour, whereby the Scales will appear still blacker, as we see the vessels in some of the coats of the eye are quite black, and I doubt not are so formed to preserve the blackness in the coats they compose.

ADDITION, BY THE TRANSLATOR.

THE perusal of this essay calls to my recollection a passage in one of Mr. Leeuwenhoek's Letters to the Royal Society, not inserted in his works, but printed in the Philosophical Transactions; in which, treating of the callus on the hands and feet, Mr. Leeuwenhoek confirms what he has here laid down, that a callus is only caused by great numbers of Scales heaped on each other. And he also observes, that on washing and afterwards wiping his hands, he was surprized at the great number of Scales which continually came away. Hence we may gather the reason why, if the hands are stained with any thing (such as the juice of walnut peel) which will not immediately wash off, the stain in a short time wears away: and why, in a long secession from labour, the hands become smooth and soft, so far as the discoloured and concreted Scales fall off, and are replaced by new ones of the natural colour and consistence.

I HAVE made many observations to discover the real formation of the Bones, and in my latest observations on the solid part of an ox's thigh Bone, I plainly saw that it consisted of four different kinds of tubes, running lengthways in the Bone, the least of which are so small, and lie so close together, that they cannot easily be distinguished in a Bone cut transversely; and even if the Bone is cut with the sharpest knife that can be had, nothing will be seen but the appearance of globules; but if the Bone is split or cleft, some fragments will be broken off in which those small tubes will be perceived.

The next kind of tubes (some of which are four, some six times larger than the former) are likewise not easily discovered; because, let the knife we cut them with be ever so sharp, the hardness of the Bone will cause many pieces to be broken off, which, for the greatest part, will close up or conceal the mouths or openings of those tubes.

The third sort of tubes, which are much larger than the last, will become so stopped up in the cutting with a knife, that it will be difficult to discover their cavities; but I have observed these tubes lying disposed in such a manner, that I was well assured a circle of those tubes formed every new concretion or addition to the Bone, almost in the same manner as I have laid it down in regard to the growth of timber, by the addition of a circle or ring of tubes, formed in the growth of the wood; and especially when I saw, that in a small space from thence, another circle of tubes was to be seen.

The fourth kind of tubes, still much larger than the last preceding, were fewer in number, so that often, in the space of three or four grains of sand, I could not see one of them.

I have made as exact a drawing as possible of a small particle, taken from an ox's thigh Bone, as the same appeared to me through the microscope, and which is shewn at *fig. 14*, A B C D. This

fragment, to the naked eye, appeared no larger than the spot, *fig. 15.* E F G is the point of a small needle to which this fragment of Bone was fixed.

The first and smallest sort of the tubes I have been describing I could not observe in this fragment; because, when the Bone is thus cut, the small tubes appear lying together in a confused manner, like irregular globules; the second sort of tubes I have represented in the figure, and these often appear like dark spots, because their orifices are stopped up in cutting the Bone, and are more difficultly distinguished when the knife does not divide them accurately by a transverse section; for if the section is ever so little oblique, it is impossible to discover them. This second sort of tubes are represented at letters H H H.

The third sort of tubes I have represented at I I I: this third sort I have not only seen adjoining each other in circular order, but also in a different order, in like manner as the large vessels in wood.

The fourth sort of tubes, which are very large in comparison with the former ones, is pointed out by the letters K K. The curved line L, and that at M, are cracks in the Bone, made by the edge of the knife, which often happens, especially when the knife is not sharp enough.

Besides the four sorts of tubes before mentioned, forming lengthways the substance of the Bone, I often imagined I saw some tubes taking a contrary course, and which seemed to me to proceed from the internal part, and terminate at the surface of the Bone; and I also thought that these were of two sizes, the least which I also imagined, were analogous to the smallest of those tubes which lay lengthways in the Bone.

The reason why I could not truly perceive the tubes proceeding from the cavity to the circumference of the Bone was, I think, this, that these tubes were far distant from each other; and, indeed, I thought that one tube lay among the longitudinal ones, as if an

opening had been made there for it. And though I could not be quite certain as to my seeing these tubes, I do not doubt that there are a great number of them in the Bone; and the rather, as I think it is to be noted, that the membrane covering the Bone is chiefly formed out of these vessels, and that it is also supported by them. And of this we cannot doubt, when we observe, that some trees have very small vessels dispersed among the perpendicular vessels, and by means of which the bark is chiefly composed.

And in like manner as we cannot with certainty point out the formation of the bark in the tree, because it is formed every year out of the horizontal vessels; so (according to my hypothesis), we are never likely to draw any other conclusion, than that the membranes covering the bones, receive their growth and nourishment from certain vessels which proceed from the cavity to the circumference of the bone, and there continuing to grow, are changed into thin and soft vessels, which protect the bone, in like manner as we observe the bark of trees are formed out of the wood, and defend it from external injury.

I know that many think the origin and nourishment of the bark of trees depend upon the root; but if it was so, we should observe those parts of the bark next the root to be very thick, and towards the upper part of the tree to grow thinner, and spread into branches, in like manner as we see the arteries spread from the heart, and the nerves from the brain; whereas, in the vessels forming the bark of trees, they are the same in the upper parts as about the roots, and what is more, the vessels in the bark of divers trees, and especially the birch, cherry, peach, and gooseberry, do not take an upward course, as in the oak, ash, elm, filbert, apple, and pear trees, but run only to the surface round about the tree; and as the barks of trees whose vessels mount upwards every year, increase in thickness; for when a tree increases in thickness, then the external part of the bark divides in fissures, and the old and dead bark adheres to the new; and for this reason, the older trees are, the thicker their bark, though only

a small part of the bark is alive, and that is what is next to the tree. But the case is otherwise, with regard to the bark of trees, where the vessels forming it, take a circular course round the tree; for when the trees increase in size, these vessels cannot separate asunder, but break; whereupon, the old bark is separated from the new and falls off, for which reason, such trees have always a thin bark, and this is plainly to be seen in the birch.

And in like manner as it has been said, that the barks of trees are formed and nourished, not from the root, but the wood, and that for the same reason, the bark is not formed in branchings. The same I also consider to be the case, in the production and nourishment of the real skin of our bodies, which is covered by an upper skin composed of Scales; for having examined the skin of several animals, I observed, that in its texture it was not produced irregularly, but I must confess, that the texture of the true skin was, throughout, an entire uniform body; and I also imagined, that all the vessels (excepting the arteries, veins, and nerves), of which our true skin is composed, do run one among another in a wonderful manner to form a skin which shall be of extraordinary strength; and that at length they grow thinner and thinner, till at their extremities, they form Scales, with which the skin is covered, and then these very thin vessels have no other termination than by being formed into Scales, so that each Scale consists of as many vessels as there were extremities composing the Scale, and that each Scale remains united to the vessels until a new Scale is formed under it.



On the calcareous substance which is found in the excrescences on the limbs of gouty persons, and commonly called Chalk-stones, with the Author's opinion respecting the possibility of dissolving the salts found therein ; and also on the cure of the Gout, by burning with the Moxa of the Chinese.

A Relation of mine being greatly tormented with the gout, and having made an incision in the heel of his foot, took out thence a quantity of that substance which physicians call Calx, or Chalk, and some of this being put into my hands, I found, upon examination, that it was composed of small irregular particles, resembling a heap of grains of sand: viewing these by the microscope they appeared of a very dark colour, and each particle to be composed of a great number of oblong and transparent figures, which cannot better be described than by a parcel of cuttings of horse-hair, with each extremity terminating in a point. I judged these to be so slender that many thousands of them would not be so thick as a hair of one's head. I have made a drawing of these, in order to shew the proportion of their length with their thickness, as may be seen at *fig. 16, A, Plate XV.* I saw many parcels of these particles lying in very regular order, as at *B,* and though I cannot say that they were so disposed throughout the whole substance, yet I believe they were originally formed in regular order of two, three, four, or more of them placed together. These small pieces of gouty Chalk I not only spread about, but also separated some of the above minute component parts, and I saw some of them lie in the position described at *B,* but many of them without any regular order, as at *C.* I also saw some of them not above half, or one third part, or a quarter the length represented at *A,* but as represented at *D;* though I do not think this

was originally their length, but that they were the fragments of particles which had been broken in the handling. Among them, also, were some irregular particles, which I judged to be globules of blood, mixed with the gouty matter.

Being desirous to pursue this subject farther, and to examine whether, by boiling the chalky substance in water, I could discover any acute saline particles pass from it into the water, I procured some newly taken out of the finger of a gouty person. This Calx was very white, and of a tough and gummy consistence.

Upon spreading this upon the surface of a new glass, I observed some globular parts lying in a tenacious or gummy kind of substance; they were of different sizes, the largest equal to grains of sand, and others adjoining to them only one twenty-fifth part of that size. The viscous matter was very transparent, mixed with many very minute transparent globules and oblong saline particles, many of which, viewed through a common magnifier, appeared like the fragments of a man's beard of a week's growth, others ten times thinner and one third the length; in a word, of many sizes, as well in length as thickness.

Having scattered some of this substance on a very thin piece of glass, I placed it before the microscope, directing the limner to make a drawing of it as it appeared to him, in order to give the reader some idea of the nature of this calcareous matter, and which is represented at *fig.* 17, I K L M. The whole of the globules here shewn, and the rest of the substance, were not altogether the size of a small grain of sand. In the figure is also to be seen the very minute saline particles I have mentioned.

I then endeavoured to dissolve this calcareous substance in clean rain water, and for that purpose put it into a new glass, and applied so strong a fire to it that the water boiled. I then again examined it, and saw the globular parts, which were of different sizes, and among which some of the oblong particles before mentioned could be distinguished, adhere as firmly together as if the water had

never been heated. These minute oblong saline particles were spread in such quantities in the water, that I did not think any of them had been dissolved in it.

I let this water stand for a time, in order to see whether the particles which gave it a whitish colour would sink to the bottom; but the water remained turbid, and when I suffered it to evaporate, there were so many minute particles left, that the whole substance appeared white.

This water, wherein I had so boiled the calcareous matter, I kept in my closet during the whole winter, and then again examining it, I saw the thin oblong particles before mentioned (which I judged to be fixed salts) lying in as great abundance as if the Calx had been newly extracted from the person's body.

From these observations, I concluded, that this calcareous substance, when in any part of a human body, cannot, by any medicine, be dissolved.

A small quantity of this Calx or Chalk, taken from the same person, I put into a small box well closed, and kept it for some months in my closet, I then put it into a new glass, and gradually exposed it to so fierce a fire, that the glass became red hot: I then perceived that, by the force of the fire, a thin pellucid liquor was extracted, and many globules of a yellow colour, but many of them afterwards, uniting together, exhibited a red appearance; but as soon as the cover of the glass, to which the thin transparent liquor and oil adhered, began to cool, all the liquor coagulated in an incredible number of saline particles, most of which were twenty-five times longer than broad, and were of various sizes, both ends terminating in points, as represented at *fig.* 18, A B C.

The oil which by the fire was extracted from this substance, and which while warm was a liquid, I saw was now changed into a solid substance, which I concluded was certainly occasioned by the multitude of saline particles coagulated in it: these particles in the oil were

much smaller than the preceding ones, but for the most part of the same shape.

I placed the glass to which these particles adhered in my closet, to see whether the particles, when the weather was rainy and the air damp, would not dissolve into a thin and pellucid liquor, and after twenty-six hours had elapsed, I saw their figures were all changed, for they were not only become shorter but assumed an irregular shape, nor did I see any saline particle which was not surrounded with some transparent liquor, and the oil itself was become thin and fluid.

After a few days I again inspected the glass, and then I saw all the saline figures changed into a thin transparent liquid, mixed with many small irregular and round particles.

But what appeared principally worthy of notice in these my observations was, that among so many saline particles there appeared none of the shape of our common salt, that is to say, four-square, or whose basis was four-square.

Upon the burnt ashes or *caput mortuum* of this calcareous matter, I poured some fair rain water, that if there were any fixed salt in the ashes, it might be mixed with the water; and having well stirred the whole together, and left them to stand until the water became clear, I then took a little of the water and exposed it in my study to the air, which was then damp, and soon after I saw in it a great number of thin flat saline particles of various sizes, which, for the most part, were of the shape represented at *fig.* 19, D E F.

I twice touched this water with my tongue, and was surpris'd at the great saltiness which so small a portion of the ashes had imparted to the water: but the taste of this salt was not like that of common salt, this being very astringent. I five or six times breathed on this fixed salt, and then I perceived most of the saline particles become liquid, but as soon as the moisture of my breath was evaporated, they again concreted in irregular figures.

I sometimes observed, upon spreading the water very thin, a great number of thin oblong particles gathered together, of the same figures as I had seen the minutest particles of the calcareous matter when taken out of the person's finger; but I thought, if there had been more water, those particles would have been larger and flat.

I often repeated these observations, to see whether I could discover any thing in this salt bearing an affinity with our common salt, but with all my attention, I never could discover any particles similar to those of common salt.

I again examined the water in which some of the calcareous matter had been boiled, and which had remained infused in it a whole winter, and though it was thick and turbid, I discovered in it an incredible number of minute particles, which all appeared round, nor did I discover any other saline figures than what are in common rain water, except that at one time I saw two saline figures in a place where the water had stood deeper than the back of a knife, but was now almost evaporated: these are shewn at *fig. 20, G and H.*

Upon the whole, we may conclude, that the salt of which the calcareous matter in the limbs of gouty people does for the most part consist, is of such a hard and tough substance, that though it be boiled, or suffered to remain in water a long time, the saline particles it contains will scarcely, if at all, liquefy or be dissolved: and hence we may conclude, as before mentioned, that it is impossible for physicians or surgeons to dissolve, discuss, or dissipate those knots or swellings which arise in the joints of gouty people, and are commonly called Chalk Stones, by any kind of plaister or fomentation; but that the only thing which promises any success in this way, must be by burning the part in the manner practised by the Chinese, who for this purpose use what they call Moxa.

This is a certain preparation used in the East, and mentioned by

H. Buschoff, a writer on the subject, in these words:—" Moxa is
 " a soft woolly substance, prepared with great art, from a certain
 " dried herb; the name of the herb I know not, but I do not
 " doubt that it is one of the most excellent in all the world.
 " No druggist or apothecary in all Europe knows the manner of
 " preparing Moxa, but the art of doing it, is held in such estima-
 " tion by the Chinese and Japanese, that they will not sell the fe-
 " cret to other nations at any price."

My opinion upon the subject is, that Moxa is only an excrescence on some fruit, like the woolly substance on peaches, and some other fruits; (and *Wilhelm ten Rhyne*, another writer on the subject, says, that it is the woolly part of a leaf) and I have at times collected divers woolly substances, which are found on some leaves and fruits, and burnt them, particularly that wool which falls from the poplar tree; this, at first sight of it, I hoped would take fire very readily, and I thought the same of the wool produced by the willow, about the beginning of June, which is of a more inflammable nature than what is gathered from the poplar. But I do not as yet know of, nor have found any vegetable woolly substance, which burns with such facility as cotton; therefore, I would recommend it to those who are afflicted with the Gout, and desire to try the experiment of a cure by fire, and cannot procure Moxa for that purpose, to use cotton, which may be had in plenty. I have also tried the experiment of burning with common tinder made from linen; but this burns so fiercely, and penetrates so deep, that I believe the use of moxa or cotton ten times, would not burn so deep as one single application of common tinder.

ADDITION, BY THE TRANSLATOR.

IT may not be unacceptable to the reader, to see what has been said on this subject by Sir William Temple, who was cotemporary with our author, and one of the most eminent persons of his time. He himself tried the application of Moxa in a fit of the Gout, and the following is an extract from his Miscellanea, in his Essay on the Cure of the Gout by Moxa.

“ Being in the seven-and-fortieth year of my age, at the Hague, about
 “ the end of February, one night at supper, I felt a sudden pain in my
 “ right foot, which, from the first moment it began, increased sensibly. I
 “ went to bed, but it raged so much all night, that I could not sleep a wink.
 “ I endured it till the next morning, and then making my complaints and
 “ shewing my foot, they found it very red and angry, and to relieve my ex-
 “ tremity of pain, began to apply common poultices to it, and by the fre-
 “ quent change of them I found some ease, and continued this exercise all
 “ that day and a great part of the following night, which I passed with very
 “ little rest. The morning after, my foot began to swell, and the violence
 “ of my pain to assuage, though it left such a foreness, that I could hardly
 “ suffer the clothes on my bed, nor stir my foot but as it was lifted.

“ By this time my illness was concluded to be the Gout, and among many
 “ other friends who came to see me, Monsieur *Zulichem* paid me a visit,
 “ and in talking of my illness, he asked me whether I had ever heard the
 “ Indian way of curing the Gout by *Moxa*. This *Moxa*, he said, was a
 “ certain kind of moss that grew in the East Indies; that their way was,
 “ whenever any body fell into a fit of the Gout, to take a small quantity of
 “ it, and form it into a figure, broad at bottom as a two-pence, and pointed
 “ at top; to set the bottom exactly upon the place where the violence of the
 “ pain was fixed, then with a small round perfumed match (made likewise
 “ in the Indies), to give fire to the top of the moss, which burning down
 “ by degrees, came at length to the skin, and burn it till the moss was con-
 “ sumed to ashes. That many times the first burning would remove the
 “ pain, if not, it was to be renewed a second, third, and fourth time, till it
 “ went away, and till the person found he could set his foot boldly to the
 “ ground and walk. This operation Monsieur *Zulichem* said, he became
 “ acquainted with by the relation of several who had seen and tried it in the

“ Indies; but particularly by an ingenious little book written of it by a
 “ Dutch minister at Batavia, who being extremely tormented with a fit of
 “ the Gout, an old Indian woman coming to see him, undertook to cure
 “ him, and did it immediately by this *Moxa*; and after many experiments
 “ of it there, he had written this treatise of it in Dutch, for the use of his
 “ countrymen, and sent over a quantity of the mofs and matches to Utrecht,
 “ to be sold, if any could be persuaded to use them.

“ I had always heard that a fit of the Gout used to have six weeks at the
 “ least for its ordinary period, a delay very distressing to one in my situa-
 “ tion, for I was at that time prest in my journey to *Nimeguen*, by his
 “ Majesty’s * commands, to assist at the treaty there. Most of the ambaf-
 “ fadors from the several parts of Christendom were upon their way, one of
 “ my colleagues was already upon the place, and I had promised immed-
 “ iately to follow. This made me ponder on Monsieur *Zulichem’s* new
 “ operation; and upon reflecting how many cures have been performed
 “ by fire, I resolv’d upon making the trial, and dispatched a messenger to
 “ Utrecht to bring me some of the *Moxa*, and learn the exact method of
 “ using it from the man that sold it, who was son to the minister at Bata-
 “ via. He returned with all that belonged to this cure, having performed
 “ the whole operation upon his hand, by the man’s direction. I immedi-
 “ ately made the experiment in the manner before related, setting the
 “ *Moxa* just upon the place where the first violence of my pain began,
 “ which was the joint of the great toe, and where the greatest anger and
 “ foreness still continued, notwithstanding the swelling of my foot; so that
 “ I had never yet, in five days, been able to stir it, but as it was lifted.

“ Upon the first burning, I found the skin shrink all round the place;
 “ and whether the greater pain of the fire had taken away the sense of a
 “ smaller or no I could not tell; but I thought it less than it was: I burnt
 “ it the second time, and upon it observed the skin about it to shrink, and
 “ the swelling to flat about it more than at first. I began to move my toe,
 “ which I had not done before, but I found some remainders of pain. I
 “ burnt it the third time, and observed still the same effects without, but a
 “ much greater within, for I stirred the joint several times at ease; and,
 “ growing bolder, I set my foot to the ground without any pain at all.
 “ After this I pursued the method prescribed by the book, and the author’s

* King Charles II.

“ fon at Utrecht, and had a bruifed clove of garlic laid to the place that
 “ was burnt, and covered with a large plaifter of *diapalma*, to keep it
 “ fixed there; and when this was done, feeling no more pain, and tread-
 “ ing ftill bolder and firmer upon it, I cut a flipper to let in my foot, fwel-
 “ led as it was, and walked half a dozen turns about the room, without
 “ any pain or trouble, and much to the furprize of thofe that were about
 “ me, as well as to my own.

“ For the pain of the burning itfelf, the firft time it is fharp, fo that a
 “ man may be allowed to complain: I refolved I would not, but that I
 “ would count to a certain number, as the beft meafure how long it lafted.
 “ I told fix fcore and four, as faft as I could; and when the fire of the
 “ Moxa was out, all pain of burning was over. The fecond time was not
 “ near fo fharp as the firft; and the third was a great deal lefs than the
 “ fecond. The wound was not raw as I expected, but looked only fcorched
 “ and black; and I had rather endure the whole trouble of the operation,
 “ than half a quarter of an hour’s pain in the degree I felt it the firft whole
 “ night.

“ After four-and-twenty hours I had it opened, and found a great blifter
 “ drawn by the garlic, which I ufed no more, but had the blifter cut,
 “ which run a good deal of water, but filled again by next night; and
 “ this continued for three days, with only a plaifter of *diapalma* upon it:
 “ after which the blifter dried up, and left a fore about the fize of a two-
 “ pence, which healed and went away in about a week’s time longer; but I
 “ continued to walk every day, and without the leaft return of pain, the
 “ fwelling growing ftill lefs, though it were near fix weeks before it were
 “ wholly gone. I favoured it all this while more than I needed, upon the
 “ common opinion, that walking too much might draw down the humour;
 “ which I have fince had reafon to conclude a great miftake, and that if I
 “ had walked as much as I could from the day the pain firft left me, the
 “ fwelling might have left me too in a much lefs time.

“ I paffed that fummer at *Nimeguen*, without the leaft remembrance of
 “ what had happened to me in the fpring, till about the end of September,
 “ and then began to feel a pain, I knew not what to make of, in the fame
 “ joint, but of my other foot: I had flattered myfelf with hopes that the
 “ vapour had been exhaled, as my learned authors had taught me, and
 “ that thereby the bufinefs had been ended: this made me neglect my

“ *Moxa* for two days, the pain not being violent ; till at last my foot began
 “ to swell, and I could set it no longer to the ground. Then I fell to my
 “ *Moxa* again, and burnt it four times before the pain went clear away, as
 “ it did upon the last ; and I walked at ease as I had done the first time,
 “ and, within six days after, about a league, without the least return of
 “ my pain.

“ I continued well till this spring, when, about the end of March, feel-
 “ ing again the same pain, and in the same joint, but of the first foot ;
 “ and finding it grow violent, I immediately burnt it, and felt no more
 “ after the third time ; was never off my legs, nor kept my chamber a
 “ day. Upon both these last experiments I omitted the application of gar-
 “ lic, and contented myself with a plaister only of diapalma upon the
 “ place that was burnt, which crusted and healed in a very few days, and
 “ without any trouble.

“ I have since continued perfectly well to this present June, and with so
 “ much confidence of the cure, that I have been content to trouble myself
 “ some hours with telling the story, which, it is possible, may at one time or
 “ other be thought worth making public, if I am further confirmed
 “ by more time and experiments of my own or of others. But this cure, I
 “ suppose, cannot pretend to deal with inveterate Gouts, grown habitual
 “ by long and frequent returns, by dispositions of the stomach to convert
 “ even the best nourishment into those humours, and the vessels to receive
 “ them ”

Thus far Sir William Temple : to which may be added, a remark made
 by Dr. Henry Bracken, an eminent physician in his time : he observes, that
 the aversion men have to endure pain prevents their experiencing many
 great cures which might be performed by fire ; and adds, that he knew an
 instance of an obstinate sciatica, or hip gout, cured by a resolute use of
 causticks : this application approaches the nearest to actual fire, and perhaps
 the fire of *Moxa* may be of so mild a nature as to be next in degree to a
 caustick.



On the nature of Stones in the Bladder, and Gravel in the Kidneys, and the Author's examination of the question whether the same can by any means be broken or dissolved.

A VALUABLE friend of mine, the late eminent Constantine Huygens, sometime before his death, did frequently, both in conversation, and in his letters to me, express a great desire to know the reason why the Stone in the bladder produces such great pain, and I having the same desire of enquiring into the nature of those Stones, procured some of them for my examination.

I put one of these Stones, which weighed about the fifty-eighth part of a pound weight, into a new glass, to which I applied so great a heat, that the salt and oil were in part expelled from the Stone, when the glass broke; I thereupon put it into another glass, and by the application of fire, I drew off the remainder of the salt and oil. Upon this I poured rain water, and I observed that they immediately united with the water. This mixture I suffered in part to evaporate, and then I discovered in it an incredible number of wonderfully minute saline figures, so small indeed, that a million of them would not equal a grain of sand; and with all my attention, I could not discover their exact shape, for the water or the particles in it impeded my view: however, I was persuaded they had four sides. I also examined some saline particles which adhered to the head of the glass, and I saw many of them to be thick and broad in the middle, and the ends terminate in a point. I also examined the vessel in which I had put a little of the salt and oil, and here, besides the before-mentioned figures, I saw some saline particles like *fig. 21* and *22*, and several like *fig. 23*, each of them with a rising on the back, and all transparent like crystal.

I kept this water in my study for some days, frequently looking at it, and I always found that much of the liquid, as it seemed to my naked eye, did not evaporate; and at length I saw many saline particles in it like *fig. 23*; but these, except a few, were so very small, that they could not be discovered without great attention.

Moreover, I poured rain water on the caput mortuum or ashes, having first pounded it small, in order to extract the fixed salt. I suffered this water in part to evaporate, and I discovered in it an incredible number of minute saline particles, which in many places were collected or coagulated together in great numbers, and yet retained their particular figures: this collection or coagulation of particles, bore some resemblance to the figures of sun flowers, so that in them I could not discover the saline particles, unless at the edges, where they represented the leaves of the flowers, as is shewn at *fig. 24*. In other places the saline particles lay in an irregular manner on one another.

I saw also many single particles, of the shape shewn at *fig. 25*, all their extremities terminating in points. Upon keeping my eye long fixed upon a place where only some few small particles of water lay, the heat which my touch occasioned, (the air being then moist), caused a great number of particles to arise with one of their ends above the water; but, on the application of a little more heat, these newly formed particles disappeared, and with still more heat, some little vapour arose, and many of the saline particles were changed into a transparent watery liquor, but soon after all that liquor in many places was changed into irregular saline particles; these saline particles were so soft, that upon breathing on them they again changed into a fluid; but when I again applied a strong heat, I saw them to be more solid. The particles of salt which had concreted in the shape of sun-flowers, and some others of the same species, remained unaltered, so that I concluded both these were fixed salt.

Farther, I drew some blood from my finger, and mixed it with different parts of the water wherein was that fixed salt, and I observed, that the globules of blood from whence its redness proceeds, appeared as if their moist parts had been extracted by heat, for each globule appeared of a different shape, some flat, and I could see that they were composed of smaller globules; others looked like dead animalcules, whose feet displayed themselves, and in a word, so many different figures, that I cannot rehearse them all.

After this, I took some water wherein the volatile salt was dissolved, and mixed it also with some blood, and I immediately perceived many of the globules of blood to be so altered or dissolved, that they could not be distinguished without great attention, some of them preserving their figure longer than others; but this appearance I did not perceive, unless where a very little blood was mixed with much water.

Another of these Stones, in weight equal to about the twenty-ninth part of a pound, I applied to a very strong fire, and observed the volatile salt, which proceeded from it, remained fluid so long as it continued warm; but when I took the black burnt part of it, it lay divided in many scaly parts like the coats of an onion, and that part which was not so divided, was the internal part of the Stone, which preserved its round shape, and was not larger than the head of a large pin. This burnt substance, or caput mortuum, I weighed, and found it contained ten parts of a pound, so that nineteen parts, consisting of salt and oil, had been driven out of this Stone; and when I poured water on the residue, and suffered it to evaporate, I found, upon weighing, that it was reduced to eight parts. While the salt and oil I had extracted remained warm, I collected a small portion of them, and saw that the oil, which was of a yellowish colour, by reason of the great number of saline particles in it, was not fluid.

After this, I put camphire in the water containing these salts, but I did not perceive them mix with the water, though the camphire stood in it for two days.

I afterwards poured off the water and poured spirits of wine on the salts, but I did not perceive them unite with the spirits of wine.

In all my observations, I never saw any of the saline particles in shape like the figures of our common salt, and I will venture to say, that the salt which we commonly use with our food, cannot produce any addition to the Stone in the Bladder, unless it could be proved, that such salt can be converted into a stony substance in the body. But I know that many may not give credit to this, and alledge, that though the Stones in the Bladder may, in part, consist of common salt, yet by applying fire to them in the way I have mentioned, they might assume the different figures I have described to be in the volatile and fixed salts extracted from thence. In order, therefore, to satisfy myself and others in this respect, I took some common salt and put it into a new glass, to which I applied so strong a fire, that the glass became red hot, and the salt dissolved into water: when the glass became cool, I broke it, and taking out the salt, I mixed it with fair rain water, and then observing it, I saw in the space of half a minute, nothing floating in the water, except particles of common salt, but none like the fixed or volatile salt extracted from the Stone in the Bladder: when this burnt salt had stood for some hours, it was changed (the air being then moist) into a watery liquor, and upon applying some heat to it, nothing appeared except figures of common salt.

And since we see that Stones taken out of the Bladder do in part consist of such a fixed salt as is produced by infusing the burnt matter in water, and which seems for a time to unite with the water, but soon afterwards coagulates, and becomes so hard, that though the water is boiled, this salt will not mix with the water; and also that by spirits of wine or camphire it cannot be dissolved;

and that the volatile falt in it (for of thefe two falts the Stone in the Bladder does for the moft part confift) when expofed to the moift air, will not difolve: that in the volatile falt there are particles fo fixed that they cannot be difolved but by the force of fire; I cannot, for my part, perceive that any medicine can be found of fufficient efficacy to difolve or break in pieces the Stone in the Bladder. And when we reflect on the fharp-pointed particles of the volatile and fixed falts which, for the greateft part, compofe the Stone in the Bladder, we are not to wonder that thofe Stones excite fo much pain; efppecially, if we confider the fharpnefs of fo many wonderfully minute faline particles, and the many nerves in the Bladder, (for with any frefh accretion of matter new faline particles are added) which, with their fharp points pierce and irritate the infide of the Bladder.

I broke one of thefe Stones into fmall pieces, and examined them by the microfcope, when I found that thefe Stones confift of fuch irregular parts as if we beheld a fubftance compounded of a great quantity of grains of fand. And in thefe obfervations I often perceived thofe wonderfully minute particles which I deemed to be falts, and which I have faid confift of volatile falt, and which appear to be of the fame nature with the calcareous fubftance in the limbs of gouty perfons. I have caufed a drawing to be made of a very fmall particle, broken off from the interior part of one of thefe Stones, partly to fhew the formation of the Stone, and partly to exhibit the wonderfully minute and fharp-pointed faline particles, but which I feldom faw in fo great a number as are here fhewn; the reafon of which I take to be, that the particles of which the Stone confifts adhere together fo very clofely.

Fig. 26, A B C D E F G H, is this fmall particle of Stone, which to the naked eye appeared the fize of *fig. 27*. B C D E F, represent the very minute faline particles which are only to be diftinguifhed at the edges of this fragment. The Stone from which this piece was taken, together with feven or eight others, had been

fifteen years ago taken out of the Bladder of a child, after its death; the outside of it was of an ash-colour, very smooth, but under the outside shell the whole Stone was as white as chalk, and, in some places, the saline particles of which it consisted glittered like fragments of glass.

I at different times mixed the water in which the volatile and fixed salts taken from these Stones had been infused, with a small portion of blood taken from my finger, and found the globules of blood to be as it were dissolved or separated into minute parts by the mixture.

An intimate acquaintance of mine, and a person of eminence in this country, with whom I had formerly travelled, came to me in a state of great pain, and complained, that now he was growing in years he was greatly afflicted with the Stone in his Bladder, and asked me my opinion, whether any means could be devised to dissolve or break it in pieces; to which I replied, that I conceived it to be impossible; and I also told him, that he had contracted this disease by his own manner of living. For, in our journies together, he had boasted that he accustomed himself to bear hunger, thirst, and cold; and he also told me, that in hunting he only held a small leaden bullet in his mouth, and by continually moving it about, he could go for a whole day without drink, when others could not dispense with it; and to which I had replied, that while we hold a leaden bullet in the mouth, its weight and coldness causes us to keep it continually in motion; and by the constant motion of the jaws, there is a continual secretion of spittle in the mouth, which we swallow, and thereby keep the throat moist, so that we do not so much require drink as other persons, but nevertheless our bodies are not the less injured by the want of it. And the moisture which is thus swallowed, and passes through the Kidneys, is very thick, and in small quantity, and more obnoxious to the coagulation of salts and other particles of which the Stone in

the bladder is compos'd, than if we were to drink in sufficient quantity.

Another gentleman of eminence, who was troubled with the Gravel, and had often been at my house, came to visit me, with an old physician of some note, and brought with him a small box, filled with Gravel Stones, asking me, whether I thought there was any remedy which would dissolve such Gravel in the Kidneys; but this I pronounced was not to be found, by reason that these Gravel Stones do in great part consist of such hard saline particles, that they cannot be dissolved otherwise than by actual fire, or very strong aqua fortis; therefore, my opinion is, that we are only to look for a remedy to prevent the further increase or collection of them; and, I think, that to drink tea or coffee promises much for this purpose, because thereby much liquid passes to the Kidneys; and further, that we should drink plentifully of beer, between dinner and supper time, and not follow the example of those who boast that they drink very little in the day, and none at all at supper.

In order to prove the truth of this my opinion, I put one of these Gravel Stones into a glass, and poured on it very strong wine vinegar: this I set by for a whole year, in which time almost all the vinegar was evaporated, and, at the end of that time, I saw the Stone remain entire. I then poured more vinegar on it, and set it by another whole year, when the vinegar was almost all again evaporated; the Stone was then still entire; but, upon squeezing it a little, it broke into many pieces, some of which were more than a thousand times smaller than sand; and I often saw a small piece of this Stone, no larger than a grain of sand, to consist of many smaller particles concentered together; and the multitude of minute particles of which these Stones are compos'd appeared to me, in this instance, much more distinctly than I had ever seen them, or could have imagined; and the minute parts into which the Stone was broken were yet very hard.

Hence we may more certainly pronounce than before, that there is no medicine of sufficient efficacy to dissolve the Stones or Gravel in the Kidneys.

I have often heard in conversation, that medical men, and particularly surgeons, are accustomed to forbid those who are afflicted with the Gravel, the use of some particular kinds of diet. But, for my part, I am well assured, that there is no kind of food in common use, which will not generate that calcareous matter which in gouty persons is called chalk stones, and likewise Stones and Gravel in the Bladder or Kidneys. But, in short, some physicians and surgeons talk of the efficacy of medicines in our bodies, and also of the constitution of our bodies themselves, in much the same manner as blind men talk of colours.



On the nature and component parts of Gunpowder, and the quantity of air produced by its explosion: together with the Author's sentiments respecting the size of great Guns, and his observations on condensed and rarified Air.

IT has long been my opinion, that all the saline particles which by the force of fire are elevated or driven upwards, from different substances, must be of a globular figure; because by the fire they are rendered soft, and are driven about with a very quick motion; in which opinion I was confirmed by recollecting the observations I had formerly made on Gunpowder. And now, to repeat those experiments, I provided several glass vessels, which I made as clean as if they had newly come out of the glass-house; some of these as deep as the breadth of three fingers, others of four, and others of six. These I heated, in order to expel from them all the moist air of the atmosphere which we experience in this country in the autumn, and to introduce in its place a more rarified or thinner air than that in which we breathe. In a glass so prepared, I placed one or more of the largest grains of Gunpowder, stopping the glass so closely as to exclude all the moist atmospheric air; and I then applied to that part of the glass where the Gunpowder lay, a sufficient degree of heat to fire the Powder. Hereupon the glass appeared filled with a kind of white smoke, in which could be seen, both at the bottom and sides of the glass, the * charcoal and sulphur, which had entered into the composition of the Gunpowder. When, in this

* It is generally known that Gunpowder is composed of saltpetre, sulphur, and charcoal, in the proportions of three-fourths saltpetre, one eighth sulphur, and one eighth charcoal; the whole well pounded and mixed together, and afterwards prepared for use in the form of small grains, resembling seeds.

experiment, two, three, or more grains of Gunpowder were put into the glaſs, the charcoal and fulphur were diſperſed more torcibly than when a ſingle grain was uſed ; and I could plainly diſtinguiſh the particles of the fulphur from thoſe of the ſaltpetre, for in ſome places they lay ſo thick together as to exhibit a yellow colour. The white ſmoke (as it appeared to the eye) was in fact the ſaltpetre. This I immediately brought before the view of a good microſcope, and ſaw with great pleaſure the inconceivable number of its component particles, of an incredible minutenefs, in a kind of circular motion, one among another ; and, to my moſt accurate inſpection, they all appeared of a perfect round figure, beſides ſeveral others which adhered to the glaſs : after a ſhort time, obſerving that theſe moving particles began gradually to ſubſide, I placed the glaſs not in an upright poſition, but on its ſide, that they might not ſink to the bottom among the particles of charcoal and fulphur. When the glaſs had remained in this poſition ſo long that I judged all the ſaltpetre particles were in a ſtate of reſt. I again examined them, and ſaw with great ſurprize, that almoſt all theſe particles which a little time before appeared globular, or to which I could not aſſign any other figure than that of globules, were now converted into hexagons, or ſalts of ſix ſides, ſome of which were very exactly ſhaped, others more irregular hexagons ; and many of them, I could plainly diſcern, riſing in a kind of piramidical ſhape, like a pointed diamond. When I examined thoſe parts of the glaſs where the particles of ſaltpetre lay in conſiderable numbers together, I could diſcover but few of them to be of an hexagonal ſhape, by reaſon that the points of the particles (at the time they changed from globular to hexagonal figures) touching each other, they tuck ſo cloſely together as to exhibit, upon the whole, only irregular figures. In another glaſs I ſaw (among the figures before deſcribed) ſome of the particles of the ſaltpetre very long and thin, ſome of which had the reſemblance of

a bundle of arrows tied together, with their ends pointing different ways.

Besides the foregoing observations, I saw, at the first firing or explosion of the Gunpowder, a very thin moisture in the upper part of the glass, which seemed to consist of globules of different sizes; which moist substance, I was well persuaded, did for the most part proceed from the saltpetre, and which I shall therefore call oil of saltpetre, though it might perhaps be mixed with some of the oil of sulphur. In order more fully to satisfy myself in this respect, I put into one of the glasses some refined saltpetre, leaving a small opening at the top of the glass, that it might not burst in the experiment, and then applied so strong a heat to it that the saltpetre boiled very much; my only view in this was, to examine the vapour or moisture which arose from the saltpetre, and to which I have given the name of oil; and I perceived, in the upper part of the glass, a large quantity of transparent and very fluid matter, which, upon inspection, could be denominated no other than oil. At another time, I saw this substance or oil so collected or run together, that it appeared like drops of water spread irregularly upon the glass.

Not content with the preceding observations, I provided some other vessels of very thin glass, in order to pursue my observations with more distinctness, and to see if I could not get a view of the changing of the saltpetre particles from globular to hexagonal figures, to which intent, immediately as the Gunpowder in the glass was fired, I brought the glass before the view of the microscope, and then I saw the globular particles of saltpetre change their figure to hexagonal, and this was performed instantaneously, or as quick as a flash of lightning to the eye, not in one or a few of the particles, but all of them, as I may say, in a moment; and though at the bottom of the glass, where the greatest number of particles subsided after their first motion ceased, I saw an immense number of small particles lying to which I could not assign any

figure; yet I doubted not they were all of the same shape with the former: and I was confirmed in this my opinion, by observing now and then a larger particle among them, which was of the same shape I have mentioned. The number of these saltpetre particles produced from one grain of Gunpowder was so inconceivably great that I dare not make a computation of them, they were more in number than the particles of sulphur and charcoal; and when I figured in my imagination any given particle, the size of a grain of sand, to be divided into a thousand millions of parts, I concluded in my mind, that the particles of saltpetre, sulphur, and charcoal contained within a grain of Gunpowder, were much more numerous. These observations were made with the greatest distinctness when I used only one grain of Gunpowder; for when three grains were fired together, the saltpetre blew up with it the sulphur and charcoal, the particles of which not only rendered the view indistinct, but also impeded the change of the globular figures into hexagons.

