

DISSERTATIONS

RELATIVE TO THE

NATURAL HISTORY

OF

M. Post

ANIMALS AND VEGETABLES.

TRANSLATED FROM THE ITALIAN OF THE

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INTRODUCTION.

IN the first and second Dissertations of this volume, I fulfil in part the promise I had long since made in my *Prospectus concerning Animal Re-productions*, in which I announced my discovery of the pre-existence of the germ in one species of frog. This discovery is related at length in the first dissertation. That so long a space of time has elapsed since the observation was made, I consider as a very fortunate circumstance; for, having examined other animals, and having found that the same thing is true with respect to them, I have still stronger reason for presuming, that the existence of the germ in the female before fecundation, is one of the most general laws of nature. While we are in quest of one truth, it generally happens that others offer themselves as it were spontaneously. Accordingly I have been led by the observations, which shew the pre-existence of the germ,

to discover that an order of animals, considered by naturalists as oviparous, is in reality viviparous.

The subject of my second dissertation is the artificial fecundation of various animals, of which the first lines may be seen in the same prospectus. This was accomplished by means of the feminal liquor of the animals themselves; and I have succeeded as well, as if the male himself had performed his proper function.

The close analogy between animals and vegetables, induced me to enquire, whether the embryos of plants exist in like manner in the ovarium before fecundation. From the third dissertation it will appear that this is really the case. These enquiries have also convinced me, that the fecundating dust is not so absolutely necessary as botanists in general suppose.

The same analogy ought, perhaps, to have led me to attempt the artificial fecundation of plants; for, though several have undertaken such experiments, and succeeded in them, I know not whether any one has done this with truly philosophical views, and in order to discover the manner of this wonderful operation. To confess the truth, I would not have avoided this task, if engagements of
another

another nature had not prevented me. I have, however, exhorted others to undertake it, and at the same time have proposed some views that may shew the way to success.

It is said by many, that fecundation is among the mysteries of nature; and, like many of her operations, an object of admiration, rather than of enquiry. Such an opinion is highly agreeable to the idleness of man. In times past, I acknowledge that generation, both in animals and plants, was involved in darkness, impenetrable to the human eye; but, since the appearance of Haller and Bonnet, this gloom has been rendered much less thick. I am very far from thinking that I have dissipated it entirely, yet I would fain hope that, by my efforts, it has been somewhat cleared, and that a light, less feeble and uncertain, now shines through it. The following dissertations will enable the learned reader to judge whether this opinion is well founded, or merely the offspring of self-love.

DISSERTATION

CONCERNING THE

GENERATION

OF

CERTAIN ANIMALS.

CHAP. I.

GENERATION OF THE GREEN AQUATIC
FROG.

I. **T**HUS I chuse to denominate the frog in question, from the green hue of the upper part of its body, and from its living in water, particularly in puddles and ditches, whereas other species live on dry land and trees; but of these last I shall have occasion to speak afterwards.

II. The head of the male is furnished with two membranous vesicles, which are very much dilated when the animal-croaks. The great toe of the fore foot has a fleshy prominence, which becomes very perceptible at the

season of their amours: the female is destitute of these parts, and has the back and thighs marked with black spots, which are seldom visible in the male.

III. This species is not to be confounded with that called by Roefel *rana viridis aquatica*. A glance of his beautiful coloured figures, will be sufficient to shew that they are totally distinct. His exceeds all other frogs in size; mine is the smallest of all, and is scarce one-third so large as Roefel's; besides his has three yellow stripes on the back and sides, which in mine are totally wanting (*a*).

IV. The amours of this species begin in April, and end in May; they are, however, influenced by the temperature of the atmosphere; during their continuance the males maintain an incessant croaking. In autumn and winter the immature eggs lie all in the ovarium, which is divided into two lobes; these lobes consist of lesser lobes, each of which is invested with a peculiar membrane. The eggs are of two sizes; some very small, so as to be scarce visible by the naked eye; others seven or eight times larger; both

(*a*) This essential difference was still more apparent when I came to compare mine with some of Roefel's, brought me by the fishermen.

kinds are globular. The smaller are of a livid grey colour; of the larger one hemisphere is white, and the other black. The slightest touch is sufficient to burst them, after which they are resolved into a cineritious viscid liquor.

v. The ovarium of this, as well as many other species, appears to be externally covered with black points, which have been the source of a memorable mistake; for Vallisneri (*a*), and other celebrated naturalists, have supposed them to be the rudiments of the tadpole. The error arose from their being contented with first appearances, and searching no further; for the spots lying close to the eggs, might easily induce the observer to believe that they form part of them, and are therefore so many feluses. But the matter is cleared up by the following experiments: 1. When the common membrane is removed, these points still adhere to it. 2. And if the eggs are inspected one by one, both externally and internally, no vestige of this black spot can be perceived. If the membrane of the ovarium be examined by the microscope, it will then be seen that these points are black

(*a*) Opere fisico-mediche, T. I. Osservazioni intorno le rane,

spots of an irregular shape. They are not peculiar to this membrane, for they are found in the area of the mesentery, and upon the heart.

VI. If the eggs be again examined in spring, we shall still find them in the ovarium, but considerably enlarged; (those I mean of the larger size, IV). and they will be found to be mature, when the male is coupled with the female. The individuals of this species couple exactly like those observed by Swammerdam and Roefel (*a*). The male climbs upon the back of the female, and passing his fore legs under her axillæ, brings them to meet upon the breast, and there clasping his fingers, holds her close, till she has discharged all her eggs. The duration of this process is inversely proportional to the warmth of the atmosphere. When this is considerable, the female will be at liberty in five or six days; but in a cold season, the embraces of the male continue for eight or nine days. This I learned by putting frogs in large vessels full of water, when their amours were about to commence, and confining them till they had discharged their eggs. And, although Vallisneri asserts, upon the

(*a*) Biblia Naturæ. Hist. Nat. Ranar.

authority

authority of a single experiment, that frogs do not discharge their eggs in close vessels, it is certain that numberless facts prove the contrary, as I, and, before me, Swammerdam and Roefel have found.

VII. It is true, as he asserts in another place, that this does not happen when the female is kept constantly separate from the male. I say *constantly*; for if they be pulled asunder when the eggs are descended into the cavity of the uterus, they will be discharged, though the female is kept separate, but they will be barren.

VIII. If the situation of the eggs, at the time of copulation, be examined, during the first days they will be found in the sac of the ovarium; and, during the succeeding, partly in the ovarium, and partly in the oviducts, and at last all in the uterus, except the small ones, which remain attached to the ovarium. The eggs, when in the ovarium, are smaller than when in the oviducts and uterus. In these situations they are enveloped with that viscid transparent mucilage, which is improperly called *frog's spawn*.

IX. Of the various trials I have made, in order to ascertain whether eggs taken from the ovarium, the oviduct, and the uterus, when the male is embracing the female, would

would be prolific, I must own that not one has succeeded. As this is a point of extreme importance, I repeated my experiments to satiety; and in my journals I find, that I have opened one hundred and fifty-six females while they were embraced by the male, of not one of which did the eggs ever bring forth young, though I immediately placed them in water; whereas those that were excluded spontaneously by the female, were all prolific. I have even taken further pains. The discharge of eggs lasts about an hour; during this process I killed a female, and put the eggs that remained in the body in the water into which those discharged by the animal fell; but the latter produced tadpoles, while the former became an offensive putrid mass. From these facts I concluded, that the fecundation of the eggs does not take place within, but without the body; whence it appears how far Linnæus was mistaken, when he pronounced, in his usual decisive tone, “Nullam in rerum natura, in ullo vivente corpore fieri fecundationem vel ovi impregnationem extra corpus matris (a).”

x. Hence we likewise see the falsehood of the strange opinion of professor Menzius,

(a) See Artedis Ichthyology, part ii. p. 32.

who supposed that, while the male embraces the female so closely, the seed is emitted from the fleshy prominence of the toe, and passing through many windings unknown to us, penetrates into the thorax, and there impregnates the eggs (*a*).

XI. But if in this species fecundation takes place out of the body of the female, shall we suppose that the male ejects his semen upon the eggs as they are discharged (*b*)? This, with respect to other species, is the opinion of Swammerdam, and adopted by Roefel; though he afterwards says, that he cannot determine whether the eggs in the uterus are impregnated by the semen emitted out of the body but afterwards arriving at them. To clear up this point, I have paid unremitting attention to every phænomenon. As soon as the eggs begin to be discharged, the agitation of the female is extreme; she darts backwards and forwards, rises towards the surface of the water, and then sinks, keeping the hind legs constantly stretched out, and croaking in a low voice. The male keeping his hind legs close to his body, throws himself into strange

(*a*) Generatio *παρα δαξος* in rana conspicua.

(*b*) In the frog, toad, newt, and several other animals, the eggs are discharged at the extremity of the alimentary canal.

contortions,

contortions, and accompanies the croaking of the female with a kind of interrupted noise, which I cannot express by words. I moreover observed, that an obtuse tumid point, which I suspected to be the penis, was elongated, and now and then brought towards the eggs nearest the vent; but I could not perceive any emission from this supposed penis. The semen, however, might be in such small quantity, or so transparent, as not to be visible.

XII. To determine the question, I placed some couples, of which the female was beginning to discharge her eggs, in empty vessels. As I knew that in these amphibious animals fecundation always takes place in water, my hopes from this expedient were not very sanguine; but fortune was more propitious than I could have expected. The male is so much attached to the female, that, notwithstanding he is taken out of his natural element, he persists in performing his office. I now saw that there darted a small jet of limpid liquor from the tumid point in the vicinity of the anus, upon the eggs hanging out at the vent of the female. This phenomenon took place only at intervals, the female from time to time discontinuing the discharge of eggs, and the male the emission
of

of semen. I was an eye-witness of this curious scene in seven couples of frogs taken out of the water; it did not entirely cease, till all the eggs were expelled from the uterus. The eggs being afterwards put into water, and bringing forth young, I hesitated not to suppose, that the liquor emitted by the male was real semen; as I afterwards found, by discovering it in the vesiculæ feminales, and ascertaining the point to be the penis.

XIII. The abbe Nollet, in a letter dated nine years ago, alluding to the hint in my prospectus, of my discovery of the existence of the tadpole before fecundation, expresses himself in this manner: “ What you say of the existence of the tadpole, before we can perceive any act of fecundation, has particularly struck me. About thirty years ago, Mr. Reaumur and myself made many researches relative to this subject. We attended for weeks very diligently and patiently, to what passed while the male embraced the female. I remember putting breeches of waxed taffety on the male, and watching a long time, without perceiving any appearance that denoted an act of fecundation.”

I will not here stop to enquire by what fatality it happened, that two naturalists, so intelligent and attentive, failed in their enquiries.

quiries. The idea of the breeches, however whimsical and ridiculous it may appear, did not displease me, and I resolved to put it in practice. The males, notwithstanding this incumbrance, seek the females with equal eagerness, and perform, as well as they can, the act of generation; but the event is such as may be expected: the eggs are never prolific, for want of having been bedewed with semen, which sometimes may be seen in the breeches in the form of drops. That these drops are real seed, appeared clearly from the artificial fecundation that was obtained by means of them (a).

XIV. Having discovered every thing relative to the situation and manner of impregnation, my next business was to observe the eggs, till the young animal should appear. The eggs lie in the centre of a white mucilage, which consists of transparent spherical masses connected together. Round the egg are two concentric membranes, of which the innermost, when pierced with a needle, discharges a fluid as limpid as water. The egg is round, and has a smooth surface, of which one hemisphere is black, and the other white. This is seen at Fig. 1. Plate I. N^o. III. shews fifteen eggs, with their mu-

(a) See Dissertation VIII.

cilage;

cilage; each lies in the midst of a circle, made by these membranes. To see the other membrane clearly, the gluten must be magnified as at N° II. C is the external, B the internal membrane, and D the gluten.

xv. When the hot season is far advanced, the observer soon perceives the lineaments of the tadpole. The egg grows for some hours without losing its round shape, it is next elongated; the white hemisphere becomes darker, and the black changes into a longitudinal furrow, terminated by two perpendicular processes. And as it increases as well in bulk as length, the internal circular membrane is dilated, and contains more fluid. Compare N° I. fig. 2. with N° II. fig. 1.

By tracing thus the progress of evolution, we come to perceive that these bodies are not eggs, as Naturalists suppose, but real tadpoles. The furrow and the processes become longer; the supposed egg assumes a pointed figure; the whitish hemisphere dilates, and the black is incurvated. The pointed part appears to be the tail of the tadpole, and the other the body. Further, the opposite end takes on the appearance of the head, in the fore part of which the form of the eyes is visible, though they are yet closed. The two processes also, by which the animal fastens himself to the smoothest

smoothest bodies, when it is tired of swimming, become evident, as likewise the vestige of the aperture of the mouth, and the rudiments of the gills.

xvi. At first the tadpole does not shew any signs of animation, when touched with a needle, or suddenly exposed to the rays of the sun, even when concentrated in the focus of a lens, he does not become sensible to these impressions, till his organs are further unfolded: he then gradually begins to move and loosen his fetters: at this time it appears clearly, that the internal circular membrane is the amnion, in the liquor of which the tadpole floats; the umbilical chord at last is seen, and becomes still more perceptible, the first day after the animal has quitted his confinement. The cord is not, as in other animals, attached to the belly, but to the region of the head. A tadpole at the beginning of its evolution, is represented at N° II. fig. 2. and N° III. fig. 1. in which at E appear the little gills. At N° III. fig. 2. is a tadpole, magnified with its back turned towards the observer. In that at N° III. fig. 3. and those N° IV. is seen the vestige of the eyes, and the umbilical cord coming out from below the mouth.

xvii. The reader will probably be surprized at this description, since it appears, that the tadpole does not come out of the egg, but that the egg is transmuted into a tadpole; or, to speak more philosophically, that the egg is nothing but the tadpole wrapped up and concentrated, being evolved in consequence of fecundation, and assuming the lineaments of an animal. These phænomena were new and unexpected, for I was firmly persuaded, that the globules of two colours, surrounded by mucus, were real eggs; all who have written concerning the generation of frogs, as Jacobus, Valisneri, and Roefel, having so denominated them. But as greater deference was due to what nature shewed so plainly, than to the authority of the most celebrated writers, it is fit to call these globules tadpoles or fetuses instead of eggs; for it is improper to name any body an egg, which, however closely it may resemble one, takes the shape of an animal without leaving any shell, as is the case with all animals that come from an egg.

xviii. But it was of importance to examine these globules by experiment before fecundation, as also while they were yet in the uterus. Upon the most rigorous and exact comparison it appeared, not only that the spheres of mucus exactly resemble each other

in size and nature, and the two membranes with respect to their position, shape and colour, but that the unimpregnated globules are not by any means distinguishable from the impregnated ones. Upon separating the mucus and the two membranes, they appear to be alike spotted with white and black, which colours remain in some measure when the evolution of the tadpole is complete. The perfect similitude, as well external as internal, is very striking. If a globule be pierced with a needle, either before or after fecundation, a semifluid yellowish white substance oozes out: if the aperture be enlarged, the whole internal capacity appears to be filled with this substance, which loses its fluidity as the tadpole is evolved, every day acquiring greater consistence. If the globules before or a little after they come out of the uterus be put into spirit of wine or vinegar, or be boiled, and then stripped of their pellicles, we shall find so many indurated masses, which when cut through appear to be homogeneous. If we next proceed to examine the external parts, the pellicle before fecundation is thin and transparent, properties which it retains when fecundation has taken place. Afterwards, indeed, as the tadpole is evolved, it enlarges and thickens just as the skin of other animals grows

grows with their growth. As the pellicle is made to adhere to the inside of the unimpregnated globule by heat or vinegar, so it continues to adhere after fecundation; and it adheres more and more firmly as the globules lose their shape, and acquire that of the tadpole.

XIX. Hence the identity between the impregnated and unimpregnated globules is manifest. But the former are nothing but fetuses of the frog, therefore the latter must be so too, and consequently the fetus exists in this species before the male performs the office of fecundation. Hence we are led to other consequences of no less importance.

1. As these supposed eggs existed in the ovarium before their descent through the oviducts into the uterus, and long before fecundation (IV, VIII), the fetus existed in the mother's body long before fecundation. 2. Although the evolution of these fetuses is never so considerable and quick, as after fecundation, it is, however, perceptible before; for let it be considered, that the fetuses in the uterus are above sixty times larger than they were a year before, when they adhered to the ovarium, as I have found by comparing them. 3. Besides the fetus, the amnion and umbilical cord exist before fecundation.

C H A P. II.

GENERATION OF THE TREE-FROG.

XX. **I**N this and the following chapters, I shall be brief: I shall only make a few additions to Roefel's account, and notice some mistakes into which he has fallen, in order to compleat the history of the animals.

The tree-frog is of very small size. The back is of a very beautiful green colour. A viscid juice, that oozes out from its feet, enables it to fasten itself to the smoothest bodies. In summer, it fixes its abode upon trees, in the spring, it descends and repairs to stagnant water, in order to propagate the species.

XXI. Though the male mounts upon the back of the female, and embraces her very closely, he does not, like the green aquatic frog, throw his arms round her thorax, but fastens his hands under her axillæ. Hence the females escape those accidents that happen to the females of other species, which are often killed before parturition, by the violent compression of the male, as appears from the contusions and lacerations upon the breast.

XXII. This

XXII. This species continues coupled sometimes, according to Roefel, three whole days, though at others it is finished in one day. Those which I observed did not continue together above a few hours. This difference I attribute to the warmer temperature of our climate, which occasions more speedy parturition.

XXIII. The same naturalist observes, that during copulation, the female plunges at intervals into the water, and remains immersed some time: during which, the male approximates the extremity of his body to that of the body of the female, and that this motion is always performed with greater briskness, while the eggs are coming forth. He confesses that he could never, with all his attention, perceive the part that characterises the male, nor any liquor emitted for the fecundation of the eggs.

XXIV. Though I could never discover any protuberance or papilla, which I could suppose to be the penis, yet by taking the animals out of water (XII), I have been able to throw further light on this point. I observed, that about half an hour before any eggs are discharged, the ardour of the male became extreme. He would frequently stretch out the posterior part of his body, and then bring

it into contact with that of the female. He would then remove it, and soon apply it again. These motions became more frequent when the eggs began to be discharged: and continued as long as that process lasted.

xxv. In the mean time, I could not perceive any emission of semen; but upon lifting up the hind part of the male, I saw a transparent liquor ejected from the anus at intervals, which I readily concluded to be the semen. When I made use of the breeches (XIII), the eggs never produced tadpoles: and the breeches were wetted with semen as before (XIII).

xxvi. Are the eggs in this species also impregnated only after they are discharged? I observed a slight variation in the present case. Those which are nearest the vent, are sometimes impregnated before they are discharged. But the singularity of the phenomenon requires that I should be a little more explicit. Roefel justly observes, that in the female of this species, the eggs descend into the uterus before she is embraced by the male; if they are now taken out, and set in water, they all putrefy. This also happens if they are taken out after they are coupled, but before the female has begun to discharge any eggs. But when the discharge has commenced, if the
female

female be opened, and the eggs of the uterus, and those near the vent, are put into separate vessels of water, several of the latter will sometimes be found to be productive, whereas all the former will spoil. This is a clear proof, that the semen sometimes penetrates a little way into the body, whether it is thrown so far by the male, or rather because the eggs already fecundated, after their discharge, are sometimes retracted, when the female is unexpectedly seized by the observer, and ceases to expel any more.

xxvii. The eggs are small in proportion to the size of this species: one hemisphere is yellow and the other black: when they are taken out of the oviducts or uterus, they are enveloped in gluten, which circumstance has unaccountably escaped the observation of Roefel, who asserts, that the gluten is not visible till the eggs have lain twelve hours in water.

xxviii. Every egg is surrounded by a sphere of mucilage, as in No. I, II, III, of fig. 5, pl. I. The sphere and egg undergo the following changes in water. They increase in bulk, and at the same time the latter is elongated, and presses against a circular membrane, which lies within the mucus and soon pierces it; as the elongation increases, the

hole is enlarged, till at last the membrane is divided into two concave segments: within which appears a second circular membrane, of much finer consistence, and of course less apt to strike the eye. This membrane, when it is broken, emits a quantity of liquor, and is no other than the amnion of the tadpole, as afterwards more plainly appears. Meanwhile the two segments separate entirely, or at least cohere but at a few points, and part from the amnion without growing any more; but the amnion becomes thicker, and many times larger than it was at first; as also does the egg, on the growth of which depends that of the membrane. The gradual enlargement is as follows:

xxix. When it has become somewhat longer and bigger, one extremity thickens, and the other becomes finer. While the observer is intent on these changes, he is surprized with the motion of the egg; it sometimes turns round, now and then writhes itself, sometimes it brings together its two extremities, and then stretches itself and returns to its original position. To these unexpected appearances another equally unexpected succeeds: from the larger end there sprout two processes and suggest the idea of the protuberances, by means of which the tadpole

at-

attaches itself to bodies (xv); above these appear two tumours, which bring to the mind the idea of eyes; and eyes appear distinctly in a day or two, and it then becomes necessary to substitute the notion of a tadpole in the place of that of an egg. Let the reader cast his eye on No. iv. Fig. 5. and he will see a sphere of mucus, the inclosed membrane and the tadpole not yet evolved. No. i, ii, iii. Fig. 6. represents the membrane dividing into two segments, which when they are entirely parted, leave the tadpole No. iv. at liberty, which appears bent No. vi. Meanwhile the amnion is seen in the dotted circle 1. full of liquor, in which the tadpole floats. The two segments are still plainer at Fig. 7. where two amnions are seen distinctly, one of which is entirely separated from the segments.

xxx. But do these tadpoles exist before fecundation as in the foregoing species? To determine this question, I had only to make a rigorous comparison between the supposed eggs, after and before impregnation, and I analysed them in the same exact and careful manner as I had the others (xviii), and found the most perfect resemblance between them; whence it was necessary to conclude, that the fetus

fetus exists in the female of this species also before impregnation.

xxxI. These fetuses are more and more evolved, though still inclosed in the amnion, where they continue longer than those of the green aquatic frog; the time is commonly six or seven days; then the rudiments of the gills begin to shoot. At first they swim slowly, but as their limbs acquire greater strength and vigour, their motions become brisker. Fig. 8. represents two tadpoles at the time of quitting the amnion, magnified by the microscope: their gills are seen at A A.

xxxII. Upon comparing these observations with those of Roefel, I cannot but dissent from him. The sum of what he says on the generation of the tadpoles of the tree-frog is as follows.

xxxIII. At first the impregnated eggs grow in bulk only; then the tadpoles, which now seem to consist chiefly of belly, though the head and tail may be clearly discerned, appear distinctly. Every tadpole floats in the white of the egg (the mucilage that surrounds it xxviii). The belly is formed by the yolk, so the author calls that which is generally denominated the egg. At the tail appears a vesicle, and afterwards another (from his figures these seem to be the two seg-

seg-

segments xxviii); these vesicles then separate, and one remains at the head and the other at the tail. Roefel conjectures, that the tadpole being now endowed with motion and life, takes its nourishment from the vesicle at its head, but he confesses that he does not know the use of the other.

xxxiv. Though it is true, that the tadpole has at first a large belly, it is false, or at least a very improper expression to say, that the belly is chiefly formed of the yolk, since the yolk is the animal itself (xxix); it would not be less absurd to say, that the belly of an animal consists of the animal itself.

xxxv. His description of the appearance of the two vesicles, shews that he did not make his observations with sufficient attention: he probably observed at intervals; otherwise he must have perceived that they were originally a sphere, which is divided into two segments by the efforts of the tadpole; consequently they did not appear one after the other, but both existed at the same time (xxviii).

xxxvi. If he had taken them up with a forceps, he would have found, that they cannot be supposed to nourish the tadpole, for as soon as they appear, which is when the
sphere

sphere is divided, they may be removed without injury to the young animal.

xxxvii. The part really necessary to this animal, and of which the removal is fatal, is the amnion, within the liquor of which it at first floats (xxviii, xxxi). As Roefel does not mention the amnion, it must have been unknown to him; and although this membrane is found in every species of frog and toad, not the least mention is made of it in his history; this circumstance the more surprized me, since it did not escape the diligence of Swammerdam, in whose footsteps the German naturalist professes to tread.

xxxviii. He has, however, remarked an accident, which happens to tadpoles that die in the amnion, or as he expresses it to eggs, that are not prolific: they become sometimes pointed, sometimes pear-shaped, sometimes oblong, and at others narrow in the middle, and so on. Four such tadpoles are represented in Fig. 5. at A B C D.

xxxix. The tadpoles of this species require great attention. They must be kept, not merely in clean water, but in that which is taken from the places where the males and females meet, as from ditches
and

and puddles (xx). This precaution was not absolutely necessary for other frogs and toads; spring water being sufficient in these cases.

CHAP.

C H A P. III.

GENERATION OF THE TOAD, DENOMINATED BY ROESEL, BUFO TERRESTRIS DORSO TUBERCULIS EXASPERATO, OCVLIS RUBRIS.

XL. **T**HOUGH Roefel mentions but one species of this ugly and disgusting animal, I incline to believe, that in this country there are two; this opinion is founded on a difference in the external configuration; not to mention a difference in colour, some having their backs and sides of a light cineritious hue, with tubercles of a tan colour, and others of a pale green, with tubercles of a dark red. But notwithstanding this difference, I shall speak of them as one species, for in what relates to generation, there is the most perfect similitude.

XL I. Of the whole class of frogs and toads with which I am acquainted, none begin their amours so soon as the species in question: they are found coupled in the beginning of March, and sometimes in February, before all the snow is melted, and while the
water

water in many places is yet covered with ice. The male being about five times smaller than the female, cannot nearly bring the toes of his fore feet to meet over the breast, a circumstance before noticed in the green water-frog (VI). He, however, holds her so fast, that she can by no means disengage herself from his embraces. The females have often got out of the vessels in which I kept them, and escaped to a considerable distance, but the males still kept their situation.

XLII. The eggs, as well as in toads as frogs, lie at first in the great sacs of the ovaria, whence they get first into the oviducts, and then into the uterus. In which of these three situations are they in the female toad with red eyes, and dorsal tubercles, at the beginning of her amours? I have found that they are indeed in the ovaria, but ready to pass into the oviducts. Copulation lasts till they are discharged, which happens in ten, twelve, or fourteen, and sometimes twenty days, if the season be cold. Such is the ardour of the males, that after the discharge is finished, and they have quitted the female, they will return to her again, and embrace her for several hours. This, however, is not peculiar to toads. I have often seen it happen in frogs.

XLIII. The male, during the whole time, makes a kind of grunting noise, which I never heard except at the season of his amours. This noise becomes louder, whenever attempts are made to pull him away, or when any other toad comes near; which he moreover observes with a jealous eye, and endeavours to drive away by throwing out his hind legs.

XLIV. If the belly of the female be attentively observed after several days of copulation, it will be seen to move in a very extraordinary manner: it sometimes dilates towards the breast, which dilatation passes gradually downwards; at others the dilatation begins at the bottom of the belly, and proceeds upwards. During these agitations, the female, now about to bring forth, evidently suffers considerable pain, probably on account of exclusion of the eggs from the ovaria, and their passage through the windings of the oviducts, and into the large cavity of the uterus; for the two last mentioned places will be found to contain eggs, if the female be opened during these alternate swellings.

XLV. In frogs the eggs are soon discharged; but in toads this is a very tedious process. Two cords, consisting of a viscid transparent matter, and containing a number of black globules,

globules, which are the eggs, issue from the vent, and are excluded so slowly, that the eye cannot distinguish any movement in them; but in a few hours they measure several feet.

These cords, consisting of eggs and gluten, certainly receive their form in the oviducts; as in a press; which may be seen upon opening the female, as soon as they have advanced a little way into those canals. The time requisite for the discharge is quite uncertain; the longest I have observed was finished in thirty, and the shortest in nine hours; it lasts generally more than twelve. A piece of these cords is represented at Fig. 9. Plate II.—Fig. 10. shews a smaller portion magnified.

XLVI. Two entire cords measured forty-three Paris feet; the number of eggs amounted to one thousand two hundred and seven: hence we cannot be surprized that the female, after the exclusion of the eggs, should have become so much smaller.

XLVII. Since the ardour of the males is so great, that they do not abandon the females, even when they make their escape (xli), it was natural to suppose, that they would also continue their embraces when I removed them from the stagnant or slowly running waters, where they assemble towards the end of spring, in order to propagate the species (xli), to a

dry and confined place, where I could watch the manner in which fecundation takes place. I shall relate what I observed during many long and tedious inspections.

XLVIII. As soon as the female begins to discharge the mucus cords, the male, that before lay close and collected, spreads himself, and stretches his hind legs backwards towards those of the female, still holding her breast close with his fore legs. This change of posture enables him to reach the cords with the extremity of his body, and to bedew them with semen.

XLIX. And this is not done by a forcible emission, but by simple contact; for wherever the cord is touched, it appears just moistened, as if with a pencil dipped in some fluid.

L. The male, after having repeated this operation many times, with various contortions, becomes weary, collects his body, and draws his hind legs close to his belly. Meanwhile a fresh portion of cord is discharged, and then the male resumes his employment, and afterwards again returns to his repose. And thus he goes on, till all the eggs are impregnated.

LI. That the act of fecundation consists in this approximation of the extremity of the
body

body to the eggs, and bedewing them, appears from the following proofs: First, if the female be killed while she is discharging the eggs, of those which lie in the uterus and oviducts, not one will be prolific, though they be kept in proper water; whereas almost all the others that have been moistened with the feminal fluid, at their exclusion from the vent, will produce. Secondly, If during the discharge the irroration should be interrupted, either by means of breeches, (XIII). or by the near approach of the observer, (for then the male through fear will bring his hind legs and thighs close to his body) the portion of eggs then excluded will be barren, while the rest will be found to have been fecundated.

LII. These last facts clearly evince, that in these animals likewise fecundation takes place without the body of the female.

LIII. But may not the bodies, which have hitherto been called eggs, be tadpoles not yet unfolded? and if so, do they exist before impregnation, as in frogs (Chap. I. and II.)? These problems were yet to be solved, and required a short analysis of the eggs.

LIV. They are black, and, to the naked eye, and even when a weak magnifier is used, appear to be globular (Fig. 9, 10. Plate II).

when examined with a glass of greater power, they seem to be marked with four furrows, which intersect each other at right angles, nearly like the husk of a chestnut half opened, (Fig. 11.): though the furrows are not bare, but covered with a very fine transparent membrane, which passes very tight round the rest of the egg. When this is taken away, we come to the skin of the egg; it is black, and is lacerated by the slightest touch; as is also the inside, which is almost fluid, of a brownish white colour, and apparently homogeneous; the particles appear globular, when observed by the microscope.

Lv. Such is the form and composition, as far as the eye can discover it, both of the eggs that have been discharged and fecundated, and of those that are taken from the uterus and ovaria; excepting, that the latter are destitute of the mucous gluten, smaller, and not so black.

Lvi. Wherefore, if we rely upon the information of the senses, we must conclude, that there is no essential difference between the impregnated and unimpregnated eggs.

Lvii. If we observe the changes produced in the former, as we have before done in frogs, we shall find that they are immature fetuses, and exist before impregnation. They
soon

soon assume the form of those little animals; first becoming thicker, then longer, and afterwards growing thin at one extremity, viz. the tail, and becoming round at the other; from which spring two appendixes, viz. the gills; and in these the circulation of the blood is very conspicuous, as well as at the edge of the body. At Fig. 14. may be seen several of those little bodies, losing the shape of an egg, and assuming that of the tadpole; they are further advanced at Fig. 15. Fig. 12. shews some of these corpuscles, losing indeed the form of the egg, but not assuming that of the tadpole, and spoiling; this appears more at large at Fig. 13: these were never subjected to the influence of the fecundating fluid.

LVIII. While the tadpole is thus evolved, the thin membrane, of which we spoke at LIV. is enlarged in proportion; this membrane is the amnion, and contains a liquor, in which the fetus is seen to float; it may be found in the uterus (LIV, LV). and therefore exists before impregnation.

LIX. Though the blood circulates in the fetus (LVII). and the heart must therefore beat, yet not the smallest motion can be excited, either by puncture, or any other

mode of irritation; it is not till the organs have acquired some consistence, that the animal begins to move; in a few days after this period it bursts the amnion, and swims about.

CHAP.

C H A P. IV.

GENERATION OF THE FETID TERRESTRIAL TOAD.

LX. **T**HIS species, which does not much exceed in size the green aquatic frog, (III). agrees in some particulars with that denominated by Roefel, *Bufo terrestris foetidus*, but differs in so many others, that it must be reckoned a species totally distinct. They are both nearly of the same size, and when irritated emit an offensive smell; they moreover agree in colour, (the belly being of a white, inclining to a green in both, and the back to yellow); if we except a list of a golden yellow colour, which, in the species of Roefel, runs along the spine, but in mine does not appear. But the essential difference consists in the absence of the web that connects the toes of so many other amphibious and aquatic animals besides frogs, from the species described by the German naturalist; in that of which I am about to treat, it is very visible, and of very great service in swimming. The second difference lies in the

progressive motion upon dry ground, those of Roefel advancing without rising from the ground; *muris instar* is his phrase; whereas mine leap like frogs. Lastly, a difference of species may be deduced from the difference of the voice, which, in the former, is exactly like that of the green tree-frog, and consists of a loud screaming, but in the latter is a kind of continued whistle.

LXI. The female, in the two other species of terrestrial toad, is five times as big as the male (XLI). But in this species the male is scarce one third smaller than the female, when she is about to exclude the eggs; afterwards they are nearly of the same size. They also agree in colour; but the upper part of the body of the female is adorned with cineritious spots and lists, from which arise some very beautiful red tubercules.

LXII. Toads of this species live on dry ground all the year, except during the season of copulation; at which time they repair to the water of pools, ponds, puddles, ditches; never to rivers, torrents, or quick currents.

LXIII. They repair to the water when the spring is pretty far advanced, in May, or at the beginning of June. Their amours last above a month; not that the male embraces the female so much longer than in the other species,

species, the contrary is rather true; but, during all this time, pairs may be found coupled; some beginning earlier, and others later, according as they are instigated by their internal feelings. The same thing happens in the two other species of toad, the aquatic and tree-frogs, fishes, and all animals that have a fixed season for their amours.

LXIV. By comparing paragraph LXIII. with XLI. the reader will perceive the great difference of time at which the two other species of toad, and that which is the subject of this chapter, begin to propagate; whence we may infer, that they are naturally different. The former begin early in March, as soon as the rigour of winter is a little abated; whereas the latter requires a warm temperature. If the weather suddenly changes, the foetid terrestrial toad abandons the business of generation, and retreats to dry ground. I was eye-witness of a singular fact which may be related on this occasion. At the end of May, 1777, I perceived upon the surface of a large pool great numbers of this species, some of which had discharged their eggs; others were discharging them, and others were only just coupled; all were in motion, and so intent upon their employment, that they suffered themselves to be taken, without attempting

tempting to escape. The males, like the green aquatic frogs, clasped the breast of the females very tight with their fore feet, and made a low grunting noise, not unlike that of the two other species (XLIII). The sky was clear, and in the shade the mercury stood at 16° (*a*), when an unexpected rain fell for two days, and cooling the air, dispersed all the toads, so that when I came on the second day of the rain, to catch some for my experiments, to my surprize I could not find one. I soon, however, was aware that they had quitted the water, to conceal themselves in the crevices of a neighbouring wall, where they continued till the atmosphere becoming warmer, invited them again to the pool.