I have also taken notice, that if a grain of Gunpowder is fired at the bottom as it lies, then the sulphur and charcoal it contains are blown up on high; but if it is fired at the top, then but little of the charcoal, and still less of the sulphur, are driven upwards.

These observations caused me to reflect on the manner in which Gunpowder is proved amongst us, to judge whether it be of a good quality: this is performed by taking the quantity of a musket charge of Gunpowder, and placing it in as close an heap, of a conical shape, as may be, on clean white paper, and then giving fire to it with a burning match; and if, in this operation, the paper is neither burnt nor blackened, the Gunpowder is esteemed to be good. Nevertheless, I cannot but think that much of this depends upon the person who gives the fire; for if the point of the burning match is applied to the upper surface of the Gunpowder, the consequence must be, that the paper will be very much blackened, and also burnt, by reason that the upper part of the Gun-

powder first takes fire, and exerts its force on the lower part, which is last kindled, whereby the fire and smoke of this last part is driven downwards: but if the person who gives the fire does, with a light touch, so apply the match that it may fire the lowest part of the heap as soon or sooner than the upper part, it must follow that the lower part of the powder will blow upwards what lies on the upper part and sides of the heap, and this before it is fully kindled, in consequence of which the paper will be little injured. In a word, one man may so fire a parcel of Gunpowder that it will be deemed good powder, and another man may fire some of the same sample in such a manner that it shall be condemned as unserviceable; that is, not sufficiently strong, or as being damp; for considering that some portion of time is required (as must certainly be the case) for the fire of one grain of Gunpowder kindled by another from beneath, to blow up the upper part of the same grain, much more time must be required in a whole musket charge of Gunpowder for the fire of one grain to be communicated to the others.

I have also turned my thoughts on observing the firing of Gunpowder in a close glass to the following consideration, namely, whether the motion of the saltpetre particles and the fire did not leave in the glass a greater quantity of air compressed together. To make trial of this, I took several small glasses, into one of which I put a large grain of Gunpowder, into another two, and into another three of such grains, and, having perfectly closed all the glasses, I set fire to the Gunpowder in each of them, and left the glasses at rest, until I judged that all the saltpetre particles were subsided, then opening the glasses, the air rushed out with violence.

I afterwards repeated this experiment with several other glasses, leaving some of them four, others five days untouched, and, upon opening them, the air rushed out with as much violence as in the former experiment.

Moreover, I took several other glasses, and inclosed in each of them three grains of Gunpowder, which (the glasses being first carefully closed) I set on fire; but in this experiment some of the glasses flew in pieces, though they were all of the same size, and the quantity of Gunpowder the same in each. The cause of this I took to be, that in the broken glasses the grains of Gunpowder might be fired altogether, or blown up more hastily than in the others. The glasses which remained uninjured, after the space of twenty-four hours had elapsed, I opened in such a manner that the air within them (which in these glasses was more compressed than the common air we breathe, and more than the glasses themselves originally contained) might empty itself into a glass globe filled with water, having a small neck, and placed not upright, but sloping, in order that I might observe accurately the quantity of air which was forced out of the glass wherein the Gunpowder had been fired into the glass globe filled with water; for so much air as was forced into the globe, so much water must be driven out of it.

To ascertain this, I took several glasses, and after having fired the Gunpowder in them, as before related, I weighed them, though to avoid prolixity, I will here only relate the computations I made with one of them. This glass weighed seventy-seven grains, and when the compressed air it contained had been made to pass into the before mentioned glass globe, I filled the other glass with water, and weighing it, I found that the water contained in the cavity of the glass weighed sixty-three grains: with this water I filled up the cavity in the glass globe, which had been made in it, by introducing the air generated by the explosion of the Gunpowder, and I found that fifty-five grains of water had been driven out of the glass globe, for I had only eight grains of water remaining after filling up the cavity or space; consequently, the firing three grains of gunpowder in the glass, produced such a compression of air within it, that when the glass was opened, nearly seven-eighth parts of the air it then contained rushed out; so that the air in the glass, which before

firing the Gunpowder in it was of the same density with common air, did, upon the firing, become so condensed or compressed as to require, for attaining the natural state or liberty, an extension of space in the same proportion.

With these observations I was not yet contented, and, among other experiments, I took a glass, into which I put a common sized grain of Gunpowder, leaving at the extremity of the glass an opening the size of a common pin; this opening, which was at a thin and pointed end of the glass, I immersed in the before mentioned glass globe filled with water, and upon firing this single grain of Gunpowder in the glass, so great a quantity of air was driven into the glass globe, that to replace the water which was thereby expelled, I had need of one hundred and sixty grains of water, over and above the water which had found its way out of the globe into the glass vessel wherein the Gunpowder was fired. This last mentioned glass vessel (which with one more were the only two of several used by me, had remained entire; for many of the glasses upon the rushing in of the water, burst in pieces), I weighed before I fired the grain of Gunpowder, in order, after the firing, to know the size of the cavity it contained, and found that the water in it weighed one hundred and fifty grains.

And now, to make a calculation with as much accuracy as possible, we must lay out of the case, the size of the cavity of the glass wherein the Gunpowder was fired, and say only this, a common sized grain of Gunpowder, in its explosion, forces so much air into a glass globe, as fills a space equal to that occupied by one hundred and sixty grains in weight of water; and to make a comparison (though it will be a very uncertain one) between the size of a grain of Gunpowder and the quantity of air it produces on being fired, we will say, that water and Gunpowder are of equal weight, though this is not so in fact, for Gunpowder sinks in water, and therefore is heavier. Now let thirteen grains of Gunpowder be considered to make the weight of a grain, these thirteen grains of Gunpowder

multiplied by 160, the product is 2080, and so many times more space does a grain of Gunpowder occupy when fixed, than what it did before.

This is then the magnitude or quantity in space of the expansion or explosion of Gunpowder, computed by me with all the accuracy I was able, though by no means to be deemed perfect, because one grain of Gunpowder will explode much more completely than another, by reason that the mixture of saltpetre, sulphur, and charcoal, is not in their proportions alike in all, as a proof of which, I have observed in firing a grain of Gunpowder, that part of it has been consumed before the rest blew up.

But one circumstance in these my observations appeared most worthy of note, namely, that with the same velocity as the air rushed out of the glass wherein the grain of Gunpowder was fired into the glass globe, with equal velocity was the water forced out of the glass globe, through the narrow opening of the glass where the Gunpowder had been fired, inasmuch as almost half to fill that glass with water; the cause of which, I conceived, could be no other than this; the fire produced by the kindling of the Gunpowder inclosed within the glass, requires great extension, whereby the air in the glass must in part be driven out, and whereas with the same rapidity as the fire was kindled, it will escape through the pores of the glass into the air, so upon the fire's quitting the cavity of the glass, some other substance must follow in its place to occupy the vacuum, whereupon this vacancy in the glass must be filled with water, because the opening in the glass is immersed under the water.

From this regurgitation or rushing of the water into the glass, when the Gunpowder was fired, after the fire had escaped from thence, I was led to reflect on the firing of Gunpowder in a Cannon or great Gun, and I concluded that a ball fired from an over-long great Gun would not have so much force, nor be driven to so great a distance as if the Gun was of a more moderate length, for

I argued thus with myself; in a Gun eighteen feet long, half as much time again is required for the Gunpowder to drive the ball to the muzzle of the piece, than in a Gun which is twelve feet long; in which length of time, much of the Gunpowder in the long Gun is consumed, before the ball gets into the open air; this consumed Gunpowder is not only wasted, but no sooner is the Gunpowder exploded, than immediately nearly one-fourth part of the expansion escapes out of the Gun, which space must be filled up by some other substance, and this is supplied from that part of the Gunpowder which first takes fire, and thus the force of the powder is diminished before the ball has quitted the Gun, in like manner as I have before said, respecting the firing of Gunpowder in a glass, namely, that so soon as it is fired and the flame is spent, the air within the glass is rendered so thin and subtle, that about a fourth part of the expansion produced by the firing of the Gunpowder must be replaced in order to fill the glass again.

In conversation with a certain gentleman of high authority in military matters, who came to visit me in company with some foreigners of rank, I mentioned my opinion of the inutility of too long great Guns, (for our discourse turned upon that subject), and he informed me, from his own experience, that upon firing two guns, one of eighteen feet long, and the other of fourteen feet long, in order to prove their greatest power, he had seen the gun of fourteen feet long, carry its ball much farther than the other, which was eighteen feet long, and hereby I was confirmed in my before mentioned opinion

It was formerly my firm belief, that there could be no other reason of the great explosion produced by the firing of Gunpowder, than first, from the saltpetre particles being put into a violent agitation and divided into smaller parts; and secondly, from the fire, and that when those particles subsided to a state of rest and the fire escaped, the remaining substances left no greater space than they had occupied before the saltpetre was by the fire divided into many

parts ; but now we see the contrary to be the fact, for the particles of air in a closed glass, wherein Gunpowder is fired, are so compressed together, that seven parts out of eight will escape out of the glass upon its being opened.

I have endeavoured to satisfy myself upon this subject, by saying that the particles of air in their natural state in the glass, before the firing of the Gunpowder, were, upon such firing, divided into still smaller particles, and that those smaller particles being of a globular figure, must (so long as they remain distinct and not forced one into another) touch each other on their superficies, and leave between them in the remaining spaces, some more thin and subtle substance ; whence it must follow, that the particles of air within the glass, will require a greater space than they occupied before ; but on the other hand, I considered, that though every particle of air which was in the glass before the firing of the Gunpowder might, upon the firing it, be divided into a thousand smaller parts, this would not produce so great an expansion as we experience ; for, let a sack of wheat be ground into fine flour, every grain will be divided into a thousand parts at least, and though the wheat is divided into many small particles, it will not occupy seven or eight times greater space than it did before the grinding. In short, with all my observations respecting the great expansion or explosion of Gunpowder when it is fired, I have not been able to explain the matter to my own satisfaction.*

* Mr. Leeuwenhoek, in this Essay, attributes the force of Gunpowder, when exploded, to some alteration produced by it in the circumambient air, rather than to the production or generation of air from the Gunpowder itself ; and though in some few places he expresses himself in a manner, intimating, that air is generated by Gunpowder when fired, he does not seem to have been fully informed of the doctrine now established, namely, that Gunpowder, or rather its principal component, Saltpetre, contains in it a great quantity of air condensed as much as possible, which, when set at liberty, upon the firing the Gunpowder, does, by its elasticity or expansive power, produce the violent and surprising effects we observe. Mr. Leeuwenhoek's experiments do, however, confirm that doctrine, and they are themselves illustrated by it.

But when, from the preceding observations, we reflect on the time that Gunpowder, when fired, requires to drive the ball out of a Gun, and how much of the charge of powder is consumed in that time; which consumed Gunpowder is injurious to the effect of the firing, and also when we consider how much of the charge may be driven out of the Gun before it takes fire, the conclusion is, that it is worthy of consideration to devise ways and means whereby the whole charge of Powder in a Gun may, if possible, be set on fire all at once; and if this be accomplished, so much powder will not be required for a charge as is now in use.

I will now explain the manner in which I make the foregoing experiments on the explosion of Gunpowder: I take a glass tube of the shape and size represented in Plate XV. *fig.* 26, LM, at one end of which I blow the glass globe HIKM, then, after dropping into it a grain of Gunpowder, I give the glass the shape represented at *fig.* 27, PONQR, whereby the grain of powder fixes itself in the tube at the place marked N. I then take a small pipe, called by silversmiths a blow-pipe, which they use to solder their work, and, first putting on my spectacles to defend my sight, I bring the tube near my eye to see the effect of the firing; then directing the flame of a candle or lamp, by means of the blowpipe, to the place where the grain of powder is lodged, it immediately takes fire, producing the inconceivable number of particles in violent agitation, resembling smoke, as I have before described; besides the multitudes of particles adhering to the glass, and indeed, the number of particles into which a grain of Gunpowder is divided, cannot be conceived, but by making the experiment.

I then blow a glass of the shape represented at *fig.* 28, STVW, taking care that it be somewhat thicker at the part T, and filling it with water, I hold it in the oblique position represented in the figure.

After this, I insert the point of the tube represented at *fig. 27*, wherein the Gunpowder was fired into the aperture of the glass, *fig. 28*, at *W*, so that the bent part of the tube *R*, may press the bottom of the glass at *T*, and continue the pressure until the tube breaks about *Q*, whereupon the compressed air in the cavity of the glass, *fig. 27*, rushes violently into the glass, *fig. 28*; but as it cannot escape through the aperture *W*, it places itself about *V*, by which means the same space of water is expelled at *W*, as of air is forced from the glass, *fig. 27*, into the glass, *fig. 28*, and upon weighing this last mentioned glass, when filled with water, and again when part of the water, as before mentioned, has been driven out of it, I can make an exact computation of the quantity of compressed air driven out of the glass, *fig. 27*.

Again, I take another glass of the shape represented at *fig. 27*, with this difference only, that the tube at *Q* is straight, and the aperture left unclosed, and then insert this into the glass, *fig. 28*, so as not quite to touch the bottom; then, upon firing the grain of Gunpowder, the condensed air rushes with great force into the glass 28; after which firing, and the heat of the flame escaping out of the glass 27, instantly, as I may say, the water makes its way through the orifice *Q*, into the glass 27, in greater quantity than one could imagine.

In these experiments, by the firing of the Gunpowder and the rushing in of the water, the glasses are often broken; therefore, in making these observations, it will be necessary to repeat the experiments many times.



OF THE LOUSE.

THIS animal, which is so troublesome to many, especially the poor, who have not the means of frequently changing their linen and other apparel, is by some writers supposed to be produced from dirt, sweat, or excrements: but to convince such persons of their mistake herein, I will give the description of several parts of this creature, as examined by me, the perfect and wonderful formation of which, will clearly prove that these animals cannot be produced otherwise than by the ordinary course of generation.

Plate XVI. *fig.* 1, A B C D E F G, is the head of the Louse, in which may not only be seen two very black eyes, but also two perfectly made horns with joints, besprinkled with hairs in many places, as shewn at C D and G F. The letters H I K B indicate only the outline of a part of the Louse's body.

When I was preparing this object for the limner, I cut off the head and breast from the lower part of the body, and placed this small part only before the microscope; for when I placed the animal before it entire, it was in such continual motion, bending its body backwards and forwards, that it was impossible to obtain a distinct view of it, so as to make a drawing; and this piece of the body remaining fixed before the microscope, the horns and feet continued in motion for an hour.

In the contemplation of several lice when placed before the microscope (and I had plenty of them brought to me for my money), I received great pleasure in contemplating the motion of the internal parts in the head and feet, and even in the œsophagus or gullet, which in this creature lies partly in the head, and through which the blood it sucks may be seen running very swiftly: this

motion in the gullet was seen in a clear liquor passing upwards and downwards alternately.

At E is a protuberance somewhat like a nipple, which, when the Loufe is preparing to take its food, it projects further, and from the extremity of it thrusts out its * piercer, in order to suck the blood. This piercer, or rather the piercer with its sheath (for there are two of them, one inclosed in the other), I have often taken out of the Loufe's head, but it was not till after repeated trials, by reason of their excessive slenderness, that I could place them before the microscope, so as to give a drawing of them both.

Fig. 2, L O P, is part of the Loufe's head; at O is shewn the protuberance, which I have likened to a nipple, as it appears when the piercers are protruded out of it: O M is the larger piercer, or, more properly speaking, the sheath which contains the piercer M N, both of which are drawn into the head when not in use. At N, the extremity of this piercer appears a little split or divided. *Fig. 3, K L*, is this sheath taken out of the head, and L M is the excessively slender piercer it contains, protruding beyond the extremity of the sheath.

When the Loufe is about to seek its food from the human body, it extends the nipple or snout I have described, from its head, and from it protrudes the sheath of the piercer; and lastly, the piercer itself, which being introduced between those small scales with which the surface of the skin is covered, it pierces the blood-vessels lying underneath and then sucks the blood, which is its nourishment; and in doing this, it places itself upon its head, the more readily to introduce the piercer into the body. The blood which the animal sucks, may be seen passing with a very swift motion into its body, and it is a curious spectacle to behold the rapid motion with

* The Dutch word is *Angel*, which the Latin version renders *Aculeus*, both words signifying a sting: but the Translator has adopted the word piercer, as more applicable to the use which this part of the animal is designed for: besides, we shall see presently, that the sting of the Loufe, properly so called, is placed in a different part of its body.

which the human blood, when received into the Loufe, is inceffantly driven to and fro both in the ftomach and inteflines, to prevent its coagulating, which would be fatal to the animal.

In my experiments and obfervations on this creature, although I had, at feveral times, had a number of them on my hand drawing the blood, yet I very rarely felt any pain from their punctures, which is not to be wondered at when we confider the exceffive flendernefs of the piercer I have been defcribing; for, upon comparing this with a hair taken from the back of my hand, I judged, from the moft accurate computation I could form by the microfcope, that the hair was feven hundred times the fize of this incredibly flender piercer, which, confequently, by its punctures, muft excite little or no pain, unlefs it happens to touch a nerve. Hence I have been induced to think, that the pain or uneafinefs thofe perfons fuffer who are infefed by thefe creatures, is not fo much produced from the piercer, as from a real fting which the male Loufe carries in the hind part of its body.

Fig. 4, B C D, represents part of this fting, fo far as it is protruded out of the animal's body, and thus far it is of a hard fubftance and a yellowifh colour like the claws: in it may be feen a kind of groove or cavity, which paffes through the whole fting, and feems as if it were defigned to convey fome liquor to the extremity when the animal makes ufe of it, though I never did actually obferve any fuch liquor. In this figure the fting is drawn with the cavity fronting the eye, in which pofition it appears quite ftraight. *Fig. 5*, E F G, is the fame part of the fting viewed fide ways, in which pofition it appears to be of a curved fhape, and thus far it was bare of any mufcular parts: the lower part G H I E, in its natural ftate, is covered with mufcular and flefhy parts, which, being cleared away, there only remains to be feen in the figure, the hard or horny part of the fting, which gives it ftrength and firmnefs.

This fting is the Loufe's weapon of offence, and which it ufes when preffed by the clothes or otherwife difturbed; for I have ob-

ſerved that, when roughly handled, they protrude their ſting as preparing to ſtrike; but as to the reaſon why the males alone are provided with it, and not the females, I have formed ſome conjectures, but not ſo as to ſatisfy myſelf in that reſpect.

The feet and claws of this animal diſplay the perfect contrivance manifeſt in the formation of ſo ſmall a creature. *Fig. 6.* A B C D E F, is one of the fix feet of the Louſe; B C D, is the largeſt claw, one of which the Louſe has on every foot, and, when the animal is on that part of the body where there are no hairs, it lays hold of the ſkin with this claw, in order the more forcibly to introduce the piercer which it carries in its head into the body, in order to ſuck the blood; but when moving from place to place, or not employed in ſucking the blood, it does not cling to the body, but to the ſhirt or other garment (on which alſo it lays its eggs), becauſe it can eaſily fix its claws into the filaments of the linen or woollen.

To lay hold of a hair it graſps it with this claw and the prominent part of the foot, which is ſhewn at D, and which is alſo provided with a very ſmall nail or claw, and more particularly the part E, which I call the Louſe's thumb, and which is alſo furniſhed with a ſmall nail. For in like manner as we, in holding any thing in our hands, uſe the thumb, ſo does the Louſe in graſping a hair, make uſe of what I call its thumb. A F G, is that part of the Louſe's leg which joins the body.

I could have given a drawing of the Louſe's claw magnified to a greater ſize than the whole foot is here repreſented, but I do not make uſe of ſuch very deep magnifiers, unleſs neceſſity requires it; for I think that an object is ſufficiently magnified when we can ſee all the parts of it diſtinctly.

Upon exhibiting this creature before the microſcope to a certain great perſonage, he obſerved to me, that his ſoldiers, who were infeſted with lice, found them more troubleſome in rainy than in dry weather: for which I gave this reaſon, that the clothes, when wetted, ſhrink and compr'eſs the body ſo cloſely, as to impede the

Louse in its motions, and cause it to use the thing which it carries in its tail, whereas, in dry weather, the clothes hanging loose on the body, the Louse has room to insert its piercer and suck its food, which it cannot do without bending its body and raising its hind parts.

The Louse is so prolific an animal, that it is a common vulgar saying, that it will be a grandfather in the space of twenty-four hours. This I could never believe to be the fact, but rather that it would require nearly a month for the offspring of a Louse to be capable of producing young of its kind; and, in order to make proof of it by experiment, I at first proposed to hire some poor child to wear a clean stocking for a week, with two or three female lice in it, and well tied or secured at the garter, in order to see how many young ones would be produced in that space of time; but I afterwards considered, that I could make the experiment with much more certainty in my own person, at the expence only of enduring, in one leg, what most poor people are obliged to suffer in their whole bodies during all their lives.

Hereupon I put on one leg, instead of the white under stocking I usually wear, a fine black stocking, chusing that colour, because I considered that the eggs and the young lice thence proceeding, would be more easily distinguished upon it. Into this stocking I put two large female lice, and cutting another black stocking into long slips, I bound it over the first above the knee, to prevent their escaping. After wearing this stocking six days, I took it off, and found one of the Lice in the same place where I had put it, and that it had laid fifty eggs, and in another part of the stocking the other had laid about forty eggs, but the parent I could not find. I opened the other which had laid the fifty eggs, and found in its body at least fifty more, and who knows how many eggs it had laid before I put it into the stocking, and how many more eggs it might then have in its body which my sight could not reach?

Having worn the stocking ten days longer, I found in it at least

twenty-five lice of three different sizes, some of which I judged were two days old, others a day old, and the rest newly come out of the egg, besides others ready to come forth, as I found upon opening one of the remaining eggs. But I was so disgusted at the sight of so many lice, that I threw the stocking containing them into the fire; after which I rubbed my leg and foot very hard, in order to kill any Louse that might be on it, and repeating the rubbing four hours afterwards, I put on a clean white under stocking.

I have caused a drawing to be made of the Louse's eggs, (commonly called nits), as viewed by the microscope, to shew from what part the young Louse issues forth. *Fig. 7*, N O P, is the egg, Q R, is a hair of the wool to which the egg is fixed, by means of some gummy substance, which the Louse emits from its body together with the egg. At N, is shewn how part of the egg is shaped, somewhat like a lid or cover, which in all probability is made very tender or brittle, that the young Louse, when grown to its full size within the egg, may be able to break it open and issue forth; whereas other young animals, who are provided with teeth or pincers, can gnaw or bore holes in the shell. *Fig. 8*, S T V, is another egg, wherein, at T, may be seen that the shell of these very small eggs is provided with a skin or membrane, the same as a hen's or other bird's egg; and under the lid or cover of this egg, which the young Louse breaks open, there is another thin membrane, which is also broken by the young Louse, and is to be seen at T.

Now since we see, by experiment, that a Louse in the space of six days can lay fifty eggs, and have as many more remaining in its body, we may easily conceive, that a poor person who has an hundred female Lice about his body, and has not any change of clothes or linen, and who, moreover, through sloth is careless of destroying those he has about him, may in a few months (if I may use the expression) be devoured by these vermin.

To give a clearer conception of the great increase of these animals, let us suppose a person to have about his body two male Lice, and as many females, and that the females in twelve days time lay two hundred eggs, and that, six days afterwards, out of those eggs are produced an hundred males and as many females, and that this young brood in eighteen days time are grown to a size to propagate their kind, and that each of those young females in the space of twelve days more lays an hundred eggs, from which, in six days time, other young lice are produced; upon this supposition, the number of lice springing from two females, will amount to ten thousand. Thus it appears that two females may, in eight weeks time, be grandmothers, and see ten thousand lice of their own offspring, which, unless reduced to actual demonstration, would seem incredible; and who can tell, whether in the heat of summer these creatures may not breed in half the time I have mentioned?

I will here put an end to this lousy discourse, which has gone to * three times the length I intended; but my design in it was, to satisfy myself, and to convince others, that some days are required for the egg laid by a Louse to produce a young one, besides the time required for eggs to be again laid by such young one, and also to ascertain in what numbers these creatures do multiply.

* In the original, the author is much more prolix and circumstantial than here set down; but the Translator has thought it sufficient, on such a subject, to give the substance and general result of his observations.



OF THE MITE.

THE Mite is the smallest animal that I have ever observed about our houses, but in every kind of dried provision, such as ham, bacon, and dried fish, they are almost always to be found.

In reflecting upon this animal, I was desirous to know the nature of their propagation, and how long time their eggs would take from their being first laid to their being hatched, and also in what space of time a young Mite will come to its full growth.

In catching these creatures, I found they were endued with a very quick sight, for after I had once touched them with the instrument with which I caught them, they afterwards avoided it in a manner which surpris'd me.

I had often seen the eggs of Mites in cheese and other substances, and I now proceeded, by the help of a magnifier, to open some of the largest mites, which I judged to be females, and viewing them by a microscope of still deeper magnifying power, I saw at three several times, not only the eggs, but also through the shells of them I perceived their inside to be composed of greater and smaller globules, exactly similar to those in the yolks of hen's and other birds' eggs, only with this difference, that the globules in the yolk of a hen's egg are each of them larger than the entire egg of a Mite.

I took a glass tube, into which I put a piece of biscuit and five or six Mites, and then, by the help of fire, I closed the orifice of the tube, so that no egg laid by the Mites could drop out; the first day of their confinement I found one egg, the next four, and the third six eggs, and one of the Mites dead: the fourth day I counted as many as twenty eggs, and afterwards still more, but

the number I did not particularly reckon; and as it was in the middle of October, and the weather growing cold, on the fourth day after the first egg was laid, I put the tube into a leather case, and carried it in my pocket, to see what time the eggs would require to produce young Mites. On the eighth day I saw a young Mite, and the following day three small Mites creeping about the glass, and then I saw only one of the Mites I had put into the glass remaining alive; and, upon minutely examining those that were dead, I judged that they had been almost wholly devoured by the survivors. The next day I saw five or six small Mites, but I was surpris'd to see those young Mites had only six feet, whereas those which were grown larger had eight.

By the microscope I every day examined the eggs, and when they were about four days old, I could distinguish the fruitful from the barren ones; those which I deemed fruitful were of a dark colour throughout, whereas the barren ones were one half dark and half transparent, which transparency was occasioned by the globules which compos'd the egg having sunk to the bottom. Afterwards I saw that from all those eggs which had remained dark throughout, the young Mites had crept out, leaving nothing but the shell behind them; whereas those eggs which were in part transparent, remained entire.

Not satisfied with these observations, I took several glass tubes, into each of which I put some small pieces of dried fish, and these tubes being clos'd at one extremity, I plac'd the open ends near a bag of meal, in which I was certain there were many Mites, in order that the Mites might of themselves, without being hurt, or being mix'd with any dirt, remove into these glass tubes to the pieces of fish, which accordingly came to pass; for, the next morning, I found in some of the tubes fifty Mites, as well large as small ones, and some so minute, that they seem'd to be newly hatch'd, and had only six feet.

In order that these Mites might not want air, I clos'd the ori-

fices of the tubes so as to leave an opening at the end in some of them, as far as my eye could judge, about the fiftieth part the size of a Mite's egg, and others larger : these glasses I then placed before the microscope, for these creatures, by reason of their minuteness, are almost invisible, and by my naked eye I could not perceive they had any life in them.

I have given a drawing of the Mite, as seen through the microscope, to shew its shape as nearly as the limner could imitate it.

Plate XVI, *fig.* 9, A B, represents the Mite's egg, the shell of which is covered with so many sharp prominent parts, that the whole appears dotted : these eggs may be seen in great numbers, in the rinds of cheese, especially when the cheese is old and Mites are discovered in it. *Fig.* 10, C D E F, is a Mite seen through the microscope when fixed to the point of a needle. This creature has eight feet, in each of which the joints are represented as accurately as could be done ; moreover each foot is furnished with two wonderfully minute crooked claws at its extremity, with which the animal can readily grasp any thing, and support its own body ; and I have seen a Mite, when fixed to the point of a pin, lay hold with its claws on the hair of another Mite and suspend it in the air, and I wondered that a single hair of a Mite should be so strong as to hold the animal's body suspended by it. At D is shewn the head of the Mite, the fore part of which is so very sharp, though with an opening on it, (from which opening I have seen something like a tongue put forth), that the mouth might be fitted to bite asunder the fibres of the flesh on which it feeds, as I have seen in the feet of a louse gnawed by a mite, leaving oblong scratches. On each side of the head was a crooked hair, beginning at the thicker part of the head, and ending near the extremity, as if it were also fixed there, which at first I thought it was, but having often seen that when a Mite touched these hairs, while cleaning its head, in the manner that cats and other animals do, the hairs when moved out of their place recovered themselves with a

kind of ſpring, whereupon I thought that theſe hairs might be deſigned to protect the eyes.

The Mite here pictured was not one of thoſe taken out of the meal or cheefe, but a piece of ſmoke-dried meat, which abounded with them; theſe Mites were of the ſame ſhape with others, but ſomewhat larger, which I preſumed might be owing to the abundance of moiſt nourishment they got from the meat, which thoſe in the meal had not; and when I put ſeveral of thoſe Mites from the dried meat into a glaſs, they could not move about readily, but remained almoſt fixed in the place, or moved very ſlowly, becauſe they could not fix their claws in the glaſs, and the hairs on their bodies, by the fatty particles they had contracted, ſtuck to the glaſs.

I have obſerved theſe animals remain many weeks without food, and alſo to endure the cold, and even lay eggs in cold weather, which in about a month's time produced young Mites.

And now, if we contemplate the wonderful regularity, as well in the formation, as in the propagation of this animal (the minuteſt as I have before obſerved, that is found about our houſes, and by reaſon of its minutenefs, unknown to or unobſerved by many), muſt not the moſt judicious philoſophers agree with me in opinion, that as it is impoſſible for an elephant to be brought forth from duſt or dirt, it is equally impoſſible for a Mite to be bred out of meal or any corrupted ſubſtance, or in any other manner than the regular way of generation I have deſcribed.



Of a Maggot, which feeds on the grafs in meadows, and a Fly bred from it in the Spring : with ſome remarks of the author on the Graſshopper and the Locuſt.

IN the month of May I was ſhewn, by a countryman, a meadow which, though good land, was very thinly covered with grafs, and the reaſon he gave for it was, that a certain ſpecies of black, thick, and ſhort maggots devoured the roots of the grafs; and he added, that the grafs would not grow, until there ſhould be ſome hot weather, by which theſe maggots (called in our language *den Hemelt*) would be killed. I aſked him whether he ever obſerved that theſe Maggots changed into ſmall Graſshoppers, or into any other flying animal, but to this I received no other anſwer than, that the Maggots would ſoon diſappear if there were but a few hot days, and then the grafs would grow plentifully: and he farther ſaid, that the lower marſhy lands, in which graſshoppers are ſeldom ſeen, were moſt infeſted with theſe creatures. And the country people ſay, that after a few hot days they often ſee theſe maggots lying dead in the fields.

This information, which I received from different countrymen, was not ſatisſactory to me, for I judged that if theſe Maggots were accurately obſerved we ſhould ſee a different appearance, namely, that they would be found to change into flying animals, and the rather, becauſe theſe Maggots are not found in numbers together, but diſperſed about ſingly.

About the beginning of May I went to a meadow which was but thinly covered with grafs, and took with me a countryman, an intelligent perſon, who lived by keeping oxen and horſes at paſture:

this man searched for the Maggots with me ; I put some of them into a box, but in a short time they partly shrivelled up and died ; others of them I put on a turf of grafs, which I placed in an earthen vessel, and watered it every day ; this vessel I set in my study, that I might remember to view it frequently, and when the grafs began to wither, I replaced it with a fresh turf : in doing which I saw more of the Maggots lie still on the earth, I thereupon took the turf which began to wither, and, taking from it the Maggots, I placed them on the fresh turf : and this I continued to do till the end of July, in all which time I perceived no alteration in the Maggots. In the mean time I chanced to be in the company of some gentlemen of respectability, with whom conversing on this subject, they told me that these Maggots would be changed into flying animals, called, in our language, *Speketers*. But, though I was well assured, in my own mind, that some such change would take place in these creatures, I determined to supply them with fresh earth, until I should see their transformation.

In the beginning of August the turf again began to dry, and, considering that so long as three months time had now elapsed, I examined the turf, and took the Maggots out of the earth, when I saw one of them, which was whiter than the others, to be somewhat contracted, whence I judged that its transformation was at hand ; whereupon I placed it and two others of the Maggots in a box, and the next day I saw one of them was changed into an aurelia or crysalis, having put off its skin, which lay beside it. I sought for another of the Maggots which was changed into a crysalis, but which I had not shut up in a box, and, not finding it, I concluded it had changed into a flying animal and escaped. The other Maggots were in a short time contracted, but did not undergo any further change.

Plate XVI. *fig.* 11, A B C, represents the Maggot called *den Hemelt*, in which, from the month of May to August, I perceived no alteration in shape. *Fig.* 12, D E F, represents the Maggot changed into an aurelia or crysalis, which at first was in frequent motion,

and did not lie still till that part which was its back was placed uppermost. *Fig. 13, G H*, is the skin put off by the Maggot.

Two days after I had perceived the change I have been describing in the Maggots, I went to the meadow where I had found them, where I saw a great number of creatures flying about, which our children call *Speketers*, and others *Mayers* ;* these are represented at *fig. 14, I K L M*. Some of these flying animals I brought home in a box, to see whether they contained any eggs, but I could find none, and the animals seemed to me to be all of the same shape. I again searched in the meadow, to see whether I could find any difference between the males and females, but without success.

In the mean time I saw, flying in my garden, a single animal of the same shape with the before mentioned ones, with this difference only, that it was something larger and its tail terminated in a point, whereas those of the others were blunted at the end. This animal I opened, and took out of its body a great number of oblong black eggs, inasmuch that, on counting those taken out of this one animal, they exceeded two hundred. Others of these animals I found in the town, all containing eggs, but I did not find one in the town of the same shape as those which I had taken in the field, which made me doubt whether both were of the same species.

In the beginning of September, on going again to the meadow, I did not at first see any animal, except those I first described, and which are represented at *fig. 14*, but after I had stood there a while I saw one of the same sort with those from which I had taken the eggs. This creature, settling on the ground at a small distance from me, extended the hind part of its body downwards through the grass, and remained in that posture a short time: this it did three or four times while I was looking at it, forcing its way between the blades of grass so closely, that when I wanted to take it, I could not

* From the figure given of these animals, it should seem that they are what are called, by children in England, *father long-legs*, and in Summer evenings are often seen in our houses.

easily separate it from them, and I saw this performed by three or four others of the animals while I stayed in the meadow.

From these observations I concluded that both the animals I have mentioned were of the same species, but those which I had taken first were all males, and those from which I extracted the eggs were females, the females being of a larger size, in order to contain the eggs they were to lay; and that the sharp points of their tails were of a hard bony nature, and designed to penetrate into the moist earth and there deposit their eggs, which otherwise, in my opinion, would be barren.

On my return home, when I examined the animals I had taken, I found six of them to be females and ten or twelve males, some of them were coupled together, which proved them both to be of the same species; and it seemed probable to me that the strong winds, which had prevailed for some days past, had brought some of the females, which had not strength to resist them, into the town.

The next day most of these animals were dead, and I concluded that their deaths had been caused by my handling them too roughly, so as to break some of the many vessels in their bodies.

Fig. 15, N O P Q, represents the female of this animal, and at *N* may be seen how sharp pointed is the hind part of their bodies, to enable them to deposit their eggs in the earth; this part is divided into four distinct parts, as joints, both above and below, which separate and open themselves.

The eggs of these creatures, of which I have said that I took more than two hundred out of one parent, are very black, and of so smooth a surface that they shine like black glass; they are about twice as long as broad. The shell of them, in proportion to their size, is very hard and thick, and yet I always found that these eggs, whether laid by the animal, when confined, or taken out of their bodies by me, did, in a short time, suffer such an evaporation of their moisture, that the shells were compressed inwards: which sudden evaporation I have not observed in the eggs of much smaller insects.

From hence I concluded that the constitution of these creatures was so ordained by Nature, that this Maggot, in the middle of Summer, when the atmosphere is very hot and the earth dry, does not undergo any change nor lay its eggs; for if those eggs were laid in hot and dry earth, they would be soon dried up and become barren, whereby the species would be exposed to perish. But, as these animals lay their eggs in the month of September, when our lands are all wetted with rains, the eggs, being laid in moist earth, do not dry up, but remain fruitful.

Having observed that the animals I had put into a box in the beginning of September, had laid some eggs in it; I did, in order to preserve them, put them into a box almost filled with moist sand, and covered them with more of the same, and carried them in my pocket for fourteen days, to give them warmth, and see whether any Maggots would be produced from them, and I daily opened some of these eggs, but could not see any appearance of young within them.

I often opened the bodies of these animals, both the males and females, and was surpris'd at the wonderful number of vessels and organs they contained, insomuch that I must say, that the spectacle would excite more admiration than to view the intestines of larger animals with the naked eye; and this was particularly the case in the hind parts of the bodies of both males and females, which, viewed through the microscope, exhibited such a spectacle, that I never saw the hind part of the body of any animal, wherein were so many organs with their joints, the use of which, though doubtless essential to the animal, are to us unknown.

Since we see now, as I have said, that the female of this animal can lay more than two hundred eggs, it plainly appears, that if all the animals of this species which are bred in one summer were to increase in the same degree, within two or three years they would so multiply, as to devour all the roots of our grass; but by droughts in the earth, great rains and storms, and severe frosts, many of

them are destroyed, and we are not infested with them equally every year.

From the generation of these creatures, we may make a transition to that of Locusts, which are found in Eastern countries, and devour the fruits of the earth, and when they have no other food, devour one another. We do not often see Grasshoppers, (which are similar to Locusts) near this town, but I had at one time a parcel of large ones brought to me, which I put into a glass and fed with vine twigs, but observing that the large ones devoured the smaller, I separated them, and found that the females laid above eighty eggs. Now, if we suppose, that Locusts do the same in extensive countries where the Maggots are not exposed to tempests, heavy rains, or frosts, (and I am well assured, that Locusts are produced from Maggots, from the observations I have made on Grasshoppers in this country, which are of the species of Locusts), we may easily conceive that Locusts in those countries may multiply to that degree, that they may visit the neighbouring countries in swarms. These are merely my sentiments on this subject, which, however, I think more consonant to right reason, than the notions of those who dream that Locusts come out of the clouds for the punishment of mankind, as I have often heard in conversation.



On a Maggot which is found in the blossoms of fruit trees, particularly apples; the Fly from whence it is produced particularly described; with the Author's reasoning on the production of minute flying animals.

ALTHOUGH it has always been my firm opinion, that no animal, however minute, is produced any otherwise than by course of generation, nevertheless, I determined to prosecute my inquiries in the examination of those minute Flies and other insects, which are found on the leaves of fruit-trees and plants in gardens.

Having heard frequent mention made of a certain black Fly, which was said to be very noxious to the blossoms of fruit trees, I examined the blossoms on the fruit trees in my garden, and particularly the apple-trees, and found that many of the flowers did not blow out fully, nor were they of a white or pink colour, but appeared shrivelled and of a kind of rusty red.

Upon opening these blossoms, I found within the leaves of most of them a Maggot of a pale yellow colour, and in some of them a crysalis, which I concluded had been changed by transmutation, from one of those Maggots; and as I, at the same time, observed many black Flies on the leaves and flowers of those fruit trees, I concurred in the common opinion respecting them, and concluded that those Flies had laid an egg on each flower, from which Maggots had been hatched, and that they, being changed into crysals, did at length produce the same species of black Fly.

At the same time, I was of opinion, that if these Maggots were not deposited within the blossoms before they were full blown, few of them would be preserved alive, because this Maggot is not inclosed in a web or case, as we observe in most other crysals, and

therefore would be exposed to be devoured by small birds, or even by common ants ; for I saw, that where an ant could find its way into the blossom, it had eaten up part of the Maggot or crysalis. But these Maggots when enclosed within the blossom, do bite or gnaw the vessels of its leaves in a manner, that not only the buds dry, but are close shut up, and thus afford a defence to the Maggot during its growth, and until it is changed into a flying animalcule.