LXV. It is the opinion of many naturalists, that the male toad embraces the female for forty days; and some think that this is the case also with the frog (*b*). Having observed that, in like circumstances, the duration of copulation is inversely as the heat of the atmosphere, I can easily believe that this may be the case in cold countries. In temperate climates, as with us, it is far otherwise. The tree-frog embraces the female a few hours

(*a*) Sixty-eight of Fahrenheit.

(*b*) Swammerdam Bibl. Naturæ,

(xxii), the aquatic about a week (vi); the two former species of toad longer, on account of the early commencement of their amours. It will therefore easily be believed that, for a contrary reason, the embraces of the species in question do not last long, four or five days for instance.

Nothing can be more falacious than the male. He not only adheres most tenaciously to the female, but does not even quit her for some time after the exclusion of the eggs. If he is separated by force, he returns to his situation immediately, though kept in a narrow vessel, and under the eye of the observer.

LXVI. About the season at which these animals meet, I procured a large quantity, and set them in vessels almost full of water, a male and female in each vessel. In some I put only a female. They were soon coupled, some sooner and others later. In two females that were opened twelve hours after the male had begun to embrace them, the eggs had not yet quitted the ovaria; in one that had been coupled two days and a half, the same phenomenon occurred. In another, that was opened at the end of the third day, some eggs had entered the oviducts, and those nearest the uterus were surrounded by mucus. Before

fore the conclusion of the fourth day, the eggs of two other females had all descended into the uterus, and were completely surrounded by gluten. The ovaria now contained only small and immature eggs. These observations were made upon females embraced by the male; though the solitary ones were kept for a fortnight, the eggs continued in the ovaria.

LXVII. While I was engaged in these experiments, a singular phenomenon occurred, which must not be passed over in silence. Of two females that were opened on the fourth day of copulation, I found the eggs of one in the abdomen, and of the other partly in the abdomen, and partly in the thorax. This accident may easily be explained. The eggs were without gluten, and therefore could not have passed through the long windings of the oviducts, and had either never been taken into them, or the ovaria perhaps burst, and hence they would fall both into the superior and inferior cavity. Such a rupture may easily be conceived to happen, if we consider the pressure made by the male against the region of the breast opposite the ovaria.

LXVIII. I have already said, that the eggs about to be excluded having passed through the canal, provided by Nature for that purpose,

poſe, were found in the uterus on the fourth day (LXVI). In the greater part of thoſe that were confined in my veſſels, they began to make their appearance at the vent on the 5th day in the form of cords, as in the other ſpecies (XLV). Theſe cords continue to be very ſlowly expelled till the uterus is empty, which always happens before the ſeventh day. Meanwhile the male brought the extremity of his body towards them, from which iſſued a ſmall jet of liquor; this was the ſemen, as evidently appeared from thoſe portions of cord that were ſprinkled with it bringing forth tadpoles, whereas the others ſpoil and turn putrid. In like manner the whole length of the cords produced nothing when the male was clothed with breeches (XIII), or parted from the female as ſoon as the eggs began to be excluded.

LXIX. As the eggs that were not moiſtened with the ſeminal fluid after they were diſcharged, were unproductive (LXVIII), I may confidently infer, that fecundation takes place without the body of the female. I have moreover frequently taken pieces of cord out of the uterus, and placed them in water, but never did I ſee one egg productive; this obſervation is likewise applicable to the eggs that were deſcending through the long wind-
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ings of the oviducts. Hence might be inferred the physical impossibility of the eggs which were shed in the cavities of the thorax and abdomen being prolific (LXVII), as experiment decisively proved.

LXX. The cords, upon examination, appear to consist of an almost innumerable series of eggs, each at a small distance from the other: they are connected by that gluten, which always encloses the fetuses of the frog and toad, and which is prepared in the cavity of the oviducts, for in them the fetuses, before naked, are invested with it. The great resemblance between the phænomena exhibited by the tadpoles that I am about to describe, and by those already described (Chap. III), renders a reference to figures unnecessary.

I was curious to know, whether the gluten is necessary to the growth of the egg, I therefore removed it entirely from some, and from others only in part, without injuring the amnion, which in this as well as in other species of frogs and toads, is very conspicuous. Those which were quite stripped of their gluten, never brought forth young, but the others, to which a portion was still left adhering, were prolific. When the amnion began to spoil, it was a certain sign, that the egg, though yet found, would spoil.

LXXI. In this species also, the little bodies, which we have hitherto called eggs, are tadpoles in miniature, as the following proofs may serve to shew. They are not broken like other eggs, to allow the inclosed animal to pass out, while they themselves become an empty and useles shell; they undergo no other change than an increase of bulk, becoming slender at one extremity, at which the tadpole soon appears, and round at the other, which the eyes, the mouth and gills soon shew to be the head; at length life appears in these fetuses, they break through the amnion and swim about the water; just as the fetuses of the other frogs and toads, which likewise have at first the shape of eggs. The advanced season causes the tadpoles of this species to exhibit their different *phases* in less than three days, whereas others being brought forth at an earlier period, require a longer space of time.

LXXII. During the evolution, I analysed these corpuscles with the utmost care, and compared them both internally and externally with others in the uterus and oviducts, but could perceive no difference except in size. From this identity then it may be concluded, that as these corpuscles are real tadpoles when they are without the body of the female, they are
are

are so also within it, and by consequence, that the fetus exists in the female before the concurrence of the male.

LXXIII. This is also true of the amnion, for I have found that membrane surrounding the fetus, when in the uterus; it is then, indeed, in contact with the fetus, and does not contain any liquid; when they are excluded, it is at some distance, and full of its proper fluid. In like manner there is reason to believe, that the umbilical cord exists before fecundation. This organ is indeed not visible, when the tadpole floats in the liquor of the amnion, yet it probably exists, since it is attached by one end to the lower part of the head of the animal, and by the other to the amnion, from which it afterwards separates, and adheres a little while longer to the tadpole; it then decays by little and little, and is at last quite obliterated.

LXXIV. I said, that in order to rear these animals, I immediately put them into water, imitating in this the example of the female. I added, that except for the tree-frog, well water would suffice, that species requiring it to be taken from certain ponds or ditches (xxxix). To these precautions, I must subjoin two others, for the sake of those who may be desirous of repeating my experiments.

First,

First, The vessels must not be kept in too warm a place, cold being less hurtful in these experiments than heat: the two first species are produced before the close of winter, and the water of the vessels in which I kept them has been frozen to a considerable depth, without doing any hurt to the tadpoles. I twice increased the cold to such a degree, that almost all the water was frozen, but the tadpoles never suffered, except when the mucus itself, in which they were enveloped, was frozen; which never happens, but in a degree of cold far exceeding the point of congelation. On the contrary, whenever I left them several hours exposed to the sun, when the thermometer stood at 35° , the greater part was sure to perish.

The next precaution, not less important than the preceding, consists in often changing the water of the vessels; if this be neglected, evolution will not begin, or if begun will stop; this happens in consequence of the gluten becoming putrid, from which a penetrating miasma insinuates itself into the tender and delicate fetus, and destroys it either before it shews any signs of life, or after it has given tokens of animation.

In order to observe exactly the amours of confined frogs and toads, it is necessary to

place something to obstruct the view of these falacious animals, between the vessel and the observer, otherwise they are disturbed at the sight of a person standing over them, and either do not begin to celebrate their nuptials, or leave them half-finished.

Nice and fastidious persons may perhaps be disgusted at the frequent mention of an animal so loathsome as the toad. They would doubtless have been better pleased with remarks on such creatures as are more familiar, and may be handled without abhorrence. But whoever purposes to explore Nature, conceives not disgust or predilection for any of her productions, but examines all indifferently, well knowing, that the ideas of what is *disagreeable, nauseous, &c.* are not in Nature but the imagination; for every creature, how vile and horrible soever it may appear, has its proper beauty and grandeur, and is the proper object of admiration, since it is the work of the supreme Architect. This did not escape the sage Petrarch, who sings

Tutte le cose di che 'l Mondo è adorno
Uscir buone di man del Mastro Eterno.

LXXV. I will close the chapter, by noticing a fact mentioned by the illustrious Bonnet in his *Corps organisés*, and connected with the present subject. He tells us, that he

he knew a pool, which, after remaining dry, for some time, was stored with the same kind of fish that it contained before, nor could he discover how they came into it. Instead of having recourse to fishes accidentally dropped by cranes for the explanation of the phænomenon, he conjectures, that the supply came from fecundated eggs that had lain in the mud without being spoiled. The instance of a species of tufted polype, discovered by the immortal Trembley, of which the eggs may be hatched by putting them into water, after they have been kept four months in the dry, affords a remarkable foundation for his conjecture; which he further confirms by the famous worms of ricketty corn; these, it is well known, after having been kept dry for many years, will move again as well as at first, if they are barely wetted. Desirous, however, on this as well as on all other occasions, of ascertaining the truth, the philosopher of Geneva wished that his conjecture might be subjected to the test of experiment, and that it might be tried, whether the eggs of fishes can be hatched after having been kept some time in the dry.

As soon as I had read the work, I made the experiment, but without success. Of the eggs of three species, which I kept in the

dry for a month, not one brought forth young; indeed this appeared to be impossible, for most of the eggs were deformed and spoiled by desiccation. Several years afterwards, being engaged in experiments relative to the generation of toads and frogs, I put tadpoles recently excluded to the same trial, and with the same ill success. Hence it appears, that in order to explain the phenomenon related by Mr. Bonnet, we cannot have recourse to the supposition, that eggs may be long kept in the dry. And I am still more inclined to this opinion, from having observed, that the eggs of fishes and tadpoles, newly laid in a shallow place, perish irretrievably if they continue in the dry for a few days, notwithstanding they are afterwards put in water. I say for *a few days*, for tadpoles do not die if they continue but a little while out of water. I put some tadpoles just excluded into a dry glass; for two or three days the evolution went on, they began to assume their true shape, and even to shew signs of life, but these appearances diminished gradually, and at last they shrivelled up and became disfigured amid the mucus, which was itself turned into a kind of hard glue.

LXXVI. But how then are we to explain the appearance of fishes where there were none

none once, and where we know that fresh ones have not been brought? The purport of this dissertation does not, as is evident, lead me to the solution of this phenomenon, which was only mentioned incidentally. And if I was obliged to treat of it, it would rather be to suggest doubts concerning its reality, than account for it. We know not how any fishes have got into places that were once dry, and are now covered with water, but is it therefore certain that they did not get there? Are we sure that nobody put them there? Is it clear that they themselves have not found some subterraneous way, or unperceived rill? Must we not of necessity draw some such conclusion, since it is certain, on one hand, that the dried eggs of fishes are steril, and on the other, that fishes come only from eggs.

C H A P. V.

GENERATION OF THE WATER-NEWT, OR
SALAMANDER.

LXXVII. **M**Y observations were made upon two species of newt. One has the lower part of the body tinged with a very beautiful orange colour, interspersed with black spots of an irregular shape; the sides and back have a ferruginous ground with grey streaks; the length of this species is four inches and upwards, and the thickness about six lines. The other species, besides being shorter and less bulky in proportion, is distinguished from the former by a list of a golden yellow, which bounds the ridge of the tail, and in females passes upwards along the spine, and reaches as far as the head. In both these species the males differ from the females, not indeed in the external parts of generation, which are alike in each sex, but in being provided with a dentated, membranous prominence, running longitudinally along the middle of the back; as also in a double silver-coloured band, which adorns the

the tail of the male, but is not perceptible in that of the female. These two sorts of newts are the same which, in researches of another nature, were the subjects of those beautiful experiments on the reproduction of the limbs, the circulation of the blood, and other interesting topics, discussed in my other works already published.

LXXVIII. During spring and summer these little quadrupeds live in stagnant, never in running waters; at the approach of winter they conceal themselves under ground, within the clefts of stones, and even in vaults; many of them, however, abide in the water during the whole winter, and more especially in such as arise from subterraneous springs, and preserves a sensible degree of warmth in the keenest colds. Hence it appears, that our newts, though in general natives of water, and therefore denominated *aquatic* by me, are, however, capable of living, and in reality do live upon dry ground; and for this reason they may also be justly termed *amphibious*.

LXXIX. The comparative state of the season at which they disappear from the waters (either descending to the bottom, or else transporting themselves to dry ground, LXXVIII) in autumn, and of that at which

they re-appear on the approach of spring, is worthy of notice. Before the middle of October, while in our climate the thermometer in the shade has scarce fallen so low as temperate, they all or almost all retire out of sight, and are lost; whereas, on the other hand, they begin to make their appearance, either by rising to the surface of the water, or swimming in it towards the middle of February or thereabouts, although at this time it continues to freeze almost always during the night, and in the day-time the thermometer in the shade is several degrees below temperate. Now whence arises this early disposition in newts to quit their retreats, while the winter is not yet over, whereas in autumn they go in quest and take possession of them, notwithstanding the season long continues mild? Does it spring from the stimulus of hunger produced by their long fast during the winter months? Or may it more justly be imputed to another principle not less active than hunger, viz. that by which the individuals of different sexes are determined to seek each other for the purpose of propagating the species? It is at least certain, that in spite of the coldness of the season, which, however, prevents them from shewing such vivacity, such fire and alertness, as they afterwards display, the males begin

begin to chase the females, to surround and to caress them after their manner, a circumstance which is never observed except at the time of their amours.

LXXX. These appearances in February are, however, only preludes to their future nuptials. These are properly celebrated in March, and till about the middle of April; this however depends, as we have already seen in frogs and toads, on the greater or less warmth of the season, which accelerates or retards their amorous encounters. These encounters are conducted in this manner: the male pursues the female, which at first makes a shew of flying, but soon stops of her own accord. He then approaches her in such a manner, that the lower part of his head comes in contact with the upper part of the head of the female; and this is done while the animals are in such a posture, that their bodies form an angle, of which the point is made by the union of the two heads. Sometimes, however, the position varies; some males, instead of placing their head on that of the female, only join muzzle to muzzle; the bodies of the male and female are always near together, so that the angle made by the two heads or muzzles, is very acute. Then the male erects that dentated membranous prominence which
lies

lies on his back (LXXVII), agitates it in an odd manner, and moves it from left to right, nearly as a mettlesome stallion waves his mane; he also moves his tail briskly, bends it in a tortuous manner, and as he waves it about, strikes very gently the sides of the female, which continues without motion. Sometimes the male, in order to preserve his position, and at the same time to lash the female with his tail, grasps with the toes of his forefeet the grass at the bottom of the ditch, or small twigs, or whatever else will serve for a support, remaining always immersed in water along with his companion; and while he thus gently lashes the female with his tail, he emits from the aperture of the anus, now unusually tumid and dilated, a copious jet of semen, which mixes with the water, and thus diluted, arrives at the vent of the female, which likewise, on this occasion, appears more enlarged as well as wider than usual. In this important operation then, the anus of the male is never in contact with that of the female, which also always maintains a greater or smaller distance, and never shews any part at all characteristic of her sex. After the male has ejected this jet of semen, he rests for a while, on some occasions quitting the female; he then returns to his employment, and

and repeats the contortions of the tail, and the emission of semen. I have seen these alternations continue more than an hour; and when they are engaged in this employment, they may be approached and taken with the hand, almost without being aware of what happens to them. And sometimes the male, from his agitation and contortions between the fingers, throws out a small portion of semen, which is of a very white colour, and in consistence resembles thick milk.

LXXXI. I first observed these curious facts in places where the newts naturally assemble and sport together, viz. in ponds, ditches, puddles, &c. afterwards I observed over again the same things at home in vessels full of water, more commodiously and with equal success, exactly in the same manner as I had observed the generation of frogs and toads. I found that this singular mode of impregnation takes place in both the species of newt which I have described (LXXVIII). Upon the whole it appears then, that copulation is not necessary to the fecundation of these animals, and I can assert with confidence, that among the many thousands which I have observed at the time of their amours, I have never seen a single pair in conjunction, not only for the purpose

purpose of generation, but on any other occasion.

Being well assured, that with respect to exceptions from certain rules supposed to be general, diffidence and caution can never be carried too far, I was very desirous of removing all suspicion. I have been several times a spectator of the copulation of *wall-lizards*, which happens in April and May, and may be said to last a single instant. In the clearest days, and in places most exposed to the sun, the male runs after the female, and when he has overtaken her, he twines himself about her, and unites his genitals to her's, but this union may be termed momentaneous, after which the two animals part. Do newts also, which on account of some resemblance, are likewise called water-lizards, during their amours as described above (LXXX), do they also, I say, copulate for a single instant? Although I kept my eyes fixed on them, so that the most trifling action or smallest motion could not escape me, I am quite certain of having never seen them in copulation for a single moment throughout the whole period of their amours. I may even assert, that the genitals of the male were always at the distance of several lines from those of the female; and I have been still more and more
con-

convinced, that no copulation of any sort takes place in this kind of animals, but that fecundation is effected by that portion of semen which the male darts into the water, and which afterwards is received into the body of the female, at the aperture of the anus.

LXXXII. I made these observations in 1766 and 1767, and gave some intimation of them in my *Prospectus concerning animal re-productions*, published in 1768: my expressions are the following: “ Naturalists are not certain whether newts copulate like the generality of animals, or in the manner of frogs and toads. As such an enquiry is of importance to the history of generation, I have paid great attention to it, and in my work shall relate all that I have observed (a).”

I was persuaded that these observations were new, for in truth I was not apprized that any person had published any thing upon the subject. And I should still have remained under this persuasion, if I had not chanced to read in Bomare’s *Dictionary of Natural History*, the article *Aquatic Newt*, in the volume printed in 1775, that is to say, seven years after the publication of my Pro-

(a) Page 77.

spectus, in which I found that Mr. Demours had turned his attention to the same point: I was not able to ascertain whether his observations are prior or posterior to my own, for want of knowing whether they are to be found in a separate publication, or were simply communicated by the author to Bomare. However it may be, I acknowledge with the most unaffected ingenuousness, that the painful feelings, which usually arise on perceiving that another, without our knowledge, has begun to reap in our own field, have been amply compensated by the pleasure of finding that the observations of the French Naturalist perfectly agree with mine upon the whole, a strong presumption that the observers have been exact. The sum of his observations is, that after various gallantries on the part of the male, such as stopping the female, bending his body in the form of an arch in the water, and standing upright with his crest or prominence erected, he opens his anus, forcibly compresses the region of the testicles, lashes his companion with his tail, and now squeezing his testicles with his utmost force, darts forth the seminal liquor, without however being in contact with the female. The liquor spreads along her sides, after slightly clouding the water: Then the male
drops

drops down as it were asleep, but soon awakes, and re-assumes his former gallantries, which are succeeded by a fresh emission of semen, after which the two animals separate (a).

These observations, as far as they prove that no copulation takes place between newts, are confirmations of mine, whatever opinion is to be entertained concerning any slight difference in the accounts of the amorous gestulations, as well as concerning the few emissions of semen observed by him, and the numerous ones remarked by me (LXXX): for with respect to the former, they are circumstances totally foreign to the main-object of attention, and as to the more or less frequent emissions of semen, these it is evident might depend on the difference of temperament, age, and vigour of the animals.

LXXXIII. In the LXXXIst paragraph I have supposed that impregnation was effected by those jets of semen which the male emits in the vicinity of the female. Let me now adduce the proof of this supposition; but that it may be better understood, it will be expedient to premise something concerning the ovaria, oviducts, and ova of these animals. When the abdomen of a female newt is laid

(a) Page 38, 39.

open at any season of the year, the two ovaria present themselves, containing a multitude of little eggs of a yellowish white colour, smaller than husked millet, and not floating loose in the cavity of the ovaria, but adhering to their sides. The little ova, at the approach of spring, increase insensibly in size, and when they are arrived at maturity, (which happens at the time of their amours) they descend into the oviducts, which consist of two white tubes, which extend from the origin of the fore-feet to the root of the tail, and include the spine between them. Both are wrinkled and full of curvatures and flexions, when they are freed from these, and stretched in a right line, they exceed about four times the whole length of the animal.

LXXXIV. During the season of amours, the oviducts always contain a greater or smaller quantity of eggs, placed in rows, and generally more numerous in that part of the duct which lies nearest the anus. And now it is, that on slight pressure on the belly, or even from the contortions of the animal, when they are held between the fingers, the eggs commonly come out from the cloaca, just as we have before observed, that the semen of the male is emitted under like circumstances (LXXX). The ova, when they quit the ovaria
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and enter the oviducts, become far larger, and they now are invested with a viscid gluten, nearly as in frogs and toads, except that the gluten which surrounds the eggs in the two latter kinds of animals is more abundant and more viscid. When the male has ejected the semen, and when from its vicinity to the female, it has insinuated itself into her anus, that portion of eggs is fecundated, which is nearest the vent, and about to be excluded, and the remainder which lies in the higher part of the oviduct, continues at that time unimpregnated, as I found from the following experiment. When the males began to pursue the females with eagerness, I put the latter by themselves in vessels full of water. They, notwithstanding, discharged their ova, but they remained barren. I allowed the males to approach, and they darted their semen as usual; I then again separated the females, when I found that the first eggs they discharged (amounting to six, seven, or thereabouts) furnished me after some interval with living newts. But this was not the case with others lying higher up in the oviducts, which were brought forth afterwards.

After the amorous encounters were over, I opened a female, and taking the eggs out of the ducts, I put them into water, with

the precaution, however, of placing those which lay in the vicinity of the anus in a separate vessel. These produced young, while the rest all failed. To these proofs, which in my opinion are decisive, let me add another. The species of newt, with the golden list upon its back (LXXVII), instead of bringing forth the ova separate like the other species produces little cords, sometimes of the length of two inches, consisting of several dozens of eggs. After the male had carested one of these females, I opened the abdomen, and took the eggs out of the ducts, which were united together by means of the glue, and formed two cords joining at an angle near the cloaca. I placed these cords in water, and marked the end which would have appeared first with threads. And the eggs at and near this end produced young, while the others came to nothing.

LXXXV. Though I have proved that these eggs are not fecundated, yet it does not follow they do not become so in the sequel. In proportion as those which lie nearest the anus are expelled, the more remote descend and take the place of the former, and are impregnated by new emissions of semen. The same thing happens to the most distant, so that all, that arrive at maturity, pass out of the ovaria,
and

and enter the cavity of the oviducts, are sooner or later fecundated. I had an irrefragable proof of this, by attending to successive discharges of ova, which all produced young alike. From what has been already advanced, it is very obvious to infer, that the males do not soon abandon the females. From observation I have learned, that their amours last sometimes twenty, sometimes thirty days, and sometimes still longer, viz. until the females have discharged all their mature eggs. For as long as any remain in the oviducts, the males never cease their careffes, and alternate emissions of semen, and intervals of rest (LXXX). I said above *all their mature eggs*, since after the amours of the newts are ended, there remain within the ovaria others of very small size, and in very great abundance.

LXXXVI. Bomare, in the article already quoted (LXXXII), asserts that aquatic newts disburthen themselves of the load of their eggs, by laying hold of them with their mouths and feet, and thus promoting the discharge from the anus, but that as they are expelled they are glued under the tail. The French writer will I hope pardon me, if I venture to assert that no fable can be more extravagant than this account. If instead of being a copyist by profession, as he appears to be from

all his publications, he had taken the trouble to observe Nature, and in consequence of this had enquired into the manners of newts, he would have seen upon a thousand occasions, that the eggs are discharged and separated from the anus without the concurrence of the mouth or feet, and that these animals never shew the least disposition to employ either the one or the other. He would have besides felt with his hands, that far from following the practice of cray-fish, which after having brought forth their ova, keep them attached to the inferior surface of the tail, that these animals expel them in such a manner, that they all fall to the bottom of the water. Of the infinite number of newts which I have kept in different years, and for different purposes, in vessels full of water, I have never found one individual that carried a single egg adhering to the tail, but when the water in the vessels was changed, they were always seen in an heap at the bottom. I remarked the same thing in those that were wandering at pleasure about the water in the open plains.

And since I am speaking of the article *Newt*, compiled by Bomare, let me point out another of the various errors which it contains. He asserts, that the cry of the water-newt
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very nearly resembles that of the frog (page 35), whereas it is sufficient to be very slightly acquainted with these animals, in order to be assured that they are quite mute. It is only when they rise to the surface of the water, to expel the old air from their lungs and to inhale fresh, that the observer hears a sort of very low whistle, scarce perceptible at the distance of four paces. But in an author, to whose lot such inaccuracy and want of discernment have, as numerous other fabulous tales, with which his Dictionary abounds, may serve to shew, such errors are less unpardonable.

LXXXVII. But let us proceed to the hatching, or rather the evolution of newts, another part of their history not less curious and interesting than the preceding. Let us then attend to what happens to the eggs after they have been brought forth. These, when put into water, sink to the bottom; if the weather be warm, a quantity of air-bubbles soon appears upon the gluten which includes them; these at first are very small, but become afterwards larger, and at last so large, that the eggs become lighter than water, and arise to the surface, bringing with them the collection of bubbles still adhering to the gluten: the bubbles then burst and disappear, and

now the ova fall again to the bottom, and rise no more, being kept down by the gluten, which fastens them to the spot on which they rest. If we continue to watch them attentively, we perceive that their shape begins to change. When first brought forth, and for one or two days afterwards, they resemble an elongated spherule; the spherule now begins to appear slightly curved, representing in miniature a kidney, or the testicle of a cock. The curvature encreases, and the bulk in the same proportion, but with this additional circumstance, that one end of the ovum becomes thicker, and the other thinner. In the mean time, it acquires twice its original size. And now it appears not to grow in bulk, but only in length; and this becomes every day more apparent to the surprize of the observer. But his greatest surprize arises from seeing the egg thus elongated, agitate itself at intervals with great briskness, and then continue quiet: and as this happens without any external exciting cause, the idea of animality necessarily arises in the mind, and we incline to believe that the supposed egg is a real newt, only in disguise, just as I have discovered that the supposed eggs of frogs and toads are not eggs, but tadpoles in disguise. This idea continues to be more and more confirmed

firmed in the sequel, from observing by a glass the self-moving egg assume the features of a small newt, the tail appearing perfectly formed, the vertebræ beginning to shew themselves as well as the little gills within which the blood circulates, and likewise two lateral protuberances, which the observer suspects to be the rudiments of the arms, and the vestiges of the head and muzzle, and lastly the outlines of the eyes lying by the side of the head, under the appearance of two inconsiderable tumours. Continuing to employ the microscope, we perceive that the little newt is not now circumscribed by the gluten, but by a transparent circlet more internal, which is nothing else than the amnion full of liquor, within which the little animal has its residence; its colour is also remarkable, the inferior part of the body is white, while the upper part is yellowish, and interspersed with several blackish streaks. N° I. Fig. 16. Pl. III. represents a newt's egg in its natural state, surrounded by gluten: N° II, III, IV, V. shew the same egg stripped of its gluten, and insensibly becoming curved and elongated. N° VI, VII. represent the body supposed to be an egg, but which from its progressive elongation and evolution, has assumed the real appearance of a small newt, somewhat

magnified. D. fig. 17. shews the same circumscribed by the amnion, which is represented by the smaller circle, but the whole was observed by a microscope of greater magnifying power.

LXXXVIII. As long as newts remain in the amnion, they are never seen extended at full length, but always incurvated in such a manner, that the tail approaches to the head, as may be observed in fig. 16. N° VI, VII. and at D. fig. 17. It cannot indeed be otherwise; since the diameter of the amnion is much shorter than the young newt, they must of necessity be bent. Whilst they are confined in this prison, they change their situation from time to time, and always with incredible quickness, on a sudden transporting the head where the tail lay and reciprocally; and this happens not only when they are struck by any substance, but even when they are left undisturbed. Mean while they increase in size from the food they take within the amnion, but when afterwards that membrane can no longer contain them, they burst it by repeated efforts, and entirely quit their envelope, and begin to swim about the water, by the help of quick vibrations of the tail. I have several times beheld this exit with pleasure; it recalled to my memory the exit of
but,

butterflies, when they lay aside the mask of the chrysalis. In fig. 17, 18, 19. are seen three newts just out of the envelope; two with their backs presented, fig. 17 and 19. and one laterally, fig. 18. In the same figure we have two others, some time emancipated from their confinement, and swimming about the water. These two young newts, as well as the two others that are coming out of their envelopes, fig. 17 and 18. shew the prominent rudiments of their fore legs, under the appearance of two little stumps. Two of these are marked C. C. fig. 17. So the letter A. in the same figure, represents one of the above-mentioned envelopes. Upon examining some, which were no longer receptacles for young newts, they appeared to be externally composed of a residuum of gluten, which, at its internal surface, had become callous, of the extremely fine and transparent membrane of the amnion, and of a little of the liquor of that membrane, which was also transparent and slightly viscid.

LXXXIX. My next object of enquiry was, the time requisite for these animals to pass from the illusive form of ovum, to the real one of newt; this I have found to amount to the space of seven days, more or less. They require three or four more before they come
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to burst the amnion and gluten, and to float at liberty in the water.

In general, to *hatch* these animals is more difficult, than to hatch the tadpoles of frogs and toads. Though the fetuses of the newt (falsely supposed to be eggs) are put into pure water as soon as they are brought forth, and this water should have been taken from the very spot in which they were deposited by the females, yet few come to perfection, unless we are careful to change the water very often; and even with this precaution, there is always, in like circumstances, a smaller number of young newts than tadpoles evolved.

xc. This difficulty does not occur in their further evolution and growth. Any kind of water, either pit, rain, river, or lake water, will answer this purpose perfectly well, provided it is pure. If there should happen to be any plant, such, for instance, as the marsh lentil, in the water which contains the young newts, they will surround and nibble it with their little mouths, selecting those parts which are best adapted to their constitution. During the progress of evolution, the arms are unfolded more rapidly than other parts (LXXXVIII), they become pointed, and are bent back towards the posterior part of the body,

body, as may be seen in the two young newts at the bottom of fig. 19. but better in fig. 20. in which the animal appears magnified. The two cones marked C. D. are the two arms; above which, and nearer the head, the gills are seen branched, as also two other smaller bundles of gills, E. F. near the eyes; these smaller gills appear later than the large ones. The young newts being still kept in water, about a week after their exit from the egg, three other very small cones, not far distant from each other, shoot from the ends of the two former; these cones are soon perceived to be fingers belonging to the little hands. The animals now begin to employ these fingers, hands, and arms. In like manner as the anterior limbs make their first appearance in the shape of two cones, so also do the posterior, which commonly are first perceived, fifteen and sometimes twenty days after the animals have quitted their envelope. Nature likewise observes the same progress in producing the toes as the fingers; and now the newt is able to walk, either along the bottom of the water, or on dry ground. It is well known, that the tadpoles, both of frogs and toads, lose their gills a few days after their birth. Young newts, on the contrary, retain

tain them long, and I have even observed them in August.

xci. There still remains an important enquiry relative to these animals, the same which has been already made concerning frogs and toads. At what period may those roundish bodies, commonly called the eggs of newts, be properly termed true fetuses? Is it when they are upon the point of being expelled from the body of the female, and consequently have been already bathed by the semen? or still sooner, when they lie in the upper part of the oviducts, where we are certain that the semen could not arrive? I flatter myself, that I have the most direct proofs that these little bodies are real animals, even when they are at the top of the oviducts; although when taken out of the body of the female, they are not developed, but come to nothing, for want of the necessary condition of being fecundated by means of semen. These proofs are deduced from the identity of the impregnated and unimpregnated corpuscles: and I hope the reader will be fully convinced of this identity, when I assure him, that I have subjected each to the same minute and rigorous analysis, that was employed upon the fetuses of frogs and toads, and which is particularly described in the LXXVIIIth paragraph; nor could

could I discover the smallest difference, either in the internal or external parts, between the corpuscles bedewed with semen, which were evolved and became newts, and others not touched by that liquor which were spoiled. I therefore conclude, that in these animals also, the fetuses exist in the females before they are impregnated by the male.

XCII. I have spoken of the two sorts of newts described in paragraph LXXVII, and which have been hitherto the objects of my enquiries, as if there had been only one, having obtained from both the same results in every thing that respects generation. I will moreover add, that these results have been the same in another species of this animal, little more than an inch and half in length, and about two lines in thickness, of which the colour is a cineritious ground, interspersed with black spots; my observations upon this species were made three years after those which are related in the present chapter.

I have hitherto related my remarks concerning the generation of frogs, toads, and newts, in such a manner, that it does not always appear, whether I have made such a number of observations and experiments as is necessary, in order to obtain safe and constant results. Now the reader may be assured, that
I have

I have done this, not because I was not in possession with a sufficient number of facts, but in order to avoid creating disgust by too frequent repetitions. I can also assure him, that every fact has been seen and examined a great number of times, for I have been taught by daily experience, that in natural history, truth can only be attained by the constant success of repeated experiments.

CHAP.

C H A P. VI.

R E F L E C T I O N S.

XCIH. **M**Y principal intention in the five preceding chapters, was to relate facts unmingled with reflections. I would now deduce consequences, and lay before my readers such a train of reasoning, as may contribute to the illustration of the subject, for this is the most important part of natural philosophy.

1. It is well known, that almost all animals except man, have a stated season for the propagation of their species. Thus the female cat receives the male in September, January, and May. The she-wolf and fox in January; the doe in September and October. The spring and summer are the seasons appointed for the amours of birds, and many species of fishes. The immense tribe of insects have likewise a determinate time for perpetuating their kind; this is the fine part of the year, and particularly autumn and spring. The last mentioned class of beings is subject to a variation that is not observed in the others.

others. Unusual warmth or cold does not retard or forward the conjunction of birds or quadrupeds; but a late spring delays the amours of insects, and an early one forwards them. Thus it is observed, that in the same country, the insects on the mountains are later than in the plains.

XCIV. The variations of heat and cold have the same effect upon the amphibious animals, that have been the subject of my enquiries, as upon insects. They too have, as we have seen, a fixed time for their amours, but it varies according to the warmth or coldness of the season. If we compare my observations on frogs and toads with those of Roefel, it will appear, that this order of animals begins earlier to propagate the species in the mild climate of Italy, than in the rigorous temperature of Germany. In his history, he speaks of a very small species of aquatic toad, which, from the beautiful flame-coloured spots that adorn the belly, he calls *Bufo igneus*, as copulating near Nuremberg in June. This species is not found in the plains of Italy, at least not in the Modanese, the Milanese, or the country round Pavia. I have sometimes found it on the hills round Modena, the male embracing the female in May, but I had not an opportunity of making my obser-

observations with sufficient accuracy. Afterwards, in my journey to Geneva and Switzerland, in the summer of 1779, I saw a great number of these toads. In the course of my excursions with the amiable and learned Mr. Senebier, on the eminences that almost surround that illustrious and cultivated city, I remarked, that there was not a lake or ditch that did not afford reception to great numbers of these animals; they were generally coupled, though it was then the end of July. I have moreover found them in August, in many waters in the vicinity of Berne, Basil, Zurich, and Lucern, and their amours were then but just finished, as appeared from the little tadpoles that had lately quitted their mucus, and were swimming about. These animals then copulate much earlier in Italy, than either in Germany or Switzerland. This is not matter of surprize, for the Swiss cantons are well known to be the highest part of Europe, and by consequence must be cold, as I perceived among other proofs, which cannot be properly introduced here, by the ripening of the corn, which is a month later than in the plains of Italy, and by those trees, which with us will not grow at all, or else only in the highest situations, thriving here wonderfully

well in the lowest vallies, such for instance as the larch, the beech, the fir, &c.