In these my observations, many things occurred to me worthy of note : first, that in all the blossoms I examined, I never found more than one Maggot in each flower ; secondly, that from the small nourishment it there received, it grew apace ; and lastly, that in a few days it became so perfect, as to be transformed into a crysalis.

Many of these blossoms I pulled from the trees, concluding that no flower which contained a Maggot or a crysalis, would ever produce any fruit. Placing some of these before the microscope, I perceived that the crysalis and the young animal it would produce, had no affinity with the black Flies I have mentioned ; and to be more certain of the fact, I put into a glass tube four of those crysalses, which I carried about with me in my pocket, and at the end of five days, I perceived one of them had changed its pale yellow into a red or blackish colour, and soon afterwards it put off the skin which inclosed it, and ran about the glass : on the eighth day the three other crysalses put off their skins, but some of them were of a darker colour than others, having respect to the time they had been in their crysalis state.

I now perceived that, with many others, I was mistaken, in supposing the blight, or rather the Maggots we perceive in the blossoms of fruit trees, to be produced by the black Flies ; for these animalcules, I was now observing, had, as I before mentioned, no similitude to the bodies of the black Flies ; first, because they were not nearly so large ; and secondly, because that which I found

within the blossoms, when changed from a Maggot into a flying animal, was provided with two shells or cases, folded lengthwise over its wings, as we see insects of the beetle tribe; moreover, the fore part of its head was furnished with a long proboscis or trunk, designed by Nature, as I suppose, for perforating the buds of the blossoms before they were blown, and through the hole to introduce an egg, whereby the egg and the Maggot produced from it, might be protected from its enemies, particularly birds and pismires; this proboscis, about the middle of it, was provided with two horns.

I again visited my garden, where many of the trees were yet in bloom, but neither on the trees, nor on the flowers, could I discover any of these flying insects, which had been brought to their perfection while carried about in my pocket; though in some of the flowers which I opened, I found Maggots, and in others crysals, yet not in one a flying animal; whence I concluded, that so soon as from a crysalis they arrived at that state, they forsook the blossoms and took their flight.

At the times when I took the Maggots I have before mentioned, out of the flowers, they twisted and bent their bodies many ways; but I never saw them move progressively; for when I placed them upon a paper, they could not change their place, but rolled themselves up in a kind of ring, for these creatures have no feet; therefore, each Maggot must be placed in a blossom by itself, and cannot injure more than one flower.

One thing seemed to me very remarkable, namely, that all the food which this Maggot took from the blossom (at least, as far as I could perceive), became part of its body, for when I examined the flowers wherein the crysalis had lain, I could never discover any appearance of excrements, but only the skin left behind by the animal upon its transformation.

In these my observations I could not but take notice how Nature, that provident parent, has created this animal so as not only to derive its support and growth from so small a portion of aliment, and in so little time, but also by appointing it to be placed within the blossoms

of fruit trees, and there brought to perfection for the conservation of its species, otherwise (as before mentioned) it would be devoured by pismires or birds: and it must also be observed, that these creatures have no ability of seeking their food at a distance.

Besides what I have mentioned, I had others of these crysals, which I kept in my study to watch their transformation, placing them in a glass, so as to give them a supply of air; but I never could perceive the least appearance of any evaporation from them, though, as soon as they were converted into living animals, so great a quantity of moisture issued from their bodies as to hinder their moving, and often to occasion their sticking on their backs to the glass, which made it necessary for me to give them more room: here is to be noted how wonderfully compact must be the skin of the crysalis, so as to suffer no evaporation of its moisture, lest the animal within should be injured by the want of it; and this is another instance of the inconceivable correctness of Nature in her works.

Finding myself mistaken in opinion, respecting the black Flies depositing Maggots in the blossoms, I determined, if possible, to examine the nature of their propagation, and the rather as I assured myself that they must be produced by crysals. I therefore opened some of these black Flies, and I can safely say that, from a rough calculation, I found in each female three hundred eggs.

Going into my garden in the morning, to take some of these Flies, that I might confine them till they laid their eggs, and I might discover what kind of crysals would be thence produced, I perceived the Flies sitting on the leaves and flowers as immoveable as if they were dead, and even, upon a gentle touch of the branches, to fall to the ground; whereas all other Flies, in warm weather, are very nimble. Hereupon it occurred to me that, when the wind blows from the North, Flies do not rove about in the air, but sit upon the trees, where we observe them in greater numbers than when the air is warm; and as in cold and cloudy weather they settle in great numbers, not in the summits of trees, but in the lower branches,

many people think they are brought to us by the North wind, or even bred in the clouds : but such men I leave in their error.

These black Flies (and especially those which I had supposed to be impregnated) being confined in two glasses, laid a great number of eggs, very long in proportion to their thickness ; whence I gathered that they would be changed into very long and slender Maggots or Caterpillars. But as all the Flies soon afterwards died, and not one of the eggs produced a living creature, I considered that perhaps these Flies might die for want of food, and that the eggs might be laid before their time, having often observed flies and moths when I happened to handle them so roughly, that they afterwards died, immediately after such handling, to empty themselves of all the eggs they contained.

I did intend here to desist from publishing any thing farther respecting the propagation of these insects, deeming that I had proved sufficiently, that all living creatures were produced by the ordinary course of generation ; but my friends in different parts of the world, exhorted me to proceed in my disquisitions on this subject, as being what many learned men desire may be prosecuted.

To proceed then in my examination of this subject. In the month of May, in the following year, the weather being warm, and my fruit trees in blossom, I sought for some of the flying insects I have mentioned, and presently saw many of them coupling together. But though they were nimble in their motions, and well fitted to fly, and moreover had six feet, and on each foot two claws, yet on my shaking the leaves or branches ever so little, they gathered up their feet and fell to the ground ; several of these small animals I brought to my study in a glass, to make my observations on them.

I found the wings of these creatures to be about twice the length of their bodies : but, if we consider that those wings are covered with thousands of hairs, and observe, that when the wing is spread, not the least fold or joint can be discovered in it, though it lies on

the body folded together, we must be lost in admiration, and the more, when we recollect the multitude of vessels and joints, and also muscles, requisite for the formation and expansion of such a wing; and we may well ask, whether any man in his senses, who is not entirely blinded by prejudice, can contemplate such a creature, by the microscope, without acknowledging that it could not possibly be produced from corruption, or the putrefied bodies of other animals, and will he not rather cry out in the words of an eminent gentleman, who lately came to see my microscopical objects, "O the depth of the Divine Wisdom, how inscrutable are his works! Can any man after this, be found to deny the being of a God?" or such like expressions.

I caused a drawing to be made of this insect, the same size as it appears to the naked eye, which is represented in Plate XVI. *fig.* 16, and to avoid giving a drawing of the whole animal when magnified, I cut off the head, and placed it before a microscope to be delineated, in order hereby, to shew how the careful parent, Nature, has created this small animal, that its species may be preserved, and the order and perfection with which the world was at first created may not be diminished.

Fig. 17, A I, represents part of the head: B H are the eyes, which, in like manner as the eyes of Flies, are formed of many optical organs, and appear with as many protuberances as the substance, called shagreen, appears to the naked eye.

C D E F G, is that organ or trunk, with which, as I suppose, the animal perforates the blossom, while yet a bud, in order to lay an egg in every bud; to which end, its extremity is provided with various piercers or teeth, and other weapons, which are, in some sort, represented in the drawing, but the whole of which, the limner could not copy, because the animal, while alive, was opening and shutting, or extending and contracting, these organs, and, when dead, they were almost out of sight; and though I did all in my

power so to fix them, that they might remain visible without the body, I could not succeed.

This organ C D E F G, is somewhat larger than it here appears, because it is bent forward, and, being viewed from behind, the bend in it could not be described, therefore I gave the limner another microscope, exhibiting a side view of the same, and a sketch or outline of its curvature is to be seen at *fig. 18, M N O.*

D K and E L are the two horns with which this organ or trunk is furnished.

After this, I again looked over eight or ten of my apple trees, and, in the space of half an hour, I collected thirty of these insects, which I shook from the leaves into a large glass tube, that they might not be hurt by my touching them, and I might be able to observe how long they could live without food, and also, whether they would lay any eggs. While taking them, I observed, that not only, upon the least motion, they drew up their feet and contracted their trunks and so fell down, but that some of them did the same upon being only touched by my hand, or by the glass.

But what I most wondered at was, that two third parts of these insects which I found on the trees, were coupled together, and I could not conceive how such small animals could find each other out in so large a space, and the rather, because in a tree, the circumference of which was more than thirty-six feet, I only found four or six of them, but all in couples.

Observing that these insects could run along or stand for a long time on any side of the glass, even with their feet upwards, I was desirous to examine accurately the formation of their feet, and, in this little creature, I saw such perfectly formed limbs, enabling it to adhere to the glass, and to run along upon its surface, as distinctly, as I had ever seen in other larger flying animals.

Fig. 19, P Q R S T, represents almost a fourth part of one of the legs, consisting of four distinct joints: the other part of the leg has only two joints. At R and S, are two claws or nails, which are

in some sort transparent, like a piece of horn or tortoise-shell, seen with the naked eye, and at Q and T, are shewn the organs, by the help of which the animal can run along the smooth surface of the glass, and also hang to it a whole night. The formation of these organs is very wonderful, for all those parts with which they are covered, and which one would conclude to be hairs, are so exactly and regularly sloped off, and particularly of such regular lengths, as if they had been all clipped with scissors, that when the animal places its foot any where, they all touch the place at the same time, and what is more, all these particles, which seem to the eye to be hairs, have at their extremities a hook, and, at a little distance from thence, two other hooks; but by reason of their extreme minuteness, though the limner confessed he saw them through the microscope, he declared he could not represent them in the drawing.

Now if we consider, what I have always experienced, that a glass, though washed ever so clean, will have many particles adhering to it, though these are so small, that the claws on the feet of small flying insects cannot take hold of them, we may easily conceive that these minute hooks, may take hold of the small particles of water or motes from the air adhering to the glass. And here we may discover the error of those who formerly supposed there were cavities in glass, wherein Flies could fix their claws and climb up.

In the spring season I observed, on several parts of my apple trees, many caterpillars gathered together, which, in moving from place to place, fastened themselves by a kind of thread, and I began to consider, whether these were produced by the black Flies, and it seems they are called by gardeners *de Wolf*. To satisfy myself in this respect, I cut off from the apple tree, two branches, on which were these caterpillars, placing the ends cut off in a vessel of water in my hall, in order that the leaves might remain fresh, and afford nourishment to the caterpillars: when the leaves began to

with, I placed fresh branches near the others, to which the caterpillars removed themselves.

When these caterpillars came to their full growth, they prepared to enclose themselves in a web similar to the silk worm, with this difference, that at one extremity of their web or case, there was a small aperture. A representation of this web is given at *fig. 20*. These animals, thus inclosed in their web, I put into a glass tube, together with several others of the same kind which I had found on the apple trees, placing the whole in a large glass. After some time, I saw come out of the greatest part of these webs certain white flying insects, having their wings diversified with black spots; they were somewhat similar to those flying animals found in granaries, and proceeding from the Maggot, which mealmen, and bakers likewise, call *de Wolf*, and of which I have treated fully in another place. I was desirous of keeping these creatures alive till I should see whether they would lay eggs, but they all died.

I have caused a drawing to be made of this flying animal, as it appeared to the naked eye, when come out of its web, and this is to be seen at *fig. 21*. To have made a drawing of it from the microscope, would have been too troublesome, by reason of the multitude of feathers with which, not only the wings, but the feet, the horns, and the whole body were covered, and also by reason of a crooked organ on the fore part of its head; the true form and make of which I was not able to observe with sufficient accuracy.

Among these flying animals, I saw in the glass two blackish Flies, which I conceived must have been produced in this manner, namely, that some black Fly of the same species having laid its egg upon one of those caterpillars, the Maggot thence produced had fed upon the caterpillar till it had acquired its full growth, and thereupon was changed into a black Fly, instead of the web producing a white flying animal with black spots.

In one of the glasses, wherein I had inclosed the webs I have mentioned, there issued from those webs not only the flying animals

I have juſt deſcribed, but alſo a great number of Flies, ſo very minute, that I ſhould not have imagined they could exiſt in the open air, for that the heat muſt cauſe all the moiſture in their bodies to evaporate.

The fight of theſe Flies cauſed me to open all the webs which had been left in the glaſs, and in one of them I found a great number of minute ſkins or caſes, from which thoſe Flies had proceeded by tranſmutation from aurelias. From whence I concluded, that a minute Fly of this ſpecies, muſt have laid many eggs in the aperture of one of theſe webs, and the Maggots hatched from thence, muſt have fed upon the caterpillars in the web, until they came to their full growth, and then within the web be changed into thoſe minute Flies; otherwiſe, in my opinion, that ſpecies of Fly would become extinct. For we muſt lay it down as a truth, that many flying animals live on nothing but other living creatures, and for want of which food many of them die, eſpecially the ſmall ones, among which may properly be reckoned Flies: for if many Flies could not find particles of fleſh whereon to lay their eggs, the maggots bred from thoſe eggs muſt periſh.

This innate diſpoſition and foreſight in ſmall animals leading them to lay their eggs in thoſe places where the young maggots may find food and nourishment, will appear ſtrange to many. But if we conſider the nature of larger flying animals which are familiar to us, and that we never ſee geefe, ducks, or ſwans, make their neſts in trees, or in fields far diſtant from the water, but always on the banks of ditches or rivers, becauſe they do not bring food to their young, who are by nature deſtined to ſeek it for themſelves; and therefore the parents, when the young are hatched, do no more than tend on them and proteſt them from enemies; whereas, on the contrary, we ſee that birds who are able to bring food to their young, build their neſts on the tops of trees and other high places, and that the young remain in the neſts, and do not endeavour to follow their parents, however hungry they may be, we ſhall ceaſe

to wonder, that an insect lays its eggs near the body of another, while in an aurelia state, where the young maggots may find nourishment. And lastly, if we see in quadrupeds, that many wild beasts have no other food than the bodies of those beasts which they devour, that there are many birds who feed only upon birds, and that the same almost universally obtains in fishes, it will not appear strange to us, that among minute flying animals, some feed on others.

I caused a drawing to be made of this very minute species of Fly, the size it appears to the naked eye, which is shewn at *fig. 22*, with a circle round it to make it more apparent; for I must confess, that when viewing it through the common spectacles with which I write, I could not discover it to be a Fly. At *fig. 23*, A B C D, I have given a representation of one of the wings of this Fly, as seen through the microscope, in order to shew the wonderful formation of so minute an animal. These wings, which are four in number, are covered both on the edges and on their surface, with a great number of hairs, terminating in points, like those on our hands.

One of these minute Flies I placed before a microscope of much less magnifying power than that from which the wing was drawn; by this we see it to have two pretty horns on its head, each composed of many joints, and every joint covered with hairs. And in contemplating the horns, I took notice of the eyes, wherein I plainly perceived many optical organs of which each eye consisted, as we observe in larger Flies. All these objects are represented at *fig. 24*, E F G H I.

In contemplating such minute animals as this, and considering that no part of them is made in vain, but that every one has its use, we see still further reason to admire the perfection of so minute a creature. And when we see the stupendous wisdom of Nature's operations in the greatest, and in the least of her productions, we may well cry out again and again, " Away with the blind croakings

“ of those followers of Aristotle, who by their writings endeavour
 “ to darken the truth, and to persuade us that flying insects or any
 “ other animalcules can be produced from corruption !”

There is another species of minute Fly, which I believe lays its eggs in ditches, from which maggots are hatched, and those maggots again become Flies of the same kind. I have not thought it worth while to pursue a minute investigation, as to its being so propagated, because the fact is, I think, already sufficiently established ; but the feathers on its head, its eyes, horns, and wings, when viewed by the microscope, are so wonderful to behold, that I have caused drawings to be made of some parts of it.

Fig. 25, is the Fly of the size it appears to the naked eye.

Fig. 26, is one of the wings viewed by the microscope.

Now, if we consider, not only the multitudes of hairs, as well round the edges of the wing, as in the other boney parts, which give it strength ; and also the incredible number of very minute hairs with which the membrane of the wing is covered, and which the limner, as near as he could, imitated in the drawing ; and that all these cannot be compared with the great number of hairs with which the Fly's body and its feet are covered ; and if we moreover consider, that each hair is not formed of a single vessel, but of many, we must needs say, that there is a greater cause for admiration and reflection, in the contemplation of so small insignificant an animal, than in that of a horse or an ox. And the deeper we endeavour to search into the secrets of Nature, the less we are able to conceive the minuteness of the particles of which bodies are composed. And to give some idea of which minuteness, I have made these remarks on the wings of small flying animals.

Fig. 27, represents one of the two horns on the head of this small Fly.

And here we must see, that there is more wisdom, perfection, and curious workmanship, in the formation of this small Fly I have

been describing, than of the large body of a horse ; and we also must conclude, which I lay down for certain, that not only this species of Fly, but every living creature upon the earth, are by no means produced from any kind of corruption or putrefaction, but derive their origin from those creatures which were made at the Beginning, or a very short time after the parts, whereof our world consists, were brought into existence. Finally, the more we reflect on the consummate wisdom and skill of the Creator of the Universe, the less are we able to form adequate ideas of his Perfections.



Of a very noxious Animalcule, which in the spring infests the young shoots of fruit trees.

HAVING frequently observed the leaves at the ends of the young shoots of gooseberry trees, and also on cherry and peach trees, to be very much contracted, and, as it were, rolled up, by which means the growth of those trees was impeded; and perceiving at the same time, many ants on the leaves so contracted, I at first adopted the vulgar opinion, that the ants alone were the cause of that contraction in the leaves, and consequently, of the impediment to the growth of the branches. In one year my cherry trees were so much infested with this contraction of the leaves, or rather by the Animalcules concealed in them, that, out of about thirty of those trees, only two or three small ones were unhurt.

I therefore determined to examine for what purpose the ants resorted to those young leaves, and what was the cause of the leaves being contracted; in doing which, I saw that the contracted leaves, and especially those on the gooseberry trees, were covered with a great number of dark-coloured Animalcules, and that those which were most full grown, and were about the size of a half grown louse, were of a darker or blacker colour than the smaller ones. These Animalcules were some of them so minute, as not to exceed the size of a common grain of sand. Upon sight of these creatures, I concluded that the ants resorted to the contracted leaves for no other purpose, than to devour these Animalcules on them; and I was confirmed in my opinion, by seeing several, both of the smaller and larger sort, to be almost wholly consumed; so that nothing except their skins and feet remained.

I cut off a shoot from a cherry tree, which I brought into my house, in order to examine these Animalcules more narrowly, and the rather, because most of the vermin which infect our fruit trees are, in their respective species, nearly of the same size, and are generated from caterpillars, which become creeping or flying animals; and, in like manner all sorts of flies, known to me, are generated.

I again looked over many of the leaves, in order, if possible, to discover the eggs from which these Animalcules were produced, and the rather, because I was certain, that their bodies were not composed of annular parts or rings, as is the case with flies, butterflies, and the like, which are produced from caterpillars, or maggots; and from those rings or divisions in their bodies, they have the name of insects, among which the flea is also to be reckoned. But all my search was to no purpose, at which I was much surprised, that among so many minute animals, not an egg was to be found. This seemed to favour the opinion of those who will have it, that small living creatures are produced spontaneously, but such a notion appeared to me altogether impossible, though, at the same time, I was at a loss how to investigate the generation of these creatures. At length, I determined to open some of the largest of them, in hopes of finding eggs in their bodies; but, instead of eggs, I found, not without great admiration, young Animalcules in the bodies of the larger ones, and in shape so like the parent, that one drop of water cannot be more like another, and I extracted not a single young one, but four, completely formed, from the same parent. Hereupon I judged, that it would be most expedient to cut off all the twigs and leaves of the peaches, cherries, and gooseberry trees which were infested with these creatures, and throw them into the water to drown them, and try whether I should not afterwards be less infested with these vermin.

These discoveries led me to consider, whether these Animalcules towards the end of summer, or when the leaves fall off, might not

lodge themselves in the trunk or branches of the trees, to shelter themselves there during the winter; and, in order to investigate that matter, having a gooseberry tree, which had been so infested with these animals, that it had grown very little during the summer, and I had therefore determined to root it up, I suffered it to stand till the 15th of the following January, when, after a long frost and rain following, I cut a branch from the tree about a span long, in a place where it was the thickness of my finger, and, examining it by the microscope, I saw among the cracks in the bark, and among the small dry leaves where the new bud was about to spring, and which fall off when the bud grows larger, and also among some fragments of moss adhering to the branch, several of these Animalcules, which all seemed full grown. They were not only dead, but the hind parts of their bodies were perforated with a round hole, and their entrails gone, whence I gathered, that provident Nature had assigned these creatures their enemies, to prevent their species increasing too fast, and also for the sustenance of other animals.

In this search, I happened upon an Animalcule eight times smaller than the former ones, which moved its head, horns, and feet; and it also appeared to me, that it had been produced by transmutation from a maggot or caterpillar, because I could plainly discover in it, those rings or annular parts we observe in maggots. I also saw adhering to this branch a substance, which, upon more accurate inspection, I took to be the web of some small maggot, and on this were four eggs, the shape of birds' eggs, but no larger than small grains of sand, and which I thought might, perhaps, be the eggs of the before mentioned living Animalcule.

But what to me appeared most wonderful was, that I discovered two Animalcules, in shape like those called by children lady-birds, but so minute, that a hair taken from my hand, was more than four times their thickness, and, upon comparing them with a grain of scowering sand, it was, in my judgment, three

hundred and fifty times as large as either of them: one of these Animalcules was alive and moved its feet, and I was certain it was of full size, because it appeared to have been produced from a maggot: and I the more wondered at the sight of so minute an animal, because I could not have imagined so small a creature could live in the open air; for, if we suppose the one half of this animal's body to consist of a thin or watery substance, one would conclude, so small a quantity would be soon evaporated; but yet, when we find that so small an Animalcule remains a long time alive, we must conclude, that Nature, our kind and provident mother, has formed the skin of this animal so hard and solid, that little or no moisture can be evaporated from it.

These were my observations during one summer and the winter which followed. On the 17th of May in the following year, I did not find, among all my gooseberry trees, above three or four branches the leaves of which were contracted. Upon opening those leaves, I found eight or ten of the before mentioned Animalcules, and among them one of a dark colour, inclining to black, and larger than the rest. This, on account of its size, I judged to be a female; and upon opening it, I took out of the body twenty-one young ones, of which one seemed to be completely formed, and of a lighter green colour than the parent; the others were of such different sizes and colours, that in the smallest, I could neither distinguish the eyes, nor the green colour.

I opened several other of these Animalcules, which I deemed to be females, and took out of their bodies many young ones; in some more than thirty, and in none less than twenty, of different sizes, and some so small that I could neither distinguish their limbs nor eyes.

On the 20th of May I cut off three branches of this year's growth from a gooseberry tree, on which branches I was certain there was no animalcule of this species; these I put into a vessel of water, and on the tops of two of them I put two and on the third branch three

of those Animalcules, which I deemed by their size to be females, in order to see how soon, and to what degree, they would bring forth young: and, in twenty-four hours, two of these produced nine young ones and a third six; they continued to increase, but there being a vessel, with flowers in it, brought out of the garden, which stood near that where the branches were, some ants, which were among the flowers, crept into the branches and killed some of the Animalcules, for I could plainly see that their bodies had been, in great part, devoured by the ants. Hereupon I killed all those ants, and placed the vessel containing the branches in a dish full of water, to defend the access to it from ants.

In these and others of my observations it appeared to me that our common ants, which I had always deemed very pernicious insects, on account of the damage they do in our gardens, by devouring the fruit when ripe, do, on the contrary, in the spring, when there is no fruit, live upon other small creatures; so that I am doubtful whether the damage they do is greater than the utility they are of in the spring.

Hereto I must add, that I have been at some times very much infested with ants, so that I caused them and their eggs to be dug up and thrown into the water: but afterwards my method was, where an ants' nest was found, to cause its opening to be strongly pressed down with the foot, to destroy the ants in their nest: and, if ants were found among the trees, to stamp the earth close round the trees, in order to keep the ants within the ground, and prevent their bringing food to their young, whereby both would be destroyed, and by this means I almost wholly freed myself from them.

On the 30th of May I again examined the branches I had placed in my study, and found many young Animalcules on them, some of which were considerably grown; and I also perceived that it was in the nature of these creatures to change their skins, for I found several cast skins, in which I could plainly see the feet, horns, hairs, and claws on the feet; and in these skins, which were very trans-

parent, I not only saw many vessels, but I could discover many of the eyes, or optical organs, with which this creature is provided, all which were a pleasant spectacle to behold.

Hitherto I had been of opinion that these Animalcules fed only on the veins or vessels of the leaves, and that their breaking or biting them caused the leaves to contract, and under which contracted leaves the animals sheltered themselves from the heat of the sun; but now I saw that they, for the most part, fed on the very small buds and also on the stalks of the leaves, the vessels of which being wounded caused the leaves to contract in their growth, and the nourishment from the young shoots being, in a great measure, taken away, the shoots grow knobbed and crooked.

Upon recollecting that, in the former summer, I had seen on my plums and apple trees a great number of flies which, as I thought, had so impeded their growth that not only the apples were very small, but also the young shoots were very defective, I now determined to examine into the true cause of that appearance.

I could not, in my search, discover any eggs of those flies in my apples, peaches, and plum trees, but I now saw all the young shoots of one of my gooseberry trees to be uncommonly contracted, and on it I not only saw a great number of those Animalcules I have described, which breed their young within them, but also many black flies, whose bodies were not larger than the bodies of those Animalcules. I therefore pulled up the tree and threw it into the water, concluding it impossible to extirpate all the Animalcules and save the tree's growth, having first cut off two branches of it for further examination.

These flies have four wings, the two largest of which are twice the size of their bodies; I could not, at first, think they were produced from the other Animalcules, but, upon examining them by the microscope, I found the bodies of both of them to be very similar, and, after several repeated observations, I saw that the first mentioned Animalcules had, on each side of their bodies, a long white

protuberant part, which, upon examination by the microscope, I found to be wings. I also opened the bodies of several of the flies, and in them I found young ones of the same form, and in like numbers, as the other Animalcules, all which plainly proved to me that these Animalcules, commonly called gooseberry lice, are changed into flies.

As I was assured that the Animalcules which had been bred in my house, and were now fourteen days old, had grown to their full size, I opened three of them, and from the body of one of them I took about thirty young ones, of different sizes, from another forty-nine, and from the third sixty. At this very great increase I was astonished, and was desirous to examine all the gooseberry and other trees in my garden, in order to extirpate these noxious animals as much as possible; and the rather, because when changed into flies, they can fly into other trees, and so infect the whole. But though we may clear our gardens ever so much, they will be still liable to be infected from neighbouring ones.

Some of these Animalcules, whose change I judged was near, I put into a glass, in order, if possible, to observe the nature of such change; and, after a day or two, I saw one of them put off its skin and assume a new form: its wings, which before had been folded up very close, it expanded by degrees, shaking them slightly, and then they appeared as straight and regularly placed as if they had never been folded up, and the same I observed in others.

This kind of propagation appeared to me more wonderful than any I had before observed; if we consider that such minute Animalcules, as those we are now considering, shall, within thirteen days after they are produced from the parent, breed within them sixty young ones, many of which can be seen to be completely formed; and moreover that these Animalcules, after issuing from their parent, shall, in their growth, many times change their skins, produce a number of young, and lastly be changed into flying animals which continue to bring forth young; this must appear wonderful, and be

a confirmation of the principle, that all living creatures deduce their origin from those which were formed at the Beginning.

And if we compute how many Animalcules one female may produce in the course of a summer, reckoning from those we find in their bodies, without considering those that escape our notice, we must be filled with astonishment.

Although these Animalcules were propagated so rapidly in my house, as I have before set down, I have found that in the open air they increase still faster.

I have also observed another sort of these Animalcules, a little different from the former, for the bodies of the former were not only somewhat broader, but, when I examined their heads and the hind part of their bodies by the microscope, I found that those parts were also different: for the former, when full grown, had a dark colour, appearing blackish; the latter were green, and so were the flies produced from them. I also saw these two species of Animalcules mix one among another, whence I concluded that they were the two sexes.

As soon as the young of these Animalcules come from the parent, they can creep about, and appear as vigorous, as if they had been long in the open air.

I have caused drawings to be made of these Animalcules, in order to shew the exact regularity and perfection which are shewn in the formation of so contemptible a creature.

Plate XVI. *fig* 28, represents one of these Animalcules of a light green colour, the same size it appeared to the naked eye. *Fig.* 29 A B C D E F G H, is the same seen through the microscope, being of its full growth and approaching to its change; the wings, folded together, begin to appear, and may be seen in the figure at W X. At K L M N, are the six legs with their joints, covered with many very thin and short hairs, and each of the feet furnished with two claws. At F, is one of its two beautiful eyes, the wonderful formation of which, as it appeared through the microscope, the linner

could not copy in the drawing. *II*, shews the trunk, or piercer, which the animal strikes into the bud, or stalk, on which it feeds: *FZZ*, are the two horns.

In the hind part of its body this Animalcule has two parts, or organs, thicker in the middle than at the ends, which are curiously covered with round scales, placed in exact order beside each other thereon, as shewn at *C* and *B*, and from these organs I often saw a small drop of very transparent liquor issue, as shewn at *B*. This small drop exhibited a very pleasant spectacle to me, for, when removed a little further from the microscope, it had the effect of another microscope, shewing the objects, as houses, steeples, and the like, inverted, and so minute and delicate in their appearance, that could scarcely be believed. When this drop of liquor was emitted from one of the organs, *B* or *C*, I saw that the organ from which it issued was immediately drawn in towards the animal's body.

DTUE, is the point of a needle, to which this animal was fastened when the drawing was made.

Fig. 30, is the same Animalcule, of the size it was when changed into a fly.

While the limner was employed in making the above drawing, I dissected some more of these Animalcules, and having taken out some of the unborn young ones, I placed the most perfect one of them before a microscope, and caused a drawing to be made of it, which is shewn at *fig. 31*, *OP*: this animal's body appeared to be covered with a membrane, but the limner could not copy in his drawing all the parts which were to be seen, because the moisture of it in a very short time evaporated, and thereby the shape was altered. I also gave the limner another microscope, before which I had placed eight unborn Animalcules, in all which the eyes could plainly be seen, and of these he made as correct a drawing as he was able, which is shewn at *fig. 32*, *QRS*.

In *fig. 28*, at *F*, I have shewn one of the curious eyes of this Animalcule, but meeting with one of the put-off skins, in which

the formation of it could be seen more accurately, I placed it, by itself, before a microscope of greater magnifying power, and caused a drawing of it to be made, in order to shew the perfection exhibited in so minute, and to us so despicable, a creature. *Fig. 33, A B*, represents this eye as seen in the put-off skin, and it appears to be composed of a great number of distinct eyes or optical organs.

After this I opened the bodies of others of these Animalcules, in order to see whether the young ones within them shewed any signs of life, and I found one of the young Animalcules, in whose body I could not only see the motions of the intestines, but also its feet to move, though they lay regularly placed on the sides of its body.

On the 6th or 7th of June, I saw that many of the Animalcules, which had been brought forth on the 21st of May, were changed into flies, and some of the leaves on the gooseberry branches whereon I kept them began to wither.

Until the 5th of June, I had often sought for these Animalcules on my cherry, peach, and plum trees, but found none: on the 7th of June, upon making a farther search, I found some of them on fourteen different branches of the cherry trees; they were not green but blackish, the young ones round about them were of a dark colour, and in one leaf I saw five Animalcules, full grown, which had one hundred young ones round about them.

I opened many of these Animalcules, and took many young ones out of them of the same kind as before mentioned, but these last young ones were of a dark colour. I also opened some Animalcules which seemed to be half grown, and found in them many imperfectly formed young ones, and, to the best of my judgment, I counted sixty in the body of one of them, besides which there were doubtless many which I could not discern; the largest of these were of a green colour.

I did intend here to have finished my observations, but seeing that these last mentioned Animalcules were only on the cherry trees, and perceiving some little differences in their formation from those I have before mentioned, I determined to try whether these last

would feed on the gooseberry tree; I therefore took three branches of this year's growth from a gooseberry tree, which I put into a vessel full of water, and put on each of them a cherry tree leaf, whereon were many of those Animalcules, judging, that as the cherry leaves began to wither, they would leave them and remove to the gooseberry leaves, which they accordingly did. But they did not remain close to each other, as they had done on the cherry leaves, where they had food close to them, but dispersed themselves among the gooseberry leaves, creeping up and down among the branches. I then put two twigs of the cherry tree in the vessel close to the gooseberry branches, and saw many of the Animalcules quit the gooseberry and remove to the cherry branches; those which did not remove to the cherry branches, I found afterwards dead; others got down to the water and there perished.

On the 23d of June, I was much surpris'd to see a great number of the Animalcules still remaining on the cherry branches, and on the 26th, I saw some of them changed into flies; but what I most wondered at was, that some of the Animalcules, which I judged had lived through the whole winter, were not yet changed into flies.

I observ'd, among other things, that as soon as one of these Animalcules had put off its skin, and was changed into a fly, it immediately began to move its wings, whereby they opened a little from the sides of their bodies, to which they before were closely attached, and in the space of two minutes acquired their full size; and, at this time, I thought I saw the parts of the wings not only unfold, but also encrease in size.

Moreover, I opened some of the Animalcules which were changed into flies, and took several young ones out of their bodies; these animals had also a piercer, which they thrust out of the organ, *fig.* 28, I I, and which is there shewn at I A: this is put off with the skin when they undergo their change.

Another circumstance appeared to me particularly worthy of note, that I saw about fifteen of these Animalcules, whose bodies were so swelled, that they seemed almost globular: some of them were dead, others moved but little, and upon opening them, I found their skins very tough, compared with others of the species, and on the insides I found no appearance of any young nor any bowels, but only a white thick maggot, which took up almost all the substance of their bodies. I put twelve of these, some of them living, others dead, into a glass, and in a little time they were all dead. But after eight days, namely the 24th of June, I opened one of them, and took out of it a living maggot, which lived for the space of three days in the glass wherein I put it: this maggot appeared to me, like those produced from the eggs of ants; for like those maggots, it seemed incapable of moving from place to place, as it had no other motion than what consisted in extending and contracting its body in a small degree.

I was astonished at this spectacle, respecting which I could form no other conjecture, than that these Animalcules had been impregnated by a female ant laying its egg in the hind part of the animal's body, which is the only way I could conceive that the maggot from an ant's egg could be found in that place.

I opened several others of these Animalcules, at different times, and found maggots in all of them; but I could not, in any instance, extract the maggot without injuring it. I left some of these Animalcules which were dead in the glass, and after a few days, I found that the maggots within them were dried up.

On the 26th of June, I found on a plum tree, on which I could not for three days before discover any, a great number of green Animalcules, the largest of them about the size of a common louse, and with which some of the leaves were so covered, that their surface could not be seen, and this for the most part by Animalcules but lately brought forth.

I took a leaf on which were only two large ones, and two or three young ones, supposing that the full grown ones had not yet brought forth many young; one of these I opened, in order, if possible, to compute the number of young I might take out of it, and I thought that the young which I could distinguish, and those particles which I considered to be young ones yet unformed, exceeded the number of seventy.

At the same time, I saw, among many others, six Animalcules changed into flies, in all which I found, upon opening them, a great number of Animalcules, and, among the rest, one that was upon the point of being voided by the parent; and I not only saw it move, but I could see the intestines within it move about as vehemently as if it had been a parcel of living creatures.

But what seemed to me the most extraordinary of all my observations was this: that in every one of the Animalcules which I opened, though but of a middling size, I found young ones, and also upon opening some whose bodies were very slender, and which, for that reason, I thought might be males, yet among these and all others of the Animalcules, I have been describing under this head, I could not find any that could be deemed males.

Farther, I opened some Animalcules, which were about an eighth part the size of the parent ones, and I took out of them a great number of round and pellucid particles, which, I doubted not, would in time have become perfect young ones; some of the largest of these contained some green particles, and moreover, they were of different sizes, the smallest of them appearing no larger through the microscope than a grain of sand viewed by the naked eye.

I afterwards opened some Animalcules twenty-four times smaller than the full grown ones, and in them I also saw some of those round particles, which I concluded would in time become young ones; and finally, I opened one, which was not much larger than

those newly voided by the parent, and, in my opinion, was only one day old; but I was not able to judge with certainty, respecting the particles which I took out of its body, (by reason of their excessive minuteness), whether they were such as would in time become young animals.

ADDITION, BY THE TRANSLATOR.

THE Translator hopes to be indulged in bearing his testimony to the accuracy of Mr. Leeuwenhoek's investigation of the preceding subject, and also as to the utility of his discoveries.

This part of the translation having been made at the house of a friend in the country, and in the month of June, the Translator was led to examine the fruit trees in the garden, expecting to find some of the Animalcules mentioned in this Essay: he saw many of them exactly agreeing with the description here given; on the gooseberry and currant trees (particularly the latter) they fastened themselves at the extremities of the young shoots, and the farther extension of the shoot seemed to be entirely prevented, for the bud in the centre was destroyed and the stalk of the shoot grown into an irregular knobbed form, and swelled to two or three times its natural size, having the same appearance as a gouty limb.

Some persons may perhaps be disposed to ridicule the great pains taken by Mr. Leeuwenhoek to investigate the nature of so minute, and, as they may think, so contemptible a creature, as that under consideration. But the state of the fruit trees just mentioned will at once demonstrate that these minute animals are of a formidable nature, and capable of destroying all the next year's produce. For if they attack the young shoots in the early part of their growth, and thereby put a stop to their further vegetation, the consequence must be that from those trees, whose fruit is produced upon the last year's wood, little or no fruit can be expected in the succeeding year.



*On certain Animalcules found in the sediment in gutters on the roofs of houses.**

I HAVE been induced to publish my discoveries respecting these creatures, in order to shew how wonderfully Nature has provided for the preservation of their species.

On the 25th of August, I saw in a leaden gutter at the fore part of my house, for the length of about five feet, and the breadth of seven inches, a settlement of rain water, which appeared of a red colour; and, upon considering that perhaps this colour might proceed from some red Animalcules, similar to those which I had seen in muddy ditches, I took a drop of this water, which I placed before the microscope, and in it I discovered a great number of Animalcules, some of them red, and others of them green. The largest of these, viewed through the microscope, did not appear bigger than a large grain of sand to the naked eye; the size of the others was gradually less and less: they were, for the most part, of a round shape; and in the green ones, the middle part of their bodies was of a yellowish colour. Their bodies seemed composed of particles of an oval shape; they were also provided with certain short and slender organs or limbs, which were protruded a little way out of their bodies, by means of which they caused a kind of circular motion and current in the water: when they were at rest and fixed themselves to the glass, they had the shape of a pear with a short stalk. Upon more carefully examining this stalk, or rather this tail, I found that the extremity of it was divided into two parts,

* This species of Animalcule is very minutely described by Mr. Baker, in his Treatise on the Microscope, and he has given to it the name of the wheel animal; also in Adams on the Microscope.

and by the help of these tails the Animalcules fixed themselves to the glass; the lesser of these appeared to me to be the offspring of the larger ones.

Moreover, I saw another kind of Animalcules much smaller, the bodies of them were very transparent; but there seemed to be an hundred of the former species to one of them.

On the 31st of August, the water which I had before observed, was, by three successive days of hot weather, so dried away, that when I pressed my finger on the muddy sediment in the gutter, no more water than about the size of a grain of sand adhered to my finger, in which water I discovered a small number of transparent living Animalcules, but all the green and red ones were dead.

The first of September the sediment in this leaden gutter was so thickened, that it appeared like stiff moist clay; but, with all my endeavours, I could not discover any Animalcules in it of the species I had before seen.

At length I discovered two living Animalcules with oblong bodies, like the largest of those which I had formerly seen in rain water, wherein pepper or ginger had been infused. These Animalcules were almost the thickness of a hair of one's head; but such of them whose bodies were full of young, were twice that size, the ends of their bodies terminating in a point; their tails were provided with six or eight minute organs, by the help of which they could fasten themselves to the glass, and the fore part of their bodies being also provided with certain organs, when they would move from place to place, they brought their hind parts nearer to the fore part, and then, loosing the fore part, they extended it in like manner as we see caterpillars do; and, in swimming, they made use of other organs destined for that purpose. Soon afterwards I observed many of the same species of Animalcules.

The matter in the bowels of these creatures was for the most part red, proceeding (as I imagine) from their feeding on smaller Ani-

malecules of that colour, but some few of them had no red colour in them, especially the smaller ones, which probably had not been long brought forth from the parent.

On the same day the weather was very hot, and, in the afternoon, I took a small part of the sediment from the gutter which was now quite dry, and I saw the surface of it completely red, by reason of the great number of red Animalcules in it, being many more than the green ones; but I could not distinguish them until I had moistened the substance with some rain water.

The following day the sky was again very hot and dry, and, about nine in the morning, I took some of the sediment which had been in the leaden gutter, which was then quite dried, and no thicker than half the back of a knife; it had also lain from the preceding evening in my study; this I put into a glass tube, about the thickness of a swan's quill, and poured on it a small quantity of rain water taken out of my stone cistern, in which water were swimming some of the before mentioned Animalcules of the smaller sort; having poured in this water, I mixed it up with the dry sediment or matter put into the tube, and which seemed very hard and compact, in order to dissolve the same; that thus, if there were still any living Animalcules in it, they might issue forth; though I confess I never thought that there could be any living creature in a substance so dried as this was.

I was, however, mistaken; for scarce an hour had elapsed, when I saw, at least, an hundred of the Animalcules before described; some of them adhering to the glass, some creeping along upon it, and some swimming about. In the evening I computed there were more than three hundred of the same kind of Animalcules, but the most of them were not of full size, as I judged by their bodies being so minute, and so empty of food, as if they were newly born; and in the bodies of some of the larger ones, I could see two, in others three, young ones, folded double: these young ones, when

newly born, were as quick in their motions as the full grown ones.

In that part of these Animalcules which may be called the breast, I saw a round particle moving with a reciprocal contraction and dilatation, in the time one might count one: this I did not doubt was the heart.

Moreover, the fore part of the bodies of these Animalcules, which may be called the head, was divided into two parts, each of the two divisions being of a round shape, set round with certain long and very slender organs, which, in their motion, exhibited a most pleasing and delightful spectacle; to form an idea of which, we must suppose we see two small wheels set round the edges with sharp points or pins, and these wheels in swift motion, as it were, from the West towards the South and East, but never to move in a contrary direction from the West towards the North and East.

This spectacle appeared to me the more wonderful and incomprehensible, because it is not to be conceived how such a motion can be produced or performed in an animal body.

In order to give a clearer conception of this, I took a glass tube with some of these Animalcules in rain water, which I placed before the microscope, and delivered the same to the limner, that he might make as exact a drawing of it as he was able.

Plate XVI. *fig.* 33, A B C D E F G represents one of these Animalcules, which had fixed itself to the glass by the help of the limbs or organs at A. The round oblong particle, appearing between B and G, I take to be a portion of its food nearly digested; that which is situated in the middle, I think is the food in the stomach and intestines; and the four oblong particles which seem, as it were, to surround the intestines, exhibit the young Animalcules in the body of the parent.

Between the letters D and E, appear the two round parts, shaped like wheels, with sharp points placed upright on them. These

wheels moving from the point D, which we will call the West, and carried round, with a very swift motion, by the South towards the point E, which we will call the East.

When one of these Animalcules is creeping along the glass, it assumes the figure H I K L M N O, *fig. 34*: the parts, H and M, being alternately fixed to the glass, and in this situation the organs, like wheels, which in the former figure are shewn between D and E, are drawn within the body, and a part like an horn, marked L, appears in sight. Besides these positions, the animal twists and turns its body into such wonderful shapes, that I have often been astonished to behold it.

The limner also saw several Animalcules of a shape similar to what is represented at *fig. 35*, P Q; these had the lower parts of their bodies of a flat shape, from which issued various minute organs, which they used in moving themselves from place to place. Now in the body of this Animalcule, were many globular particles, and in the same water were many still smaller Animalcules, whose whole bodies appeared no larger through the microscope than one of those globules in the former Animalcule.

I have often placed the Animalcules I have before described out of the water, not leaving the quantity of a grain of sand adjoining to them, in order to see whether, when all the water about them was evaporated and they were exposed to the air, their bodies would burst, as I had often seen in other Animalcules. But now I found that when almost all the water was evaporated, so that the creature could no longer be covered with water, nor move itself as usual, it then contracted itself into an oval figure, and in that state it remained, nor could I perceive that the moisture evaporated from its body, for it preserved its oval and round shape unhurt.

In order more fully to satisfy myself in this respect, on the third of September, about seven in the morning, I took some of this dry sediment, which I had taken out of the leaden gutter and had stood almost two days in my study, and put a little of it into two separate

glafs tubes, wherein I poured fome rain water which had been boiled and afterwards cooled.

This fediment confifted of a fmall portion of earth, fome fand, pieces of mortar; and among it were mixed fome pieces of hair, threads of wool of different colours, and bits of ftraw, which things we may fuppofe to have been brought thither by the winds; and the furface of it confifted of thofe red and green Animalcules, apparently dead.

As foon as I had poured on the water, I firred the whole about, that the fediment which, by means of the hairs in it, feemed to adhere like a folid body, might be the fooner mixed with the water: and when it had fetled to the bottom of the glafs, I examined it, and perceived fome of the Animalcules lying clofely heaped together. In a fhort time afterwards they began to extend their bodies, and in half an hour at leaft an hundred of them were fwimming about the glafs, though the whole of the fediment which I had put into it did not, in my judgment, exceed the weight of two grains. After five or fix hours had elafed I faw two feveral forts of Animalcules fwimming in the water, the leaft of which were fo minute, that many thoufands of them would fcarcely equal the fize of a grain of fand.

The preceding experiment I afterwards repeated, and met with the fame event.

Thus we fee that thefe Animalcules, when the water dries away, contract their bodies into an oval fhape, and, even in the heat of fummer, preferve their oval fhape for a long time: and, that when they are again fupplied with water, they, in a very little time, unfold and extend their different limbs or organs, ufing them in the fame manner, and with the like motions as they did before the want of water caufed them to contract themfelves. And this I obferved, not only in the full grown ones, but in the moft minute of thofe Animalcules.

Hence we may conclude, that in like manner as the fhells of the eggs of moths or butterflies, whence caterpillars are hatched,

and also the membranes of grubs or cryfales produced from caterpillars, are of fuch solidity and firmnefs that the moiſture in them will not evaporate, fo the ſkins of the Animalcules I am now treating of are formed of fuch a ſolid texture, that they do not permit the leaſt evaporation: and, were it not fo, I will venture to affirm that theſe creatures in very dry weather, being deprived of water, muſt all periſh: and this, which we find to be the caſe with theſe, we may fairly conclude takes place with Animalcules a thouſand times lefs.

We can now eaſily conceive, that in all rain water which is collected from gutters in ciſterns, and in all waters expoſed to the air, Animalcules may be found, for they may be carried thither with the particles of duſt blown about by the winds. And not only fo, but Animalcules, millions of times ſmaller than a grain of ſand, may be carried up in particles of water, if not to the clouds, yet to ſuch a height as to deſcend with the evening dew: or by the winds they may be raiſed from the earth, and ſpread on all ſides. Nor will this appear incredible, if we conſider that, in great ſtorms, the ſea water is daſhed with ſuch force againſt the ſhore, that the particles of it are carried many miles, fo as by ſettling on the leaves of trees and herbs to give them a ſalt taſte.

The preceding kinds of experiments I have many times repeated with the ſame ſucceſs, and in particular with ſome of this ſediment, which had been kept in my ſtudy for above five months, and upon pouring on it rain water, which had been boiled and afterwards cooled, I ſaw in a few hours time many of the Animalcules before deſcribed. And if, after being ſo long in a dry ſtate, theſe Animalcules, upon water being given to them, can unfold their bodies and move about in their uſual manner, we may conclude, that in many places, where in ſummer time the waters ſtagnate, and at length dry up, there may be many kinds of Animalcules, which, though not originally in thoſe waters, may be carried thither by water fowl, in the water or mud adhering to their feet or feathers.

From all these observations, most plainly we discern the incomprehensible perfection, the exact order, and the inscrutable providential care with which the most wise Creator and Lord of the Universe, has formed the bodies of these Animalcules, which are so minute as to escape our sight, to the end, that the different species of them may be preserved in existence. And this most wonderful disposition of Nature with regard to these Animalcules, for the preservation of their species ; at the same time that it strikes us with astonishment, must surely convince all of the absurdity of those old opinions, that living creatures can be produced from corruption or putrefaction.



On the circulation of the blood in the tail of an Eel, and in the tails and fins of other fishes: with a particular description of the Author's apparatus for viewing it.

AMONG * other objects, wherein I have viewed the circulation of the blood, was an Eel, and in the fin of its tail, I saw the blood circulating through an incredible number of arteries and veins of various sizes; and although those blood-vessels were dispersed promiscuously one among another, yet I could very plainly distinguish the arteries and veins.

Upon fixing my eye almost at the extremity of the tail fin, I there saw such a number of minute blood-vessels, that it was impossible for me to discern the circulation in the minutest of those vessels. This fin being a little dirty, I took a piece of linen cloth, and gently wiped it twice, to get a better view; but I found that in that slight touch, many of the external blood vessels were so injured, that many particles of blood issued forth, causing the extreme parts of the fin to appear red, and they were also a little distorted: I also plainly saw in all the arteries, however minute, a strong and quick successive elevation, with a protrusion of the blood; and, upon attending to a large artery in the tail, I there saw the protrusion of blood, derived from the heart, to be much more vehement.

I also placed before my sight one of the two fins which the Eel has next its head, and there I saw the blood circulating in as many distinct places as I have just mentioned respecting the tail fin. And when I endeavoured to pursue the circulation to the extremity of this

* See more on this subject, in Vol. I. p. 89. *et seq.*

fin, I there saw its motion, both in arteries and veins, in such an inconceivable number of excessively minute vessels, that it looked as if the fin in that part of it was composed of nothing but blood vessels.

In this and many other Eels, I saw that the blood vessels, where the circulation comes to its full extent at the extremity of the tail fin, turned back with a small bending, many of them taking their course transversely, or crossing one another, in their return back towards the heart. For instance, the circulation appeared to me as in Plate XVII. *fig.* 1. A B C D E, which represents a blood-vessel situated between two of those small bones which give strength to the extremity of the tail fin. In this figure, A B C is to be called an artery, because in it the blood moves forward from A by B towards C, and from C by D, it returns to E; therefore C D E must be called a vein, though each of these vessels, so called, is but one and the same blood-vessel prolonged; and in its return it twice crosses itself. Near to this was one of the small bones of the fin, and close to it a second blood-vessel, in which also the circulation forward and backwards was performed; and this vessel in its return also twice crossed itself, but with somewhat smaller bendings than in the other vessel. Here, F G H, is an artery, wherein the blood was carried forward as far as H, and, bringing back the blood towards the heart, in the direction H I K, it is then to be called a vein. On this occasion I saw, (and which I had observed in several other instances) that in the first mentioned artery A B C, there arose, at C, a small vessel which conveyed the blood from A B C, into the other vessel at I.

To give an idea of the relative size of these vessels, and of their distance from each other, the reader will understand, that the distance from E to K was almost equal to the breadth of four hairs of one's head.

To make these observations, my method at first was, to wrap the Eel in a piece of paper, or a cloth, leaving out only that part of

the fin which was to be examined; but, with all my care, I found that, in doing this, I either impeded the circulation or injured the fin; I therefore adopted this method: to take Eels about the size of one's finger, and let them swim about in the water for a time, till they were at rest, and then examine their tail fins while in the water, for in that situation they spread out their tails, but when taken out of the water, the very extremity of the fin gathers up in wrinkles.

In order to impart this pleasing sight to others, I invented and compleated some instruments, whereby I exhibited the same to several gentlemen of eminence in this country, who all declared, that they did not think any more delightful spectacle, nor more worthy of observation could be shewn.

I have determined to give drawings of the instruments by which I exhibited this circulation of the blood, and by which those who desire to view the same may be easily gratified.

I first prepare a piece of brass about half the thickness of the back of a common knife, and of the shape represented at *fig. 2*, A B C D E F; this I hammer upon an anvil some time, to make it the tougher, and make two large holes in it, as shewn at G H I and K L M. I also drill six smaller holes at P Q R S and N O. I then bend this piece of brass at the end A B F, and at the dotted line B F, so that it may form a right angle with the other part, and also bend the other end C D E, in like manner.

I then take another flat piece of brass of the same thickness, which, being well hammered, will answer the purpose of a spring; this is represented at *fig. 3*, A B C D, and I hammer it into something of a hollow shape, and file in the lower end of it a cavity, in order that it may in part surround the glass tube I shall hereafter describe; this round cavity is shewn at A D. I then drill four holes in it at E F G H, to correspond with those marked P Q R S in *fig. 2*; and with brass rivets I join the two pieces together. I then prepare another brass spring, as at *fig. 4*, I K L M, in which

I also make a hollow, as at K L, to embrace the glass tube, and having drilled two holes in it at A B, to correspond with the two holes Q R, in *fig. 2*, I place it on the contrary side of the plate, *fig. 2*, and rivet it with two brass rivets through the holes Q R.

I then prepare another piece of brass more than twice the thickness of the former, to be screwed close to the plate, *fig. 2*, and to which the microscope is to be fixed; this piece of brass is represented at *fig. 5*, N O P Q R S T, and in this I drill two holes to correspond with those at O and N in *fig. 2*; these holes, V and W, are made to receive screws in them, in order to fix by them the plate, *fig. 5*, to the plate, *fig. 2*. In this piece of brass, I also drill a third hole with a screw fitted to it, by which the magnifier may be adjusted at a proper distance, which hole is shewn at X. I make likewise in this piece of brass a fourth hole, rather larger than the former, in order that the magnifier, when adjusted by the screw, may be moved to the one side or the other: lastly, I bend this piece of brass, at the place marked with the dotted line P R, to a right angle.

I then make three screws, one of which is shewn at A B, *fig. 6*, and with two of them I screw the brass, *fig. 5*, to the plate, *fig. 2*, passing one screw through the hole O, *fig. 2*, and V, *fig. 5*, and the other through the holes N, *fig. 2*, and W, *fig. 5*: the third screw is to fix the magnifier by the hole Q, to the piece of brass, *fig. 5*. I also make a fourth screw, as at *fig. 7*, C D; the only use of which is, that, being passed through the hole X, in *fig. 5*, it may serve to fix the magnifier at a proper distance from the object to be viewed.

Fig. 8, E F G H I, are two thin plates of brass or silver joined with five rivets, between which the magnifying glass is placed at L; and at K is shewn a hole made to receive a screw, by which the magnifier is fixed to the brass instrument before described.

I have also caused a drawing to be made of the whole instrument, as viewed on one side, (before the magnifying glass is joined to it),

which is shewn at *fig. 9*, and in this figure LMNT, shews that part which, in *fig. 2*, is represented at ABF: and the round hole in that figure is in this shewn at abc. In this figure also OPQR is the part represented in *fig. 2*, at CDE, and the round hole, def, in this figure, is the hole KLM, in *fig. 2*. The brass spring VWX, is that which, in *fig. 3*, is shewn at ABCD, and the spring ABCD, in this figure, the same which is shewn, in *fig. 4*, at IKLM. The piece of brass shewn in *fig. 5*, NOPQRST, is represented in this figure at EFG, and the two screws, by which it is made fast, are shewn at Y and Z; the brass screw, in *fig. 7*, marked CD, is in this figure shewn at H, where it is seen screwed in its place.

I then directed the limner to make a drawing of the entire instrument, with the microscope joined to it, and the glass tube inclosed in it, the same being adjusted ready for use, and so placed before the view; this is shewn at *fig 10*, ABCDEFGH; the silver or brass plates, inclosing the magnifying glass, are shewn at IKLM; the brass screw, by which the magnifier is fixed, is shewn at 2; the screw, by which the object is placed at a proper distance, is marked at 3, and the glass tube, containing the Eel, and fixed in the instrument, is represented by Q R S T V W X Y.

It is my practice to use glass tubes of a size suitable to the Eels which are to be viewed; and though but a small Eel be placed within the tube with its head towards the bottom, yet the glass tube will project so far below the instrument, that it may be held in the hand by that part, so that it will not be necessary to touch the instrument with the hand, and with the other hand the magnifier, which will be held next the eye, may, by means of the screw at 3, be drawn out, or compressed inwards, until the part of the object to be viewed is brought to a proper distance.

When I wish to take out or put in the glass tube, which I generally do at the upper end, I remove the magnifier a little on one side, that it may not be scratched by the glass tube; this position or removal is marked in the figure by the dotted line NOPF.

I also made another instrument of like shape with the former, in which the spring represented in *fig.* 9, at *A B C D*, is a little shorter; and to this I screwed the brass containing the magnifying glass, as represented in *fig.* 11, *H I K L*, on which magnifier or microscope, I fixed a little dish or concave reflector, to throw more light on the object; to which intent I filed away as much as I could of the brass about the microscope, as may be seen at *fig.* 12, *M N O P*, where this is shewn on the opposite side.

I have also given a drawing of the glass tube containing the Eel, which is shewn at *fig.* 13, *Q R S T V*. The Eel placed in this tube, I at first used to cover entirely with water, but I found that when the tail was left out of the water it would, being wet, spread itself on the glass, and be more easily viewed (this fin of the tail is shewn at *W*); and I always, before putting in the Eel, wetted the inside of the glass, for otherwise the glutinous matter from its body would adhere to the dry glass and impede the view.

I also recommend to those who make these experiments, as soon as ever the Eel is taken out of the tube, to let the glass be well cleaned, for otherwise the glutinous matter from the fish will dry on the glass.

I have caused many glass tubes to be blown for these experiments, but the rounder they are, and the thinner the glass, the better: some of them I caused to be made closed at one end, as here shewn at *Q*, others I closed with a cork.

I have also viewed the circulation of the blood in the tail fin of a Perch, and that in such minute vessels as would only admit one globe or particle of blood to pass through at a time.

I once saw in a large artery in a Perch, (I call it large in respect of the most minute ones, though this was not thicker than an hair of one's head), and which is represented at *fig.* 14, the blood protruded from *A* to *B*, though but faintly; from *B* to *C*, I could only perceive a small agitation of the blood, and beyond *C* it was coagulated: from this artery there arose a branch as represented in the

figure at B E, which carried almost all the blood from A B, through B E, into the vein D E F, in which vein the blood was conveyed from E to D, and all the blood which lay between E F was coagulated.

When those Perch which I examined were very lively, and their tails unhurt, I could not discern the large vessels in their tail fins, but when the blood began to coagulate, some of the vessels, in a short time, appeared fifty times larger than their natural size. There were other vessels in which the blood had circulated, and were not swelled, but here the blood was at a stand, and the vessels themselves, by reason of their minuteness and there being no circulation in them, were not easily discerned.

Moreover, it was my opinion that many of the large circulations of the blood, which I saw, when it began to stagnate, were not performed within the coats of the blood-vessels, but that, when the blood in the arteries was impeded in its course, the continued and strong propulsion from the heart caused it to form new canals, where the fish's skin made the least resistance, and that it was by this means the oblique current of blood, shewn in *fig. 14*, at E B, was formed. And, indeed, I am of opinion that all those very minute currents, whether we call them arteries or veins, are not performed within the coats of vessels, but that they are formed where the blood, in its protrusion from the heart, meets the least resistance. But if we suppose one of the very smallest blood-vessels to be provided with a coat, and such coat to consist of three distinct membranes, as it is said the coats of the veins and arteries are formed, it necessarily follows that the coats of those minutest vessels must be of a thinness which is inconceivable. For, let us suppose, as I have often said, that the axis of an hundred globules of the blood, from whence its redness proceeds, are no more than equal to the axis or diameter of a large grain of sand; it follows, that ten thousand globules of blood may pass together through a vessel, no larger than to admit a large grain of sand to pass through it. Now, supposing the coat of such

a vessel, as will admit a grain of sand to pass through it, is twenty-five times thinner than fine post paper, how much thinner must be the coat of a vein which will only admit the ten thousandth part of a grain of sand to pass through it? Therefore, if these very smallest blood-vessels are provided with coats, such coats must be so thin, open, or spongy, that the very thinnest watery parts of the blood can as easily pass through them as water through a fine sieve.

After this, I took one of those fish called a Jack, which I placed so that about the breadth of a finger of its tail was above the water, and, upon examining the extremities of this tail fish by the microscope, I could not perceive the least motion in the blood in that part; whence I judged that those parts of the tail were mortified, for the fish, from its appearance, seemed to me to have been caught some days. But when I examined the tail fin, nearer to the body, I saw the blood slowly moving in an artery, and, a little nearer to the body, there issued from this artery a small branch, through which the blood was carried with a very swift current, and this branch, taking a small bending, carried the blood into a vein which brought it back towards the heart. Let *GHI*, in *fig. 15*, represent this artery, in which the blood from *G* to *H* was driven forward with its usual swiftness, and from *H* to *I* its motion was very slow; *HKM*, is a smaller vessel, through which the blood was carried from this artery into the vein *LMN*, at the point *N*, and in that vein conveyed back to the heart.

After this, I bought a parcel of Trout, which had been caught in the river Maes two or three days before, and among the rest was one, not quite seven inches long, which, upon view of its scales, I judged to be about five years old. This Trout, whose tail fin was a little injured at the extremity, I placed with its tail about a finger's breadth above the water; and, when it was become quiet, I viewed the tail by the microscope, and I could not discover any circulation at the extremity, but, examining it a little nearer to the body, I saw the blood in the arteries and veins stagnating and coagulated; and I

also perceived the arteries to be uncommonly large, which I conceived to proceed only from this, that the circulation being impeded, they were by the impulse of the blood distended to that size.

Upon viewing the fin still nearer to the tail I saw the blood running both in the arteries and veins, and what seemed to me very worthy of note was this: I saw the blood stagnating in an artery, shewn in *fig. 16*, at *OPQ*, from *P* to *Q*, and so on towards the extremity of the tail, and in the same artery from *P* to *O*, and also towards the fish's body, the blood was carried in its usual motion, namely, from *O* towards *P*. From this artery there arose a branch, represented at *P*, in which the arterial blood, after it had been driven thus far, was carried back towards the heart, so that this branch, *PV*, arising out of the artery, *OPQ*, must be called a vein.

At about three hairs breadth from the before mentioned artery, there was another artery, nearly of the same size, which is shewn in the figure, at *RST*. In this the blood between *S* and *T*, and also beyond it, was coagulated, and the blood which, from *R* to *S*, was driven forward, issued into a branch arising out of that artery, at *S*, which also led back towards the heart, as is shewn in *SV*; where, at *V*, both the veins, *PV* and *SV*, are joined, and constitute a larger vein, *VW*.

Moreover, I took a Carp, in order to view the circulation, but it was so restless that, after many trials, I determined to wrap it in a cloth, in order to view the fin on its back, which, being nearer to the heart, I judged the circulation might be stronger: and here I saw the blood moving in an artery, which divided itself into two other arteries, so small as only to admit a single globule or particle of blood at a time; one of these minute arteries was joined to a vein, so large that the blood in it began to assume a red colour, the other I could not follow with my eye, because its course was hid behind a small bone in the fin. *Fig. 17*, *AB*, is this small artery, which, at *B*, was divided into two branches, one of which, *BC*, was joined to the vein *DCE*, at *C*; so that the blood which, in the artery, was

driven from A to B, from the heart, and took its course from B to C, in C, being emptied into a vein, it returned back, from C to D, towards the heart: the other small artery, B F, took a course out of my sight at F. This circulation gradually decreased till, at length, I could not perceive any motion in the vessels A B C and B F.

These observations I also pursued in several other sorts of fish, and in all of them the circulation was very apparent, particularly the Flounder, in the tail fin of which I saw such multitudes of minute blood-vessels, that I may say there is not a space in that fin, so small as a grain of sand, which does not contain a vessel, either conveying the blood from the heart, or bringing it back towards the heart. This fish, called the Flounder, is one of the best adapted to view the circulation of the blood, because it lies quiet a long time, and also will live a long time out of the water.

With regard to the instruments I have described, they might, I doubt not, be made more perfect and convenient; for I myself was never instructed in the working in brass and iron, otherwise than by observation of workmen in the making and using their tools; by which means, however, I have been enabled (rather in a rude way) to make these instruments sufficient to answer all my purposes.

ADDITION, BY THE TRANSLATOR.

AS the reader may have a desire to know of what nature were the Microscopes by which Mr. Leeuwenhoek made the wonderful discoveries, which are the subject of his works, the Translator has thought proper to introduce, in this place, the best account of them he has been able to collect.

Mr. Henry Baker, in his Introduction to the use of the Microscope, has these words:*

“ The famous Microscopes of Mr. Leeuwenhoek are the most simple possible, being only a single lens set between two plates of silver, perforated with a small hole, with a moveable pin before it to place the object on, and

* Microscope made Easy, p. 7. and 8. Ed. 1711.

“adjust it to the eye of the beholder. Several writers represent the glaffes
 “Mr. Leeuwenhoek made use of in his Microscopes to be little globules,
 “or spheres of glafs; which mistake most probably arises from their under-
 “taking to describe what they had never seen; for, at the time I am writing
 “this, the cabinet of Microscopes left by that famous man, at his death, to
 “the *Royal Society* as a Legacy is standing upon my table; and I can assure
 “the world that every one of the twenty-six Microscopes, contained therein,
 “is a double convex lens, and not a sphere or globule.”

And in another treatise, published by Mr. Baker some years afterwards, he writes as follows:*

“An accurate description of the twenty-six Microscopes, and objects be-
 “longing to them, contained in a small cabinet, which Mr. Leeuwenhoek,
 “at his decease, bequeathed to the Royal Society, was presented many years
 “ago to that Society by Martin Folkes, Esq. and may be seen No. 380 of
 “the Philosophical Transactions. And a farther account, setting forth the
 “magnifying powers and other particulars concerning the said Microscopes
 “(which were three months under my examination for that purpose), was
 “presented by me to the Royal Society in the year 1740, and published in
 “Phil. Trans. No. 458. But neither of these accounts has any drawing of the
 “Microscopes: it is therefore hoped the curious will be pleased to see a draw-
 “ing of them, taken, with great exactness, from those in the Repository of
 “the Royal Society, which are all alike in form, and differ very little in size
 “from this drawing, or from one another.

“The two sides of one of these Microscopes are shewn at *fig. 18* and *19*:
 “the eye must be applied to the side *fig. 18*. The flat part, *A*, is composed
 “of two thin silver plates, fastened together by little rivets *b, b, b, b, b, b*.
 “Between these plates a very small double convex glass (called by mathema-
 “ticians a lens) is let into a socket, and a hole drilled in each plate, for the
 “eye to look through, at *c*. A limb of silver, *d*, is fastened to the plates
 “on this side by a screw, *e*, which goes through them both. Another
 “part of this limb, joined to it at right angles, passes under the plates, and
 “comes out on the other side (see *fig. 19*) at *f*, through this runs, directly
 “upwards, a long fine-threaded screw, *g*, which turns in, and raises or lowers
 “the stage, *h*, whereon a coarse rugged pin, *i*, for the object to be fastened to,
 “is turned about by a little handle, *k*, and this stage, with the pin upon it,

* Employment for the Microscope, p. 131. Ed. 1764.

“ is removed farther from the magnifying lens, or admitted nearer to it, by
“ a little screw, l, that, passing through the stage horizontally, and bearing
“ against the back of the instrument, thrusts it farther off when there is oc-
“ casion. The end of the long screw, g, comes out through the stage, at m,
“ where it turns round, but acts not there as a screw, having no threads that
“ reach so high. These Microscopes are plain and simple in their contrivance :
“ all the parts are silver, fashioned by M. Leeuwenhoek’s own hand, and the
“ glasses, which are excellent, were all ground and set by himself.

“ The magnifying powers of these glasses come short of some now made,
“ but are fully sufficient for most purposes. Of the twenty-six Microscopes
“ I examined, one magnifies the diameter of an object 160, one 133, one
“ 114, three 100, three 89, eight 80, two 72, three 66, two 57, one 53,
“ and one 40 times.”



On Frogs, and the manner in which their young are produced from Tadpoles, with the circulation of the blood seen in them: also on the shape of the component particles of the blood.

WE have two sorts of Frogs in this country, one which used to be found in great numbers about the town of Delft, but of late years only few of them have been seen, by reason, as I believe, that their spawn has been devoured by a small kind of fish; and I have sometimes seen this spawn in the small ditches which divide our fields, lying in such quantities, that the surface of the water seemed almost covered with it. The other species of Frogs is much fewer in number, but of a larger size, and they leap farther than the others, and the hind parts of their bodies, or rather the thickest parts of their legs, are used by the French, as good food. Respecting the propagation of these last, as a particular species, I was for some time in doubt, not having observed them couple together, nor seen any of their eggs or spawn.

But one day, at the end of the month of May, walking in a meadow for my recreation, and not thinking of this subject, because the time of the other sort of Frogs laying their eggs was long past, my attention was excited by the noise or croaking, which these last mentioned Frogs make both by day and night, in hot weather, whereupon I went to the side of the water, and I thought that I saw some of their eggs adhering to a green leaf in the water, and so in fact it proved. These eggs are not so easily to be discerned, as those of the common Frogs, because the glutinous matter or jelly which contains them, swims deeper in the water, and is also less in quantity.

I caufed fome of the leaves, to which thefe eggs or fpawn adhered, to be brought to my houfe, and put them into two earthen veffels filled with the water that runs through our canals; and then I examined thofe eggs by the microfcope, and found that, for the moft part, they were dark coloured on one fide, and yellow on the other: but looking at them again the next morning, I found that the yellow colour was almoft vanifhed, only a fmall part of each egg remaining of that tinge, whence I concluded that the eggs had been but lately laid by the Frog when I took them up.

Moreover, I took feveral eggs out of the tranfparent glutinous matter or jelly, which contained them, and I found this glutinous fubftance, by which the eggs feemed to be preferved diftinct in their round fhape, to be very ftrong and tough, fo that it could not be feparated from the egg itfelf, without being torn; and when I handled it ever fo gently, the egg did not retain its round figure, but broke to pieces. I examined many of the eggs after I had taken them out of the jelly which inclofed them, and found them to be contained in a very thin membrane, formed with black parts or fpoats, in fhape agreeing with the knobs or protuberances in that leather which is called fhagreen.

The egg itfelf, as far as I could difcern, confifted of a watery matter, (as it appeared to the fight) and an incredible number of globules, each of which again confifted of a great number of leffer globules, all having a larger globule in the middle; fo that every one of them had the appearance of an egg with a very fmall yolk.

The figure of many of thefe eggs altered every day; for, from being round, they affumed an oblong fhape; afterwards minute tails were formed; and I alfo thought that I faw the appearance of heads.

Every day I opened feveral of thefe eggs, and until the feventh day after they had been brought to my houfe, when I faw the young Frogs (which are called Tadpoles) to be fo far formed, that they began to move. But, with all my attention, I could perceive

nothing in them except globules ; and upon opening a Tadpole, which of itself crept out of the jelly which had contained it, and was swimming in the water, and in which, when it was entire, I could distinguish the back bone, I could not, when dissected, discover any bowels, much less arteries or muscles.

Hereupon it seemed to me, that the whole body of this animal, called a Tadpole, consisted of no other particles than globules, and especially the belly of it, which was of a yellowish colour, and seemed to be made of that part of the egg which had continued yellow. This seemed strange to me, that in so large an animal which I had killed and dissected, I could not discover any vessels or fibres.

After all my observations respecting these eggs, I could draw no other conclusion, than that the glutinous matter or jelly, encompassing the eggs, was only formed by nature for the preservation of the egg when deposited in it, as the shells of birds' eggs cover and protect the whites and yolks.

And as we see, that the whole substance of a hen's egg passes into and becomes part of the chick, (except the shell and the membrane within it, which are designed for the preservation of that substance), so in the present case, the whole of the egg passes into the body of the Frog or Tadpole, and the viscous and glaucy matter, which surrounded the egg, remains after the young has left it. So that we may say of the eggs of Frogs, what I have said of the eggs of birds, namely, that they are designed to support and nourish the young creature until it is able to provide for itself.

When I observed that the viscous matter or jelly I have mentioned, contained in it multitudes of air bubbles, I concluded the use of them was, that where there were no leaves floating on the surface of the water to which the eggs could adhere, these air bubbles might make the jelly float on the water, whereby the eggs might receive the warmth of the sun, and thereby, as I may say, be hatched.

Plate XVII. *fig. 20*, represents the egg of the Frog I am now describing, inclosed in the tenacious or viscid substance I have mentioned; and when it is so far grown, that the animal begins to move, the tail appears somewhat bent. *Fig. 21*, represents the animal, when so fully formed in the egg, as to be able to swim in the water; this was taken by me out of the egg, and laid on a glass, where it died, and the body of it dried up.

Fig. 22, A B C D E F, represents the same animal as it appeared to the limner through the microscope, in which the head can easily be distinguished from the fore part of the body, as is represented at A B F.

FE is the belly of the animal, which was of a yellowish colour, being formed, as I have said, out of that part of the egg, where there remained a yellow spot; but this drawing is not exactly like the animal, because it was so broken and torn, that it only exhibited the appearance of globules.

C D E represents the tail of the animal, in which a spine or back bone could very plainly be distinguished, and is here represented in the figure in the best manner the limner was able; and though I opened many of the tails of those animals, which had this appearance of a spine, I could not discover in them any other parts than globules.

These little animals, or Tadpoles, in swimming, move their tails with great force, and as soon as they cease the motion, they sink to the bottom of the water, whence it appears that their bodies are specifically heavier than water. But these animals have a property (while in their Tadpole state) to fix their heads to the glass, so that they are able to fasten themselves to any thing they find in the waters, and there to remain without sinking to the bottom.

Moreover, I placed a Tadpole, which was alive in the water and had fixed itself to the glass, before a microscope, directing the limner to make a drawing of it as it appeared to him.

Fig. 23, G H I K L M N O P Q R S, represents this Tadpole alive,

as it had fixed itself to the glass, with its belly next the eye of the spectator ; this Tadpole had but a few hours before quitted the egg and jelly in which it had been inclosed.

L M N O P, represents the head ; H I R S, the belly ; and G H S, the tail. In the upper part of the head of this animal, its skin appeared somewhat thicker than the rest ; so that I concluded, this was a part of the skin which in time would cover the whole body of the animal : this is represented at M N O. The mouth is shewn at T, which I did not perceive this newly hatched animal to move. At V V are two dark spots in the animal's head, of a very round shape, and being much more protuberant than I have seen in any other animal, and which many would conclude to be the eyes. But if eyes, they would not be seen by us in this position of the animal ; but rather be placed on the other side out of sight.

I K L, and P Q R, are six transparent parts hanging down from the head, three on each side, and these parts were the only reason of my giving a drawing of this Tadpole ; for in each of them I, with great delight, saw most distinctly the circulation of the blood, which was driven from the parts next the body to the extremities, and there, with great swiftness, performed an incessant circuit back to the body. This circulation had not an equable or even motion, but was performed at intervals by quick repeated protrusions ; and in the intervals, any one for the moment, would have thought the circulation ceased ; but scarcely had the current of the blood begun to make a stop, when a new protrusion immediately followed, causing a continual course of the blood in this creature ; and, upon accurately computing the time in which their pulsations or protrusions were performed, I may say, that an hundred of them were performed in less time than one could count an hundred. From these appearances, I concluded that at every one of those protrusions, the blood was driven out of the heart, and I could perceive this motion (namely, the protrusion of the blood from the heart, and its passage from the

arteries into the veins, where they are united), as distinctly as any person can possibly imagine.

When these Tadpoles were some days old, those dependent parts, wherein the circulation of blood I have described was performed, were no longer to be seen, being, as it seemed to me, grown over by the animal's skin. But I even then saw something of a rapid motion, as before described, performed on each side the head, though I could not distinguish it to be the circulation of the blood; at the same time, the head became so conjoined to the body, that it could not be distinguished from it.

The circulation of the blood, as seen in the tail of the Tadpole, has been noticed in another place,* therefore no more need be added here, than that my observations on this creature were not made upon a single inspection of the subject; but I often repeated the same, on account of the pleasure I took in the sight, and in different Tadpoles, but I always found the appearance to be the same. And one thing is here worthy of note, that in the very small vessels at the greatest distance from the heart, as in the extremity of the tail, there did not appear such a forcible and vehement protrusion as in the vessels near the heart; but though the blood in those small vessels appeared to move in an uninterrupted course, yet it could plainly be seen, that at every pulsation of the heart the course was a little accelerated.

I have in another place said, that the particles or globules of blood, from whence its redness proceeds, are so minute that a million of them taken together would not equal the size of a large grain of sand, and from hence we may easily conclude how excessively small must be the vessels in which this circulation is performed.

As many persons have never seen a Tadpole, I have caused a drawing to be made of this creature, which, despised as it is, has afforded me the greatest pleasure; for the circulation of the blood, in this animal, is more admirable to behold than in any other creature or fish I ever saw: and such has been my delight in viewing it, that

* Vol. I. p. 92.

I think no fountain whatsoever, either natural or artificial, or indeed any other spectacle, could have afforded me such pleasure as these small animals.

Fig. 24, represents a Tadpole, when grown to such a size that both its hind feet were formed, and it could use them; the two fore feet were also visible, but as yet lay hid under the skin.

Moreover, I examined the Tadpoles when they were so far perfected as to become Frogs and leap about the fields, and in these I also discovered a great number of wonderfully minute blood-vessels, which with their bendings constituted those vessels we call arteries and veins; so that it most manifestly appeared to me that the arteries and veins are only a prolongation of one and the same vessel. And this was most particularly manifest in the extremity of those parts in the foot, which may be called the fingers, and of which the Frog has four in each of the fore feet and five in the hind feet.

These blood vessels, which we call veins and arteries (though in fact they are one and the same), were to be seen in great numbers at the extremities of those fingers, and each of them had a round bending, so that the distinct course of each of them could not individually be discovered. All these vessels were so small and thin that they only admitted the passage of the blood in single globules. But when I examined them at the first or second joint of the fingers, then I saw the arteries and veins were larger, so that the blood in them was of a red colour.

I did not only examine these young Frogs in parts of their bodies only, but placed their whole bodies before the microscope, and the before mentioned blood-vessels appeared to me as I have already described them. This circulation I shewed to two respectable gentlemen, who could not behold the sight without great admiration, especially in those places where they saw the blood running in such small vessels as to be only pervious to one of those particles, whence its redness proceeds.

I also examined larger Frogs, and in their feet I saw the circulation of the blood, but with much difficulty, and, unless I had before discovered it in the small ones, it would have been impossible for me to discover the complete circulation in the smallest vessels. But when I examined other parts of the bodies of these larger Frogs, I there completely discovered the circulation.

Among other things, I at one time saw the blood in an artery (large enough to admit three globules of blood at a time) driven in a contrary direction to the ordinary one; but this retrograde motion lasted no longer than while one could tell four, after which it resumed its usual course. To illustrate this: I saw the blood running as described in *fig. 25*, N R O P, passing from N to O; out of which artery arose a branch or small artery; but here the sight which attracted my notice was, that the blood in the artery, P Q, not only suddenly stopped in its motion, but was driven back from Q to P, and emptied into the artery N R O P. The cause of this, I think, might be, that the blood in the small artery, P Q, or in the smaller branches into which the artery, P Q, was divided, had met with some small obstruction, or that some muscle, adjoining to these small vessels, so pressed upon them, that the course of the blood was thereby impeded, whence the blood was not only stopped in its progress, but driven by a backward motion into the artery adjoining; for, after the short time I have mentioned, the blood resumed its motion in the same course and with the same swiftness as before.

In another place I saw the course of the blood, in the same kind of artery, very much retarded for a short space, and immediately afterwards in the same artery a sudden protrusion, and directly afterwards another stop or short interruption in the circulation. This protrusion and interruption took place five or six times successively, after which followed a swift and regular motion, and all this was performed in so short a space of time, that I could scarcely have spoken ten words in the interval.