I have observed in the tree-frog, and fetid terrestrial toad, in the river at Geneva, just the reverse of what I had seen in the *Bufo aureus* in Switzerland. In March 1780, I remarked that the two former species had made their appearance, and were coupled in the ponds and other reservoirs of fine gardens near that city, whereas in Austrian Lombardy, they had not yet quitted their subterraneous abodes. The reason why frogs, toads, newts, and the numberless tribe of insects, require a certain degree of atmospherical warmth, while other animals are not influenced by it, is, I should think, because the latter have an internal source of heat, which animates them, even in the severest cold, whereas the former are almost entirely destitute of any such source. As therefore the exercise of their functions depends on the heat of the atmosphere, their amours also depend upon this cause, and will of course be later in cold than hot climates, and in both will vary with the season.

xcv. II. It has been shewn in the preceding chapters, that the round bodies produced during the season of generation by the female frogs, toads, and newts, that fell under

der my inspection, are not, as it has been universally believed, eggs, but real fetuses; for they are never inclosed in membranous or crustaceous envelopes, like animals that come from eggs, but are quite naked; nor do they ever leave any shell or membrane behind them, after they have been impregnated, except the amnion, which is the case with all viviparous animals. It follows, that these species ought to be removed from the class of oviparous animals, to which they have been referred by naturalists and nomenclators, and placed among the viviparous. There is a circumstance here that deserves to be noticed. All viviparous animals have this in common, that their fetuses are at birth full formed, and retain the lineaments which they then have through their whole life: they are only more unfolded. We are further certain, that they have long before birth the form of the species, as is evident from human abortions, as well as those of beasts. In like manner, animals that come from eggs are formed, not only when they are hatched, but long before, as we see in the eggs of birds, various reptiles, crocodiles, &c. If the eggs are broken and examined, we perceive the fetuses more or less advanced, provided they have been fecundated and set to hatch. I have made the

same observation on the eggs of insects; when I found they were nearly hatched, I have frequently opened the pellicle, and discovered the embryo formed, and endowed with the power of motion. On the contrary, the fetuses of the amphibious animals, that have been the subject of my researches, are quite shapeless at the time of exclusion, and have only the appearance of globules; it is not till afterwards that the limbs begin to appear, and that they assume the lineaments of the species. Now I think that upon reflection, I can assign the physical cause of this striking difference. The fetuses of other animals have, indeed, at the time of birth, the characteristic form of the species, but they do not acquire it for some time after fecundation. They are at first shapeless, as we see in birds in the egg, which, before they assume their true figure, must undergo the most surprizing changes, as has been shewn by Haller, and before him by Malpighi. Fetuses then, in general, are not perfectly formed till some time after fecundation. Now in the amphibious animals of which I have been treating, impregnation does not take place till the fetus is excluded. It is, therefore, no wonder that they are shapeless; it is indeed to be expected, that they should take the form of tadpoles

tadpoles after they are brought forth. And we might conclude, that the globular figure which these animals have, as long as they lie in the ovaria, might perhaps be the universal model for unimpregnated germs, if they were not the production of infinite wisdom, and therefore probably infinitely varied.

. xcvi. III. Though these animals are properly denominated amphibious, since they are capable of living both upon dry ground and in water, yet, if we except the common frog and the newt, they generally live out of the water, and only repair to it in order to propagate the species; and as if conscious that their fetuses, if brought forth upon land would irretrievably perish, they invariably deposit them in water, the only element where they can be unfolded and grow. They do not, however, place them in any water that may happen to be near: they are never to be seen in torrents or rapid rivers, but always in ditches, lakes, ponds, where the water stagnates or runs but slowly. The reason of this is very obvious. A rapid current would carry the fetuses away, for they are specifically lighter than water, and by continually shaking and dashing them against the banks and other obstacles, would destroy the greater part. The same danger does not subsist in stagnant

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waters,

waters. Besides the tadpoles here find their food, which chiefly consists of water lentil, a plant that does not grow in quick streams.

These animals then, as well as others, carefully provide for the continuation of the species. In insects in particular, these provisions are an inexhaustible subject of admiration. The curious observer of these minute beings, is constantly surprized by the care they take to deposit their eggs, where the young are sure to meet with proper food. An immense number of both nocturnal and diurnal butterflies and moths, come from caterpillars that live upon various herbaceous and woody plants. They never fail to deposit their offspring precisely upon these plants. There is not the least danger lest they should mistake one vegetable for another. That which comes from the caterpillar of the oak never lays its eggs on the elm, and reciprocally. The same may be said of other plants; for should such a mistake happen, the caterpillars would die for want of fit nourishment. Another class of insects, the numberless and various tribe of flies, affords equal cause of admiration: they all deposit their eggs in certain stated places; some in the pith or within
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the bark of trees, or upon the leaves, or within the empalement of flowers; some in the ground or in water; some upon dunghills, in sepulchres or dead bodies, and others, in short, within the body or upon the skin of living animals.

Equal industry and sagacity is conspicuous in the race of cantharides, beetles, &c. And to come to animals, more nearly resembling those concerning which we are enquiring, many fishes, belonging both to salt and fresh water, conceal their eggs in places where the water is shallow, and consequently more warmed by the rays of the sun; hence the young fry is sooner produced. Besides these places generally afford more aquatic insects, the food most agreeable to fishes. Turtles, which are themselves amphibious, furnish an example still more closely connected with the subject of these dissertations, excepting that they repair to dry ground, and conceal their eggs in the sand.

xcvii. iv. In the four first chapters, I mentioned the close embraces of the male frogs and toads. This phenomenon has given rise to discordant opinions. Vallisneri thinks, that the compression contributes to separate the eggs from the ovaria, and to fa-

cilitate their entrance into the oviducts (*a*). Swammerdam, on the contrary, supposes that it is more likely to prevent, than forward their passage through the oviducts, as it may close their orifices. Hence he thinks, that the male does not begin his embraces, till the eggs have passed through those canals (*b*). But this can only be determined, by opening some females at the commencement of their amours. In some species, indeed, bare inspection will shew, that the pressure made by the fore-feet of the male, cannot contribute to force the eggs out of the ovaria; for they do not come forward upon the breast, to which the chief part of the ovaria lies opposite, but press against the lower extremity of the abdomen, as I have myself observed in the *Bufo igneus* of Roefel, and as indeed is evident from his figure. I kept two females apart from the male, but this did not prevent them from discharging their eggs. Hence it is more than probable, that this would also happen in the species, denominated by that naturalist, *Bufo aquaticus allium redolens*; for the male does not press against the breast, but like the igneous toad against the abdomen of the female (*c*). There seems, how-

(*a*) L. C. (*b*) Bibl. Nat. (*c*) See Roefel's figure.

ever, some ground for believing, that the embraces of the male may contribute to the exclusion of the eggs, in those species in which he clasps the female round the breast; as in those frogs and toads, of which I have described the mode of generation in the four first chapters. If the females of the aquatic frog be kept separate from the male, I have found, that they do not discharge their fetuses (vii). Here then we must conclude, that the embraces of the male influence this function.

Now I know not how this influence can be exerted, except by the pressure made against the breast, by which the fetuses are forced out of the ovaria, and get into the mouths of the oviducts. But in the tree-frog, notwithstanding pressure is made against the breast, it does not contribute to the descent of the fetuses, since they get into the uterus before the male begins his embraces (xxvi). When I wrote the history of the terrestrial toad, with red eyes and dorsal tubercles, I knew not whether the female could bring forth without the male; and I only said, that his embraces begin before the fetuses quit the ovaria (xlii). I afterwards kept two females in a vessel full of water, which, without the approaches of the male, discharged

charged their long and viscid cords, but the fetuses all perished for want of fecundation. If we now compare the facts related in this paragraph, we shall find, that Swammerdam's opinion is not universally true; for although the embraces of the male are posterior to the descent of the fetuses into the uterus in the tree-frog, yet this does not happen in the aquatic frog and in toads. Secondly, it will appear, that the strong compression of the male is very far from being always, as Vallisneri imagined, the cause of the separation of the fetuses from the ovaria. We cannot therefore on this, any more than on numberless other occasions, lay down any general rule, but must be attentive to the variation of Nature, in the endless multiplicity of her operations.

xcviii. v. I shall, perhaps, be asked to point out the cause of the male's perseverance in his embraces, which in some cold countries last above forty days, as Swammerdam observed in Holland. It may also be enquired, why these animals, during their amours, not only abstain from food, but disregard their own safety, for at this time they will not make any efforts to avoid being taken.

I can

I can assign no other cause, but that physical necessity which compels the individuals of different sexes to approach each other at this season. Under the dominion of this influence, which, in these amphibious creatures, as in other animals, is probably more strongly felt by the male than the female, they go in quest of each other and copulate. They must continue coupled till their mutual necessities are satisfied; this necessity, in the female, consists in the expulsion of the fetuses, and in the male, in the emission of semen; for the amorous impetus is occasioned and exalted by this fluid, with which the spermatic vessels are gradually filled during their embraces. Their pertinacity may have indeed another origin; they may be afraid, lest if they should quit their female, she should be occupied by another male; we know that the brutes are not exempt from jealousy; and I have had occasion to remark, that it is particularly vehement in those of which we are speaking. Their amorous ardour, which, at this season, seems to be their only feeling, may render them insensible to the call of hunger and to danger. - The long adhesion of the male to the female, and the neglect of themselves, is not peculiar to frogs and toads, but extends to various other animals, as we every

every day observe in insects, and in some animals of considerable size; as in the turtle, of which naturalists have observed, that the male and female continue attached to each other for several days; and in this situation, the fishermen easily take them together.

XCIX. VI. This blind ardour of the male, has furnished me with an opportunity of attempting some uncommon experiments, with an account of which, I hope my readers will not be displeas'd. In paragraph XLI it is said, that if a couple of the species of toad with red eyes and dorsal tubercles, happened to escape out of my vessels, when coupled, the male did not quit the female, notwithstanding she got to dry ground. I was further desirous of knowing, whether a separation could be effected by violence. I therefore suspended a male, by a thread tied to one of his hind feet, for a quarter of an hour; he would not, however, loose his hold, notwithstanding the long continuance of this violent attitude, and the weight of the female, which was so much larger than himself. I next pricked him with a needle in the hind legs, the thighs, the back, sides and head, till the blood issued out at every puncture. He would move and writhe, and stretch out and contract his body, but did not quit his hold.

hold. I afterwards cut the body in various parts with scissars: but this expedient was still ineffectual; nor would he forsake his situation, when I cut away small pieces of flesh; I even amputated a thigh without effect, and it was not till after thirteen hours, that the male, after so much torture, parted from the female, and expired the same instant.

I put another male to this female, and as soon as he had clasped her closely, cut off both his thighs without causing a separation: and what is more surprizing, the female began to discharge the cords three hours afterwards, and the male, with his blood flowing all the time, continue to impregnate them with semen, till the whole was discharged. The impregnation was effectual, and, as usual, the greater part of the fetuses were evolved.

I separated the male of another couple by force, and then cut off his two thighs. He was left with the female, that it might be seen, whether he would resume his employment; an event which actually took place, but he died before the exclusion of the cords.

My last experiment consisted in cutting off the fore-feet of a male, and putting him to a female.

female. Now the fore-feet enable the male to clasp the female so closely. The subject of my experiment instantly leaped upon her back, as if he had suffered nothing, and maintained his position, embracing her with his bloody stumps, till he had sprinkled the cords with seminal fluid.

c. I will not enter into the particulars of other like experiments, that were made upon the fetid terrestrial toad and frogs. I will only observe, that they coincided with those I have related: the amputation of the limbs neither preventing the embraces nor fecundation. Nay, even the decapitation of a frog, did not prevent either the one or the other. It is well known, that these animals are so tenacious of life, that this operation does not immediately take it away. That of which I am speaking was thrown into convulsions, but neither the fore-feet nor legs quitted the breast of the female, which brought forth her fetuses in an hour and three quarters, and I was an eye-witness of the male's besprinkling them with semen; that they were fecundated, there can be no doubt, since they came to life at the usual time. As soon as he performed this operation, he deserted his situation, and died four hours afterwards.

The

The toad mentioned in paragraph xciii, was also the victim of an experiment of the same sort. In August 1779, at Genthod, the delightful villa of Mr. Bonnet, I cut off both the thighs of a male of that species, while it was embracing the female; but without effecting a separation, which did not take place till many hours afterwards, a little before the death of the animal. Besides Mr. Bonnet, Mr. John Trembley was present at this experiment. I was asked, whether this pertinacity of the male was the effect of stupidity, insensibility, or amorous ardour. Though this appeared to be one of those questions, of which the determination requires, that one should enter into an animal without at the same time becoming one; yet I hesitated not to say, that I thought, as I still do, that this perseverance was less the effect of obtuseness of feeling, than vehemence of passion, which, as we have seen, renders them insensible to the call of hunger, and careless of their safety (xcviii). Such a degree of stupidity is not, I think, consistent with the great irritability of these animals, and the signs they shew of acute feelings, whenever they are wounded, or have their limbs cut off during their amours.

CI. VII. Swammerdam, in his account of the generation of frogs, supposes, that the eggs do not immediately pass from the ovaria into the oviducts, but that they pass first through the abdomen. He rests his opinion upon the instance of a frog, in which he found the eggs partly in the ovaria, and partly in the abdomen, besides those which were already in the oviducts and uterus. Roesel, speaking of the dark-brown terrestrial frog, says, that he has found many eggs in the abdomen, without, however, adopting or rejecting the opinion of Swammerdam. Of these eggs, or more properly, of these tadpoles, lying loose in the cavity of the abdomen, I have already spoken (LXVII); but if I may confess what I think, I have no inclination to accede to the opinion of the Dutch naturalist. I should imagine, that if the fetuses were to pass through the cavity of the abdomen before they entered the oviducts, they would frequently be found there in females during copulation, since they are so often found in the oviducts and uterus. Yet the contrary is true. I wish to avoid the charge of ostentation, when I observe, that having kept an account of the number of frogs and toads, which I have opened at the time of their amours, for this and the following

lowing dissertation, I find that it amounts to 2027; nor did I ever find the fetuses in the cavity of the abdomen, except on three occasions, of which two are mentioned in paragraph LXVII, and the other in CXXII. I am much more disposed to believe, that fetuses pass immediately from the ovaria to the oviducts. Hence I think, that if ever they get into the cavity of the abdomen or thorax, it proceeds from their never having got into the oviducts, or from some laceration, as is intimated in paragraph LXVII. Let the impartial reader determine.

CII. VIII. The fetuses, after having traversed the long and tortuous canal of the oviducts, are all collected in the uterus, whence they pass into the rectum, and then out at the vent. The discharge, in one species of toad, is aided by the male. Placed as usual upon the back of the female, and clasping her with his fore-feet, he waits impatiently for the expulsion of the cord (in this species there is only one cord); at the instant of its appearance, he lays hold of the end with his toes, and draws out a piece of the cord; this manœuvre is repeated till the whole cord is extracted. So intent is the male upon his employment, that he may be taken and placed upon the hand. And though this may cause

some interruption, yet he will soon resume his task with equal ardour. This information is owing to a casual observation of Mr. Demours, on the small species of terrestrial toad, as he denominates the animal, he saw nothing from which he could collect, that the male bedews the eggs with semen, while he is employed in extracting them (a).

It is to be lamented, that the French observer did not particularly describe the species. It is certainly different from either of those, of which I have treated; since I could never observe, that the male assisted in bringing forth the young, while it was easy to see, that he bedewed them with semen. Roefel, who mentions this fact incidentally, adds, that he never observed any such appearance: it might likewise have been expected, from the accuracy of the observer, that he should have taken notice whether the eggs were productive, since we might have hence collected, whether they are impregnated within the body of the female. It were also to be wished, that so interesting an observation should have been repeated. In short, the fact being solitary, and happening unexpectedly, should have been confirmed, and the narration ought ra-

(a) The reader will find this very curious observation related at full length in the Appendix. T.

ther to awaken than satisfy the curiosity of the philosopher.

CIII. IX. Daily experience shews, that in an immense number of animals, fecundation takes place within the body of the female. It might perhaps be thought, that we are warranted to conclude, from analogy, that this is an universal law of Nature. And it has accordingly been admitted as such by vulgar reasoners. But as on many other occasions, when the law was supposed to admit of no exception, so on this, analogical arguments have been found to disagree with experiment. Swammerdam first shewed, that impregnation is effected without the body of the female in one species of frog; and Roefel extended this discovery to another amphibious animal of a similar kind. I have had the satisfaction to discover this external fecundation in other species of frogs and toads, and have, moreover, clearly beheld the fecundating liquid issuing from the male, and falling on the fetuses, after they were expelled from the uterus of the female (Chap. I, II, III, IV).

CIV. But we know, that besides the species, in which it has been found that impregnation is external, there are many others included under the genus of frogs, as well as that of toads, both European and foreign.

We shall be convinced of this, by opening any modern nomenclator, as Linnæus for instance. What then are we to conclude, concerning the mode of fecundation in them? From these conjectures we may be willing to believe, that it is the same in the species that have not been examined. But we cannot be certain without experiments. Among those, which in this respect require to be examined by the naturalist, the famous toad of Surinam, denominated by the natives *Pipa* or *Pipal* (*a*), so remarkable for its property of bringing forth the young at the back, would, in my opinion, deserve the preference.