In my several observations on the circulation of the blood in fishes I have not been able clearly to satisfy myself with regard to the shape of the globules or component particles of the blood, for they sometimes appeared of a spherical, and sometimes of an oval and even a flat shape, and sometimes an irregular figure; this I sometimes attributed to my glasses not being of sufficient magnifying power to distinguish them, and sometimes to the position in which they appeared to the eye, for, while in circulation, they tumbled one over another, sometimes presenting one part and sometimes another to the view; and I also thought that it might be owing to the straitness of the vessels, in which the particles of blood, being of a yielding nature, might, by the compression, lose their spherical figure.

In order to satisfy myself in some degree on this head, I cut off pieces from the tails of several small flat fish, such as Plaice and Flounders, in order to view the blood when drawn out of the vessels, and the rather, because I could not persuade myself, that the natural shape of the particles of blood in fishes was an oval; forasmuch, as a spherical seemed to me to be the more perfect form. For I was of opinion, that the particles of blood in fishes were composed of six globules, in like manner with the blood in man, and in terrestrial animals: and I several times saw the particles of fishes blood, the original texture of which was broken, and in which I could distinctly see four or five, and in some few of them six component particles. I, however, thought it worthy of note, that many of these particles of blood appeared to me of an oval shape, some few others roundish, and others of a perfect spherical figure.

In order farther to prosecute my inquiries on this subject, I took the blood of a Salmon not quite dead, which was received into a glass tube, about the size of a small writing pen: this blood, after a short time, became coagulated; but having restored it in part to its fluidity, I put it into a smaller glass tube, in which I viewed it, holding it so, that the particles of blood might be kept in motion continually, by which means many of the particles appeared before my

sight with a flat and oval shape; in others, the sides of which were turned towards me, I could scarcely perceive any sensible thickness; and in short, others, where their sides were not exactly turned towards me, appeared somewhat broader in proportion to their size; but I could not discover one particle of blood of a perfect spherical shape.

The blood of a Salmon appears, to the eye, of a blackish colour, by reason of the very great number of those particles which give the blood its red colour, and which are more in number in this fish's blood than in others.

Moreover, I spread a small portion of this blood upon a clean thin glass, and I observed where the particles of the blood lay thinnest, that they were of oval shapes, and in many of those oval particles, it could be seen that they were composed of globules; and I saw a few of them, wherein I could distinguish six globules lying in two rows, three and three.

But where the particles of blood lay in numbers close together, they were so coagulated, that no oval figures could be seen in them, and all I could observe was some confused particles, six of which made up one entire particle of blood.

These renewed inquiries of mine were, with intent to discover whether I had before been in an error, by saying, that the particles of blood in fishes, were not oval, but spherical; because all those persons who had seen the circulation of the blood at my house, were of opinion, that the particles were not oval, but spherical.

I have heretofore said, and do still believe, that the blood-vessels in many parts where I have viewed the circulation, are so extremely slender, that if a large grain of sand was divided into a million of parts, not one of those parts could pass through those small blood-vessels, unless they were as soft and flexible as the particles of blood.

I have used all the diligence in my power, in order, if possible,

to discover these oval particles at the time the blood was in its natural course in the veins ; for which purpose, I selected for examination, the very smallest blood-vessels ; but though I was very attentive, I could not satisfy myself, for sometimes I saw a darkish particle, then one much more transparent ; and when I viewed the very smallest vessels, in which the blood had little or no motion, as divers vessels at the extremity of the fins, the particles of the blood lay so very thin and scattered, that I could not see any thing except a liquid without a motion, which was somewhat of a yellow colour.

I have heretofore caused drawings to be made of the particles of blood, representing them of an * oval shape, though the limner had not the originals before him. I have also diluted blood with water, because the multitude of its particles impeded a just view of them ; but now, for the greater satisfaction of myself and others, I gave into the hands of the limner a microscope, before which was placed a portion of the blood of a Salmon, in order that he might make a drawing of the particles as they appeared to him.

Fig. 26, A B C D, shews the oval particles of the blood of a Salmon, which weighed about thirty pounds.

At A B are represented those particles which did not come in a straight line before my eye ; the others, shewn at C, floated straight before the sight : in most of them was a luminous spot, though in some larger than in others ; these the limner also observed, and represented them as nearly as he could in the middle of the particles in the drawing. These particles appeared black, and I disposed the serum in which they floated, in such a manner as to make them sink to the bottom, though with some difficulty ; and in this position the limner had an opportunity of seeing some thousands of those particles.

* See Vol I. p. 94.

If I had made a drawing of these particles, as they appeared to me, I should have represented them twice the size here shewn, so that here is an instance of the diversity of sight in different people.

Moreover, I placed before a microscope the blood of a small Flounder, not diluted with any other liquor, but the particles were floating in their own proper serum; these particles are shewn at *fig. 27*, between E and F.

Although these particles are represented somewhat smaller than the former ones, I could not perceive any difference in their size, and I am certain, that there is not such difference, but that the particles of blood, whence its redness proceeds, are no larger in a whale than in the smallest fish.

I also put some particles of the blood of a small Flounder in a clear and very clean glass, and placed them before a microscope of less magnifying powers than the former, in order that I might shew them, even in the dark days of winter, by day-light alone. These particles of blood, in the middle of which the light could be seen, and which was observed by the limner, are shewn at *fig. 28*, between G and H.

I had also some blood lying in a glass before deeper magnifiers: from these the thin serum liquor was evaporated, and in some few of these oval parts (which were so separated, that they did not touch one another), it could be seen that each of them, as far as the eye could distinguish, consisted of six globules: these six globules the limner imitated as near as he could, as appears at *fig. 29*, between I and K.

After these observations, I devised means to view the circulation of the blood through deeper magnifiers than what I had used before, in which attempt I succeeded so as to see the rapid course of these oval particles in the blood-vessels, by several different microscopes, and the greater the magnifier, the plainer was the course of

blood to be seen: in order to abate this rapid motion, I sometimes pressed the small arteries for the space of two minutes, when I saw in divers small blood-vessels, some oval shaped particles were so separated from each other, that in such vessels no particles of blood could be distinguished, not even those of which six go to make up one entire particle of blood, but only a fluid substance passing through the vessels, which was almost colourless, and in one of the large blood-vessels in the tail, which was an artery, the blood ran very slowly: in this last vessel I plainly saw that the particles of blood in it were oval, and I not only saw them plainly, but I could, more distinctly than before, observe the component globules, of which all, or the greater part, of these oval particles were composed.

It is easy to conceive how six globules of a yielding or flexible nature, in continual motion and striking against each other, may form a perfect spherical figure, as I have elsewhere observed: for example, let *fig. 30*, represent the original composition of a particle of blood, consisting of six globules, five of which appear to the eye, and the sixth is out of sight. I have myself with wax made up such a globule, consisting of six smaller ones, as is pictured at *fig. 30* and *31*, and each of those six, of six others in order to expose to the view of the curious, the make of these globules of blood; for I may lay it down as a fact, that each globule of our blood consists at least of thirty-six globules; these before mentioned globules, pressed together, in constant motion, and flexible in their nature, are, as it were, mutually compacted together, and assume a perfect spherical shape, as shewn at *fig. 31*.

From this disposition of the parts, we may conceive how the globules of blood, in men and animals, have a round figure, but how those oval particles of blood I have been treating of are compounded, and made up of six globules, is not easy to comprehend.

I have before said, that I believed every globule in our blood, I mean those, six of which go to make up a perfect globule, as is shewn in *fig. 30*, is itself composed of six globules, and who knows how this is performed? For in how small parts soever we, in imagination, divide a globule of blood, there may, nevertheless, be particles of which such a globule consists, infinitely smaller, and I wonder that any will be so bold as to publish what they do, respecting the original particles of matter; for my part, I think, that could I divide, even in imagination, a globule of blood into a thousand million of smaller particles,* I should not go the extent of its component parts.

Since we now see, as before is observed, that the particles of blood can, by pressure, be divided, and so united with the thin liquor in which they float, that we take it for a simple or uncompounded liquid, we may imagine, than when a horse presses his breast with a heavy load, the globules of blood in the vessels, where the pressure is greatest, may be dissolved or divided in the vessels, and united with the fluid, which physicians call the serum.

I have also thought, whether or no the particles of blood, so divided or dissolved, may not, when removed from the incumbent pressure, assume their pristine figure, in like manner with the particles of fat or tallow; for, if a portion of ox's or sheep's tallow is exposed to the fire, the particles of fat, which we call globules, as having the nearest resemblance to that shape, are dissolved and exhibit only a clear transparent liquid, even to the microscope, but when the heat ceases it again appears in the shape of globules, and if melted ten times over it still, when cold, assumes the shape of spherical particles.

I have likewise laid it down, as my opinion, that no blood, which is carried in a vein to the heart, can become arterial blood, unless

* The Latin translation has it *decem millics*, ten thousand: but the author's own words are, *duysent millioenen*, a thousand millions.

it has first passed into the heart; but since, in one of the observations I was making only for my amusement I saw the contrary, I have caused a drawing to be made of that appearance, in which drawing I directed the blood-vessels to be represented rather larger than they appeared to me, and in the middle of the small vessels there are no dots: those dots, in other parts of the figure, denote the globules of blood, in order more plainly to describe the circulation.

Let us suppose A B, in *fig. 32*, to be a vein in which, by the microscope, the blood may be seen driven with great swiftness from B to A. From this blood-vessel issued two small branches, represented at the letters C and D, which were united between the letters E and F.

On the other hand, the letters H I exhibit an artery through which the blood was, with like swiftness, driven from H to I, and from this artery, H I, proceeded a branch, shewn at the letters K F L.

Now the blood running from K towards F is united to the vessel F at that letter, and by this means the blood issuing out of the artery is in part infused into a vein and carried from F to G; and the same quantity of blood (as according to my most accurate observations appeared) as was carried from K F to G downwards, so much blood of that, which was carried from C E to F, was carried upwards from F to L; so that as much of the arterial blood as the vessel, K F, conveyed into the vessel, F G, so much of the venal blood did the vessel, C E, convey into the vessel, F L. And, though I have often enjoyed great pleasure in viewing the circulation of the blood, this spectacle, which I have been just describing, was more delightful than any other, because I could most clearly and distinctly see the objects I have described, and also, because I never before saw such a conjunction or communication between the blood-vessels.



ON PHOSPHORUS.*

A CERTAIN German gentleman, who said he was a Doctor of Physic, and newly come from England, paying me a visit, with the compliments of several of my friends in London, after sitting with me sometime, produced a small vessel filled with water, at the bottom of which lay some small pieces of a whitish substance, inclining to a tinge of yellow; these he took out of the water, and with one of them he traced upon paper some letters about the size of a joint of one's finger; and though at first, nothing of these letters was to be seen on the paper, yet upon removing it into a dark place, the paper seemed to be on fire in every place where the letters had been traced: but this fire was very pale or faint, and several hours afterwards viewing the paper in the dark, the letters had still a lucid appearance. He also took a small piece of this substance and put it between two pieces of whited brown paper, and rubbing them briskly six or eight times with his cane, in the place where the before mentioned substance lay, to my great surprize, I saw the paper, by means of the friction, burst into a flame.

This medical gentleman told me, that this substance was prepared by distillation from urine which had been long kept, and that it could not be preserved, unless kept under water: he gave me a piece of it, and with a particle thereof, about the size of a pin's head, I repeated the before-mentioned experiments three several

* Mr. Leeuwenhoek calls this substance *ligt der nature*, which the Latin Translator renders *lumen naturale*, in English, natural light: from the description here given, it can be no other than the Phosphorus of the shops.

times, not only with the same event, but I also found, that when the letters had been so traced, and the paper was brought so near the fire as to warm it, the places where the letters had been traced, were immediately inflamed, and the flame spread farther over the paper.

Soon after this, a friend of mine, who was on his travels, sent me, by a messenger, a small vessel full of water, in which was a substance about half as big as a pea, and informed me that it was a mineral, and called natural light; being a part of what had been given to a chymist in the town where he then was, by a person who had received it as a present from some other, and the half of it was now sent to me. My friend also informed me, that he had himself seen, that when taken out of the water and laid in a person's hand, a vapour and smoke issued from it, and, upon being carried in the hand into a dark place, it emitted a light similar to that produced from rotten wood. Upon viewing this, I immediately judged it to be the same kind of substance as that which I had received from the German gentleman; for, upon viewing it with the naked eye, and trying the same experiment of its effect, the whole were as exactly alike, as if it had been broken off from the same piece; I thereupon set about a diligent examination of this substance, and noted the following particulars in it.

I poured the water and this substance in it out of the vessel, and cut off, while it was under the water, a piece or particle, about the size of a pin's head, having first prepared a glass to receive it and make my observations. Having put this particle on the glass, I immediately placed it before the microscope, and saw lying about it a small portion of some kind of moisture, and the particle itself appeared like a dark body, excepting only, that in two places, there was a small luminous appearance: I also saw a great number of particles, which I deemed to be globules, in violent agitation one among another; these globules were collected in two places on the

particle, and then were, as it were, driven off from it, and when they had got to about five or six hairs' breadths from the substance of the particle, they disappeared. All these globules, viewed by the naked eye, exhibited the appearance of smoke: soon afterwards, several small luminous parts appeared on the particle, which by degrees increased so far, that through the microscope, they gave me the representation of a piece of burning turf, with some ashy parts here and there covering its surface.

But what I most wondered at was, that what I have said gave the appearance of smoke, did not disperse itself in the air, nor mount upwards, as we observe common smoke to do, but fell back on the glass, so that round about this particle of substance, called natural light, there was not only more moisture than before, but at the distance of half an inch from it, there lay a very transparent moisture or liquor, consisting of minute round particles of different sizes, and these in such numbers, that it was wonderful to behold: many of these seemed to be watery, others oily particles.

After this particle had laid a whole night, I examined it again in the morning, and then I saw that the substance which seemed watery, was increased, and that the minute globules, exhibiting the appearance of smoke, still issued from the particle. I then carried it into a dark place, to see whether the same kind of light was emitted from it as I had seen the evening before, but I could not distinguish any.

After it had remained twenty-four hours in my closet, exposed to the air, I again examined it, but I could not perceive any thing expelled from it. I then lighted a candle, excluding the day-light, that I might see more acutely, and then I saw some globules expelled, which I think was principally caused by the heat of the candle; for I observed, that all the moist part of the substance which had been expelled, was evaporated, and some saline particles of irregular figures were coagulated.

Moreover, I prepared a glass tube, in which no liquor had ever been, and taking one half of this substance, called natural light, out of the water, I wrapped it in blotting paper to dry up any part of the water which might adhere to it. Being thus dried, I put it into the glass tube, with intent to close the orifice by heat, first using the necessary precaution that the glass might remain cold, excepting the bare end, which I closed by applying it to the flame of a candle.

Having thus closed the glass, I examined the substance I had put into it, and saw an incredible number of globules issuing from it, producing round about it an appearance of vapour or smoke, and at length forming a collection of watery and oily matter, which gathered about the particle in such quantity, that, after an hour's time, they impeded my view of the globules which were expelled: whereupon by strongly shaking the glass, I removed the particle into a fresh place, and then, again examining it, I saw the globules expelled from it in as great numbers as before, and after three hours, they formed such a collection of moisture, as again to intercept my view of the globules as they were expelled.

I brought the glass, containing this substance, near to the heat of the candle, and immediately placed it before the microscope; and then I saw, not only a great number of globules expelled from it, but also many globules expelled from that part which had been converted into a liquid; and in some places I saw that the heat had changed the liquid into hard or solid corpuscles, to which, by reason of their minuteness, I could not assign any determinate shape, and which, to the naked eye, represented a white appearance.

I again brought the glass to the candle, and exposed it to a greater degree of heat, imagining that when, in this closed glass, I had driven the liquid from one place, I should find it collected in another, but in this I was mistaken, for all the moisture was changed into rigid corpuscles, and though, on the next and following days, I

examined the substance in the glass, I could not perceive any moisture.

The piece of this substance which I had left remaining I put into a glass tube, the cavity of which was not larger than the tip of a child's little finger, whereas the former tube was more than three times that size. This I examined by the microscope, placing the particle in a somewhat oblique point of view, and then, not without wonder, I perceived the globules expelled from it; which, to the naked eye, appeared as smoke, not rising up on all sides, as is universally observed in common smoke, but, on the contrary, that they tended downwards.

To illustrate this, let A B C D, Plate XVIII. *fig.* 1, represent this piece or particle of substance, which is called *natural light*, and let A be supposed the lower and C the upper side of it, and in this position the globules, expelled between E D and G, were driven downwards, that is, towards H, and, in like manner, the globules between E B and F were driven towards I; and when I made B the lower part, all the expelled matter took its course towards K, and if D was made the lower part, then it all tended towards L. This expulsion of globules lasted only two or three days, and, as I saw no farther alteration for the space of two or three weeks, I laid the glasses aside.

This substance was sent to me on the fifteenth of April, 1693, and in the middle of August, in the same year, I looked at the first mentioned glass, when I was greatly surprised to find that it seemed to be all vanished out of the glass, whence, at first, I doubted whether this was the real glass which I had used: but, seeing a great quantity of transparent moisture in it, I became more satisfied that it was the same, and the more so when, upon warming the glass and the moisture which adhered to it, towards the warmth I saw the moisture immediately change into a white substance. I then examined the other, and the smaller glass tube, which was still closed up, and I could not discern any alteration in the contents.

In order to give a better idea of the manner of making my observations on this subject, I will explain how I prepared the second and smallest glass tube which I used.

I took a glass tube, of the length and size represented at *fig. 2*, A B, and by the help of a brass pipe, which goldsmiths and other artificers use to folder small pieces of work, which they call a blow-pipe, and by applying with it the flame of a candle I blew at the end of it the sphere or glass globule, *fig. 3*, F D E.

When this sphere and the whole glass was cooled, I dropped into the opening, C, the particle of the before mentioned substance, called *natural light*, which rested at the place E: I then took the sphere between my fingers, that when I should again approach the tube to the fire, there might the less heat reach to the sphere, then, directing a stream of fire to the tube, at G, until the glass was hot enough to be extended, I gave it the figure shewn at *fig. 4*, H I K L. I then broke the glass at the slender part, H, and the tube then was of the shape shewn at *fig. 5*, M N O P, and when the glass was again cool, I brought the orifice, M, to the flame of the blow-pipe, whereby the glass immediately melted, and the orifice was closed. This glass having remained closed up for so long a time as I have mentioned, I broke off a small piece at M, causing an aperture about the size of a pin, and from which the air rushed out with so much force as to produce something of a noise. I then immediately observed the substance in the glass, and saw the globules driven from it in as great numbers as if it had been but newly put into the glass. I examined it several times the same day, and constantly saw the expelled matter tending downwards, as I have before described.

The following day the matter was so diminished that two small cavities were formed in the middle of it, and the moisture was so much increased that the matter swam in it. In the evening of that day two third parts of the solid substance were wasted: I then took the piece out of the water, and saw the globules expelled from it as large as ever.

On the morning of the third day after the glaſs had been opened, I examined it, and ſaw that but a ſmall part was remaining, yet the globules were expelled with the ſame force; in the evening, about nine o'clock, it was diminished to leſs than the ſize of a large grain of ſand, and yet globules were expelled from it, though in no great number, by reaſon of its minutenefs.

On the fourth day there remained no more than the ſize of a ſmall grain of ſand, yet the expulſion of globules ſtill continued: in the evening there was nothing remaining of it deſerving to be mentioned, nor did I ſee any more globules iſſue from it.

During the courſe of theſe my obſervations I have often conſidered what might be the cauſe why flame is ſo eaſily produced from this ſubſtance, but have not been able to ſatisfy myſelf on that head. This, however, ſeems certain to me, that this ſubſtance may, in great part, be kept out of water undiminished, provided it be preſerved from communication with the air, and cloſed up in ſome veſſel, as I have mentioned.

If any perſon ſhould imagine, on reading my deſcription of the glaſſes fabricated by me for theſe and my other obſervations, that I am verſed in the art of glaſs-blowing; I muſt inform him, that the only knowledge I have therein was acquired from thoſe artiſts who, at our fair-times, came to exhibit the manner of blowing in glaſs by the candle or lamp; and, by obſerving their manner of working, I have learned ſufficient to qualify myſelf for preparing glaſſes to anſwer my ſeveral purpoſes.



The Sting of a Gnat, as described by Swammerdam, shewn to have been erroneously pictured by him. The Author's description of the Gnat's Sting, and also of that of the Horse-fly, and the Feathers on the wings of Gnats.

DOCTOR John Swammerdam has lately printed and published to the world a figure of the Gnat, as drawn from the microscope, and particularly its sting out of the case or sheath, which sting is pictured exceedingly pointed and slender, and extended to a great length from the sheath. Upon viewing this figure, I could not persuade myself that the sting was, in reality, formed in the manner there exhibited; for I was well assured that, if of such a shape, it must either be bent or broken when thrust into the skin; nor could I easily believe that in the head of a Gnat, or in the fore part of its body, there could be contained muscles or organs of sufficient length and strength to give the sting its necessary firmness. For these reasons I determined to examine into the true formation of the Gnat's sting, and I was also desirous of discovering, if possible, the reason of the great pain it excites, and of the swelling on the skin caused by the puncture.

I could not discover that the Gnat, when stinging, protruded the sting from the extremity of the sheath (though I thought I saw, rather obscurely, a small part of the sting so protruded), but I always found that the Gnat made a wide opening on one side of the sheath, in like manner as if one was to hold a sword in a sheath, the leather sides of which were laid together in such a manner that, when the sword was to be used, it would not be necessary to draw

it wholly out, there being an aperture left on one side of the sheath, which might be opened by a touch.

Having taken out the stings of many Gnats, through this opening in the side of the sheath, and having examined several Gnats, who protruded their stings, some in part and others to their full extent, through the same aperture, I fancied that I saw the extremity of the sting to be pointed like a spear, and to be barbed or jagged on each side. But, on more narrowly examining the stings I had extracted, I found I had been mistaken in this: that what I at first took for one single sting was, in reality, composed of four parts, for, out of what I before deemed to be only one I took two stings, each having hooks or barbs at the ends; the third part, from which I took those two (and which might also be called a sting), was open on one side, in like manner as I have mentioned respecting the sheath, and terminating in a point, and appearing, through the microscope, like a quill cut sloping: the fourth piece, which was exceedingly thin, seemed to be placed round about the last mentioned sting, but, when I examined it more narrowly, I saw that it also lay in the cavity of it.

When I had separated these four pieces of the sting from each other, they did not preserve their straight figure, but became somewhat bent, especially those two which had hooks on them, so that I could not place these two last before the microscope to my wish. Hereupon I was obliged to cut asunder those four pieces, and give distinct drawings of each of them from the microscope, in order to shew to all men, and in particular to my own countrymen, (who I suppose are more tormented with these insects than the people of other nations, by reason of the many standing waters we have, where they breed in great multitudes), this wonderfully formed and mischievous weapon in the Gnat.

To place all these things in a clear and distinct view, I will first exhibit the figure of the Gnat's sting, as described by Swammerdam, and which he says he himself has seen. Plate XVIII. *fig. 6.* A B C D E, is, as Swammerdam says, the sting of the Gnat, with its

sheath or case, *A B D E* being the case, and *B C D* the sting, which sting, he says, is so sharp at the extremity, that he could not, with his best microscopes, discern the point.

Fig. 7, represents the sheath and the stings of the Gnat, as placed by me before the microscope, and drawn by the limner from the life, by my direction. *F G H I* is the case or sheath, which the Gnat opens on the side, at *G H I*, in order to protrude the stings when it is going to strike: this sheath is covered with hairs, and between them with many small feathers, but which feathers are so closely joined to the body of it that they are rarely to be seen. *H K* is a part of those four organs or stings, as they are regularly placed beside each other, and at such a distance from the sheath as the Gnat protruded them of its own accord, and not forced out by me, unless in killing the animal I did it inadvertently. At *K* are seen the barbs or hooks with which the stings are furnished. The colour of these stings is like that of transparent tortoiseshell. *F I* is that part of the sheath and stings cut off next the Gnat's head.

Fig. 8, *L M N O P*, represents that part of the sting, or second case, from which I extracted the two other stings, the points of which are furnished with hooks. This has an opening from end to end, through which the other stings may be protruded, in like manner as I have mentioned respecting the first sheath: I have also, through this opening, oftentimes drawn out all the three stings which lie within it.

L M Q O, is another piece, lying upon the former, and, being somewhat broader and longer than the other, seems as if designed to cover it like another sheath, but I have often drawn it out of the cavity of the piece *L M N O P*.

Fig 9, *R S T*, represents in part all the stings, and here may be discerned the two inward ones that are barbed, exhibiting the same appearance as at *H K*. These two interior barbed stings are wonderfully thin but not flat, for if so they would, by their thinness, be unable to bear any force, and still less have power to perforate the

skin. But they have a third side on the back of each of them, and their figure, in that respect, is like that of a small sword, which, by reason of its smallness, must necessarily have a third side to give it strength, agreeing, in that respect, with these stings of the Gnat.

Hitherto it has appeared no otherwise to me than that each of these barbed stings has the barbs or hooks only on one side, and when they lie on the inside of their case, or more properly, within the thickest of the stings, their flat sides are close together, and those sides which are barbed lie on the outside, so that when all the pieces of the sting are placed together in order, they exhibit, when taken out of the sheath, the appearance of a single sting barbed or hooked on each side.

Fig. 10, V W X, represents one of the two barbed stings taken out of the cavity of the other piece, one of them being a little longer than the other.

Fig. 11, a b c, represents a part of one of the two last mentioned stings, which, by reason of its exceeding thinness, was bent in this shape, but in this position, the hooks or barbs cannot be distinguished; placing the flat side of this before the microscope, the point of it appeared as at *fig. 12*. Upon turning it round a little, the point appeared as at *fig. 13*, and turning it still more, the hooks became visible, as is shewn at *fig. 14*.

If we consider the formation of these stings, though we know not how the Gnat strikes them into our bodies, or moves them about when there, we may, nevertheless, easily conceive, that when driven within the skin, they may make a very sensible, though minute wound, and by reason of their length, a much deeper one than is occasioned by lice, fleas, or other small vermin. Again, so long as the Gnat is sucking the blood which issues out of the wounded vessels, there will not any swelling appear. But when it draws out the stings, the juices of the wounded vessels continuing to issue, there must necessarily arise a greater swelling than usual, because, as I before said, the stings enter so deep, and another rea-

son is to be attributed to the minuteness of the stings, which make so small a wound, that the skin, especially that part next the surface of the body, closes immediately upon the extraction of the sting.

If any person should be desirous to follow my example in the examination of the Gnat's sting, I caution him to arm himself with patience in the pursuit. I have often opened the sheath or case inclosing these stings, and taken them out as they lay regularly placed beside each other, but to separate the four parts or pieces of this sting, and to place them before the microscope, so as to give a distinct view of them to others, requires no small labour and pains. I have destroyed above an hundred Gnats in accomplishing this purpose, and have been obliged to repeat my observations many times, for though I could see all the pieces, and did my best to fix them before different microscopes, it often happened, that while I was busied with one of them, I lost sight of another; for which reason, I was obliged to make new trials and observations many days together.

I thought it would not be amiss in this place to give a figure of one of the two stings which the large Flies, commonly called Horse-flies, carry in a sheath in their heads near the mouth; which, as drawn from the microscope is shewn at *fig.* 15, A B C D, and I have caused this drawing to be made, not only to shew the nature of this sting, being of a flat shape and very sharp, and with which the animal torments horses to that degree, that they kick and leap about the field even at the sight of that Fly, but also to point out that as the sting from D to C, is exceeding thin and sharp; so from C to B, where it tapers to a point, it grows thicker and thicker to the very extremity at B, by which means it is of the same strength throughout; so that, in a word, we cannot but observe the greatest perfection in the formation of the smallest animals.

The feet of a Gnat, and its whole body, are covered with very beautiful feathers, one of which, drawn from the microscope, is

shewn at *fig.* 17; the wings also are covered with feathers. *Fig.* 18, is one of the wings of the size it appeared to the naked eye; *fig.* 19, A B C, is the same wing somewhat magnified, in order to shew, not only that the whole border of it, A B C, is covered with large and small feathers, but also the nerves or bony parts, D D D D, which give strength to the wings. One of these feathers magnified, is shewn at *fig.* 20. The membrane or thin skin between these parts, appears, when viewed by the microscope, to be covered with a great number of exceedingly minute particles elevated above the skin, and these, upon a closer examination, I found to be, in truth, hairs: they are to be seen at *fig.* 21, which is a sketch of a small portion of the whole wing. A B C, are the feathers on the border, and A D E C, the hairs on the membrane of the wing.



On the Nature of Insensible Perspiration, with the Author's method of computing the quantity of moisture which issues from the human body, by that evacuation.

HAVING had a discourse with a certain medical gentleman on the subject of what is called Insensible Perspiration, or the great quantity of matter or substance which issues from our bodies, and which we are unconscious of, I determined to make an experiment on this subject, by an observation of the perspirable matter issuing not from my whole body, but from one of my hands only.

For this purpose, I took a glass jar, wide enough to admit my hand, which jar, as far as I knew, never had any thing put into it, except clean rain water; and having wiped it as dry and clean as I could, I put my left hand into it, stopping the aperture round my wrist with a cloth, that none of the perspirable matter might escape from the glass, and I then began to drink tea until it not only warmed me, but brought on a moderate perspiration.

After some time had elapsed, I perceived the perspirable matter issuing from my hand, collected on the inside of the glass, exhibiting the same appearance as when in summer time, a bottle of wine is brought out of a cool cellar into the warm air, whereupon the moisture in the air will condense and settle on the glass round the wine. Soon after this, the moisture was so increased, that it adhered to the glass in small drops, and at length those drops ran down and settled at the bottom of the glass. After I had kept my hand in this situation three quarters of an hour, I took it out of the glass, and with all the accuracy I was able, I weighed the perspirable matter which had issued from it, and found it to be the sixteenth part of an ounce.

In the latter end of the month of January, I repeated my observation, by again putting my hand, while it was very cold, into the glass, and sitting down by the fire, I began to drink tea, so hot and so plentifully, as to produce a copious perspiration; and after keep-

ing my hand in the glafs a whole hour, I collected the perfpirable matter, and found it to weigh three thirty-second parts, being a sixteenth and the half of a sixteenth part of an ounce.

Hereupon, I began to consider and reason thus with myself; if the perspiration in every part of one's body is in the same proportion, as in the experiments with the hand, how great a quantity of moisture must issue from our whole bodies, and how necessary must it be when we take any medicine to promote perspiration, that we should also recruit our strength by some restorative liquor, such as either wine and water sweetened and boiled with the yolk of an egg, or else drink meat broth; especially if we consider, that the health and strength of our bodies depends on the juices.

In order to make an estimate of the proportion the hand bears to the whole body, I filled the before mentioned glass jar with water to the brim, and having placed it in a larger vessel, I thrust my hand into it: the water which ran over, and was equal to the size of my hand, I collected, and found it to weigh eleven ounces. I am indeed aware that we cannot make a true computation of the surface of the body by that of the hand, because the hand is furnished with fingers, from which the perspirable matter issues. However, if the perspiration is the same throughout the body as in the hand, I will venture to say, that according to my preceding observations, I should perspire in an hour's time, about the quantity of twenty ounces. For I reckon my body to weigh one hundred and fifty pounds, and my hand eleven ounces, and I compute that eleven ounces weight of water contain eighteen one-third cubic inches, and this I set down as the solid contents of my hand. Farther, I reckon that sixty-five pounds weight are a cubic foot of water, and contain 1728 inches: and according to this calculation, we shall see that the size of my whole body is almost two hundred and eighteen times larger than that of my hand.



On the propagation and rapid increase of the common Fly: the manner in which the common Nettle produces pain and inflammation explained.

A SURGEON of some eminence in these parts, happening to meet with me, shewed me a piece of glandulous or fungous substance, about the size of a finger's nail, which he had taken from the diseased leg of a certain gentlewoman, whose leg from the foot to above the knee, had for some years been uncommonly covered with those kind of tumors, and he told me, that having washed this substance in brandy, and afterwards cut it open, he had perceived in it a number of minute maggots: these he produced to me, but they were so small, that I could not distinguish them without my spectacles. A piece of this substance was put into my hands by the surgeon, in order that I might examine into the nature of those maggots.

Upon my return home, I examined them by the microscope, and was immediately convinced that they had been produced from eggs laid by some Fly upon the diseased part, and I had no doubt, that from them would be produced other Flies of the same species with that which had laid the eggs. This I communicated to the surgeon, who, at first, did not give much credit to it, as not being able to conceive how any Fly could find its way to the part to lay those eggs.

In pursuit of my inquiries, concluding that the piece of flesh on which these maggots were found, would very soon be consumed by them, I supplied them with other pieces of meat, which they also devoured; and I continued to feed them with fresh meat until the fifth day, which was the last day, when, preparing again to feed

them, I found, to my great surprife, that having left the box which contained them open to give them air, they had all crept out of the box, and it was not till after a diligent fearch, that I found many of them (for they had been fifty in number), in the corners and clinks of my ferutoire: they had in thefe five days grown to fuch a fize, that each of them was as long as one of my nails; and the reafon of their quitting the box, I concluded to be, that having grown to their full fize, and requiring no more aliment, they concealed themfelves in holes and corners, in order to undergo their next transformation.

The next morning, being the firft of Auguft, one of thefe maggots, whofe body had been fharp or pointed at one end, was contracted one third in length, fo as to be of an equal thicknefs at each end, and exhibited the figure of a fmall barrel: in the afternoon of the fame day, four others of the maggots had affumed the fame fhape, and they were changed from their original white, to a yellowifh colour: the next day they became red, and fo all the maggots, from day to day, changed from a yellowifh to a red, and at length to a blackifh colour.

Two of thefe cryfales or grubs I put into a glafs, and carried about in my pocket, with intent to expedite their change into Flies, but after five or fix days, I found the heat was injurious to them, for they began to shrivel up, and confequently, I judged, would not produce any living creature. The others I placed on a paper, covering them with a glafs, and at the end of nine days, I opened three of them, and took out of each a perfectly formed Fly, but very moift, and without any motion that I could difcover; they were inclofed in a thin membrane, befides the outside hard fhell which contained them. I could not at firft difcover their wings, but examining them more narrowly, I perceived the wings folded in exact order on their bodies, and having feparated them from the bodies, I found them to be perfectly formed; upon opening the bodies of thefe unborn Flies, I took out of one of them a great quantity of eggs.

On the fourteenth of August, I saw four fully formed Flies flying about the glass, and that the shells or vesicles from which they had issued, had a hole at one end: upon my putting some sugar under the glass, the Flies immediately fed upon it.

The next day all the other aurelias or grubs produced living Flies except two which I had injured in the handling, and at the same time I perceived many other Flies on the glass in my closet, which I concluded came from the maggots which had hid themselves as before mentioned. I placed before those Flies some pieces of raw flesh, but none of them would feed on it; nevertheless, on the eighteenth of August, they all fed greedily on a piece of flesh.

On the twenty-eighth of August, I opened three of these Flies, and took out of one of them a great quantity of oblong eggs, each of which was a twenty-fifth part larger than the eggs I had taken out of the Flies which were not hatched; and what appeared to me worthy of note was, that to each of these eggs was fixed an exceeding small black vessel, through which I conclude each egg had received its nourishment. All these minute vessels arose out of larger and darker coloured vessels, and those again out of a much larger one; all which vessels I therefore concluded to be arteries. Examining these arteries with great attention, I very distinctly perceived them to be formed of annular parts, like the vessels in the lungs of animals, but these annular parts were so exceedingly minute, that, viewed through microscopes of very great magnifying powers, they appeared as slender as a fine hair of one's head seen by the naked eye.

Though these Flies appeared very vigorous, yet a small touch or pressure would cause them to die; for happening to break one of the glasses in which I kept them, whereby they escaped, and flew about my study, though in catching them I handled them as gently as I was able, they died in a few days; some losing their wings, by which I had caught them, others the use of their legs, and laying on their backs motionless for several days; and I concluded their

deaths to be occasioned by this, that in touching them, some of those minute vessels might be injured, and many of the eggs depending on them be broken off, and putrefying in the body, might occasion death. At length, on the seventh of September, I had only two Flies left alive, one of which had lost a wing. These I judged to be a male and a female.

On the ninth of September in the morning, I found one hundred and forty-five eggs laid, as I judged, by one Fly: some of these eggs, with a piece of dried flesh, I put into a glass and carried in my pocket, the weather being cold, to see in what space of time maggots would be produced from those eggs, and I found some of them hatched the very same day. The next morning all the others were hatched, and I found that, in that one night's time, they had all grown twice the size of the eggs.

I again put some more eggs into a glass, and carried them in my pocket, and in five hours time they were all hatched, and in seven hours more they were grown to twice their original size, so that I concluded for certain, that the maggots which had been brought to me on the piece of flesh taken from the gentlewoman's leg, had been produced from eggs laid on it at the last dressing, by some Fly, and that when brought to me, they had been hatched but a few hours.

I caused drawings to be made of the Maggot, the Grub, and the Fly, I have here described, because these Flies are the largest sort found in our country. Plate XVIII. *fig. 21*, is the maggot when grown to its full size and five days old. *Fig. 22*, is the crysalis, aurelia, or grub into which the maggot was transformed, and at one end of it appears the hole through which the Fly issued. *Fig. 23*, is the Fly: and unless I had been convinced by my own experience and inspection, it would have seemed incredible to me, that so large a Fly could proceed from so small a grub; but we must consider, that the wings, and also the hairs with which the Fly is covered, are placed as close as possible to its body, while in its aurelia or crysalis state; but when it becomes a perfect Fly, they separate from the body,

and rise up at some distance from it, and consequently appear larger than they are in reality. *

I know many people are of opinion that flies are produced from corruption, and they pretend to bring many instances in support of that notion, which occurred to me lately in conversation with a certain learned gentleman, who argued thus upon the subject :

“ I have observed,” says he, “ in a parcel of grubs or aurelias, produced from some caterpillars of the same species, four butterflies produced, all of the same kind and shape, and from the fifth aurelia, which had an aperture like the others and was transparent within, three common Flies issued. The cause of this appearance I could not account for.”

To this gentleman I made answer, that these reasonings of his made no difficulty with me, because I conceived the matter might be accounted for as follows :

Flies, and almost all living creatures which are not able to nourish their own young, have it implanted in them by Nature to lay their eggs in those places where the young, when hatched, may find food. When, therefore, Flies of any description cannot find any flesh, fish, or offal, they often lay their eggs in those places where their instinct informs them their young will find subsistence, and this is in the grubs or aurelias of caterpillars; the maggots hatched from these eggs laid by the Fly can easily perforate the thin coat or case of the aurelia, and use for their nourishment that substance within it, which was destined to the formation of a winged creature of a different species, so that from such an aurelia a Fly instead of a butterfly may be produced. With this argument of mine the gentleman declared himself to be satisfied.

Now, I lay it down for a certain truth that it is equally impossible for a Fly, or other living animal, to be produced from corruption, as for rocks to bring forth horses or other beasts.

* Another reason may be assigned for this appearance, namely, the rapid growth of flying insects immediately after their coming forth from their aurelia state. See the Translator's remark on this subject in a note, Vol. I. p. 28.

Many persons cannot sufficiently wonder at the immense quantities of Flies with which the inhabitants of a besieged town, of any note, are infested. But we may easily solve this difficulty, when we consider that it is impossible for the commanding officers to cause all the bodies of the slain to be interred, and that from them, and from the entrails and offal of beasts, left exposed in the fields, the number of Flies must increase beyond measure. For, let us suppose that

<table style="width: 100%; border-collapse: collapse;"> <tr> <td style="border-bottom: 1px solid black;">114 Flies in the first month.</td> </tr> <tr> <td style="padding-left: 20px;">72 of which supposed females.</td> </tr> <tr> <td style="padding-left: 20px;">144 eggs laid by each female.</td> </tr> <tr> <td style="border-top: 1px solid black; border-bottom: 1px solid black;">288</td> </tr> <tr> <td style="padding-left: 20px;">288</td> </tr> <tr> <td style="padding-left: 20px;">72</td> </tr> <tr> <td style="border-top: 1px solid black; border-bottom: 1px solid black;">10368 Flies in the second month.</td> </tr> <tr> <td style="padding-left: 20px;">5184 of these females.</td> </tr> <tr> <td style="padding-left: 20px;">114 eggs laid by each female.</td> </tr> <tr> <td style="border-top: 1px solid black; border-bottom: 1px solid black;">29736</td> </tr> <tr> <td style="padding-left: 20px;">29736</td> </tr> <tr> <td style="padding-left: 20px;">5184</td> </tr> <tr> <td style="border-top: 1px solid black; border-bottom: 1px solid black;">746196 Flies in the third month.</td> </tr> </table>	114 Flies in the first month.	72 of which supposed females.	144 eggs laid by each female.	288	288	72	10368 Flies in the second month.	5184 of these females.	114 eggs laid by each female.	29736	29736	5184	746196 Flies in the third month.	<p>in the beginning of the month of June, there shall be two Flies, a male and a female, and the female shall lay one hundred and forty-four eggs, which eggs, in the beginning of July, shall be changed into Flies, one half males and the other half females, each of which females shall lay the like number of eggs; the number of Flies will amount to ten thousand: and, supposing the generation of them to proceed in like manner another month, their number will then be more than seven hundred thousand, all produced from one couple of Flies in the space of three months.</p>
114 Flies in the first month.														
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Considering this we need not wonder at the great multitudes of Flies observed where the bodies of great numbers of men or animals lie unburied.

There is a wonderful circumstance, and well worthy of note, in regard to Flies, namely, that the maggot from which a Fly is produced will come to its full size in the space of five days: for, if a month or more was required for this purpose, as is the case with other maggots, it would be impossible for Flies to propagate their kind in the heat of summer, because the Fly's maggot can scarcely ever have any food than what is found in the place where the egg was first laid. Now this food of theirs, namely, fish, flesh, or offal, lying in the open air and exposed to the scorching heat of the sun,

would continue but for a very few days to be fit food for the maggots, therefore the All-wise Creator has implanted in those maggots the property of acquiring their full growth in a very few days, when, on the contrary, other maggots which can have a continual supply of food, are months before they undergo any alteration.

I have, at times, carried several of these maggots about me in a glass, giving them every day a supply of flesh, and shewed them to several curious persons, that they might with me observe their wonderfully rapid growth; I have, indeed, brought them to their full size in the space of four days, so that I conceive in the height of summer the eggs laid by Flies may, in less than a month's time, become complete Flies, so as again to lay eggs. Lastly, it is worthy of observation that these maggots do not void much excrement, so that the greatest part of the substances they consume for food enters into the composition of their own bodies.

AT the time I first turned my thoughts on the nature of our common stinging Nettles, I imagined that the great pain and swelling they occasion arose from the sharp points of the stings or prickles, which are thick set on their leaves and stalks, being broken off and left within the skin: but happening one day, while gathering asparagus in my garden, to be stung between my fingers by a very small Nettle, it produced so uncommon a pain and swelling, that I examined more narrowly the formation of Nettles by the microscope, and I found that the stings or prickles are not only hollow, and contain within them a very transparent juice, but that, at the time when they are in their most vigorous growth, this juice issues from the stings, and may be seen to settle on the points in the shape of a very small drop or globule.

Upon seeing this, I formed a different opinion on the subject, and I conceived that, though we may be pricked by the Nettle no deeper than the external cuticle or skin, and though the point of the sting may not be left behind, yet we shall experience both pain and

swelling, if the liquid, which is at the extremity of the sting, or can by any means be expelled from thence, penetrates within the sensitive part of the skin, and there touches or wounds any of the vessels; whereupon some acute salt, which this liquor contains, principally produces the pain and swelling we experience; and this I rather take to be the case, because, on examining Nettles which had passed their full growth, I found that the juice in many of the stings was dried up, whereas those that were still growing were not only quite full of juice, but some of it issued from their points as before mentioned. And I observed the points of those which had come to their full growth, to be for the most part broken, which I attributed to the wind agitating the leaves, and striking the stalks one against another.

I know many people say, that if we boldly grasp a Nettle it will not sting, but the only reason is this, that if we seize a Nettle with the whole hand our fingers are close together, so that the Nettle only touches the skin on the inside of our hands and fingers, which is generally so thick and tough that the stings cannot pierce it, but are either blunted or broken, and therefore we feel no effect on the insides of our fingers, much less the palms of our hands; but the parts between our fingers, where the skin is thin and soft, are those liable to be injured.

In order to shew the formation of the stings of the Nettle, I have caused drawings to be made of them from the microscope.

Plate XVIII. *fig.* 24, A B C D E, is the sting of a Nettle, as it grows on the leaf or stalk, when in its most flourishing state. At C is to be seen a round drop or globule, being part of the juice with which the cavity of the sting is filled. A B D E is a soft, green, flexible part, which has this appearance while on the plant, but, when separated from it, in a little time more than half of it dries away. B C D is the sting itself, which is very transparent when filled with juice.

Fig. 25, FGH IK, is the sting of a Nettle in its vigour, at the end of which, *H,* no juice is emitted.

Fig. 26, LMNOP, is the sting of a Nettle come to its full growth, and in which the moisture which it contained in its cavity is dried up, as is seen between *MNO*; and as in this sting we see several particles, which in the figure are marked 1, 2, and 3, I imagine that this evaporation of the juice from the sting was not performed in one day, but at several times, one day more than another, according to the heat of the weather. Between 3 and *N,* this sting appeared very dark and of a greenish colour, which doubtless was the solid substance of the juice in the sting there coagulated.

Fig. 27, QRSTV, is the sting of the Nettle cut transversely to shew the cavity within, as appears at the letter *S*; and *QRTV* is the green and soft part dried up thus far.

Fig. 28, WXYZ, is a sting of the Nettle cut off nearer the point, where the letter *Y* indicates the cavity.



OF THE SHRIMP.

I HAVE often reflected on the nature of some sorts of shell-fish, such as Shrimps, Lobsters, and Crabs, in this respect only, that they bear their multitudes of eggs on the outside of their bodies, which, if we reflect, must necessarily be so, for otherwise it would be impossible for those fish to produce so great a quantity of young as we find they do, because their bodies being covered with an hard shell, cannot be distended, and would not allow space for the growth of their eggs, if they were always within their bodies.

The eggs of our Sea Shrimps are not produced and brought to maturity at any one particular time of the year as is the case with most other fish; for I observe Shrimps to have eggs at all times of the year; I speak here only of those Shrimps which are caught near this coast; for I am told that the Shrimps about Amsterdam, at some seasons, are all loaded with eggs, and the remainder of the year are destitute of them.

But what I observed beyond my expectation, and which is well worthy of note is this: that every Shrimp's egg, when come to maturity, contained in it nothing but a complete minute Shrimp, which not only I could see lying in the egg in a circular position, with the tail turned over its head and laying on the back; but, when taken out of the shell, I could see some of its scales, together with the tail and fins; also the head and eyes, and those limbs or organs which issue from the head, and which lay as it were folded together near the feet. In short, Shrimps are completely formed in the eggs before they are separated from the parent.

These eggs, for the reason I have mentioned, not being capable of expansion within the body of the Shrimp, are carried about by

the parent, being fixed between those organs or limbs, which we call its feet, and there they are nourished and brought to full maturity by a small string or ligament; being thus protected by the parent until the young Shrimp within is able to break through the shell and seek food for itself.

In order to view the nature of these unborn Shrimps, I took several out of the shells, and having placed them before the microscope, I sent for the limner, whom I directed to make as accurate drawings of them as he could, without informing him what the objects were, and while he was employed in so doing, he often said, "I do not know what I am drawing, but it seems to me to be a Shrimp."

Plate XVIII. *fig.* 29, A B C D, is one of these unborn Shrimps, taken out of the egg, which I extended as much as possible from the rounding position in which it lay; but though I endeavoured to take these Shrimps out of the eggs with the greatest care, yet I could not do it so completely, but that in one of them the tail appeared most plainly, in another the head, and in another the feet. In this figure, A B D denotes the head of the Shrimp, but wherein the feet and other prominent parts, which we see in full grown Shrimps with the naked eye, cannot be distinguished, because all those parts are, as it were, closely folded together next the head, and are extended towards B. *Fig.* 30, E F G H I K, is another unborn Shrimp, which drawing I caused to be made only because here, at the letter E, the fins can be plainly seen, together with the bony parts destined to give strength to the fins. E F G is a part of the yolk of the egg yet remaining.

Upon comparing the size of the eggs from which these young Shrimps were taken, with grains of common scowering sand, I found them, for the most part, of the same size, though some were rather larger and others smaller.

Some years after this I renewed my observations on Shrimps, in order to discover the circulation of the blood in those animals, and

in doing this my eye was taken with those two protuberant parts, which are placed in the fore part of the Shrimp's body, and which are commonly called its head. These protuberances are the eyes of the Shrimp, which it has a power of moving in every direction, so as to view all surrounding objects.

Having taken out the tunica cornea or horny coat of these eyes, and well cleaned the insides of them, I found that the tunica cornea in this creature was very soft and flexible, in comparison with those in the eyes of flies; and when placed before the microscope, in order to view objects through them, I found that such objects appeared with much less distinctness and brightness than when viewed through the optical organs of a fly. The reason of this difference I took to be, that in Shrimps, and many kinds of crabs, but more particularly in Shrimps, they, being always immersed in water, do not require to be hard or tough, and, therefore, when exposed to the air and becoming dry, they contract; inasmuch that these eyes which, in their natural state and united to the animal's body, are of a spherical figure, when dry lose that spherical shape, and from being convex become concave, by contracting inwardly.

I caused a drawing to be made of one of the eyes of the Shrimp, the same size as it appears to the naked eye, and this may be seen at *fig. 31, X*. *Fig. 32, A B C D*, shews a great part of the Shrimp's eye, as seen by the limner through the microscope, and here may be seen a number of optical organs in that single particle or spot which we commonly call the eye. But, as in many flies each optical organ seems to be surrounded with a kind of border of six sides, here, in the Shrimp, each optical organ is contained in a kind of square.

In the figure, that which in every optical organ is represented by a small circle, is to denote a cavity or sinking, but which originally was of a perfect protuberant spherical figure, until the optical organs in the Shrimp, upon drying, became contracted.

Fig. 33, E F G H I, exhibits another large portion of the Shrimp's eye, placed in a direct line opposite to the spectator. Between the

letters E F G K appears a kind of muscular part, void of optical organs, and this part I concluded was what served to move the eye.

Farther, I concluded for certain, that in like manner as every optical organ in the eye of a fly has its optic nerve, so in the eyes of Shrimps every optical organ must have its proper optic nerve. I therefore opened many of the eyes of Shrimps, and not only discovered a great number of minute optic nerves, but also a great quantity of pellucid particles, every where mixed with those optic nerves. Hereupon I began to consider whether or no those pellucid particles (which often appeared to me rather oblong and somewhat crooked) might not each of them be the crystalline humour of one single and individual optical organ, and that their being of the oblong and crooked shape I have mentioned, might be occasioned in the dissection of the tunica cornea, whereas those particles were, in their natural state, of a perfectly spherical shape, and filled the whole internal cavity of their respective optical organs. And, in this opinion I was confirmed when, upon more carefully attending to the Shrimp's eye, I perceived the tunica cornea to have a whitish appearance, and the optic nerves, in the parts towards the inside of the head, to be of a dark appearance, and, when separated from each other, exhibited a violet colour. I also concluded, that if it were possible to take out the eye of a Shrimp, in such a manner as to preserve the crystalline humours within the cavities of the tunica cornea in their perfect shape, uninjured, I might place them before the sight, and that, doubtless, I should see the objects through them as clearly and distinctly as when viewed through the eyes of flies.

In the next place, I was desirous of knowing what was the food of the Shrimp, and I had observed that the part in these creatures, which we call the head, and which when we eat them we throw away, was not only the head, but, for the greatest part, the belly of the Shrimp, whence I concluded that there its stomach might be found.

The first thing I examined was the mouth of the Shrimp, which I found on all sides provided with various organs, and each of these again furnished with many weapons, the whole of which seemed contrived for the purpose of catching the Shrimp's prey, and conveying it to the mouth.

Upon opening the mouth I saw that it had on each side a tooth : the fore part of this tooth, to be used in biting, was not smooth or even, but made with indentings or notches ; and, upon applying one tooth to the other, I found that the rising parts in one tooth most exactly fitted the cavities or notches in the other. These teeth in the fore parts or edges were a little broader or thicker and of a yellowish colour, but the lower ends of them were white.

I caused a drawing to be made of the Shrimp's tooth, as seen by the microscope, and this is shewn at *fig*, 34, K L M N O, in which figure the letters K L M denote that part of the tooth chiefly used in biting.

Coming to the stomach, which I took out of the bodies of several Shrimps and laid open, I saw that some of the animals had fed on small fishes, mixed with minute Shrimps bitten in pieces, and in one Shrimp I found some large fragments of another Shrimp which had been of a middling size, from whence it appeared plain to me, that a Shrimp can open its mouth much wider than I had before imagined.

The stomachs of other Shrimps were empty ; many others I saw containing a great quantity of thin particles, which I deemed to be small bones, which had been part of the fins of some small fishes, and also some of the organs of other Shrimps ground to pieces in the mouth.

These thin oblong particles I have mentioned, when viewed by the microscope, exhibited the same appearance as the fragments or shavings of one's beard viewed with the naked eye, with this difference only, that these particles terminated in a point at one end. I also saw a particle, one side of which was armed with teeth notched

like a saw ; and I also saw the claws or pincers of a young crab, which had been no larger than a grain of sand.

The stomachs of others were filled with fragments of small bones from the spine of minute fishes, appearing like the back bones and tails of haddocks. I likewise saw a small bone of a shape and make different from any I had ever before seen in fishes.

I also saw in the stomachs of many Shrimps, but not in equal quantities in all, small shells, in shape similar to those shells which are found in the greatest quantities on our coasts, nor did I find many fragments of such shells, whence I concluded that, about the beginning of September (for it was about that time these observations were entered upon) many of the small shell-fish, newly produced from the parent, had been caught by the Shrimps, and their shells broken by their teeth, in order to pick out the young fishes from the shells and use them for food.

Many of these minute shells were no bigger than large grains of sand, and I was moreover well assured that all those shells which I took out of the stomachs of the Shrimps had been but a few days brought forth by the parents, because many of those shells were quite transparent, others of them were in part about their outer edges rather obscure, by reason, as I imagined, that they had had an addition in growth since the fishes were brought forth.

I caused a drawing to be made of one of these minute shells, but which was one of the largest of those I had extracted ; and, at *fig.* 35, this shell is shewn of the same size as it appeared to the limner by the naked eye. *Fig.* 36. P Q R S, is the same shell seen through the microscope: the letters P Q R T shew that part of it which appeared bright and transparent, so that all without the letters P Q R T which was not transparent, was added to the shell by growth, after it was separated from the parent.

In some of the Shrimps' stomachs I found some exceeding minute shells, which, on account of their round figure, I called snails, and which were not larger than a large grain of sand ; and to shew the

pretty shape of these shells, I thought it would not be amiss to give a drawing of one of them: this is to be seen at *fig. 37, A B C*, as viewed through the microscope.

I next examined the intestines of the Shrimp, to see whether I could there discover any of those small bones or shells, but finding none, I began to consider that, perhaps, when Shrimps have kept those shells, or other hard substances, which cannot be digested or ground to pieces by the action of the stomach, so long as that all the nutritive particles which can pass into the intestines are extracted, whether they do not throw up the remainder, and void it at their mouths.

The passage out of the stomach was provided with certain organs, like teeth, for the purpose (as I think) of grinding the food a second time, before transmitting it to the intestines.

I have often reflected on the great number of eggs which Shrimps are accustomed to carry, fastened to those limbs or organs placed on the lower parts of their bodies, and which are commonly called their feet; and especially when I considered that those eggs, which are by some means fixed on that part, did not seem to grow any larger, and that all those which I saw were of the same size, from whence I could not satisfy myself in the belief that those eggs had been formed in that place.

Hereupon I began to dissect the belly of the Shrimp, which is in that part commonly thought to be its head, and then I saw that the eggs were chiefly placed there, and the remainder of them next the back, where the body of the Shrimp is thickest.

The eggs which I took out of the bodies of Shrimps were not of equal size in all, but some larger than others: and between those eggs which had been put forth from the Shrimp's body, and were fixed to the lower part of the body, and the largest of those which still remained within the body, I could not discover any difference in point of size.

It was to me a pleasant spectacle to view the eggs, as they lay in the bodies of the Shrimps, for every one of them had in the middle of it a small round bright spot, in like manner as if we were to see with the naked eye, a transparent egg, with a still more transparent yolk in it.

Having made these discoveries, I concluded for a certainty, that when the eggs within the Shrimp's body are come to their full size, that they are soon afterwards emitted out of the body, and, by the Shrimp itself, fixed to those organs commonly called by us its feet, and that they are also placed as close as possible to the animal's body, so as to be no impediment to those organs in the performance of their usual functions. But though each of these organs is furnished with a number of still smaller organs, yet all taken together would not be sufficient to hold fast and secure the great number of eggs with which they are loaded, nor could these eggs be securely fastened, unless each egg had a kind of string or ligament, by means of which they are all fastened together, like a chain or string of beads.

Fig. 38, D E F, represents a very small portion of these eggs, which were dried and placed before the microscope after being taken out of the body of the Shrimp.

Fig. 39, G H I K L, represents the receptacle of the egg, of its natural size, taken out of the Shrimp: the letters *G H K L* indicate that part which was in what is called the Shrimp's head, and *H I K* that part which lay in or next to the solid part of the Shrimp's body. Now, when these eggs increase in size, then that part, marked *G L*, in part surrounds the stomach of the Shrimp, and, in proportion to the quantity of food in the stomach, it either extends farther or is repressed by the distention of the stomach.

I have also observed that, when dissected, some Shrimps whose eggs which were fixed on the outside of their bodies had arrived at that maturity that, I judged, they would produce young minute Shrimps in a few days, within the body of the same Shrimp I could perceive

a great number of eggs, which I thought were grown to that size, that within a few days they would be emitted from the body and placed on the outside of the Shrimp.

As we now see that those Shrimps, which are taken in the sea near our coasts, do fasten their eggs to the lower parts of their bodies until the young are able to issue forth and swim about (whence it is, that Shrimps newly hatched can, in stormy weather, retire to the deeper water, and avoid the agitation or ground swells of the sea upon the shore); on the contrary, among those Shrimps which are caught in the inland sea, round about Amsterdam, few or none are found bearing their eggs about them in that manner: hereupon it seemed probable to me, upon reflection, that perhaps these last mentioned Shrimps, when their eggs are grown to a size to be excluded from their bodies, resort to the creeks and shallows of that sea, where they can find a sufficient quantity of leaves and rushes to which they can fasten their eggs.

In the month of May I examined the bodies of sixty-four Shrimps, forty-nine of which had eggs fixed to the lower parts of their bodies, and the young shrimps in these eggs were some of them more perfectly formed than others, and the darker the colour of the eggs the more full grown were the young within: of the remaining fifteen, I found eggs within the bodies of twelve.

I oftentimes repeated these my researches, and always with nearly the same success. And when I met with Shrimps which had no eggs fastened to the outsides of their bodies, I began to consider whether or no those Shrimps might not have fastened their eggs to leaves, litter, or other substances at the bottom of the sea.

I afterwards examined many parts of the intestines of this creature, which were formed so wonderfully, and with so much art, that to attempt a particular description of them would be only losing time and labour. In a word, if we could place all those organs before the microscope, make drawings of them, and could assign to each part its use, we might cry out, What depth of wisdom is here!

what hidden wonders in so contemptible an animal! and, how little is it that we know!

The mouth of the Shrimp is not, in my judgment, calculated to catch fishes, but only to pick up its food, but that defect is well supplied by two organs placed on the fore part of the Shrimp's body, and which may be compared to arms and hands; these are furnished with many joints, and at the extremities have each of them two nails or claws, the larger of which is moveable, and the smaller fixed and immoveable: by the help of these claws the Shrimp can, in my opinion, catch and hold fast many minute fishes, and also convey them to its mouth, there to be ground smaller by the teeth.

I have given a representation of these claws, as seen by the microscope: *fig. 40*, M N O P Q R S T, is one of them, which I may call the Shrimp's hand. The part denoted by the letters N O P is the nail, which I think is immoveable, and only serves the Shrimp to pierce into the bodies of such small fishes as it catches. P Q is a sharp bony part, which serves to hold the prey more firmly, and is covered with short hairs, some of which extend beyond the edges of it. Q R S is the larger claw, which the Shrimp can open and shut at pleasure, and it is hollowed on the inside, the more firmly to keep its hold. *Fig. 41*, V W X Y, represents the same organ, which I call the Shrimp's hand, in such a position that the larger claw is closed: V W represents the smaller claw, and V X Y the larger, which, when closed, reaches a little beyond the smaller, as may be seen at V.

I have sometimes seen these claws broken, and in a state of decay at the points, caused, as I suppose, by their struggling with fishes too strong for them.

From the view of these claws in the Shrimp I was able to satisfy myself as to the use of those very strong claws which we observe crabs to be provided with, and which, I think, are not intended

merely for catching their prey, but also for grinding or breaking it into small pieces, before it is taken into the mouth.

I have observed Shrimps to have eggs on their bodies during the whole of the summer, but in the month of November no eggs were to be found on them. But when we consider, that all the time Shrimps carry eggs on the outsides of their bodies, other eggs are forming within them, who can pretend to say how many times in a year Shrimps bring forth young?

I never was able to satisfy myself that I had found a Shrimp of the male sex; and all the pains I could take in the search more and more convinced me that there are no males among these creatures, herein resembling those small flies which I have discovered, and described in another place.

These things being considered, namely, that all Shrimps indifferently do bring forth young, and that many times in the year, and every time in very great quantities: these considerations, I say, fully solve what was once with me a difficulty, in regard to the great quantities of Shrimps which are continually brought for sale, inasmuch that many persons in our large towns make a living by that traffic, besides the people who are employed in taking the Shrimps in the sea.



Of the Salts contained in Pepper, and in Tea, with the Author's reasonings thereon: the Salts found in Cantharides described.

I HAVE often reflected on the nature of Pepper, and particularly what might be the reasons that the particles of Pepper excite such a pungent sensation in the mouth, when the same, taken into the stomach or intestines, do not cause any irritation, so as to promote an evacuation.

I at one time thought, on looking at the internal or mealy part of Pepper, that the particles composing it might many of them be very sharp pointed, and thereby produce that kind of pricking on the tongue, which many call heat or burning. But I afterwards rejected this opinion, because, upon contemplating a great number of these particles, I saw they were all of different shapes.

I made a drawing, from the microscope, of five of these particles, many of which together constitute one grain of Pepper, and these are shewn in Plate XIX, at *fig. 1, A.* Many of these particles are, indeed, very long and sharp pointed, which I never observed in any other seed I have examined, for the mealy particles in other seeds are more of a globular form. Besides being of an oblong shape, the particles of Pepper are flat on some of the sides and irregular on others, but in such manner that there are no cavities between the particles, for otherwise each grain of Pepper, when come to maturity, would not be a solid body.

But what is very remarkable, these small particles of Pepper, when laid in water, do neither swell nor become soft, like the mealy substance of wheat, rye, pease, beans, and the like, but preserve their shape (at least as far as I have been able to observe).

Many of these particles of Pepper are transparent like glass, and others of them may be seen to be composed of still smaller particles.

Having examined the parts of common Pepper, I took some of that which is called long Pepper, and because I believe that many persons are unacquainted with this kind of Pepper, I have caused a drawing to be made of it, which is represented at *fig. 2*, B C D E, in which B C is the stalk, and C D E the fruit or Pepper itself.

Upon cutting open this long Pepper, I perceived it was not a single seed of Pepper, but what is called a pericarpium, that is, a case or shell containing many seeds, and in this shell were contained above an hundred small grains of Pepper,

Several of these small grains, each of which is contained in a particular skin or covering closely adhering to it, I took out entire, and held them in my mouth, in order that by the moisture the coverings or skins inclosing them might be softened, and I might the easier discover the mealy substance of which these small grains of Pepper are composed. In doing this I found that the grains, though not broken, excited as strong a sensation on my tongue as if they had been pounded Pepper, whence I concluded that the mealy substance of these grains, which in size and shape agreed with our common Pepper, did not excite that sensation by the acute particles in them, which I have said are to be discovered by the microscope, but rather by some saline particles exciting on our tongues that kind of pricking which is called heat.

I therefore took some of this long Pepper, which I put in clean paper and pounded it on an anvil: I then put it into a new glass vessel, pouring rain water on it till it was covered to about the third part of an inch; after this water had stood about two hours I poured it off, but it being evening I let the water stand all night; the next morning I saw, in the place where the water was most evaporated, an incredible number of saline particles, many of which were almost twice as long as broad, but one side always longer than the other, both sides parallel, and the two shorter sides sloped from the

longer, as represented at *fig. 3, F*. All these saline particles were exceedingly thin. There were many others formed in the same manner, but rather narrower, as at *fig. 4, G*: I also saw many saline particles in which I could not discover any acute angles, as *fig. 5, H*; but the greatest number of particles, being more than all the rest put together, were broad in the middle and drawing to points at the ends, as represented at *fig. 6, I*. These last particles were of various sizes, and some so minute as to be scarcely distinguishable.

After the water had stood a whole day and night, I saw some saline particles collected together in an irregular form, so that they had two, three, or more points at the extremities, as appears at *fig. 7, K*, which I concluded was caused by some of the particles adhering together.

From these observations I concluded that the heat which we feel in our tongues from Pepper, is nothing but the salts or saline particles passing from the Pepper to the tongue, and the sharp points of which so prick the tongue as to excite the sensation we call heat, as before mentioned; but when these saline particles by degrees coagulate, as I have before observed, and get into the stomach and intestines, they may, by such coagulation, so alter their shape as to be prevented exerting such a stimulating power as to cause evacuation, or at least these particles may alter their figure; to which we may add that the juices in the throat, stomach, and bowels, may promote such coagulation, or other disposition of the pungent saline particles, as to prevent their irritating the stomach or intestines, if they are not taken in too great a quantity.

To satisfy myself farther on this subject, I took a new glass and put into it some long Pepper, and, by applying fire to it, I drew off oil from it; the oil, or spirit, which was first driven off by the fire, I often examined, but saw nothing in it worthy of note; the oil that came off last, being heavier, sunk to the bottom, but in this I remarked nothing particular.

I afterwards mixed water with the oil, and by the fire I mixed the oil and water as much together as I was able, but in this I could observe nothing except a few saline particles, like those represented at *fig. 6, 1*; but these I could not well distinguish on account of the oil, the water being evaporated. I also saw in the oil various particles, which I thought were salts irregularly joined together.

Upon the caput mortuum, or substance left behind by the fire, I poured some clean rain water, and let it stand for some hours, that the fixed salts it contained might be drawn off by the water; after this I poured off the water as clear as I could and exposed it to the air in my closet, the weather being calm and the sun shining bright, but the water did not at first evaporate: but, after standing two days and nights, I saw a great number of saline particles swimming in the water, like those pictured at *fig. 6, I*. I also saw many small flat particles of six sides floating about, some of which were so small that I could not distinguish the shapes of their sides without great attention. I concluded that this liquor was, for the most part, composed of fixed salt; therefore I exposed it to some greater degree of heat, and then I saw, as I may say, the whole of it converted into irregular saline particles, which were as transparent as glass, and in such numbers that they looked like a heap of sand.

Upon breathing my warm breath on this newly formed salt it again changed into a transparent fluid liquor.

From these observations I necessarily concluded that there was but little volatile salt in Pepper, because I observed few particles of that sort in it.

After this I took common white Pepper, because I think that there is no other difference between white and black Pepper, than that the outward skin or shell, which is stripped off the former, is left on the latter.

This Pepper I also put into paper and pounded it into small parts, and infused it in water. After the water had stood about two hours I poured it off as clear as I could, and in the evening placed it in my

closet: the next morning I examined it, and with great pleasure I saw in it all the same kinds of saline particles as I have pictured at F G H I, these I could discern most distinctly, and in great numbers, yet much fewer than those I had before seen in the long Pepper, though the quantities of white and long Pepper, and of water, in which I had infused them both, were nearly equal.

I also drew off the oil and spirit from white Pepper, but I could see nothing in the oil except a few particles, which I thought to be salts, in the thinner oil or spirit I saw some saline particles, the same as represented at *fig. 6, I*. These were so small, and so hid by the oil, that they could not be distinguished without great attention.

Farther, I poured fair rain water on the caput mortuum, or solid substance left of this Pepper, and after it had stood for some hours, I poured it off; and when the greater part was evaporated, I saw some saline particles lying in it which had six sides like an equilateral triangle, with the three angles or points a little cut off, as shewn at *fig. 8, L*. Others had six equal sides: there were also some exceeding thin quadrangular figures, as at *fig. 9, M*, and some parallelograms, as at *fig. 10, N*, which were wonderfully thin. Also many small particles like those pictured at *fig. 6, I*, some of which were blunt at one end, and sharp pointed at the other. I saw also various particles, at the extremities of which I could not discover any point. All these saline particles were surrounded with an exceeding thin transparent liquor, which upon the least heat being applied, was changed into irregular saline particles, which caused all the before mentioned particles to alter their exact shape. But with the same ease as this liquor was converted into salts, it was again resolved into a fluid only by breathing upon it twice.

A gentleman of my acquaintance received from the East Indies, among other presents, a small jar filled with the salts extracted from

Tea, which he considered as a present remedy against fevers. A parcel of this he gave to me, which having left for some hours on a sheet of paper, I found it soft, as if it was going to melt away, therefore I put it into a new glass, and poured rain water upon it, in order to dissolve it entirely.

I exposed three different portions of this water to the air, that part of it might evaporate, and by this means some particles of salts became visible, and after a few hours were expired, I saw a great number of particles swimming in the water, which were almost all of the same shape, namely, oblong, and terminating obliquely at the extremities, as at *fig. 11, O*. But these figures were not flat, but somewhat rising in the middle in a sort of ridge, and sharp at the ends, and transparent like crystals. Others of these saline particles were not so perfect, because one half of them was something broader than the other half, as is shewn at *fig. 12, P*.

After this water had stood about two days, I saw the saline particles in the place where the water was not entirely evaporated to be somewhat increased in size, but retaining their former shape.

I saw also in other parts of the liquor not evaporated, a number of small roundish particles, in which I could not distinguish any exact shape, though I viewed them very narrowly and with a very deep magnifier, and besides them an incredible number of wonderfully minute particles, the shapes of which I was still less able to distinguish: these last particles I concluded to be so minute, that a thousand millions of them together would not equal a large grain of sand. However, upon using my sharpest and greatest magnifying microscopes, I saw the saline particles, which, as I have said, appeared round to me, to be, for the most part, of six sides, and some few of them three. And among the minute particles that I saw in such incredible numbers, I was able to distinguish many of them to be of the shape represented at *fig. 3, F*. Others were so small, that I could not with certainty assign any particular figure to them.

After this liquor had stood more than two days exposed to the air in my study, the weather being very warm, with a dry east wind, and about half the moisture remained, I concluded that it then mostly consisted of salts, which were too soft to coagulate, I therefore applied it to a moderate heat, and then I saw an incredible number of saline particles formed in it, many of which were of six, and others of four sides. But whereas our common salt, dissolved in water, when it again forms itself into salts, assumes an exact quadrilateral figure, or if a little of an oblong shape of four sides, yet each angle is always a right angle, or contains ninety degrees, as far as the eye can judge; on the contrary, those saline particles I am now describing, were not only composed of irregular angles and sides, but their sides did rise up in a pyramidical form, like our common salt, so that they appeared to me like irregular quadrilateral cubes. But I must needs say, that none of the saline particles which I ever saw extracted from plants of any kind, appeared so regularly formed in their crystallization, or coagulation, as the salts produced from the first infusion of Tea, and which are represented at *fig. 11, O*, and *fig. 12, P*.

After this, I took a parcel of my best Tea, which I put into a new glass, and, by degrees, applied to it so strong a fire that the glass was red hot, collecting, with all the care I could, the spirit, oil, and volatile salt, which proceeded from it. I then observed that the oil, as soon as it was cooled, was not only very thick, but could not be rendered liquid, save by heat. Upon examining this oil by the microscope, I thought that its tenacity was occasioned by the extraordinary great number of volatile salts it contained, and the number of these salts was so inconceivable that it is impossible to give a true idea of them, so as to conceive that such a small quantity of Tea could produce so many particles of volatile salt. All these saline particles were of the same shape, that is, very long and pointed at both ends; these are represented at *fig. 13, Q*.

I afterwards endeavoured, for my farther satisfaction, to discover, if possible, how many saline particles would be produced from a single leaf of Tea, but having reckoned up only a part of the volatile salts contained in one leaf, I forbore any farther observations, because the number I had already reckoned up was so great that I durst not publish it, as I had purposed to do. And, indeed, many persons could not believe that the leaf itself could be divided into so many parts, visible by the microscope, as I saw volatile saline particles produced from one single leaf.

Moreover, I took the caput mortuum, cinder, or ashes of Tea, on which I poured clear rain water, in order to extract the fixed salt it might contain. After this water had stood on it one, two, and some of it three days, I poured it off as clear as possible, and exposed it to the air to evaporate: I then observed in it many particles of what I before mentioned to be volatile salt; these were not only covered, as it were, with a watery moisture, but many of them I saw lying in the water, so that I was convinced they were particles of volatile salt, which had been so closely united to other salts, and to the caput mortuum or ashes, that the fire I had applied had not been sufficient to expel all those particles of volatile salt.

As I concluded that the watery moisture which did not evaporate, and in which the before named saline particles lay, was nothing but a parcel of salts, though I could not distinguish their figures; I applied some heat to it, and I immediately perceived that, in fact, the moisture was for the most part salt, for it conereted in irregular saline particles, and in such numbers that the substance became white. But soon after it again changed into a fluid and transparent liquid, like water. But after I had suffered this liquid to stand a little longer, I perceived various saline particles of six sides formed in it, which are represented at *fig. 14, R.* Among these, many were so small that they almost escaped the sight, even though assisted by my very best microscopes. I also saw some few exceeding minute saline particles, with figures of four sides.

After this I, at two several times, examined the water which had stood a little longer on the caput mortuum, or ashes, and then I saw a greater number of the particles with six sides: I also saw a few particles like those represented at *fig. 11, O*.

Moreover, I put a parcel of Tea in cold clear rain water, and afterwards some Tea in hot water, in the same manner as it is commonly made for drinking; this infusion of Tea I suffered to evaporate in part, and, in various observations upon it, I always found that there were formed in it many saline particles, coagulated or concreted together, as represented at *fig. 13, Q*, and *fig 15, S*; these were of various sizes, and many of them so minute as to be almost invisible.

These saline particles could not at first be easily distinguished, by reason of the multitudes of small particles which were floating in the fluids, in such numbers that they made the water thick or muddy, so that it seemed to me impossible that hot water could, in so short a time, extract such a number of saline particles from the Tea: and I formed an opinion that the many saline particles in Tea were of such a nature and property as to coagulate, or rather condense, the particles in water, which we call watery particles, and that most of those wonderfully minute particles, which I have said rendered the water turbid or muddy, did not derive their origin from the Tea, but from the water itself. Whence I concluded that all the virtue or effect of Tea, when taken into our bodies, consists in this, that it occasions a great coagulation or condensation of the thick particles of the chyle in the stomach and intestines; whence the other particles or substance of the chyle, which passes to the nourishment of our bodies, being rendered very fluid, the blood may be in a short time diluted or rendered thinner, by which diluting or thinning of the blood we are often relieved from pain in the head, and our whole body refreshed.

I might here add my sentiments as to the reasons why drinking Tea is so very salutary to many people, though the use of it does not agree with some few. But, as these my sentiments might not

be pleasing to some, I have omitted them, and the rather because I meet with men, whose whole aim is to find fault with what I advance.

To these observations I will add what I have observed respecting Spanish flies, or Cantharides. I have examined these Flies by the microscope, and found that their feet were covered with many wonderfully sharp particles; but, as these are seen on common flies, I could not think that these particles could produce such an irritation that when Cantharides are mixed with any ointment, and laid on the body in the shape of a plaster, they raise a blister on the part.

I put, therefore, some Cantharides into a clean paper, and, having pounded them, I poured clear rain water on them. Some of this water I exposed to the air, that the saline particles in it might coagulate, and, after about half an hour had elapsed, I saw a very great number of saline particles, which were three times longer than broad, though I could not discover their exact shape.

I then took a larger quantity of water, and let it stand a whole night, to see whether thereby the saline particles would concrete in larger figures. In this time almost all the water was evaporated, and the saline particles were so concreted together, that I could very distinctly discover the shapes of each of them. These particles lay in such numbers that they made the remainder of the liquid appear white; and it was a very pleasant spectacle to me, to see so many particles lying together of an uniform shape.

Many of these were square at one extremity, making two right angles; and at the other end two oblique sides, making an acute angle, as represented at *fig.* 16, T: others of them were oblique at one end, and the other shaped like the preceding figure, as is shewn at *fig.* 17, V. Some few of these saline particles had the two ends oblique, as *fig.* 18, W, and a few others two sides oblique, but one side rather longer than the other, as *fig.* 19, X. The longest

sides of all these particles were parallel to each other, and the particles were very thick in proportion to their length.

Moreover, by the help of fire I drew off the oil and spirit from Cantharides, but neither in the spirit nor the oil could I discover any saline particles. After this I mixed some clear rain water with the spirit and oil, and shaking them together, to promote the coagulation of the salts, I saw many saline particles, which at first appeared flat and thin, and rounded at the extremities, but the longest sides of them turned or rolled themselves together, as is shewn at *fig.* 20, Y.

After this, I saw various saline particles floating in the same spirit and oil, of the same shape with the former but much smaller, at the same time I saw some saline particles of six sides, but so minute that they almost escaped the sight. I likewise perceived a very great quantity of particles, which I deemed to be salts, but by reason of their extreme minuteness I could not, with all my attention, assign any particular figures to them.

The caput mortuum or cinder left of the Cantharides, I also infused in clear rain water, in order to discover the fixed salts it might contain; and, after the water had stood on it about two days, I suffered part of it to evaporate, and then I discovered a great quantity of saline particles, the shapes of which I could not distinguish by reason of their minuteness. But when almost all the water was evaporated, I saw a few saline particles of a regular square shape and exceeding thin, and among them a few whose sides rose up in the shape of a pyramid, like our common salt, and also some oblong figures of four sides, some of them exceedingly thin; likewise some particles pointed at each end, as is shewn at *fig.* 21, Z, and some of these last seemed, as it were, divided or notched at the other extremity, and with two points, as is represented at *fig.* 22, *u*.

The little moisture in which these very minute particles lay, evaporated on the least heat being applied, and then some oblong and

irregular particles appeared coagulated. But when I breathed upon them two or three times they again dissolved into a transparent liquor.

I have mentioned, as my opinion, that there is no difference between white and black Pepper, except that the white is black Pepper stripped of its skin. And, to prove this by experiment, I took half a pound, or eight ounces, of black Pepper, which, in about three quarters of an hour, I stripped of all the shells or husks, and was thereby confirmed in my opinion, for this Pepper might have been sold for white Pepper. From whence it appeared to me that white Pepper is better than black, not only because it is largest and ripest, but also because there is little virtue in the husks or shells of black Pepper, and also because among the black there are found many parts which may be taken for grains of Pepper, when, in fact, they are only unripe grains, or only husks of Pepper. When I had thus converted my black Pepper into white, I weighed it again, and found I had not so much as five ounces, so that out of eight ounces I had lost more than three, though I must confess I had not been very careful in the operation.



*Of the young Plant discernible in Seeds, and on the Blossoms,
Fruit, and young Wood, in the Buds of Fruit Trees.*

AFTER examining the young plant in Wheat, I turned my attention to that in Rye, and after stripping off the external skin or husk from the seed, I took out the young plant, and placed it before the microscope.