The celebrated Merian, who, with courage truly heroic, went from Holland to America, towards the close of the last century, to observe the insects of that unwholesome climate, first made us acquainted with this animal. It was afterwards examined by the acutest naturalists, by Ruysch, Folkes, and Baker, and found exactly to agree with the description left by that illustrious lady. Its back has cavities or cells, each containing a young toad. I had myself the satisfaction of seeing this rare and indeed singular phenomenon, in the public Institute of Bologna, and to still

(*a*) *Pipa* (*Rana*) *Digitis* *anticis* *muticis* *quadridentatis*, *posticis* *unguiculatis*. Linn. S. N. T. I.

greater

greater advantage last year at Geneva, in company with my respectable friends Bonnet, Senebier, Abraham and John Trembley. Nay, the possessor, who had many years kept it in spirits of wine, obligingly afforded Mr. Bonnet and myself an opportunity of making a number of observations upon it; and above all, to examine at leisure the dorsal cells, which were very numerous, and of which each contained a fetus. These observations, together with others, which my illustrious colleague made after my departure from Geneva, may be found in a Memoir printed in Rozier's Journal, which at once shews the learning and good sense of the author, as well as his impartiality with respect to his own opinions. Having denied, in his *Corps organisés*, the existence of the cells, on the authority of a celebrated professor at Leyden (a), he not only confesses in the Memoir, that they are to be found, but describes them in a manner so particular, as to remove all doubts concerning their existence. He concludes with proposing several questions, calculated to illustrate the natural history of this wonderful native of Surinam, which is yet dark and imperfect. It is easy to suppose, that the Genevese philosopher would not overlook

(a) Mr. Allamand.

what relates to the manner of impregnation. But we cannot hope for the solution of this, any more than the other questions, except upon the spot where the pipa lives, and propagates its species, if we cannot naturalize it, as Vallisneri naturalized the cameleon of Africa.

But besides frogs and toads, it is supposed, that fecundation is external in scaly fishes. When the female has laid her eggs, the male is said to go in quest of them, and sprinkle them with semen. There is then no real copulation between these animals, though the male approaches the female at the season of their amours, and is sometimes seen to rub his belly against that of his mate; this appearance takes place, because the male impregnates the eggs at the instant they fall from the female, This is the opinion of Buffon (*a*); and the manner in which he delivers it, would lead us to suspect, that he had the most certain proofs of it, though he has in truth no better foundation for it, than the notion which was generally prevalent till the time of Swammerdam (*b*), that the cetaceous class impregnates the eggs without copulation. This notion was not, I think, sup-

(*a*) Nat. Hist. T. 2. p. 313.

(*b*) Bibl. Nat.

ported by any observation upon which reliance could be placed. Hence it is no wonder that other naturalists, and among the rest the illustrious Haller, should incline to believe, that fishes really copulate for several reasons, which cannot properly be recited here, but may be seen in his great work. Though these reasons have far greater weight than the bare assertion of Buffon, yet I cannot think them decisive, as they are destitute of facts, by which alone the problem can be solved.

The mode of fecundation in fishes is very extraordinary, according to Linnæus; he supposes, that the female pursues the male while he is emitting the semen, and devours it, and and thus is impregnated (*a*). In the time of Vallisneri, there lived at Rome a physician, who taught that pigeons, sparrows, and many other animals, were fecundated by the mouth (*b*). Both these opinions are palpably false. Female fishes have, indeed, been observed to swallow the semen, not because it then serves to impregnate them, but simply for food. The male devours it with equal greediness for the same reason. The same observations are applicable to the eggs.

(*a*) Spons. Plant.

(*b*) Vallisn. op. in fogl. T. 3.

cv. Hence it appears, that we know not certainly the mode of fecundation in fishes. The element they inhabit, is so much more difficult of access than the land and air, that we cannot wonder the natural history of fishes should be so little advanced. Having often meditated upon this subject, which is yet among the mysteries of nature, I have been struck by a thought, which my other occupations have not yet permitted me to bring to the test of experiment. I willingly lay it before my readers. The golden fishes of China (*a*) are now very common. Italy in particular abounds with them, there being scarce a pond that is not supplied with them. The beauty of their colours induces many to keep them in vessels in their apartments. The amours of these fishes happen several times a year, and as they have little timidity, they do not desist from their employment when the observer stands close to them. The reader is by this time in possession of my ideas. Let these fishes be carefully watched when they are propagating their species, and we shall soon know, whether they really copulate, or whether the male darts his semen

(*a*) *Cyprinus auratus*, pinna ani gemina cauda transversa bifurca. Linn. S. N. T. 1,

upon the eggs after they are discharged, or, in short, whether fecundation takes place in any other way. If it is external, the asperſion of the ſemen cannot eſcape the attentive obſerver, as it is of a turbid colour. I will not ſtay to point out the means of aſcertaining whether the eggs are fecundated, and in what manner, as the reader may gather them from the preceding obſervations on the generation of amphibious animals.

CVI. Recent obſervations ſeem to have eſtabliſhed the certainty of external fecundation in bees. If the ingenious obſervations (*a*) of Mr. Deſbrow may be relied upon, the eggs of this induſtrious inſect are impregnated after the queen has diſcharged them. She, as it is well known, deposits them in the cells. The Engliſh naturaliſt obſerved, that when they are accompanied with a whitish liquor, which the male voids from the poſterior part of his body, they never fail to be productive; but when this liquor is not preſent, they are ſure to periſh. The author kept an hive without drones. The queen laid her eggs, as uſual, but as they were not impregnated with the white liquor of the male, they produced no young.

(*a*) Philoſoph. Tranſ. Vol. LXXII.

The experiment was varied in the following manner. An unimpregnated hive was divided into two parts, of which one was set under a glass-bell; in this was a queen, together with working bees, but without males, the eggs were not productive; but the contrary happened in the other part of the hive, which was likewise set under a glass-bell, and contained, beside a queen and common bees, a quantity of males. And the eggs were impregnated by the white liquor which the males void in the cells containing eggs. The dissection undertaken by the English observer proves, that this liquor is the true semen; for he found it in those vessels of the male, which are universally supposed to be appropriated to the reception of the seminal fluid.

From this it appears, that the two naturalists, who have written so well concerning bees, have committed mistakes. Swammerdam thinks, that exhalations from the males are absorbed by the female, and serve to fecundate the eggs; while Reaumur has been led by deceitful appearances to suppose, that these insects perpetuate their kind by real copulation. Moreover, the suspicion entertained by the celebrated Maraldi, is completely verified; this writer, in his observations on bees, conjectures, that the eggs
are

are impregnated after they are laid, by the whitish matter above described; he, however, was not diligent to enquire further.

From what has been said on external fecundation, it appears, that the number of animals, in which we are certain that this mode takes place, is very inconsiderable. We may presume, that the industry of observers will increase it, for many other discoveries have been extended to a multitude of subjects, though they at first seemed to be confined to a single species.

CVII. x. Fecundation in newts is accompanied with circumstances, that are not common to other animals, and deserve to be considered. The fetuses (for the elongated bodies, supposed to be eggs, are only young newts not yet unfolded, LXXXVII), are impregnated within the body of the female. But the male does not introduce into her any part that characterizes the sex, for, in truth, he has not any such part; he only darts his semen into the water; the semen gets into the anus of the female prepared to receive it, and thus she is fecundated. And here occurs another singularity that deserves to be noticed. According to the general opinion of the best anatomists and physiologists, the ovaria are the seat of fecundation; but this
cannot

cannot be the case in the newt. When the feminal fluid gets to the inferior orifice of the oviducts, it cannot possibly pass any further, for its passage is stopped by the fetuses, which now occupy more or less of these canals, and are generally thickest at the mouth. The feminal fluid then stops here, and moistens the fetuses as they come forth. These are the only ones that are fecundated; but they are succeeded by others, which are in like manner fecundated by new emissions of semen, and so on till all are impregnated. I suppose the reader to recollect paragraphs LXXX, LXXXI, LXXXII, LXXXIV, LXXXV. upon which these deductions are founded.

CVIII. XI. Though the chief object of this dissertation is generation, yet I have hitherto considered it in a few animals only. I shall now extend my ideas, and make use of the data which my experiments afford, as principles that will prevent me from falling into error in this intricate enquiry. The most celebrated systems concerning generation may be reduced to two: the one attempts to explain the formation of animated beings mechanically; the other supposes, that they are already formed and pre-existing, and that the act of fecundation only unfolds them, and renders them visible. Those who favour
this

this second system, are divided into two parties; some naturalists being of opinion, that the fetus pre-exists in the female, and others in the male. It is generally known, what efforts the eloquent Buffon has made to bring the former system (which is called the system of Epigenesis, and is of more ancient date) into repute, by means of his famous *organic molecules*. The powerful opposition that has been made to this system, is also well known. It has been ably refuted by the great Haller, not only in his physiology, but also in a separate publication, entitled *Reflections on the System of Generation of Mr. Buffon*. The arguments adduced by Mr. Bonnet in his *Corps organisés*, are not less cogent. Observing, however, that the objections of both Haller and Bonnet, though of great moment, are not direct, since these writers have not enquired into the existence of the organic molecules, which constitute the base of Mr. Buffon's edifice, I conceived it necessary to enter into an examination of them, and have found that this system, like all his other favourite hypotheses, is the product of his fervid fancy, which so represents shadows, as to make them appear like realities to those, who have not a great share of discernment.

I flatter

I flatter myself, that my Effays (*a*) prove the truth of my assertions.

The preceding observations on amphibious animals, furnish another irrefragable argument against the French naturalist. He thinks that the fetus does not exist before fecundation, but is formed during this act, for then the organic molecules, which are, according to him, the essence of the semen of the male and female, meet and combine in the uterus, and by virtue of certain relations, are modelled into an organized body. But my observation on frogs, toads, and newts, are diametrically repugnant to this imaginary theory: They prove, that the fetus exists in the female long before impregnation (xviii; xix: xxx. liv, lv, lvi, lvii. lxxii: xci). Now this is probably the case in other animals. My experiments, indeed, were all made upon animals of cold blood; and this circumstance may afford room to doubt, whether the conclusion is to be extended to those of warm temperature. But all suspicion must be removed, now the same observations have been made upon this class. I allude to this luminous discovery of Haller, who has shewn, that in birds the young exists in the female

(*a*) Opuscoli di fisica animale e vegetabile. Modena 1773.

before fecundation; but as this discovery is very generally known, it will be unnecessary to relate it here. As then we have, both in the class of cold and that of hot animals, instances of the pre-existence of the fetus, I can see no reason, why we should not apply the observation to the rest. We have at least, till the contrary shall be proved, good grounds for believing that this is the case.

CIX. But these observations lead to other consequences. I have remarked, that those who admit the pre-existence of the fetus, are divided into two parties, of which one believes, that they lie in the female, the other in the male (CVIII). According to the latter, the fetuses are the worms that float in the semen, and pass, during coition, from the male into the female. But the falsity of this opinion is now obvious. In paragraph VIII. I observed, that after the oviform corpuscles, or the mature fetuses of the green aquatic frog, have descended into the uterus, the ovaria contain others of a smaller size, which serve the year following to continue the species. The like remark was made (LXVI and LXXXV), concerning the fetid terrestrial toad and water-newt. I may now add, that I have discovered the same thing in the other amphibious animals mentioned in this work;

so

so that we may safely say, that the little fetuses are to be found in the ovaria; at least a year before these animals seek each other for the purpose of generation: they do not, therefore, pass from the male to the female during the act of fecundation.

Here it is proper to answer a question that may be asked. It appears from the observations of naturalists, that these several species of amphibious animals begin to propagate the second year. And it is probable, that they continue to do this as long as they live; that is to say, for a considerable series of years; we have at least Roefel's authority for believing that frogs live ten years or longer (*a*); and it seems likely, that toads come near them in this respect. Let it then be supposed that they propagate for nine years. The females will exclude nine successions of fetuses. If we examine the females during the first year of their growth, we shall not find any fetuses in the ovaria. We are not able to distinguish any before the second year, when two sets appear, viz. the mature ones,

(*a*) If any conclusion may be deduced from the curious account of a toad in the Appendix to the British Zoology, the term here assigned is too short. The individual there described is said, if I remember right, to have lived at least thirty-six years. T.

those which are to be brought forth that year, and the immature ones, which will be produced the succeeding year. That year the third succession of fetuses becomes visible, and the fourth year the fourth succession; and in this manner one succession only every year. Now it may be asked, whether these orders of fetuses, which successively appear in the ovaria, pre-existed in them without being visible; so that they are only evolved, and rendered conspicuous by time, or rather whether they are formed in succession, a new order being annually generated.

I should reply without hesitation, that as it is by no means proved, notwithstanding the efforts of the modern favourers of Epigenesis, that any such formation of organic bodies takes place, either in the animal or vegetable kingdom, and as all Nature abounds with such evolutions, according to the accounts of the most judicious philosophers of this age, it is natural to suppose, that these orders of fetuses, which annually make their appearance in the ovaria, are not successively generated, but co-existed with the female, and are only unfolded, and rendered visible in progress of time, by the supplies of nutritive liquor that come from the female. This co-existence of successive orders of fetuses,

which become visible in the ovaria, is analogous to that which takes place in the limbs. Tadpoles have at first no legs. These parts appear only when they are about to assume the lineaments which characterize the species. Shall we therefore conclude, that they do not exist at first, but are generated when the tadpoles approach their metamorphosis? Is it not infinitely more philosophical to suppose, that the limbs co-exist with the tadpoles, and are invisible, only because they are too small to strike the senses? And if it is reasonable, to adopt this opinion concerning the limbs, shall we not also admit it with respect to the fetuses of these animals?

CX. XII. If I passed over in silence the singular, and I think, new opinion of Mr. Gautier, concerning the generation of frogs, I should deviate from the sincerity which every philosopher ought to practise. In a publication entituled, *Observations sur l'histoire naturelle, sur la physique, &c.* after speaking of some small worms, which he found in a vesicle in the cavity of the abdomen of the male, he adds, that they are the real efficient cause of generation. I must quote his own expressions: *La grenouille male montée & fortement attachée sur la femelle attend les instans que les oeufs s'écoulent*

lent de la femelle: il jette alors ses embrions tels que je les ai apperçus, ils s'attachent aux oeufs, & s'en nourrissent pendant quelques jours, jusqu'à ce qu'ils soient en état de se nourrir d'alimens plus grossiers. Ces embrions conservent la même figure qu'ils avoient dans la vesicule du pere, pendant l'espace d'environ un mois, temps au quel ils quittent cettent figure, comme font les vers a foyé dans le cocon. Ils developpent leurs pattes posterieurs qu'ils ecartent enfin: ce sont ces pattes qui unies dans l'embryon, forment la queue du têtard embryon de la grenouille.

As the book was published in 1752; I have had sufficient time to examine the discovery. My first step was to seek for the bladder containing the worms in the abdomen of the male. It was easily found, being no other than the urinary bladder, as may indeed be collected from the description of the author; in this I moreover found the worms; they are about the thickness of a thread, of a yellowish white colour, without rings, a line and a half in length; they are almost perpetually in motion, and have one extremity attached to the inside of the bladder. So far I agree with Mr. Gautier; but I cannot, with him, consider the worms as the fetuses of the
 I 2 frog,

frog, for the following reasons, which I think decisive. They are likewise to be found in the bladder of the female, whereas they ought to exist only in the male, if they were what the French naturalist supposes them to be. Secondly, I have opened an immense number of frogs during the time of copulation, but have by no means found these worms in all of them. Thirdly, They never exceed twenty in number, whereas the fetuses, from my observations and those of Swammerdam, appear to amount to about a thousand in every female. Fourthly, After fecundation the male ought not to contain any, but I have found that the number is not lessened. Fifthly, As these worms adhere to the supposed eggs, and feed upon them for several days, I must have also seen them, especially as they are visible to the naked eye. I can, however, truly assert, that notwithstanding all my attention in examining the eggs, both externally and internally, I never could perceive any vestige of them. I shall deduce a sixth argument from the artificial fecundation spoken of in the next dissertation; which consists in touching the fetuses extracted from the females with the semen of the male, though that fluid sometimes appeared destitute of living inhabitants, even
when

when examined by the microscope. These reasons oblige me to reject the pretended discovery of Mr. Gautier. I will not pronounce it to be a mere fiction: I will rather suppose, that some fallacious appearance has misled him, in consequence of his inexperience in observing frogs, and his ignorance concerning their internal structure, though that is exceedingly obvious. That this is the case every naturalist will perceive, from the passage in which Mr. Gautier affirms, that the hind legs of the tadpole, by their union, form the tail of this animal.

Baia che avanza in ver quante novelle
 Quante mai differ favole o carote
 Stando al foco a filar le Vecchierelle.

Those who shall have the curiosity to read his book, will learn, that the female frog has no uterus, that the tongue is fixed to the anterior margin of the palate, that the kidneys of the frogs are also the testicles, &c. It is not, therefore, matter of surprize, that Roefel should treat him with ridicule, and conclude, non solum itaque asserere audeo parum in anatomia Ranarum profecisse Gautierium, sed addere etiam non ambigo, ipsas ranas eundem vix habere cognitatas. And as the Parisian naturalist, in the relation of his discovery, modestly observes, that if Py-

thagoras had made one equal to his, he would have sacrificed another hecatomb to the Gods. The German observer retorts; Ego vero crediderim, si fieri posset ut Gaudierius Pythagoræ quæ invenerit, enarraret, hunc ipsi non silentium biennii vel quinquennii, quod discipulis suis imponere solebat, sed perpetuum esse injuncturum.

CHAP.

C H A P. VII.

EXAMINATION OF SOME RECENT OBJEC-
TIONS MADE AGAINST THE SYSTEM OF
THE PRE-EXISTENCE OF THE GERM IN
THE FEMALE.