Plate XIX. *fig.* 23, A B C D, is the young plant in a grain of Rye, as seen by the microscope, A a B D, are the parts from which the roots and stems, or straws, will proceed; C is the top of the first leaves in the young plant. Upon cutting this young plant transversely at the end next A, I could only perceive the beginning of one stalk or stem cut across; but when I cut a little farther towards B or D, till I came to a, I perceived two stems, and still farther to B D, I saw the beginnings of three stalks or stems; and though I could not at first see more than the beginnings of three stems or stalks, yet I was very well convinced in my own mind, that every grain of Rye has in it the origin of four stalks or stems; for A a, are two principles or rudiments of the largest young stalks or stems, and B and D are two other principles or beginnings: at length, after dissecting transversely many grains of Rye, I perceived that I had cut through four several trunks, stems, or stalks at one cutting. All these young plants are placed in a kind of meal, which is not so white as the rest of the substance of the Rye.

I have not given drawings of these young stems or stalks cut transversely in the grains of Rye, partly, because I could not place them before the limner's view so distinctly as in Wheat, and also

because there was very little difference between those in the Rye and in the Wheat, except that those in Rye were much smaller.

I also examined the feeds of Barley, and took from thence the young plant with its leaves. But because I perceived very little difference between them and those in the grains of Wheat and Rye, I did not think it necessary to give drawings of them.

But what is worthy of remark in Barley is, that in every grain there are to be seen the rudiments or beginnings of five distinct plants, therefore I have caused a drawing to be made of them, as they appear at one cutting of the seed transversely.

Fig. 24, A B C D E F G, is a small piece of a grain of Barley cut transversely near the end of the seed, where the origin of the young plant is formed, in which may very plainly be seen, a transverse section of five several particles of an oval shape, in each of which, the vessels composing them may be distinguished. The reason why these young beginnings of plants appear oval and not perfectly round, I take to arise from the evaporation of the moisture, as well in the plants, as in the mealy substance surrounding them; for we must consider, that the external part of the seed in drying will contract itself, and that when it is afterwards moistened with water, it will, in a short time, again extend itself, and then the plants, instead of an oval, will assume a round figure.

These young plants are placed in a very small quantity of mealy substance of a darker colour, and not composed of such transparent globules as the rest of the meal of which the Barley consists. This dark meal is placed between *G A B C D E*, and between *E F G* is the mealy part, which for the most part composes the substance of the Barley; and though this dark mealy substance may here seem to be in great abundance, yet it must be observed, that this does not extend farther in the Barley, rising no higher, (in a sloping direction), than where the young plants extend.

I afterwards examined Oats, and found that each feed contained in it the beginning, or first rudiments, of young plants, and the be-

ginnings of leaves; and that the young plants were three in number in this feed.

Now as we see, that the principle of vegetation, or young plant, in every one of the before mentioned feeds, is capable of producing two, three, four, or five shoots or stems, according to the fertility of the earth in which they are sown, we are not to wonder, that from single grains of Wheat, Rye, Barley, or Oats, such numbers of shoots or straws are produced, as we observe.

Among many other feeds which I examined, I observed a remarkable variation in the feed of Buck-wheat, and in my endeavours to dissect this feed, I was obliged first to take off the hard shell, and then steep the feed in water for several hours; for the leaves of the young plant were so intimately connected with the mealy substance of the feed, that, until they had been thoroughly wetted, it was impossible to separate them. When I had taken out the young plant, I found the leaves so twisted one among another, that it was not without great difficulty, I could spread them apart by small pins, so as to be distinctly seen, for these leaves were extremely weak and tender to handle.

Having placed these leaves before the microscope, I discovered in them a vast number of vessels, (which, in a full grown leaf, we should call fibres or ribs, though in fact they are nothing but vessels), appearing like what we see by the naked eye in vine leaves. But whereas, in the leaves of the young plants of Wheat, Rye, Barley, and Oats, the vessels run lengthwise along the stem, in like manner as the vessels do in those leaves when full grown; on the contrary, the vessels in the leaves of the young plant in Buck-wheat are spread all over the leaves, in like manner as may be seen in the leaf of that plant when come to its full growth. I should have given a drawing of this young plant, as seen by the microscope, were it not that it would have taken up too much room on the paper; for, (contrary to other young plants that occupy but a small part of the feed) these young plants spread all over the

feed, and the leaves are extended as far as the outer shell or husk of the Buck-wheat. I have, however, given a drawing of the plant so far magnified, as to shew it distinctly; and likewise a small slice of the feed cut transversely, in order to shew in what manner the young plant is placed in this feed.

Fig. 25, H K L M, is the young plant taken out of the feed of Buck-wheat, and separated from the mealy substance in which it lay, and in which, as much as possible, the several twists or folds are laid open, and the whole (as I said before) magnified just enough to shew the leaves and the future plant distinctly.

H K L, is a large and very thin leaf, within which lies another leaf much smaller and thinner, which it is very difficult to distinguish. This small leaf, I separated a little from the larger one, and let it dry in that position, that the limner might the better distinguish it; this is shewn at letter *K*, and the part from which the root and stem will proceed is noted by the letter *M*.

Another of these feeds I separated from the hard shell and membrane inclosing it, and cut it transversely near the point where the root of the young plant is situated: this I placed before the microscope, and caused a drawing to be made of it to shew in what a wonderful manner this young plant lies among the mealy substance of the feed.

Fig. 26, N O P Q, shews the circumference of this piece or slice of Buck-wheat, cut transversely, as above mentioned, in which *N O P* is the lower part of the leaf as far as it lies contiguous to the husk, and is that part of the leaf, which in the preceding figure is shewn at *L* or *H*.

From *P*, the leaf runs inwards into the meal, and embraces or defends the half of that part from which the root or stem will proceed, as is denoted by the letters *P V T*.

The other side of the leaf begins adjoining the inside of the husk, at *P*, lying close to it, half way round the feed, as far as *P Q N*; so that in this part of the feed there is no meal next the husk, but

only the leaf, though this is not the case throughout the whole of the feed; for if we cut it across the middle, we shall there find much meal next the husk. From N the leaf proceeds inwards, as the letters N D S indicate; so that the two lower parts of the leaf represented in *fig. 25*, at L and H, embrace or defend the whole of the future stem, the external part of which stem is shewn in *fig. 26*, at the letters E and F. In this part of the stem are plainly to be seen a great number of vessels, and likewise in the leaves themselves may be seen many vessels, which here appear cut transversely, and some of them longitudinally.

The places marked by the letters T Q V and D O S, are those in which the meal is contained, and which it was impossible to represent on the paper, therefore those spaces are left vacant. This mealy substance consists of globules larger than those in the meal of Wheat, Rye, or Barley; and these again composed of smaller particles or globules, so that these last are less than the globules in the before mentioned feeds.

But what appeared to me worthy of note, in regard to this mealy substance was, that the large globules I have mentioned, were not of a perfect spherical form, but every one of a peculiar shape; one of six, another of eight or more sides. In short, I can not any otherwise describe their figure than by supposing a great quantity of globules of wax, or any other soft flexible substance, thrown promiscuously into a tub or box, and that, sinking together by their own weight or softness, and thereby almost all the air which had been between them, while they were of a spherical form, being excluded, they lost their shape, and became compacted or squeezed together. And these irregular figures they must retain, until, being diluted in a sufficient quantity of some fluid, they may have room to expand and resume their first globular figure.

I have examined * many other feeds, and among the rest, a small feed called Manna, which is used to be boiled in milk, and which I remember in my childhood to have often eaten, it being esteemed a very wholesome diet. Concerning this feed, there is a vulgar report that it does not grow, but falls from the clouds in Poland at a certain season of the year, and that it is gathered in cloths before sunrise, while the dew is on the ground. This notion I never gave any credit to, and having procured some of this Manna, I saw upon the bare inspection of it, that we are much deceived respecting it, for I could discern in every grain, the place where the young plants had laid. But the true state of the case is, that before this Manna is brought to us, it is ground in mills, in order to strip off the external husk, as is practised with Rice, by which means, the feeds are broken in pieces, and the young plants they contain, for the most part, destroyed. I discovered, however, many entire feeds, and in them the young future plant; from whence the futility of the above idle tale is apparent. But it may be true, that it is the practice to reap and gather in this feed early in the morning, by reason that otherwise the heat of the sun might cause the pods containing the feeds to burst open and shed the feed.

Being once on a journey in Gelderland, where much Tobacco is planted, I desired to have some of the feeds of that plant purchased for me, having never seen any.

This is a very small feed, being no bigger than a large grain of sand. Upon placing one of them before the microscope, I saw that its outer covering or shell, was more curiously formed than any feed I had ever viewed with my naked eye; and indeed it affords so pleasing a spectacle, beyond any other feed, that, had we such an object visible by the naked eye, we should not hesitate to

* The author has given specimens of several other smaller feeds, but as they are all reducible to one or other of the descriptions before given, they are omitted in this translation, to avoid prolixity.

give it a place in a cabinet of curiosities; and, when stripped of its outer coat or shell, and of an inner covering or membrane, the beauty of it is still more conspicuous. To give a view of this pretty object, I caused a drawing to be made of it from the microscope, and this is to be seen at *fig. 27, P Q R*.

I also caused a drawing to be made of the young plant taken out of the seed, as it appeared when viewed by the microscope: this is shewn at *fig. 28, S T V W*, wherein *T V W* is the part which would grow up into a plant, and *W S T*, the root. I had another of these young plants placed before a microscope, of which I have also given a drawing at *fig. 29, X Y Z A*; and here again we may see how this small production of Nature exceeds, in beauty, many of her larger works.

Having for some time contemplated this seed, I took my little brass box, which I filled with wet covering sand, and placed therein a few of the seeds: the box I carried * in my pocket in the day time, and at night placed it in bed beside me, that it might always be warm, and thereby the vegetation of the seed be expedited, as I had done before with other seeds; for I was desirous of seeing whether the young plants pictured at *fig. 28 and 29*, would agree in shape with the plant when in vegetation.

These seeds I examined every day; the first and second day I saw no other alteration, than that they were somewhat swelled: the third day, I saw that the shell was burst open, and at the lower part of the opening the plant had sprouted about two hair's breadth from the shell. On the fourth day, I saw with great surprisè, that the young plant was grown six times as large as the original seed, but, as I could not then conveniently get a drawing made of it, I deferred it till the following day, when the young plant was become twice as long, as it was the day before. *Fig. 30, H I K L*,

* Mr. Leeuwenhoek means the pocket of his *broek*, (Anglicè breeches), which is made large, and being next the body always warm.

represents the two leaves which are shewn in *fig.* 28, at T V W, and in *fig.* 29, at Y Z A.

And now I was not only convinced that, in dissecting the seed, I had formed a true judgment as to the different parts of it, but here I plainly saw the multitudes of vessels of which the leaves in great part were composed; and of which in the two former subjects, pictured at *fig.* 28 and 29, I had only seen, as it were, the shadow. I could moreover distinctly see a great number of valves or joints in each vessel or tube, as well in the leaves, as in the stalk and root, according to what we see in the formation of larger plants.

While I was preparing this object for the limner to make a drawing of the root, as shewn in *fig.* 30, at M O N, I saw so many of the grains of sand adhering to it, that I was obliged to use several expedients to clear them away; and in doing this, many of the small branchings from the root were broken, by reason that they had twined most closely round the grains of sand in a manner wonderful to behold; and if all the ramifications which I saw, had been represented in the figure, the number of radicles branching from the larger root, M O N, would have been twice as many as here shewn: and what is more, in many of these small radicles, might be seen their exquisitely minute vessels.

The larger root, *fig.* 30, M O N, in a very little time became so dried, that in order to make a true drawing of it, and to represent its vessels properly, I found it necessary to place five or six different plants before the microscope one after another.

Now since we see that in so few days time, this small seed will grow to a perfect plant, and moreover consider, that the same perfection in its parts which we observe in the plant, is contained in the seed, though hidden from our eyes, we are not to wonder that in a bird's egg, duly impregnated, a young bird shall in a fortnight's space or less, be completely formed, equal in size to the egg itself.

In like manner as in feeds, ſo in the buds of trees the future produce may be diſtinguiſhed; for I have, in the middle of winter, cut off a twig from one of my beſt bearing fruit trees, and on opening ſome of the buds, and viewing them by the microſcope, I could diſcover the bloſſoms, though wonderfully minute. At the ſame time alſo, I cut a twig from a currant tree, and opened the buds, wherein I ſaw, not only the ſmall currants, which were incloſed in a double covering, and lay in as compact and perfect figure as ſmall bunches of grapes, but I alſo ſaw the germ or firſt ſhoot of the future young wood of the tree, which was already formed and prepared to iſſue from that part where the bunches of currants ariſe in the bud. *Fig. 31, B C D*, are two bunches of theſe currants ſeen through the microſcope, and *E F G*, is the germ or ſhoot of the young wood.

Now, if we do not content ourſelves with merely examining the formation of different feeds, but weigh and conſider in our minds, the wonderful and incomprehenſible perfection in the operations of Nature, or, more properly ſpeaking, of the all-wiſe Creator, who in many feeds has created the future plant, with a mealy ſubſtance for its ſupport and nourishment, and in others has provided for the growth of the young plant, without any ſuch ſupport. When I ſay, we duly weigh and conſider all theſe things, we may conclude and be aſſured, that all the trees and plants now growing on the ſurface of the earth, have been produced in regular ſucceſſion from trees and plants of the ſame ſpecies which were created at the Beginning of the World.



On the Formation of Rushes, and on the Structure of the Nerves.

IN pursuing my observations on the nature of the vessels or tubes of wood in trees, I turned my thoughts to the observation of Bulrushes. These grow in great quantities in the shallow parts of our larger streams and rivers, and the seats or bottoms of our chairs are generally made of them; and, as they have many cavities into which water will not easily penetrate, they are used in time of war for the making of bridges to pass troops over small streams or canals.

Plate XIX. *fig. 32*, A B C D, represents a piece of Bulrush, which I cut at A, in an oblique direction, that the large cavities with which the inside of it is for the most part formed, and which are visible to the naked eye, may be seen.

These large vessels or cavities in Rushes, are furnished with valves through their whole extent, without which the Rushes would be very brittle, and their sides easily compressed together; and likewise, were it not for these valves, if only one end of the Rush was laid in water, the whole would soon be filled; but the water is by these valves prevented entering the cavities otherwise than very slowly, and the Rushes, by reason of the many cavities they contain, can bear, when floating on the water, a great weight in proportion to their size.

I was also desirous to shew this cavity in the Rush when cut lengthwise, in order to place before the eye the valves as they are placed in each of those cavities. *Fig. 33*, E F G H I K, represents a single vessel, with its valves forming one of the cavities of the Rush, which I shall presently shew, as it appears through the microscope, together with the nature of its formation.

I also placed before the microscope two sides of one of these tubes or vessels in the Rush, five of which generally form the cavity of the tube, in order, if possible, to discover the nature of their formation. This at first appeared to me to be something similar to the basket work, or hurdles, with which our country people fence their fields against cattle, and which in other countries are placed on the outsides of houses, and afterwards daubed over with clay. *Fig. 34, LM N Q*, represents one side of this tube in the Rush, drawn from the microscope as nearly as the limner could copy it. *Q N O P*, represents another side of the tube.

What appeared to me particularly worthy of note in this object was, that the before mentioned sides of the tubes were not composed merely of the vessels running along those sides, and of which they seemed to be composed, in like manner as I had hitherto conceived, that all the tubes of wood received their growth, or consisted of their own proper vessels; but the contrary here appeared to me, for I saw that each tube in the Rush arose from, or was composed of, various peculiar small tubes.

In *fig. 34, Q N* represents a small tube in the side of the cavity in the Rush, to which I may not improperly give the name of a rushy blood-vessel, and from which the side *LM N Q*, in part proceeds, and from which also the side *Q N O P*, is composed or produced. This vessel, which I so call a blood-vessel, and also all the vessels of which the sides of the large vessels in the Rush are composed, do again consist of many oblong parts, or rather of wonderfully minute vessels, from which vessels a great number of small vessels arise, and of these, the sides of the large tubes in the Rush are composed.

These minute vessels, which I call blood-vessels, and are, as I have said, composed of still smaller vessels, give rise to a great number of horizontal vessels, which take their course in as regular and exact order among the other vessels, as we may see in the joints of the bamboo or reed from Japan.

In order to exhibit more clearly to view, the formation of this ascending vessel, which I call a blood-vessel, and from whence the sides of the large cavities in the Rush proceed, I took one of the smallest of those vessels, the sides of which I tore asunder as exactly as I could, and placed them before the microscope, that I might cause as accurate a drawing as possible to be made of them.

Fig. 35, R V, represents one of these small vessels, consisting of many still smaller ones, out of which small vessels arise a great number of horizontal vessels, and of these last vessels, as I have said, the tubes of Rushes are composed, and these vessels as broken off from those tubes of the Rush are pictured at *S T* and *W*.

This blood-vessel (as I call it), *R V*, from which such a wonderful production and so many vessels arise is so thin, that measured by the naked eye, I must say, that it is not a twenty-fifth part the thickness of a hair.

But what seemed to me still more wonderful was, that I thought I saw from the ascending blood-vessels represented in *fig. 34*, at *Q N*, or *fig. 35*, at *R V*, and from the horizontal vessels which arose from those blood-vessels, a matter or substance protruded, which immediately extended itself in length, and appeared to form a membrane. But when I examined this nearer, I saw that the side of the Rush's tube, *Q L M N*, was not a simple membrane, but that each separate part forming that side was manifestly hollow, and that the cavity was surrounded on all sides with a wonderfully thin membrane. This cavity is shewn in *fig. 35*, about and before *S*.

Upon discovering this, I placed the sides so broken off from the tube of the Rush, before a deeper magnifier, and directed the engraver to make a drawing of two of the parts, in which he could most plainly distinguish the cavities; and these are shewn in *fig. 34*, between *X* and *Y*.

Now if we suppose that all these cavities, and each of those whereof the side of one of the tubes in a Rush consists, cannot be formed but of an incredible number of small vessels, in like manner

as we see the finallest membranes to be in the bodies of animals, we must again exclaim, “ what inconceivable minuteness is here !”

Moreover, I cut a Rush transversely, in as thin a slice as I was able, that I might the better conceive the formation of what I have called the ascending blood-vessels, and bring them into view.

Fig. 36, A B C D E F G H I K L M N O P, represents a small piece of Rush cut transversely. In E F K L S, is shewn, as nearly as the engraver could represent it, one of the before mentioned valves or divisions which are shewn in *fig. 33*, at E F G H I K, and, as I have before said, oppose the entrance of water and give strength to the Rush. This formation consists of an incredible number of vessels and membranes, each vessel being in a contrary position from that adjoining to it. This valve or division has five distinct sides, as at S E, E F, F K, K L, and L S, and thus (as I have before said) almost all the tubes in the Rush are formed.

Now let us suppose one side of the tube in the Rush, which in *fig. 34*, is described at L M N Q, to be the same as the side which is represented cut across, in *fig. 36* at L M; and moreover, that in *fig. 34*, N O P Q, is another side of the same tube, shewn in *fig. 36*, at M N, and also, that what I have called a blood-vessel, and represented in *fig. 34*, at N Q, is the same with *fig. 36*, where an oval hole is shewn between M and O, and that this vessel is cut across.

Having made these observations, and moreover, seeing that in various cavities of the large tubes of the Rush, there were formed many membranes, especially when the Rush began to be so thin, as is shewn in the partition of two of the tubes in *fig. 36*, between D E S R and D R Q C. I concluded that the sides of the large tubes in the Rush were formed of the small ascending tubes, which I call blood-vessels, as follows :

From the ascending vessels placed between B P Q, the sides of the vessels P O, Q R, B C, B A, and P T, are in part formed; and also from the ascending vessel placed in R, in part is formed the side of the tube, R Q.

Now when the nutritious matter is protruded from the vessel placed in **Q** towards **R**, in order to form the side of the tube **QR**, it meets the nutritive substance protruded from **R** towards **Q**, to make also the side **RQ**, and when this matter or substance, so protruded for the formation of two distinct vessels, and one side of a tube, is formed in great abundance; it is so compressed on all sides, that in the cavities of the tubes, it grows into irregular membranes, as is here shewn in *fig. 36*, between **CRQ**.

And this is also the case with the superfluous matter protruded from the ascending blood-vessels (as I call them), in **D**, **R**, **S**, **E**, for the formation of the sides of the tubes **DR**, **RS**, and **SE**, where also, from the compressed matter, the membrane placed between **DESR** is formed.

This formation, namely, each side of the large tubes, being formed of two distinct ascending vessels, is necessary, for otherwise, the large tubes in *Ruffles* would not adhere firmly. But as the sides of the large tubes are made so compact, that little or no aperture can be discovered in them, so, on the contrary, the membranes, which, as I have said before, are accidentally formed in the cavities of the tubes, are very irregular, and have in them many apertures, and also broken parts, some of which have a cavity in them, and their irregular formation seems to me, to proceed from such extra nourishment not being sufficient to form entire membranes.

But what seemed most worthy of note in these membranes was, that their external edge seemed a little thicker, and that, in so thin a substance as were these membranes, various streaks could be distinguished, which I considered to be vessels. And indeed, I am persuaded, that however thin and transparent the membranes might be, they were no otherwise formed than of vessels joined together.

I have often, with great pleasure, observed the texture of the Nerves, which are composed of very minute vessels of an incredible thinness, and which, running along by the sides of each other, constitute a Nerve; the cavity of each of these small vessels is about two thirds its diameter; and in order to examine them I directed the spinal marrow of three cows and one sheep to be brought to my house, that I might extract from thence the Nerves.

I consider it as a misfortune, that I have not been able to exhibit the cavities in these Nerves to others, but no sooner do I bring them before my sight, than in a very short time, even less than the space of a minute, they so contract by evaporation, that the wonderful object vanishes and cannot be restored. And I not only saw the size or circumference of these vessels, (some hundreds of which go to the composition of a nerve no larger than a hair of a man's beard), but some of their cavities I could as plainly distinguish, as if we were to pierce many holes in a paper with a small needle, and hold them up against the sun. And although these cavities, or the orifices of these vessels are so wonderfully minute, I have seen living creatures in the waters, which could have moved and swam about in them with freedom. In short, the minuteness of some things on this earth is such as seems to exceed all belief.

I cut some of these nerves into small slices, or round pieces, each little or nothing larger than the hair of a man's beard, and wetting them, I placed them on a glass to dry; when dried and viewed by the microscope, I saw in them many small knobs or risings, which I concluded to arise from hence, that the many small vessels of which a single nerve is composed, became contracted upon the evaporation of the moisture they contain, and thence arose in protuberances.

It often happened to me, that when I moistened one of these small slices of a nerve after it had been dried, and looked at it through the microscope, that I saw some small particles in great numbers, swimming about in the water: these particles I judged,

proceeded from out of the vessels, for the knife with which I cut the pieces off was as sharp as a razor.

I am sensible, that what I relate here will not be credited by some persons, who are persuaded that what I advance cannot be proved by experiments or observations: but these sort of objections weigh little with me. I am indeed, by the vulgar, treated as a conjurer, and that I publish descriptions of objects which do not exist in nature, but we will leave these men to talk in their own way.

I was lately exhorted by a respectable gentleman to go on with my inquiries, notwithstanding my * advanced age; forasmuch as, (he said) those fruits which ripen in autumn, are by nature the most durable. I have, therefore, set down the observations I have, with no little pains, made in my decline of life. And, indeed, it is no small trouble to pursue the inquiry into such minute divisions; and it is scarcely to be conceived how the smallest nerves can be divided into so many branches.

Now, though I cannot give a drawing of a nerve cut transversely, to shew the figure it presented to my eye, I will give a figure of one viewed sideways. *Fig. 37, A B C D E F G*, represents a very small portion of a nerve, viewed in this position; the part shewn at *A G F* is only a branch proceeding from the larger nerve.

In this piece of a nerve I could not only distinguish the fibres of which it was composed, and which perform the office of vessels, but I also saw certain cavities in each of those vessels, and the particles or contents within those component vessels seemed also visible to me. Having several times cut transversely, as accurately as I could, some of these minute Nerves, and moistened them with water, I succeeded in exhibiting to the linner not only the small vessels composing the Nerve, but in every filament or vessel, of which I have said the Nerve is composed, we both of us saw an oblong stroke or line, which, in fact, was no other than the cavity or orifice of the vessel

* The Author was at this time eighty-five years of age.

compressed or collapsed, as we find is the case with blood-vessels, when the flesh that incloses them becomes dry.

Fig. 38, B C D E F, represents this small Nerve, the multitudes of vessels composing which are cut transversely, and in which the lines or strokes denote the cavities or orifices of those vessels. This Nerve is surrounded, in part, by five other Nerves, represented in the figure by G G G G G, but in these I have only represented their external coats or membranes, omitting the vessels they contain within them. At four places in the same figure, namely, at B N C, C H I D, E K L F, and A F M, are represented particles of fat, with which particles I have sometimes seen a small Nerve wholly surrounded: but in lean animals I apprehend the Nerves are only separated from each other by their membranes; whence it follows that the juices destined to form themselves into fat, when distributed through the body, will insinuate themselves between the Nerves; and, in some part of this same collection of Nerves, of but a few hairs' breadth, I saw seven small Nerves, and no fat between them.

After this, I pursued my dissection of the spinal marrow, until I could discover the parts extended lengthways in it; a small portion of which spinal marrow, as far as art can imitate Nature, I have caused to be represented in *fig. 39*, M N O P. I also made several transverse sections of this spinal marrow, and at length, with great pleasure, I saw its component parts to be placed exactly in the manner represented in *fig. 38*, except that they appeared rather larger to my eye than here appears. Moreover, in many places I could see the light of the sun through the orifices of the vessels which I had cut across, and I pointed out the same to the limner; but as the object, in all other respects, perfectly agreed with *fig. 38*, I did not think it necessary to make a fresh drawing.

While I was separating the strong outer coat which incloses the spinal marrow, from the marrow itself, I saw many minute Nerves, and, in different places, take a course out of the spinal marrow; some of these were so minute, that what I had at first taken for a

single small Nerve I found to be five at least, and each of those five as minute as that represented at *fig. 37, G A F.* But I wondered to see that these small Nerves, as soon as they issued from the spinal marrow, were inserted into the membrane that surrounded it, and became in a manner united to it. But when they again quitted the membrane, they seemed larger, and to be covered with a new membrane or coat. But in this place I found the Nerves, which I had cut off near their origin, so covered with fat, and surrounded with such strong membranes, that I could not separate them to my mind.

These observations I afterwards repeated in other objects, for the most part with the like success, and in some of them I saw the cavities or orifices of the small vessels, composing the Nerves, still more distinctly; and, upon shewing the same to a learned friend who called upon me, he declared that he could distinctly see the same.

Whoever wishes to make similar observations, must be careful in cutting across such minute vessels, so to hold his knife and to make the incision, that it may not be in the least oblique. The knife also must be very sharp, and the edge of it as thin as possible, otherwise the cavities of the vessels will, in the cutting, be compressed and closed up.



* *The Author's experiments and observations respecting the quantity of Air contained in Water and other Fluids.*

I HAVE seen an engine in form of a pump, designed to extract or pump out the air from water, but the operation itself I never saw. Since that time, I turned my thoughts to the making some small instruments for this purpose, and from which I hoped to derive more success, than from the pump I had seen in the hands of other persons.

For this purpose, I took some very round smooth glass tubes, and of an equal breadth or bore throughout; that extremity, however, which seemed (if any) widest of the two, I prepared for the introduction of a piston or sucker.

Plate XX. *fig.* 1, A B C E D, represents one of these glass tubes drawn upon a reduced scale, for the part marked A B, was upwards of fourteen inches long, and B C, twelve; the cavity or bore was, throughout, nearly the seventh part of an inch in diameter. The cavity in the small glass tube, which by the flame of a candle I had joined to the larger tube, was one sixth part the size of that larger one, consequently, the contents of the larger glass tube would be thirty-six times the tube C E D: this last tube, C E D, was two inches and two thirds of an inch long.

I then took a brass wire, as *fig.* 2, G H I K, at one end of which, with a file, I made three notches, to which end, I fastened a small piece of leather, binding it on with fine silk, which silk was very

* This Essay, and the next following, though not pertaining to microscopical subjects, are inserted to shew the Author's diligence in his inquiries, and the accuracy of his observations in other branches of Natural Philosophy.

firmly twisted in the middle at I: the other end of the wire I hammered flat, and fastened it to a small wooden ball.

Having thus prepared the tube, I filled it with fair rain water, and inserted into it the brass wire by the end H I K, having first well tallowed the leather bound round this end, and I pressed it so forcibly into the tube, that no particles of water could pass by it: indeed, so violent was the pressure, that in this experiment, I broke four of my glass tubes.

This glass tube being thus filled with water, that no air could be seen at M, I pressed the instrument L M, which I will call the piston, slowly into the tube, until the lower part of it, O N, at the extremity N, came near to the oblique or curved part of the glass. In doing this, the water issued out of the aperture in the small tube D, in a stream like a fountain, until there was no water in the tube, but from D E C B to N.

I then held the small glass tube from E to D, to the flame of a candle, until the heat drove out the water from E to D, which being done, I applied the aperture D to the flame, whereby the glass melted, and closed the orifice.

Having thus managed the glass tube, that there was no air (as far as appeared) between N B C E D, unless from E to D, being one sixth part of an inch, I poured a little water into the aperture A, letting it run down and settle upon the piston at O, to the intent, that if there should chance to be any aperture or unevenness in the cavity of the glass, water and not air might pass by the instrument N O, when used as a sucker.

After this, I gradually drew up the piston or sucker out of the tube, and, while I was doing it, I saw, that not only the tube from E to C, immediately became empty of water, and was filled with air, but the water in the larger tube was depressed from C towards B, for the space of three inches; and, during this operation, many bubbles of air arose out of the water, and bubbled to the surface, and still the more when I shook, or with a tap struck

the glaſs. At length, I ſaw no more bubbles of air than what aroſe from the bottom of the water. But what appeared to me in theſe obſervations to be moſt worthy of note is, that the bubbles ariſing from the bottom, were at firſt ſo ſmall, as to be ſcarcely viſible to the naked eye; and by reaſon of their ſmallneſs, they aſcended very flowly; but as they roſe higher, they gradually became larger, and aſcended to the ſurface with greater velocity.

I then again thruſt in the piſton with as much force as I thought the glaſs would bear, and in doing this, I ſaw that the whole ſpace of air from D E C, and three inches farther from C to B, was ſo compr'eſſed, that the ſubtile particles of air which probably had found their way into the glaſs, while I withdrew the piſton, had not only again eſcaped out of the glaſs; but that alſo the ſubtile particles of air mixed with the before mentioned common air from E to D, and alſo the thin particles of air from the bubbles which aroſe out of the water, had, by this violent preſſure, been ſo forced and driven out of the glaſs, that there was no more ſpace of air than about the ſize of two grains of ſand to be ſeen in D.

After this, I again very flowly drew out the piſton, and then again ſo many ſubtile particles of air got into the glaſs, that they filled the whole of the ſmaller tube and a ſpace in the larger tube, the extent of three inches from C towards B. Again I drew the piſton very flowly out of the glaſs, left by the too ſudden irruption of air, the glaſs ſhould be broken. For it had happened to me three ſeveral times, that by drawing out the piſton too haſtily, the glaſs tubes broke at C or D.

The pains I took to make theſe experiments, was for no other end than to diſcover, if poſſible, what was the quantity of air extracted from the water: this quantity, though to the eye it ſeemed conſiderable, yet in fact could be but little; for each of the air bubbles ariſing out of the water is much increaſed by being mixed with the particles of common air. Having taken the piſton out of the glaſs, I ſaw the water in the tube ſettle itſelf at P; ſo that ſuppoſing all the

before mentioned air to be in its natural state, that which came out of the water could not occupy a larger space than from P to F. Now the diameter or bore of the small tube D E C, being only one sixth part that of the larger tube B C, the bore of the larger tube is thirty-six times the size of the smaller one, as before has been mentioned.

The water in the larger tube from B to C, stood at the height of twelve inches, but I will only suppose it eleven inches, and, reckoning from this, I compute that all the air bubbles dispersed through large collections of water, and extracted from it, do not amount to one two hundred and fiftieth part of the bulk of the water. And upon another computation made by me, I found that the bubbles of air extracted from the water did not amount to one two hundred and eightieth part of the water.

After this, I took boiled water and treated it in the same manner I have described as to rain water, but after thrice repeating my experiments, I could not pump out or extract any air from it; though, indeed, I once saw a single bubble arise, but this, I rather concluded to have found its way through some inequality in the glass, or from some water not boiled, which by accident was in the tube before the boiled water was put in.

But because many people may think that, in my experiments, I could apply very little force in glass tubes, in proportion to what may be done by others with larger tubes, pumps, or syringes, I think it right to demonstrate how much force I could apply in my tube.

We will then take the diameter of the piston, or bore of the tube, to be the seventh part of an inch, and supposing that with this piston, I could apply a force equal to ten pounds weight, the conclusion is, that in a pump or syringe of an inch bore, a proportionable power must be applied, which we shall find must be equal to four hundred and ninety pounds, because the piston of this pump or syringe is forty-nine times larger than the other; and if the bore

of the larger syringe was two inches, the force applied must be equal to a weight of nineteen hundred and sixty pounds, following the same rule of proportion. This proves the great power I can apply with my small instrument, which I have made use of for many years. And those persons who are any wise skilled in the rules of geometry and statics, will easily see the truth of what I advance.

In further prosecution of my inquiries on this subject, I ordered the blood of a calf to be brought to me in a clean earthen pot: after it had stood seventeen hours, and the thin liquor which is called the serum of the blood, stood at the top, I put some of that serum into a glass tube, concluding that if any air was to be found in the blood, it would be in this part called the serum, because the globules which give the blood its red colour, are heavier than the serum, and, consequently, must contain a less portion of air.

This serum I treated in the same manner as I had done the water, and I saw, that in drawing out the piston, a great quantity of air bubbles arose from the serum; these were ten times larger than those I had seen issue from the water, and adhered to the glass like froth. At length, having quite extracted the piston from the tube, which was done very gently, for fear of breaking the glass, I saw that all the air bubbles, except a few, disappeared. This serous matter being thus without any pressure, I saw that the space occupied by the air which had issued from it, stood in the tube at the height of one inch, and I computed that it was one three hundred and ninety-sixth part of the said serum.

I repeated this experiment on the serum, which I took as carefully as I could, from the blood of a calf after it had stood only two hours out of the animal, and I found that the bubbles proceeding from it, were not so many by far, as those in the preceding experiment, but of a larger size. The space of air in the glass when the piston was almost wholly withdrawn, was from C to B, four inches and two thirds of an inch. The piston being very slowly taken out of the glass, I saw all the air bubbles vanish, and

the large space of air which in the large tube occupied four inches and two thirds, had so escaped out of the glass, that in the small tube, from C towards E, there only remained of it the sixth part of an inch. So that, according to my computation, the air in this blood only occupied the one two thousand three hundred and seventy-sixth part of the bulk of the blood from which it was extracted.

I then went to a butcher's, at the time I knew he was about to kill a calf, in order to extract the air from the blood while it was yet warm; and for this purpose, I took with me my instruments, and also a glass vessel two inches diameter, and eight inches deep, in order to catch the blood as it issued from the vessels of the animal; which having done, I put some of it, as soon as possible, into my glass pump, and treated it in the same manner as before described, and I immediately saw several air bubbles arise, which when they came to the surface, became very large. I also saw the blood sink in the glass tube from C towards B, so that the tube was filled with air to the height of five inches. Before I began my experiment, I had accurately observed what space of air was in the small tube when I had closed its orifice at D, by the means of fire. Upon drawing out the piston as slowly as possible, I saw all the subtle air, which as I before mentioned, had filled the glass tube five inches from C towards B, had either escaped out of the glass or returned into the blood; for I could not perceive any the least increase of air in the small glass tube from C to D. I then opened the glass tube at D, and poured out all the blood as carefully as I could, and then put into it a fresh portion of blood, and repeated my former method of drawing out the air; and then I saw that the glass tube from C towards B, being about five inches, was filled with air, and that some air bubbles arose out of the blood, which were very large. After I had very gently drawn out the piston, I saw that the air in the small tube was increased by the addition of two air bubbles covered with some blood, neither of which bubbles

exceeded the size of a common grain of sand. But whether this small quantity of air came out of the blood, or was air in the water with which I had washed out the tube, before I put the second portion of blood into it, I cannot determine.

I then gave directions that the glass vessel in which the blood had been taken, should be brought to my house, and after it had stood an hour, and the blood was yet warm, I poured some of that which was towards the surface and was now grown a little thinner, into the glass tube, and having stopped the orifice at D, I again repeated my operation of pumping, and I found that the air produced by drawing out the piston, occupied a space in the tube from C towards B, of almost four inches, and, from the most exact observation I could make, I found that the air in the small tube was augmented only about $\frac{3\frac{1}{2}}{5}$ part of the quantity of blood. I then left the glass tube with the piston extracted, and the orifice at D stopped, for six hours, at the end of which time, I saw that the last produced quantity of air was diminished more than half.

After the blood had remained eight hours after taken from the animal, and there was a quantity of serum collected on the surface, I employed myself to extract the air from it. But because there were eight or ten air bubbles in the small tube among the blood, I could not make so exact a computation as before, but from the inspection of my naked eye, I thought that there was rather a greater quantity of air issued from the blood.

When this blood had stood in the glass twenty-six hours, I took some of the serum off it, which I put into the glass tube and drew off the subtle air, which appeared in many minute air bubbles rising out of the blood, and I found the tube from C towards B, filled with air to the height of three inches and three quarters, and drawing out the piston as carefully as I could, I thought the air produced was $\frac{4\frac{1}{2}}{7}$ part of the blood.

After the blood had remained forty-four hours in the glass, I repeated the experiment of pumping, but I found such a small additional quantity of air produced, that it was scarce worthy of being noted.

After twelve hours more had elapsed, I again drew off air from the blood, but I perceived no alteration nor any increase in the air produced.

After the blood had stood about five days and as many nights in my study, I took some of the serum from the surface, and put it into a new tube, wherein no liquor had ever been put: the length of this tube was seventeen inches from B to C, and eleven inches from A to B.

This I treated in the same manner as before, and perceived that the air bubbles arising from the blood, and the external air which found its way into the glass, occupied a space of five inches and three quarters from C towards B. During my operation, I attended carefully to the air bubbles as they arose, and which grew larger as they ascended, as I have before observed; and I saw some of them which were larger than others, rise to the surface faster, and some of those larger ones ascended with five or six times the velocity of the smaller ones. Some of these larger bubbles frequently drove the smaller on one side, to make way for themselves in their ascent; but I never perceived any of these air bubbles unite together, which I believe was only caused by the viscosity or tenacity of the serum in which they were formed.

Hereupon I figured to myself the reason why a larger air bubble sooner reached the surface of the liquor than a smaller one. For, if the axis of a small bubble is as one, and that of a large one is as three, the proportion of a column, which the power of ascent in the smaller bubble will overcome, is to the larger one as one to nine, and the bulk of the bubble whose axis is one compared with that whose axis is three, will be as one to twenty-seven, and consequently according to this proportion, the larger bubble will ex-



ceed the smaller in the velocity of its ascent towards the surface. And as this is the case with air bubbles in their ascent, the same will hold good in falling bodies of equal density, as in balls of different sizes, discharged with equal force from cannon.

But to return to the subject, I drew out the piston very slowly, and having observed the whole with the greatest attention I was able, I found that the air produced in the small tube from C towards D, by the operation, amounted to a seven hundred and twentieth part of the bulk of the blood.

I know it may be said, that my method of making these experiments was not sufficiently accurate, to which I can only answer, that I conducted them in the best manner I could devise.

I made many other observations which I did not note down, because I think I have sufficiently proved, that while the blood is in the veins, or while it remains warm after taken out, there is no other air in it, than that thin and subtle fluid which pervades not only our bodies, but the substance of all solid bodies.

Now if it be so, that the blood contains no other air than this subtle fluid or medium which can pervade all bodies, and that the quantity of common air found in blood after it has stood some hours, is so small as not to deserve notice, we shall reject the doctrine of many medical and chirurgical persons, who, when they are not able to assign the true cause of a disease, say, that the blood is in agitation, or in a state of fermentation, and the like. But I think we may more assuredly than ever, lay it down as a maxim, that every motion in the blood depends on the heart alone, and that we ought rather to say, the blood is too much condensed, and cannot easily be driven through the small vessels, wherefore its circulation is not duly performed.

But when the blood is so much thickened that it cannot pass through many of the small vessels, and thereby its course is obstructed, and still is driven from the heart with its usual force, I think that by this means it may be made to burst through some of

the small vessels, and thereby produce red spots on the skin; and I have often thought whether something like this might not be the cause of those small ulcers on the skin, which we see in the small-pox. But these are only my own private speculations, which I submit to the judgment of those who make these matters their study.

In further prosecution of these experiments, I took rain water well boiled, and after it was cooled, I put it into a clean glass, first washing out the glass with the same water, and let it stand in my study uncovered for two days. On the third day, I placed the glass in the window, which fronted the North-east, where it remained all day, the sun shining bright with a cold wind from the North. The following day I took my largest glass instrument, which I rinsed out with some of this water, and then filled it with more of the same water, then closing the end of the small tube, and applying my operation of pumping, I saw air bubbles rise out of this water in greater quantity than I had seen in rain water not boiled, but taken out of the cistern. Having drawn out the piston, I saw that when the subtle air had escaped, a greater quantity of common air remained in the small tube than I had before seen in the water which was not boiled, and according to my computation, the quantity of air produced was a two hundredth part of the bulk of the water.

I requested a surgeon of my acquaintance, that when he should bleed any person, he would send me some of the blood; in consequence of which, I received from him a vessel with some blood which had been about half a quarter of an hour exposed to the air before I received it. This blood was so thick, that in two essays to fill the glass tube with it, the air found its way into the tube, and it was not till the third trial that I could fill the tube with blood only; by which means I apprehend that more air was introduced into the blood, than if I could at once have put it into the tube. Upon applying the operation of extracting the air, I saw large air bubbles arise out of the blood; but upon taking out the piston, I found, according to the most accurate computation I

could make, that the quantity of air extracted was $\frac{1}{1273}$ part of the blood.

I was invited by another surgeon to be present when he should open a vein in one of his pupils; but upon this occasion the blood issued very slowly, and as it were, drop by drop. Some of this blood I put into a glass tube which had had rain water in it, and applied the operation of drawing out the air, which I found amounted to $\frac{1}{2058}$ part of the blood.

Now, since we see that blood, when it first issues from the veins, contains in it no air, we may be confirmed in the opinion of rejecting those sayings of medical men, who, when they are called to sick persons complaining of pains in this or that part, tell them, that the wind has got into the part, or that it is a windy complaint, and the like; whereas they ought rather to say, that the vessels in that part are obstructed, that the blood or juices have not their free course or circulation; and these kind of obstructions cause pain, and sometimes swellings in different parts of the body.



Description of an Invention by the Author, for illustrating his Ideas respecting the Effects of the Earth's diurnal Motion on the Clouds in the Atmosphere.

A FEW years since, the eminent Christian Huygens, of Zuylichem, paying me a visit, our conversation chanced to fall on the diurnal motion of the Earth; whereupon I produced to him a glass globe, of my invention, which is represented in Plate XX. *fig.* 3. And, upon my putting it in motion, he was so much pleased with the effect, that I made him a present of a similar globe: and having since frequently reflected on the subject, I determined to publish this my invention, as illustrating my ideas of some of the effects produced by the Earth's diurnal rotation on its axis, which the generality of mankind do not attend to.

I caused some glass globes to be blown, about seven or eight inches diameter, and with a small neck or aperture. Having filled one of these with water, I took some red sealing wax, reduced to a fine powder, and put the same into the globe. I then took a small leaden bullet, which would pass through the neck of the globe, and boring a hole in it, I fixed to it a thread, and passed the same through a cork fitted to stop the opening of the glass globe, the hole in the cork being of such size that the thread, to which the bullet was suspended, might stick in the cork at any length required; then putting the bullet into the globe, I caused it, by means of the thread, to hang at a small distance from the bottom, stopping the mouth of the globe close with the cork; then, with some twine, I made a kind of net-work round the globe, twisting the pieces of twine together, to the length of about a foot beyond the neck of the globe.

I then placed the globe on a table, and, taking hold of the end of the pieces of twine, I twisted them round several times, which, upon lifting up the globe from the table, caused it to spin or whirl round. In this contrivance I purposed to represent, by the leaden bullet, our earth; by the water, the air we breathe; and by the wax, the clouds floating in the atmosphere.

Now, in the circumrotation or spinning round of the glass globe, the leaden bullet, moving slowly, seemed as it were, suspended in equilibrio. But the particles of wax, which, while the globe was at rest, lay round about the leaden bullet, now flew off from it, and dispersed themselves as far as the inside of the glass globe would permit them.

Then lowering the glass globe, while it was yet in motion, I suffered it to rest on the table, upon some paper or a cushion, to prevent the glass being broke; and when the globe was so placed, I saw the particles of wax, at first in a confused and irregular motion, but at length they all subsided and settled round the leaden bullet, so as almost to cover it.

Now in like manner, as by the motion of the glass globe, the particles of wax which at first surrounded the leaden bullet, were driven away and separated from it, so I am persuaded that the clouds, by the diurnal motion or revolution of our earth, are kept suspended in the atmosphere. And as, when the motion of the globe ceases, the particles of wax subside, and collect themselves round about the leaden bullet, such, I am of opinion, would be the effect if the motion of the earth were to cease; for that all the clouds, the watery parts of the atmosphere, and other bodies that have gravity, could not remain suspended in the air, but would fall to the earth, and there remain.

Again, if while the particles of wax lie round about and cover the leaden bullet, the cork which serves as a stopper to the globe be taken out, and the thread which is fixed to the bullet, be so lengthened as to suffer the bullet to lie at the bottom of the glass;

then, the thread be twisted round, and the bullet lifted up from the glass, it will be seen by its motion on its own axis, to drive off the particles of wax: and hence also we may gather this conclusion, that the revolution of the earth round its axis, is wisely designed to drive off the vapours and moisture from its surface on all sides.

A B C D E F, represents the glass globe as it lies on the cushion, and G H, are the pieces of twine gathered up and twisted together. I, is the leaden bullet fastened to the thread K L D, passing through a hole in the cork or stopper.

In the bottom of the globe at A, and round about the leaden bullet, are the particles of wax; but when with the hand at H, the whole is lifted up from the cushion, then by the twisting of the twine, the globe will be swiftly whirled about, and the particles of wax be driven to the sides of the globe, as at B F. But when the revolution of the globe is suddenly stopped, we shall immediately see that the particles of wax quit the sides of the globe, and are hurried about in an irregular manner one among another, and then collect round about the bullet, where at length they subside and settle.

In the next place, I remove the cork stopper and draw it on the thread D M N P, as far as the place marked N O M, and then take hold of the thread at P, with my finger and thumb, and gently twist it, having first laid down the twine G H, by the side of the globe, and then lifting up the thread N P, in like manner as I had before done the twines G H, so as to raise the bullet a very little from the bottom, we shall see how the bullet, in its motion, drives off from it the particles of wax.



Certain Positions laid down by the Author, respecting the Circulation of the Blood in an Human Body, with his Opinion respecting the manner of exhibiting the Circulation by the Injection of Quicksilver.

I HAVE heard it said that the blood is circulated throughout our bodies fourteen times in the space of an hour; but no reasons have ever been assigned to me from whence this conclusion was drawn.

This subject has been much in my thoughts, especially since I have seen so much of the circulation myself; and I have been considering that the blood which is carried to the extremities of our feet, must pass through nearly three times the distance from the heart as that which is circulated to the top of the head. For let us suppose the extremities of the feet, in any human body, to be four feet and an half distant from the heart, we must at the same time reckon the top of the head, in the same body, to be only one foot and an half distant from the heart; and, consequently, the blood which passes through the head may perform three circuits, while that which is carried to the feet is performing one. Hereupon I have considered with myself, how to lay down certain axioms or positions whereby I might investigate this matter, upon sufficient grounds, without paying any regard to bare assertions; and only admitting this single fact, that the blood actually does circulate to and from the extremities of the hands and feet, as well as the rest of the human body.

Upon reflecting how swift a motion the blood appears to have in the arteries, when viewed by the microscope, though when exa-

mined without the help of glaffes the circulation feems to be flow; I have, in my mind, divided a minute of an hour into feventy-two parts (inftead of fixty feconds, as is the common way of dividing time), and I reckoned each of thefe feventy-two parts or portions of a minute, to be of fuch a length, that in it a perfon can diftinctly pronounce a word of four fyllables.

I then took an eel, about a foot long, and placed it before the microfcope, and, from the moft correct obfervation which I could make, I judged that, in the feventy-fecond part of a minute, the blood in one of the arteries, which was of a fize to admit three or four globules of blood at a time to pafs through it, had proceeded the fpace of a fifteenth part of an inch; therefore the blood, in one minute's time, paffed over a fpace of four inches and four-fifths of an inch; and this number being multiplied by fixty minutes, it follows that the blood in this eel could be carried or driven forward 288 inches in the fpace of an hour.

Now, let us fuppofe that the heart (which in eels is placed near the head) was in this eel diftant from the end of the tail, where the fartheft extent of the circulation is performed, 11 inches, it follows that the blood, in its circulation, muft be carried twice this diftance, being 22 inches, before it returned back to the heart; and dividing the before mentioned number, 288 by 22, we fhall find that in this eel the blood circulated to the extremity of the tail and back to the heart, fomewhat more than thirteen times in the fpace of an hour.

In the next place, we will fuppofe that the ends of the blood-veffels in the head, and in the fins next to the head, were in this eel one inch and an half diftant from the heart, and, confequently, that in thefe veffels the blood only performed a circuit of three inches; it will follow, by the fame mode of computation as before, that in thefe parts the blood might perform its circulation ninety-fix times in the fpace of an hour.

Lastly, if we advert to the blood-vessels in this eel, in and near the heart, where probably the blood is not protruded the fourth part of an inch before it is carried back to the heart, we shall find that in those vessels in and near the heart, the circulation may be performed an inconceivable number of times in an hour.

I have many times endeavoured to see the current and circulation of the blood in quadrupeds, which always live on land; but I never could discover it: among birds, I found a species in which I could most plainly see the motion of the blood, but I could not distinguish the complete circulation, by reason of the great number of blood vessels, which caused the part I viewed to appear red.*

Now, if we take for granted that in our own bodies the blood is carried through the arteries with the same degree of swiftness as in eels, and that the extremities of the feet or toes are distant from the heart in any human body four feet and an half, or fifty-four inches, making the blood perform a circuit of 108 inches, it will follow that the blood is carried to the extremities of the feet, and brought back to the heart, in the space of an hour, only two times or circuits, and two-thirds of another circuit.

In the next place, supposing the blood-vessels at the extremities of our fingers to be two feet and three-quarters, or 33 inches, distant from the heart, making a circulation of 66 inches, we shall find, by the same mode of computation as before, that here the circulation can be performed $4\frac{1}{4}$ times in an hour. By the same rule, in those blood-vessels in the breast, and other parts in the body, only twelve inches distant from the heart, the circulation may be performed twelve times in an hour; and in those vessels in the farthest part of the head, which may be supposed 18 inches distant from the heart, the circulation may be performed eight times in an hour.

* A cock's comb and gills, Vol. I. p. 90.

But, if we make a calculation on those vessels in the body which are within twelve inches distance from the heart, and consider moreover how many vessels there are very near to the heart, and even in the heart itself, here we shall find that the circulation may be performed in a very short space of time.

Upon the whole, it may probably be true, that in an hour's time there passes through the heart fourteen times as much blood as the whole body contains: but it cannot be said that all the blood in our bodies performs its circulation fourteen times in an hour, so far as we have seen, from what is before laid down, that in many parts of the body this cannot be the case; and in particular in the feet, where it appears that the blood can, in an hour's time, perform no more than two complete circulations, and two-thirds of another.

I am aware, that many may object to these my positions, respecting the circulation of the blood, and say that I have seen it, in frogs and in fishes, only at the extremities of their bodies, where the motion of the blood is exceedingly slow; but that, if it could be seen in the vessels near the heart, there its motion would be found to be much swifter: and, in proof of these opinions, they reason by analogy to what we observe in the motion of a solid body in the air, such as an arrow or a bullet, which moves with great swiftness when first discharged, but whose velocity continually diminishes: so, say they, the blood must move with much greater swiftness in the vessels next the heart, than in those vessels of the same size which are near the extremities.

To this I answer, that the motion of the blood in our bodies, or in the blood-vessels in fishes, has not any analogy to the motion of a solid body in the open air; and in proof hereof I have caused a figure to be drawn, which may be seen in Plate XX. *fig.* 4. A B C D E F, which we will suppose to represent a leaden tube, from whence various smaller tubes branch forth, as at B G, C H, D I, E K, and F L, and that this tube and all its branches are filled with

some liquid, such as water or blood, or any other fluid; and in the larger tube let there be a piston or forcer, as a A B. Now, I say, that if the liquid in this tube be compressed, the small branch, B, which is next the forcer, will not endure a greater pressure than the branch, F, which is the most distant from it.

And though the smallest tube, F, be a million times less than the largest one, the pressure of the liquor will be the same in both. To illustrate this by another example. Let the diameter of this largest round tube be represented by a square vessel, *fig. 5*, A B C D E F, the measure of one of whose sides shall be as 5, the square of which is 25; then let the small round tube be represented by a square one, as at *fig. 6*, which call 1, being formed of one of the divisions of *fig. 5*, then this small square tube will be to the large square vessel in the proportion of 1 to 25.

Now, let us suppose the large square vessel to be filled with some liquor, which is pressed with a force equal to one hundred pounds, the bottom of the vessel, A F E, besides the weight of the liquor, will suffer a pressure of one hundred pounds; but to press the base of the vessel, *fig. 6*, H, with the like force, will only require a weight of four pounds; and if so, the pressure on the base H will be exactly proportioned to the pressure on the base A F E, because this is twenty-five times larger than H. And we may even say, that if only the weight of a grain of sand was impressed on the part a A B, in *fig. 4*, there is no reason why this pressure should not be felt in all the smaller tubes at the same moment as in the larger one; for, as the whole of the water or other fluid is to be considered as a solid body, there is nothing to prevent such impression being so felt.

Their reasonings of mine will be very easily understood by any one who is acquainted with the principles of hydrostatics, as laid down by Stevinus.

But if we suppose the part A, in *fig. 4*, to be some feet higher than F or L, it may be said that the water in the small tubes, F and

L, will be more compressed than in the tube B, solely by reason of the gravitation of the liquor, if B is placed higher than F and L; but if the liquor in these small tubes has a passage into other small tubes, in which the liquor can rise to the height at Q, then the pressure on the liquor will be the same in all the other tubes; for the pressure is the same in every part of the tube as at its base, regard being had to the diameter or breadth of those tubes.

In a word, the current of the liquor will be of the same swiftness in every one of these branches.

Now, if we consider the large tube, *fig. 4*, A B C D E F, to be an artery, and out of it various small arteries and their branches arising, as B G, C H, D I, E K, and F L, and that M N O P Q R is a vein into which the arterial blood is discharged, we must necessarily say that L M, K N, I O, H P, and G Q, are veins, because G H I K L are the places where the blood begins to take its course backwards.

But the circulation of the blood will be very much retarded in the extreme parts of our bodies, when the limbs are cold; whence it comes to pass that the blood-vessels in those parts are so distended that our hands and feet are swelled, and therefore there cannot be supposed to be so rapid a motion of the blood in the extremities, as in those vessels which lie in the more internal parts of the body, unless all the parts of the body are equally warm.

A certain Doctor of Physic, to whom, among other persons, I had shewn the circulation of the blood, told me that this circulation had also been exhibited to him by a surgical gentleman; and on my desiring to know how it was shewn to him, he said by injecting quicksilver into an artery, which circulated back again through a vein; but when I asked him how they were assured that one of the vessels in which the experiment was made was an artery, and the other a vein, he answered that they were not certain as to that point. I also asked him what was the size of the vein in which the quicksilver, so injected, was circulated; to which he answered, that

it was above a thousand times larger than those vessels in which he had seen the circulation of the blood at my house.

Hereupon I told him, that in this case a person might very easily be deceived; and that the vessel or vessels to which the names of arteries or veins were given, might, in my opinion, be either one or the other; for that I had myself, at times, seen various vessels in which I concluded the blood had performed its circulation, and which were so large that they might be discerned by the naked eye, but that, in those instances, I had always been mistaken; for, upon a more close examination, I had found that those several vessels had arisen out of a branch of one and the same blood-vessel. To illustrate this by an example:

Let A B C D E F G, in *fig. 7*, represent a vein, whose two branches are both cut off at B and E, so that we do not know that each of these is a branch issuing from the vein A. These two vessels, B C D, and E F G, we find again united by the vessel E F, as the blood-vessels are often joined together; and out of these vessels, by squeezing, pinching, or rubbing, we may extract any blood that might be left in them. Now it may very easily happen, that in extracting the blood, or by the strong injection of quicksilver or wax, the valves (if any such there are in the vessels) may be broken, and then the quicksilver or wax, so injected, may be forced through that part of the vessel marked B C F, and return from F to E; whereupon B C D will seem to be an artery, and E F G a vein, and it will be thought that the circulation is performed through C F. When vessels of this kind have formerly been viewed by me, I have found myself to be mistaken in such conclusion; for, upon farther examination, I have found (as before mentioned) that both the branches B C D, and E F G, arose out of the same vein, A.

Again, should we meet with an artery, as represented in *fig. 8*, I K L, into which we inject quicksilver until the whole vessel I K L is filled, so that we may be certain that in this vessel the blood is brought back from K to L towards the heart; nevertheless, we

cannot say that I K is an artery and K L a vein, because in those vessels, which are visible by the naked eye, the blood is not carried from and also back to the heart in circulation; for where I have observed these arteries to have a bend, I have often perceived that they were spread into branches, as is here shewn at L, and these again into smaller branches; so that at first sight we might say that the circulation is performed in M N O, and that M P Q, N P Q, and O P Q, are veins.

I was formerly of opinion, that the blood which, in the arteries, is carried to the extremities of the body, did, in its passage, gradually lose part of its thinner juices, by the continual pulsation of the blood forcing those juices through the coats of the arteries. But, after frequently contemplating the circulation, I am assured that the coats of the arteries are so formed, as not to permit the least particle of the blood to pass through them, until those arteries become divided into such very small branches, as give passage to no more than one, two, or three globules of blood at a time; at which places the circulation or return of the blood to the heart takes place; and here, through the extreme thinness of the coats of the vessels, the finer juices of the blood may find a passage on all sides for the nourishment of the parts adjoining. For were it otherwise, not only the external parts would want a due proportion of nourishment, but the circulation of the blood itself could not be performed to answer its intent; for the external parts of the body require the greatest supply of thin juices, by reason of the great expence of moisture they are exposed to from perspiration and otherwise. But if the blood was carried both to and from the heart in circulation in the vessels, which are so large that we can inject quicksilver or warm wax into them, and are of such a size as to admit two or three thousand globules of blood, or more, to pass through them at a time, and the coats of those arteries being, as I conceive, so solid and impervious that they will not suffer the very fine juices to pass through them, the consequence would be, that in those large ves-

fels, the blood would be brought back to the heart without benefiting the body; whereas we may be assured that the blood is circulated through the vessels for no other purpose than to distribute nourishment to every part.

If we cut open an artery or vein of any animal, out of which the blood is emptied, we shall find the inside of it as clean and white as if no blood had ever been in it; which I think is sufficient proof, that these vessels are formed in such a manner as to retain the thinner juices of the blood in every part, except where they are divided into the most minute ramifications. Nor will this seem strange to us, if we consider those fowls which swim on the water, and there seek their food; for we know, that how long soever they remain in the water, or how often soever the water covers their feathers, yet those feathers do not at all imbibe the water.

I have stripped off the very thin membrane, or internal coat of an artery, and placed it before the microscope; when I saw, with admiration, that it was composed of an inconceivable number of excessively thin parts, interwoven one among another, and linked together in the manner of a net: and when I examined the other coat of the artery, which covered the last mentioned, I saw that the parts of which it was composed, were carried round about the artery; so that the strength of such coat consisted in its being capable of great extension and contraction in breadth, more than in length.

Seeing this, I concluded that every time the blood is driven from the heart, all the arteries expand themselves, which must occasion a pulsation throughout all those arteries; and as by this means they are every time stretched beyond their usual width, they are so formed and constituted as immediately afterwards to contract themselves, which contraction promotes the propulsion of the blood in those arteries, until it reaches those vessels we call veins.

But the contrary, in my opinion, takes place in the blood-vessel which we call a vein; for a vein, when emptying a part of its con-

tents into the heart, muſt neceſſarily at that time be contracted more than when in its natural ſtate, becauſe there cannot be a vacuum in the vein, and there is no ſubſtance at hand to occupy the place of the blood poured into the heart ; for the blood cannot flow from the extremities, or even the parts adjoining, in ſo ſhort a time as would be required, whereupon muſt follow, as before obſerved, a more than ordinary contraction of the veins.

But thoſe parts, of which the coats of the veins are compoſed, which almoſt all take their courſe round about the vein, theſe, upon the effuſion of the blood into the heart, being put in motion, by their undergoing ſuch contraction as before mentioned, will, by their eluſtic tendency to recover their natural ſtate, draw the blood from the moſt diſtant parts towards the heart; ſo that this may be conſidered as a third cauſe of the conſtant circulation and current of the blood.



On the nature of Lime, and other kinds of Cement.

IN order to discover the reason why Lime and Plaster, being moistened with water, do, upon drying, acquire a degree of hardness like stones, I took a piece of Limestone, which, by being burnt, is rendered very soft, and divided it into very small particles; these I moistened with pure rain water, and placed the mixture before the microscope: I did not at first observe it to undergo any alteration; but, continuing my eye steadily and incessantly fixed, both on the smaller and larger particles, I perceived, after ten or twelve minutes had elapsed, a faint appearance of excessively thin and minute saline particles; at the end of fifteen minutes I saw them very distinctly; and in half an hour I saw, not only in every particle of the broken Limestone, but also in the water itself, such a multitude of saline particles as is almost incredible. These minute salts were not formed in straight lines, lying side by side, but for the most part they crossed each other in all directions. For example: I saw a particle of Limestone lying distinct by itself; this particle was more than a thousand times less than a grain of sand, and in it more than fourteen salts or saline particles were formed; they were not all of the same size, but some larger than others, and also of different thickneses; but the two longest sides were all straight, and parallel to each other; the two ends, or shortest sides, were obliquely shaped, yet parallel. This particle, with the salts which shot or proceeded from it, as seen through the microscope, are represented by the figure in the margin.



By the sight of this great quantity of salts lying together in this irregular manner, and especially where the particles of Lime lay

closely heaped together, I was fully satisfied as to the cause of the hardness or cohesion which Lime acquires after being moistened ; for the salts shoot from it, one among another, in all directions, and being soft when first formed, they must, as they harden, become closely coagulated, and fixed to each other, so that not a single salt is formed in the substance, but it is united or connected with others : and I am fully convinced, that the hardness or strong cohesion we find in Lime when made into Mortar, arises only from the saline particles or minute salts produced in it, which are of such a nature, that when once formed, and thoroughly hardened and compacted together, they can never afterwards be dissolved, unless by the powerful effect of fire.

In reflecting on the nature of Lime, as thus described, I began to think that if the Mortar of old buildings were burnt over again, the saline particles formed in it would be so dissolved that it might be again moistened with water, and become serviceable as before. In order to make trial of this, I took some pieces of dry Mortar from the roof of my house, and heated them red hot ; when cold I found they were become so soft, that they might be crumbled to powder between the fingers ; and, upon wetting this powder with water, I found it to possess all the properties of new-made Lime.

Now, since we find that the saline particles in Lime or Mortar are the only reason that Mortar or Cement, when dry, becomes hard or stony, and that if the same be again burnt it becomes soft, all those saline particles being dissolved, inasmuch that we can crumble it between our fingers, we may be well assured that Lime is unfit to make Mortar or Cement for walls of buildings wherein great fires are to be made, and that the proper substance for such kind of work is good Clay, which becomes the harder the more it is exposed to the fire.

I also concluded, that this shooting or formation of salts in common Lime, must also take place in that kind of Cement which is called *Tras*, or *Terras*, which is of that nature that it acquires a stony

hardness, even under water, and is therefore used in the construction of water-works, and in other buildings where more than ordinary strength and solidity are required.

This substance called *Tras*, is brought to us down the Rhine, and is ground to powder in windmills constructed for that purpose, and being mixed with common Lime, it is beaten or worked together a very long time, even for several days; for it is said, that the longer it is beaten the better it is: and, in order to make work firm and strong, it is customary to use three parts of this Cement or *Tras* and four of Lime.

A piece of this mixture of Lime and Cement, taken from an ancient building, I heated red hot, and when it was grown cold I found it so soft, that it was easily reduced to powder. This powder I mixed up with water, and perceived a great quantity of bubbles arise from it, the same as we observe in fresh Lime; after this had stood for an hour, it was grown stiff. I then beat it, or mixed it up again, and in a short time it became exceeding hard.

This repeated beating, or working up of the *Tras* or Cement, is of very great use, because, by this means, the air bubbles formed in it are expelled; for want of which being done the Cement cannot acquire its proper firmness. This appeared in the small portion of Cement which I have just said that I mixed or worked up only twice; for, after it was become dry, I broke it in pieces, and found in the middle of it many round cavities, which I concluded to have been caused only by the bubbles of air in it. But, when the Cement is beaten or worked together for several days, not only the air bubbles are expelled, but, after a certain time, no new ones are formed in it; and I am certain that if our common Lime was to be beaten or worked up for as long a time as the *Tras* or Cement, the bubbles of air which are formed in it would be all beat out, and the labour more than twice repaid by the goodness of the Mortar, especially in cases where solid and durable work is required.

ADDITION, BY THE TRANSLATOR.

On the same Subject ; from Smeaton's Narrative of the Construction of the Edystone Light-House.

IN this work, Mr. Smeaton has given an account of his examination of all the species of Lime or Cement that came to his knowledge ; and he ranks among the first, or most powerful, the Terras described by Leeuwenhoek ; and also Puzzolana, which is a Cement of the same kind, brought from Italy, as the former is from Germany. Both of these are supposed to be volcanic productions ; and if so, they are substances prepared by Nature, in the same manner as Lime is by art, namely, the operation of fire. Mr. Smeaton recommends, for water buildings, the mixture of Terras or Puzzolana, with Aberthaw Lime, and he approves of repeated beating of the Mortar so made, as it thereby acquires the greater strength.

Mr. Smeaton's sentiments on the preparation of these Cements, entirely agree with those of Mr. Leeuwenhoek, though our English Engineer seems to have been ignorant of the cause of the cohesion of Mortar, and of the reason why it is improved by beating ; which have been so accurately investigated and minutely described by the Dutch Naturalist.



The Author's Discoveries and Observations respecting Wood consumed by Maggots.

CASTING my eye in the winter season on some pieces of oak, (called in this country, *Tel-hout*), which I had kept in my house about six years, for fire-wood, I saw that it was almost wholly covered with a kind of white powder of the same colour with the wood. Conceiving that this dust was caused by some maggots concealed within the wood, I took out of the heap a piece of wood very much covered with this powder; it had been cleft lengthwise, was about an inch thick, and eighteen inches long, and of five years growth; but I could not see on the surface of it any of those maggot holes which we see in many woods, although I had wiped off all the powder it was covered with. I then cut it in half, and having split the pieces, I took out of one half of it seventeen white living maggots, all of the same size; their bodies were short, and when taken out of the wood, they rolled themselves up in a kind of circle: their fore part was twice as thick as their hind part: moreover, when taken out of the wood, they were not able to creep along; their feet were very short and covered with many hairs, and on each foot a straight claw. These maggots had closely compressed the particles of wood which they had gnawed off, and also the excrements left behind them in the winding channels they had made in the wood, which I wondered they could do, because their bodies were very soft, except the organs they had in the fore part of their heads, and these were not white, but of a yellowish colour. Although I had no doubt that these maggots in the summer time would be changed into flying animals, I nevertheless put fourteen of them into a little box with a cover, which screwed on, and these I carried about me, to discover how long they would live.

In the prosecution of my observations on this subject, I was astonished to see that the wood, on the surface of which there was no appearance of biting or perforation, was yet so gnawed through, that it would easily break. Now when people in general see such pieces of wood, which on the outside appear quite uninjured, and yet so brittle, and contain so much dust or powder, they think that the wood has a natural tendency to decay, or that it is consumed by maggots bred in the wood by spontaneous generation, because, as I said before, they see no holes in the surface.

But if we consider how small those eggs must of necessity be, from which so minute an animal is produced, as proceeds from these Maggots by transmutation, and that a Maggot newly hatched from such an egg may creep into the chinks or cracks, or even the large vessels of the wood, and pierce holes in it so small, as at first to escape the notice of our eyes, we may in some measure solve the difficulty.

I was well satisfied in my mind, that the flying animals to be produced from these Maggots would be of the species whose wings are covered with shells or cases, and to satisfy myself in this respect, I took about a third part of the same piece of wood (from which part I had not taken any maggots nor could I discern any holes perforated by them in the surface), and inclosed it in a glass tube, each end of which I stopped with a cork; this tube I put in my closet, in a place where it would be often exposed to my view, and that thus I might discover what would become of the Maggots, if any there were, in it. This piece of wood I often looked at, but could not perceive any alteration, except that there appeared some particles scraped or bitten off the wood which lay in the glass, and also a Maggot which had crept out of it and was dead.

On the 21st of May, seeing no farther alteration in the wood, I took three other small thin pieces of the same fire-wood, which I broke and took out of them about thirty Maggots, which were now changed into aurelias or cryfales, and as yet of a very white colour :

these crysals were very prettily shaped, for in them I could plainly see their eyes, curiously formed of various optical organs, having the resemblance of bunches of grapes; and I also thought that each eye consisted of more than two hundred such optical organs, besides which there were plainly to be seen the like joints, rings, or creases as in the maggots, also the horns on the head, the feet, and the shells or cases for coverings to the wings; and, in a word, there appeared so many and so great perfections in this insignificant animal. that I could not view them without astonishment.

In about three weeks from this time, my occasions having called me out of town for a few days, I observed at my return that all the animals which I had put into a box were changed into flying creatures, excepting two or three who were dead and their bodies dried up, and these I supposed I had injured in taking them out of the wood. I also saw eight flying animals, which had issued from the small piece of wood I had put into a glass tube, sitting upon the wood, and several round holes pierced in the wood, through which I concluded they had escaped

These flying animals were, in my judgment, six or eight times larger than that which is produced from the maggot which feeds on the oily substance of Mace, of which I have treated at large in another place. The shells or cases also, which cover the wings, are nearly of the same shape in both species.

From hence we see that such young shoots of oak, as I have before mentioned, are more liable to be consumed by maggots than the larger pieces growing in this country, and much more still than those which grow in warmer regions: and for this there can be no other reason assigned, than that those smaller pieces of wood, which are the shoots which spring from the roots of old timber trees, and grow up into faggot wood, are in this country very slow in growth, and consequently very porous; whereas the pieces or shoots of oak which are of quick growth are firm and solid, and these maggots

can make but little impresson on them, and the reason * of this strength and firmness in oak timber has been explained by me in another place.

Moreover, those pieces of wood which are left with the bark on, are more obnoxious to be injured by maggots than those which are stripped of it, because, as I conceive, the maggots, while young, are more able to gnaw into the bark than into the wood itself; but when they have fed upon the bark for some time, they by degrees grow stronger, and are better able to gnaw the wood: of this we have a proof in baskets, which are much less liable to be injured by these maggots when made of white wands than of those which have the bark on.

* Vol. I. p. 5 and 6.



Description of a minute Fish found adhering to the shells of Sea Fish: the Author's Examination of the Eyes of Flies and other small Animals, and his Observations and Conclusions thereon.

I HAVE often taken notice of certain knobs or risings which sometimes are seen in great numbers on the shells of muscles, though on other shells none are to be found. I have also seen them on oysters, and I have counted as many as fifty on one shell: these knobs or swellings, are called by our common people, pustules, or pimples. They are considered by many to be only casual excrescences on those shells; but, if we examine them carefully, we shall find, that they are in reality small fishes fixed to the shells of muscles or oysters, where they gradually increase in size; so that I once saw on an old muscle one of them, whose diameter at the base was three quarters of an inch, and there were many other smaller fishes of the same species, but of different sizes round about it.

These small fishes, which I call Pustule-fish, or Pimple-fish, are fixed by their shells on different parts of the oysters or muscles. But whereas, oysters and muscles, when seeking their food, open their shells a little, these small fish are inclosed by a solid shell on all sides, except an opening at the top or point of each shell, in which opening are placed two very minute oblong shells, not fixed to the other shells, but to the fish itself, and by the help of which it can close the opening in the other solid shell. These two minute pieces of shell have each of them a straight side, exactly fitting each other; the other sides are rounding and sloped to a point.

Upon putting some muscles, to which several of these small fish were fixed, into salt water, I saw the fishes protrude these small shelly parts beyond the aperture, and open the extremities of them about as wide as the back of a common knife, and upon my touching those parts lightly with a small needle, the fish immediately closed them, and also drew those shelly parts within the solid shell

which thus became shut. These small fishes lived only two days while in my possession, but how long the muscles to which they were fixed, had been out of the water when I got them, I do not know.

I have often taken these minute fishes as gently as possible out of their shells, and spreading their parts asunder, placed them before the microscope, and I must say, that I never with my naked eye, beheld any fish, in the formation of which there appeared such wonderful art, as in these minute fishes; therefore I determined to give a drawing of the greatest part of one of them.

Plate XX. *fig. 9*, A B C D, represents part of a muscle shell, in which, at E, is shewn the shell of this small fish, as fastened to the muscle shell, and which, as I have said before, is called a Pustule, or Pimple.

The wonderful and curious formation of this little fish, the limner could not represent exactly after Nature, because the beauty and regularity of its shape were lost when the moisture evaporated from it; and besides, the several parts of its body were so exactly and delicately joined together, that when I endeavoured to spread them asunder, some one part or other was always broken.

This fish is provided with a receptacle containing a great number of eggs, but which, at the least evaporation of the moisture, are so contracted, that nothing of them can be distinguished.

Fig. 10, A B C D E, represents the body of this fish as seen by the microscope. E F, is one of twelve organs or limbs with which it is provided, besides the other organs marked K L M, N O, and P Q R. All these organs lie bent in a kind of curved position, and are so twisted one within another, that they cannot be distinguished unless they are gently spread asunder.

I have said, that the eggs were so soft, that on the least evaporation they lost their shape and vanished from the sight, therefore I determined to boil some of these Pimple-fish a little, and by so doing, the eggs acquired such a degree of hardness, as to keep their figure, and enabled the limner to see them distinctly and represent them in his drawing, as may be seen in *fig. 10*, at G H I. But I directed,

that only a few of the eggs should be represented, for if all those eggs which lay about that part of the body marked A B C, had been inserted in the figure, the number would have been ten times as many as here shewn: moreover, it is to be observed, that all these eggs were held together by a kind of membrane, and in such exact and regular order, that it was a pleasure to behold them. On farther examination, I perceived in each egg a round corpuscle like a yolk, and each egg was composed of a great number of larger and smaller globules, as I have always observed all eggs, or at least the yolks of them, to be formed.

I have given a drawing of this minute animal, for no other reason, than to convince those persons of their error, who absurdly maintain, that a living creature can be generated from corruption, by shewing the wonderful formation of so minute and contemptible an animal, and which not one man in a thousand knows any thing of, not to mention that many vessels, nerves, and organs, worthy of admiration, may be seen in this small creature, when first taken out of the shell, and while the parts of its body remain moist.

I have often made repeated dissections of the eyes of various kinds of flying insects, merely on account of the pleasure the contemplating them afforded me.

In particular, in the summer season, I examined the eyes of that large flying insect called the * Dragon Fly. These being placed on a sheet of clean paper, with a small hair pencil and fair rain water, I cleared away the many vessels which fill the inside of the tunica cornea, or horny coat of the eye, leaving only the tunica cornea remaining. This I contrived to place in such a manner, that it might not, as it dried, contract in wrinkles, and placed it before the microscope, and I often contemplated it with great admiration, for I not only saw the many parts or optical organs of which it was composed, each of which was a perfect portion of a sphere; but each of these spherical parts was inclosed in a partition separating

* See page 47, of this volume.

it from the others, exhibiting at the parts of separation a kind of luminous appearance, so that each spherical part was surrounded by six sides, as is shewn in *fig. 11, A B C D*.

When I removed this tunica cornea a little from the focus of the microscope, and placed a lighted candle at a small distance, so that the light of it must pass through the tunica cornea, I then saw through it the flame of the candle inverted, and not a single one, but some hundreds of flames appeared to me, and these so distinctly (though wonderfully minute) that I could discern the motion or trembling in each of them.

Directing my view, through the same tunica cornea, to the steeple of our new church, which I have measured by a quadrant and found to be two hundred and ninety-nine feet high, and I judge to be about seven hundred and fifty feet distant from my study, I saw the representation of a great number of minute steeples inverted, and which seemed no larger than the point of a needle seen by the naked eye.

When I looked at the neighbouring houses, I could see, through the tunica cornea, not only the buildings, but the doors and windows, and could plainly distinguish whether the windows were shut or open.

It must be observed that in *fig 11, A B C D*, a very small part only of the tunica cornea is represented, but to give an idea of the whole of it, and to shew the multitudes of the protuberances or optical organs it contains, every one of which is a perfect eye, I counted how many of those protuberances were contained in the diameter of the whole tunica cornea, which amounted to about one hundred; but if we reckon them to be only ninety, the number of optical organs in the whole surface of the tunica cornea will amount to above eight thousand.

In the same manner I also examined the eyes of a fly, and I perceived the same perfection in the formation of the optical organs it contained (though these were smaller than in the dragon-fly); and

through every one of them I could most plainly distinguish all surrounding objects.

Fig. 12, EFG, represents a small portion of the tunica cornea in the eye of a fly, in which those protuberant parts or optical organs were not fewer in number than in the eye of the dragon-fly. And to shew the incredible perfection of each of these optical organs, I took a large grain of common scowering sand, and I must say, that above a thousand of those protuberances, in the eye of a fly, taken together, were not equal to the size of that grain of sand.

What I have here said, respecting the eye of a fly, must also be understood of the eye of a gnat, regard being had to the comparative smallness of the gnat's eye.

About the time of my making these observations, I found it asserted in the writings of a certain author, when treating of ants and moths, that ants have no eyes in their heads, on account of their smallness; but, meeting with some ants in my garden, I found, upon dissecting them, that each of their eyes contained fifty optical organs; and in the brains of those creatures I saw, with admiration, the wonderfully minute blood-vessels they contained.

Resuming the examination of the eyes of the dragon-fly, I was desirous to know whether the tunica cornea in them consisted of many scaly particles laid one on another, as I have in another place described to be the formation of it in the eyes of men and quadrupeds. This dissection, after some difficulty, I accomplished; but I observed that the least alteration made by dissection in the tunica cornea, prevented my distinguishing objects through it: I, however, discovered that it was in reality composed of many particles or scales, laid one on another, of which I could distinctly count thirteen series.

Now, if we consider the multitude of parts of which these scales or skins must be formed, which escape our researches and our sight, and moreover, how closely and exactly they must be laid together, to give free passage to the light, we shall be lost in wonder at such perfection in the eyes of such insignificant creatures as these flies:

at the same time we must admire the regularity of Nature's works, all whose operations are performed in similar ways, both in the formation of the eyes of these minute creatures, and in those of larger animals, and also of the human species.

Now, since we see the regularity of the propagation of the minute marine animal I have been describing, and when we contemplate the wonderful formation and incomprehensible perfections in the eyes of flies, and the admirable construction of their many optical organs—when, I say, these things are known, surely none will be so absurd as to retain the notion that any animal, however contemptible in our eyes, can be produced spontaneously or bred from corruption.

But I will conclude with this wish, that the eyes of all may be opened to the truth of the regular propagation of all the animals and vegetables on this earth; and to admire the infinite wisdom of the Creator, in the formation of all things at the Beginning, and in the wonderful and infinitely diversified provision made for their propagation through all succeeding ages of the world.

END OF THE SECOND VOLUME.



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