CXII. **D**R. Pirri, a celebrated physician and philosopher at Rome, is the author of these objections. To a sensible work on the theory of putrefaction, wherein he declares in favour of the Count de Buffon's system of generation, he has prefixed some reflections concerning the reproduction of organic bodies, in which he attempts to overturn the principal arguments of those who adopt the hypothesis of pre-existence of germs. In the first place he quotes and attacks two discoveries, one made by Haller on the chicken (CVIII), the other by me on frogs, and mentioned in my Prospectus. Let us bestow a little attention on both, and begin with that which belongs to me. "The learned Abbé Spallanzani, a name now so dear to his country, says he p. 7, has related another fact not less interesting than those

already mentioned, nor in appearance less favourable to the doctrine of Palingenesis, or the pre-existence of germs in the eggs of the female, and by which the importance of the function of the male is very much lessened. He watched the instant when the eggs are extracted and fecundated by the male. During this operation he killed the female, and by the aid of the microscope, that instrument which has so often imposed both on our senses and understanding, he found, that both the impregnated and unimpregnated eggs, which lay in the uterus, contained a tadpole bent in such a manner, that the tail was contiguous to the head. The young animal was distinguished by its black colour. In the impregnated eggs it was alive and in motion, but in the others it lay in a profound lethargy." It was proper to transcribe the expressions of Dr. Pirri, as they by no means agree with my relation of the discovery in the Prospectus. The reader must allow me to quote my own words. After remarking, that the impregnated and unimpregnated eggs of the frog are perfectly alike, and that the latter are not productive, I add, p. 51, " but this is far from being the case with the former. They lose their round shape, and elongate without sensibly increasing in bulk, though they afterwards

terwards manifestly grow larger. The surface of the whitish hemisphere turns darker, and upon the black hemisphere appears a longitudinal furrow, terminated by two processes, which become gradually longer, and take the direction of the long diameter of the egg. The furrow and processes grow, and in time nearly burst out from the side of the egg, which yet retains the shape of an oblong globe, with a prominence on one side. Meanwhile the whitish hemisphere dilates, and the black one is incurvated, while the prominence increases in length, and it now appears, though this is still more distinctly seen afterwards, to be the tail of the tadpole: the curvature on the black hemisphere is discovered to be the back, and the dilatation on the opposite side the belly. The other extremity now takes on the figure of the head; in the anterior part the vestige of the eyes, which are yet closed, becomes conspicuous: the two processes, by which the animal adheres to bodies, however smooth, come in sight, as well as the rudiment of the aperture of the mouth; and lastly the gills, through which the blood may be seen to circulate.

The animal does not yet shew any signs of life or motion, when it is pricked with a needle, or suddenly exposed to the rays of the

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the sun, even when collected into a focus, though it is afterwards sensible of these impressions

Such are the phænomena which gradually appear in fecundated eggs, whence it is evident, that these bodies are not, as it has been generally supposed, eggs, but the tadpoles themselves concentrated and folded up.

It is therefore clearly proved, that the tadpoles exist before fecundation; but this interesting truth may be more fully shewed thus: the fecundated eggs differ not in any respect from those which have not been fecundated, but the former are the tadpoles concentrated and folded up, the latter must therefore be the same; wherefore the fetuses of frogs exist before fecundation, and to be evolved, want only the fecundating fluid of the male."

Upon comparing this passage with the quotation of Dr. Pirri, it is easy to perceive, that at the time of writing, he had not the Prospectus before him. First he advances what is not true, when he asserts, that I made use of the microscope, "*that instrument which has so often deluded both our senses and understanding;*" except in the passage where I say, that I examined the inside of the egg with the microscope, I never speak of it,
having

having had indeed no occasion to make use of it; for the eggs are so large, as to be capable of being examined sufficiently by the naked eye. If even I had been obliged to have recourse to this instrument, I hope I should not have incurred the danger which the learned physician mentions. The microscope has, indeed, sometimes been the source of error, either when it was ill constructed, or in the hands of unexperienced persons. But it has, nevertheless, when these inconveniencies have not been present, enriched, and every day enriches natural history with the most important discoveries. Whoever, indeed, shall question this, will risque the imputation of being supposed deficient in common sense.

Secondly Dr. Pirri says, that I found both the impregnated and unimpregnated eggs to contain tadpoles. These words suggest an idea very different from that which I have expressed. I do not say, that I have found the tadpole to exist as well in the latter as the former egg, but that both are nothing but tadpoles. The expression of Dr. Pirri supposes the existence of eggs, whereas mine entirely excludes them.

Thirdly, The tadpole was, according to the objector, so bent, that its tail was contiguous

tiguous to the head, and it was distinguishable by its dark colour.

In my Prospectus I do not say that the colour is entirely black, but that one hemisphere of the tadpole, while it retains its round shape is black, and the other of a dirty white. With respect to the position of the tail, the author is far less accurate; for I never dreamed of asserting, that the tail is bent towards the head, but that it appears like a prominence or appendicula, and increases in length as the tadpole grows. This is confined to the impregnated tadpoles; for the others have no tail, though Dr. Pirri would make the reader believe, that I attribute this part also to them.

Fourthly, I found this difference, that in the impregnated eggs the tadpole was alive and in motion, whereas in the others, it was motionless, and in a profound lethargy.

I shall finish these strictures upon the extract with observing, that I never mentioned any profound lethargy in unimpregnated tadpoles; and so far was I from seeing any motion in the others at first, that I expressly mention the contrary. Let the reader again peruse the quotation from my Prospectus, and he will find that these strictures are just.

CXII. I shall now examine the objections of Dr. Pirri. They may be reduced to two, of which the following is the first. My discovery of the pre-existence of the tadpole, rests upon an observation in which I fully confided, as it has the authority of Swammerdam; from whom we learn, that in frogs fecundation does not take place in the uterus, but without the body, since the eggs are touched with the semen of the male, after they are discharged. The Roman naturalist, relying upon the remarks of Roefel, suggests various doubts with respect to this observation. “How, says he, can we be certain that the eggs of frogs are impregnated after they are laid, when Roefel himself confesses the uncertainty of this, having seen an instantaneous contact, which may afford room for supposing, that the semen was thrown into the uterus? (p. 15.)”

When I composed my Prospectus, I was not unacquainted with the doubts of Roefel, but I was aware, that they did not destroy the validity of the contrary observation, since that is positive. Roefel himself was sensible of this, and he does not, therefore, in speaking of this momentaneous conjunction, question the fact related by Swammerdam, a circumstance which Dr. Pirri ought to have known.

known. But further, he does not only not question what Swammerdam tells, but strongly confirms his observation in another passage of his work, where he speaks of the generation of the green frogs. He says expressly, that the male bedews the eggs with his semen, after they have been discharged by the female. “*Simulac autem femella ova sua per anum emittit, masculus eadem suo conspergit semine; id quod ipse domi meæ non solum vidi, sed iteratis etiam vicibus fieri non sine admiratione observavi (a)*.” And to render this external fecundation still more evident, he represents the male mounted upon the female in their natural colours, the eggs as they are discharged, and the feminal fluid bedewing them, Fig. 2. Pl. 13. It might have been expected from the ingenuousness of my opponent, that he should not have suppressed this important observation, or rather that he should have spared his objections. If he entertained any doubts concerning the mode of generation in these animals, I hope they will be dispelled by the four first chapters of this dissertation.

CXIII. In the second objection, I am accused of a paralogism in the relation of this discovery. In speaking of the pre-existence

(a) Hist. Ranar. p. 56.

of the tadpole, by the word tadpole I mean the embryo of the frog, or the frog in a very small state, disguised under the appearance of the tadpole. This offends Dr. Pirri, who considers the frog and tadpole as two distinct animals. He concludes, that “to make use of this fact as an unanswerable argument against the system of Epigenesis, is a fallacy that has escaped the penetration of the Abbé Spallanzani. It consists in confounding the appearance of the tadpole with that which belongs to the frog; and falsely supposing, that the tadpole and frog are one and the same animal.”

I was indeed apprehensive, on account of my confined talents, that my book on animal reproductions would contain mistakes, but not that to which my learned adversary objects. My confidence was grounded upon Swammerdam, Vallisneri, Roefel, and many other excellent writers, who all agree in considering the tadpole and frog as the same animal. We know that many insects pass through the three different states of worm, nymph, and winged animal: and those who are at all acquainted with natural history know, that these different states do not constitute three distinct animals, but that the same animal assumes these various appearances;

ances; so that the insect equipped with wings, existed under the membranes of the worm and nymph, from which, when it is freed, it issues forth in a state of complete evolution. Swammerdam observes, that what the nymph is to the winged insect, the tadpole is to the frog. For he found the frog in miniature under the disguise of the tadpole, and the winged insect under the cover of the nymph. Both continue in this state only till they are arrived at a proper period or state of maturity, when they throw off their old habiliments, and assume their proper form. From these observations I had reason to conclude, that the tadpole and frog are identical; and this might have sufficed as a reply to the objections of Dr. Pirri: my supposition was moreover founded on certain observations, his was gratuitous. My esteem, however, for this physician, who is advantageously known by several other publications, and my desire to ascertain a fact of so great importance, induced me to undertake a more particular and exact enquiry into the identity of the tadpole and frog than that of Swammerdam. To prove this completely, it is necessary to shew, that the internal structure and organization of the parts of the tadpole continues the same in the frog. If we find
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in each the same system of arteries, veins, nerves, and muscles, if the heart, liver, lungs, and other viscera be unaltered, if there be no difference in the organs of sense and the disposition of the bones, there can remain no doubt concerning the identity of the animals.

CXIV. Soon after Dr. Pirri's book came to my hands, in the spring of 1777, a season very convenient for the purpose, I began this enquiry upon the green aquatic frog, of which I treat in the first chapter. To be as brief as possible, I will only mention the bare result, beginning as soon as the internal structure of the tadpole can be examined, and ending at the time when it takes on the appearance of the frog. On the twenty-sixth day the intestines are distinguishable, though the integuments of the abdomen, rolled up in a spiral, and in the region of the thorax; the pulsation of the heart is perceptible. Upon opening these cavities, we find the mesentery resembling in thinness and tenderness a spider's web; along it wind small red streaks, which, when viewed with the microscope, appear to be arteries and veins. The kidneys, lungs, and liver are then very conspicuous; to the last viscus is appended the gall-bladder, full of a transparent liquor

without any bitternefs. The heart is conical, and confifts of an auricle and ventricle; a little nearer the head lies the bulb of the aorta, which is divided into two branches that are implanted in the mufcles of the breaft. The defcending aorta is alfo vifible, and the vena cava, together with the beginning of their ramifications. The dorfal and lumbar vertebræ, as well as the cranium, have nothing of the confiftence of bone, and the brain is gelatinous, as are alfo the nerves; thofe arifing from dorfal vertebræ are very diftinguifhable. The noftrils are open, and the iris is of a golden yellow colour. If we take the eye out of the orbit and open it, we fhall find the aqueous and vitreous humours, and the chryftalline lens, which has fome confiftence and is very transparent. The gills, which in more advanced tadpoles, appear on the outside of the body, are as yet lying under the integuments of the thorax.

On the thirty-fifth day the viscera are the fame, but larger and more firm. The liquor of the gall-bladder is ftill transparent, but by this time fomewhat bitter. The arteries and veins are of a deeper red, and by confequence more confpicuous. The cranium and vertebræ begin to turn cartilaginous, and the brain,

brain, spinal marrow, and nerves are not so gelatinous.

On the forty-sixth day, these several parts are further unfolded and firmer. The bile is bitterer, and the rudiments of the hind legs begin to appear. Somewhat too of the fore legs may be discerned, but they are as yet buried under the integuments of the thorax.

The fore legs do not appear till fifteen or twenty days afterwards; for the time varies in tadpoles that are brought forth at once. In other respects the structure continues the same.

Nor does it essentially vary during the subsequent days, when the legs being unfolded, and the tail growing shorter, and being at length obliterated, the tadpole puts on the form of the frog.

This happens about the eightieth day, when the old integuments come away. And now the animal becomes a true frog, differing from the adult only in size. It does not, however, differ from the tadpole, but has the same organs and viscera, the same system of arteries and veins and nerves, the same conformation of bones, with numberless other parts which I shall not describe, lest the reader should find me tedious. These new observations confirm the identity be-

tween the tadpole and frog; they demonstrate that Dr. Pirri, in combating this supposition, has contended against truth. If any remaining affection for this harmless mistake should suggest to him, that the gills and tail of the tadpole are not to be found in the frog, and that the frog has four legs, while the tadpole has at first none, let him recollect, that the chicken, at its first appearance within the egg, has the shape of a worm with a large head and long tail; that the heart has afterwards the form of a half ring; that incubation continues some time before the legs and wings shoot, and that it loses the umbilical cord when it breaks the egg (*a*). And yet, notwithstanding all these appearances of metamorphosis, no one, I believe, has ever imagined the pullet in the egg, and the cock to be different animals. And so much concerning Dr. Pirri's objections to what I have advanced.

cxv. Let us proceed to that which he has brought against the discovery of Haller. Desirous of knowing the sentiments of this great physiologist, between whom and myself there had subsisted a friendship of long continuance, I sent him Dr. Pirri's book, but it came to his hands at a time when he

(*a*) Haller. Form. du Poulet.

was oppressed with those maladies which were soon to destroy him. Instead, therefore, of writing his sentiments at length, he replied in these laconic terms, Berne Nov. 5, 1777, “ Je vous abandonne ce M. Pirri; il est en bonnes mains, vous sçavez assez defendre la bonne cause de la Nature. Il est toujours temeraire d’attaquer des experiences par des raisonnemens.” Thus he imposed upon me the task of answering Dr. Pirri, and I would certainly have undertaken it, if upon more maturely weighing the objection, and comparing it with Haller’s discovery, I had not perceived, that I might safely defer it at least, if not entirely decline it, without offending the objector himself. I would, therefore, beg the ingenious naturalist to read the relation of the discovery over again with greater attention; for from his extract and reply, he appears to have overlooked more than one essential circumstance. This will become evident, if we compare the expressions of the original with those of the extract.

“ The yolk of the egg, concludes Haller, from his observations on the chicken, is a continuation of the intestines of the chicken; the internal coat of the yolk is continued with the internal coat of the small intestines and of

the stomach, and pharynx, and with the skin and cuticle. The external coat is the external coat of the intestine; it is continued with the mesentery and peritonæum. The cover which encloses the yolk, towards the end of incubation, is the skin itself of the fetus." He then reasons thus, "if the yolk is continued with the skin and intestines of the fetus, it must have co-existed with the fetus, and is really a part of it. But the yolk existed in the female, independently of any commerce with the male; therefore the fetus itself also pre-existed."

The same great writer expresses himself with still greater precision in his physiology. B. xxix. Sect 2. Denique directa demonstratio adest, quâ ostendes, certe in avibus, pullum in matre fuisse. Pulli enim intestinum continuatur cum vitelli involucro & adeo intestini interior membrana cum epidermide animalis, exterior cum cute; denique cum involucro vitelli eadem est.

Dr. Pirri uses the following terms, "Haller having shewn that the membrane of the yolk, which pre-exists in the impregnated egg, is transformed by incubation into the small intestines of the chicken, concludes that the chicken pre-exists in the unimpregnated eggs."

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Let the Roman physician determine concerning the fidelity of his own copy. To this he adds another fact taken also from Haller, and endeavours to confute him. But the author did not consider the latter as of equal weight with the former, which, therefore, ought to have been faithfully quoted.

I am aware, that these strictures cannot be pleasing to Dr. Pirri, more especially those which shew his inaccuracy in relating the discoveries of others. I would, therefore, willingly have omitted them, as well on account of the real esteem I entertain for his merit, as some degree of friendship I have contracted with him since the publication of his book; in consequence of which, I have been studiously mild in my expressions, and have opposed things and not words to him. But, when I was treating this subject, to omit them entirely was impossible. I will moreover observe, that I do not consider these inaccuracies as intentional, but as proceeding from want of reflection, or rather of leisure; for he confesses, that he composed his considerations on the reproduction of organized bodies in a few days. I cannot, therefore, but urge him afresh to examine these two facts, which prove, that the fetus belongs entirely to the female, at greater leisure, and

with deeper attention; I wish that to these he would add the further proofs I have adduced in this and the following dissertation. That he may be a good judge of these facts, he must moreover allow me to say, that he ought to be in possession of the difficult art of making observations and experiments with skill. He may then repeat those which I have made, and will be enabled to give his opinion concerning them with greater freedom and safety.

CXVI. The author likewise disapproves of Mr. Bonnet's doctrine concerning the involution of germs, though he does not directly attack it; but declares that he does not subscribe to it; at which I do not wonder, because the opinion of every one is free, and Dr. Pirri manifests too great a partiality for the Epigenesis of the illustrious Buffon. I was, however, not a little surprized, when I came to read in page 32, a passage of the Genevese philosopher; whence it would appear, that he is himself an opposer of the system of involution. My surprize was increased, when I learned that the evolution of bodies is, by the confession of Mr. Bonnet himself, deduced from equivocal facts, and from premises that by no means lead directly to any such conclusion. I was instigated, by the
desire

desire of knowing how my illustrious friend would reconcile these contradictions, to write to him at Geneva, making him at the same time acquainted with Dr. Pirri's sentiments concerning my discovery, and the organic molecules of Buffon. I did not long wait for an answer; which, as the author desired me to publish it, I shall transcribe in this place.

Genthod near Geneva.

Nov. 29, 1777.

“ cxvii. I was not acquainted with Dr. Pirri's book. The short account you give me of it surprizes me exceedingly. Is it possible that the eighteenth century could have produced a writer capable of asserting, that the tadpole and frog are two essentially different animals? Can this writer ever have read Swammerdam? And yet how can he treat of frogs without reading, or at least occasionally consulting him? Was not your illustrious countryman Vallisneri, whom he must doubtless have seen, sufficient to shew him the falsehood of his opinion? To me, this strange assertion of Dr. Pirri appears altogether incomprehensible. It is probable, that some secret interest has led him into this
mistake.

mistake. An opinion so singular, does not deserve to be very anxiously confuted. Every naturalist will excuse you from taking much pains for that purpose.

“ You inform me that Dr. Pirri declares, that my reflections on organized bodies have not persuaded him of the pre-existence of germs. I am not surprized that a naturalist, who believes the tadpole and frog to be two distinct animals, should not be satisfied with my proofs. I should be much astonished if he were.

Your partizan of Epigenesis will surprize those who have not sufficiently reflected on the influence of opinion. He owns on the one hand that you have fully proved the organic molecules of Buffon to be true animalcules, yet on the other he maintains that such molecules exist, though they are invisible. If they are not perceptible, how does he know that they exist; you tell me that he deduces this conclusion from consequences. I ought to be made acquainted with these consequences if I am to judge of their force. But the reasoning of this author does not incline me to think favourably of his skill in logic.

A mistake in reasoning may indeed easily be forgiven, but want of accuracy and fidelity in quoting authors, is not so readily to be excused.

cused. He attacks the involution of bodies, by mutilating a passage of my *Corps organisés*; he might doubt of his cause from this alone, if he could form a proper judgment of his own conduct. I well know the arguments, says he, usually adduced in proof of the possible tenuity of matter; nor am I unacquainted with the geometrical demonstration of its infinite divisibility. But I also know that these are mere illusions, to surprize the imagination, and cloud the reason, as Mr. Bonnet has ingenuously confessed, Art. cxxvii. of his *Corps organisés*, where he thus expresses himself concerning involution. “The infinite divisibility of matter by which involution, or the hypothesis, which supposes one germ to be contained within another is supported, is a mathematical truth and physical falsehood. Every body is necessarily finite; all its parts are of necessity fixed and determined.”--- Who would not conclude from this extract, that I was combating the doctrine of involution? Yet this is the very passage in which I endeavour to prove its possibility.

Dr. Pirri, in order to persuade his readers that I concur with him in opinion, dexterously separates four lines from the paragraph, suppresses all the rest, and then praises my ingenuousness: I am sorry I cannot return
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the compliment, but the truth is, that he makes me assert just the contrary of what I was endeavouring to prove: My words are, “ the hypothesis of involution is not without probability, but it is not necessary to suppose an endless involution, which would be an absurdity; the infinite divisibility of matter, which might furnish ground for maintaining such an opinion, is a mathematical truth, and a physical falsehood; every body is necessarily finite, all its parts are fixed and determined.” I then proceed thus, “ we are utterly ignorant of the farthest division of which matter is capable; and this ought to prevent us from considering the involution of germs as impossible. We need only open our eyes and look round us, in order to learn that matter has been prodigiously divided. The scale of corporeal beings is the scale of this division. How many times is a mucor contained in the cedar, the mite in the elephant, the aquatic flea in the whale, a grain of sand in the terraqueous globe, and a globule of light in the sun? An ounce of gold can be drawn by human art into a wire, eighty or one hundred leagues in length. The microscope shews us animals, of which several thousands do not equal the smallest grain of dust. An hundred such observations might be mentioned,

tioned, and shall we, without hesitation, pronounce the theory of involution to be absurd?

In the CCCXLII^d paragraph I treat professedly on involution, and transcribe a long passage from Bourguet, in order to destroy the force of those calculations by which the celebrated Hartsoecker affected to overwhelm the imagination. How then could Dr. Pirri fail to perceive, that so palpable a deviation from good faith would discredit his book?

I am yet more astonished at another passage, in which this author has the assurance to assert, that “according to Mr. Bonnet’s own confession, the involution of bodies is a system founded on equivocal facts, and on observations that do not directly lead to any such conclusion.” An assertion so pointed, and at the same time so false, can impose on those only who have never read me; for who among my readers does not know that I have ever regarded the evolution of organized bodies, as established upon the most unequivocal facts, and the most conclusive observations? All my works are full of the doctrine of the evolution of organized bodies; no author has said more concerning it, or endeavoured to confirm it by stronger proofs. It seems morally impossible, that Dr. Pirri could have continued in his mistake a single moment in a
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matter so evident; and therefore since he puts into my mouth a proposition, which he knows to be repugnant to my way of thinking on this subject, I may safely conclude, that his book was not dictated by the pure and dispassionate love of truth. But I have bestowed too much attention on an author so regardless of public esteem, as to expose himself voluntarily to the heavy charges of suppression and bad faith. I think you should but just mention his work, for if you confute him at length, you will confer upon him a degree of celebrity, to which he is by no means intitled."

END of DISSERTATION I.

A DIS-

D I S S E R T A T I O N

CONCERNING THE
ARTIFICIAL FECUNDATION OF
CERTAIN ANIMALS.

C H A P. I.

ARTIFICIAL FECUNDATION OF THE TER-
RESTRIAL TOAD WITH RED EYES AND
DORSAL TUBERCLES.

CXVIII. **T**HE first attempt to effect artificial fecundation was made by my immortal countryman, Malpighi; having taken the eggs out of the ovaria of the moth produced from the silk-worm, he moistened them with the seminal liquor of the male. The event did not indeed correspond to the wishes of the curious naturalist, for the eggs proved barren. The learned Bibiena, formerly professor at Bologna, was in like manner disappointed when he repeated and varied the experiments of his illustrious fellow-citizen (*a*). Such a project, however, though it fail in moths, does not seem un-

(*a*) Act. Acad. Bon. T. 5. Part I.

likely

likely to succeed in those animals in which fecundation takes place without the body of the female, as in frogs and toads. Hence my celebrated friend Mr. Bonnet, ever since I communicated to him in 1767, my discovery of the pre-existence of the germ in the frogs, has been constantly urging me to attempt the artificial fecundation of this animal. And as I conceived that if this experiment should succeed, it would throw new light upon the natural history of animals, and more especially upon generation, I resolved to undertake it. When I published my *Prospectus* in 1768, I gave an intimation of my design. Other occupations, however, prevented me from carrying it into execution till the spring of 1777, and the present year (1780) in the course of which I enjoyed more leisure. The animals mentioned in the five first chapters of the preceding dissertation, were the subjects of my experiments.

CXIX. I began with the terrestrial toad with red eyes and dorsal tubercles, which begins earliest in the spring to propagate its species. I have already observed, that the female, with the male on her back, discharges slowly at the anus two shining viscid cords, full of black globules. These globules are minute tadpoles, which the male fecundates at the time
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of expulsion, by besprinkling them with semen (XLV, XLIX, L, LI, LVII). As therefore the tadpoles are at this period best disposed for fecundation, I tried to effect it in the following manner. Just before parturition, of which I was apprized by the excessive swelling of the belly, I parted the male from the female, and set the latter by herself, in a vessel full of water. In a few hours the two cords began to appear; as soon as about the length of a foot was excluded, I cut them off, and left one in the vessel, while I took out the other, in order to wet it with semen, which I procured from the male that had been just separated from the female. It is easy for any one who has the slightest skill in comparative anatomy, to find the seminal vesicles. In this animal, they lie below the testicles, and cover part of the kidneys. At the time of coupling, they are always full. I laid open the vesicles, and receiving the liquor which had the transparency of water, into a watch-glass, I spread it on the piece of cord with a pencil; but the quantity was only sufficient to go over two-thirds; after the operation, I placed this piece in a vessel of the same water as that in which the unimpregnated portion lay. This experiment was made on the 16th of March; as the weather

was rather cold, and consequently unfavourable to the evolution of the tadpoles, I was forced to wait the longer in suspense for the event, about which I was not a little anxious. I examined the cords with the closest attention several times a day, without perceiving the smallest difference for the first five days. In both the mucus was equally enlarged, as well as the tadpoles: and they retained their globular shape. It was not till the sixth day that I began to conceive hopes, that the application of the seminal liquor had not been ineffectual. Many of the tadpoles comprehended in the two-thirds of the cord, over which the pencil had passed, began to assume an elongated figure, while the others preserved their round form, as did also those of the piece of cord left in the other vessel. The seventh day was still more favourable to my hopes; for together with a manifest elongation, an increase in bulk became visible; these appearances grew more evident every day, in so much that there no longer remained any doubt of a considerable evolution of the tadpoles. On the eleventh day, I perceived them moving within the amnion, and on the thirteenth they quitted the membranes, and swam about the water. On the other hand, the unimpregnated tadpoles began to corrupt,

and

and in time they were quite decomposed, and turned putrid. When I had thus called into life a number of animals, by imitating the means employed by nature, the reader will conceive the satisfaction I received from the success of an experiment so precarious and uncertain. He will easily imagine, that I was not disposed to stop at my first discovery, but that I determined to repeat and vary my trials, in order to deduce such consequences as might illustrate the subject.

cxx. All the tadpoles in the portion of cord that was wetted with semen, were not evolved. They were in all a hundred and seventy-six, of which sixty-three spoiled. This probably arose from their not having been touched by the fecundating fluid. I now determined to repeat the experiment upon another piece of cord, which a female just separated from the male, was about to discharge. As this piece was only five inches long, I could bathe it completely with the seed of two males. I may here remark, that if we wish to find the vesicles full, we must open them at the time of copulation. The largest quantity of seminal liquor afforded by a single male, is generally two grains, though it sometimes amounts to almost three. When the males are not mounted upon the females,

there is either no fluid at all in these reservoirs, or only a very inconsiderable quantity: they are indeed so much shrunk, that there is some difficulty in finding them. But to return to my subject; I found that a greater quantity of semen caused more tadpoles to be evolved. The season being now a little further advanced than at the time of my first experiment (CXVIII), the tadpoles began on the fifth day to take on an elongated shape, on the tenth they shewed manifest tokens of animation, and on the eleventh were swimming about the water. They were in all an hundred and seven, out of which number eight only failed; whether it was because they had not been impregnated, or more probably because they were vitiated. In the natural process, among a great number of tadpoles, a few always spoil.

CXXI. In these two successful experiments (CXIX, CXX), the cords had been discharged by the female into water. Natural fecundation always takes place in this element. But I had learned from observation, that it succeeds just as well when the animals are removed to a dry place. It was thus, that I was fortunate enough to discover the natural mode of impregnation (XLVII, XLVIII, XLIX). I therefore supposed, that artificial fecundation would succeed in the same circumstances;
a long

a long portion of cord discharged in a dry vessel was moistened with seminal fluid, and then put into water, along with another piece brought forth by the same female, but not impregnated. In twelve days the tadpoles of the latter were half putrid, while those of the former were evolved, and swimming about the water. While employed about these experiments, I was careful to observe whether artificial fecundation is slower in its effects than natural. Having two toads coupled, I waited till the female had discharged part of the cords, and the male had besprinkled it with semen. I then removed the male, and cutting off the cords, close to the anus, left them in the water. As soon as the female had discharged another portion of cord, (which happened in a quarter of an hour) I cut it off, and impregnated it with the semen remaining in the vesicles of the male; and this piece was put into the same vessel with that which had been naturally fecundated, that it might be seen which would soonest produce complete tadpoles. But evolution and animation kept an equal pace in both, an observation which I afterwards saw confirmed both with respect to toads and frogs.

CXXII. My attempts to produce fecundation by artificial means, have hitherto been

made, only when the tadpoles had arrived at the place destined by nature for impregnation and evolution. But suppose it was attempted within the body. We know that they are at first lodged in the ovaria, then pass along the oviducts, and at last get into the uterus. Will artificial fecundation succeed alike in these three receptacles? These reflections stimulated my curiosity, and I believe I have now data sufficient for the solution of the problem. I began with the uterus. This organ is divided by a membranous partition into two cells, which are quite filled with tadpoles, as soon as they have passed through the oviducts. They are inclosed in the glutinous cords, which are very much entangled; it is not, however, difficult to draw them entire out of the uterus with a forceps, if they are managed gently. I opened the abdomen of several coupled females when the cords began to be excluded, for at this time the uterus is always full, and disentangling part of the mass which lies in this viscus, I bathed it with seed, and set it in a vessel of water. The rest of the cords was put at the same time into another vessel of water; but not one tadpole contained on the latter portion was evolved, whereas all those which had been impregnated became complete tadpoles. It is therefore

to be inferred that these germs, by the time they get into the uterus, are arrived at such a state of maturity, as renders them capable of being fecundated.

While I was engaged in these experiments, I was eye-witness of an accident that deserves to be mentioned. Having often observed the seminal liquor of the toad, I found it very full of spermatic worms, which, like those of the frog, have an oblong shape, and writhe their body as they move. Upon two occasions I have been greatly surprized at finding this fluid totally destitute of such inhabitants. I was induced to try, whether it is also destitute of fecundating virtue, but I found that it was just as effectual in this respect, as that which most abounds with these diminutive animals.

CXXIII. I next proceeded to try whether I could impregnate those germs, which in coupled females are often to be found in the oviducts. I succeeded upon many of the largest, those which lie nearest the uterus, but those which were situated in the narrow part, in the vicinity of the heart, where the other extremity of the oviducts is placed, all baffled my attempts. These different results may perhaps be thus explained. The gluten in which the tadpoles are imbedded, and which forms the two long cords, is produced as the

germs pass through the oviducts, for they are totally destitute of it before they enter into these canals. Those tadpoles therefore which have passed along the greater part of the oviducts, and of course are nearest to the uterus, will be surrounded by moist gluten. This substance has been appointed by nature for the first nourishment of the fetuses. If therefore they happen to be provided with a sufficient supply, as those are which lie nearest to the uterus, the asperision of seed will not be ineffectual; but the contrary will happen with respect to those which have little or no gluten; for should they be animated by the act of fecundation, they will soon perish from want of nourishment. The necessity of gluten during the first period of evolution, is evident from another experiment, in which the tadpoles having been entirely stripped of it, were wetted with semen, but they came to nothing; nay, very few of those were evolved from which part only was taken. Upon opening one day a female during copulation, I found the tadpoles in the abdomen, instead of the uterus and oviducts. Their colour was black, as it is when they are arrived at a state of maturity, but they were without gluten, having never entered the oviducts. These fetuses, after being moistened with seminal liquor

liquor were not evolved, as the reader will naturally suppose.

cxxiv. For the same reason those taken from the ovarium would not grow. This viscus is divided into two large lobes, each of which consists of smaller lobes. All are full of little tadpoles, till the season of amours arrives, when they separate from their stalks, in order to go whither nature has destined them. I besprinkled many of those tadpoles with seed when they were about to quit the ovarium, but in vain. This experiment suggested a curious idea. I had found that the abdomen of female toads, and this is also true of frogs, may be opened, not only without immediately destroying them, but frequently without preventing them from bringing forth their young, which also are afterwards evolved, either in consequence of natural or artificial fecundation. It is only necessary, that the female thus maltreated should be kept in the dry; otherwise the water will enter in at the wound, and occasion death before parturition takes place. Now what will happen, thought I, if the abdomen of a female should be opened, and the seminal fluid be spread upon the ovarium? May it be expected that the tadpoles, after quitting this viscus, passing through the oviducts and uterus,

rus, and being discharged at the anus, will grow? Though I had little expectation from the experiment, I was determined to perform it. Having with this view perforated the abdomen of two females, I threw a drop of seed upon each ovarium from a syringe introduced through the wound. I moreover made a perforation in the membrane that invests the ovarium, and injected another drop of seed, which must have come into immediate contact with the tadpoles. One of the toads died five hours after the operation with the tadpoles in the ovarium, but the other discharged part of them at the anus imbedded in gluten, as if she had been in a state of the most perfect health. I kept them very carefully in water, but not one came to perfection; whence I was obliged to infer, that the gluten is so indispensably necessary for the nutrition of these delicate beings, that should it not be present at the time of aspersions with seed, they will perish, even though it should be afterwards supplied.

cxxv. Having established upon a number of decisive trials, the possibility of artificial fecundation with semen (cxix, cxx, cxxi, cxxii, cxxiii), I thought of trying whether the expressed juice of the testicles would answer the same end. I supposed this not unlikely,

unlikely, the testicles being the organs in which the seed is formed, or rather perfected and rendered fit for fecundation; but on account of their small size in this animal, I could not hope to obtain it pure: and my only resource was therefore to squeeze out the juice, and use this for my experiments. The testicles in the present species of toad are situated at the upper part of the kidneys; they have a yellowish colour and an oval shape, but are a little gibbous on one side, like those of the cock. At the season of generation they contain a brown, dense, and somewhat viscid liquor. Having collected a little of this liquor in a watch-glass, I moistened with it about an hundred tadpoles, part taken out of the uterus, and part discharged by the female, after the male had been removed. I succeeded perfectly; the tadpoles grew, and I frequently repeated the experiment with equal success. Upon making a comparison between the efficacy of the seed, and the juice of the testicles, I found that both fluids produce their effects in the same time; but the latter did not cause the evolution of so great a number as the former, though I did not then know whether this arose from its inferior activity, or rather from its density, a quality that prevents its diffusion over an equal extent of surface.

CXXVI. In May 1780, while I was composing the present chapter, the fishermen brought me a species of toad, concerning the generation of which I have said nothing in the preceding dissertation, for I was totally unacquainted with it; I shall here give a short description of it, as it relates to the present subject. This species is smaller than the fetid terrestrial toad, but is of the same colour, if we except the belly, which is of a brighter white. But the diversity of manners and organization shews, that the two species are totally different. The skin upon the back of the fetid toad is rough like shagreen, the body is rather long, and the animal, like frogs, moves by taking long leaps. The other has a smooth skin, a thick short body, and jumps a very little way. The male of the former species utters a sound resembling a man's whistle. The croaking of the latter is very low and indistinct, a clear proof that the organs of the voice are very differently constructed. The structure of the parts of generation also differs; the female of the fetid toad brings forth two cords, but that of the other only one; the former derives its name from its bad smell, but the odour of the latter is at most not more offensive than that of garlic. The male of the fetid toad throws his arms round
the

the thorax of the female, but that of the new species clasps the abdomen. Hence it is apparent, that this toad ought not to be confounded either with that with which I have been contrasting it, nor with the *Bufo igneus* of Roefel, in which the lower part of the body is ornamented with flame-coloured spots (CCCCLXIV); for in the species in question, not a vestige of such spots is to be perceived.

The first that fell into my hands was coupled, and the female was discharging her cords. I had here an opportunity of admiring the curious spectacle described by Demours (CII). When they were placed upon my hand, they seemed at first a little timid, but soon afterwards the female continued her discharge, though I did not observe that the male used his hind legs to extract the cord. I saw, however, what he did not observe, I mean, that the male sprinkled the tadpoles from time to time with semen emitted from the anus, which in this species is terminated by a point. I watched this phænomenon for about thirteen minutes, and then resolved to open both the male and female, for the sake of the following experiments. Having extracted the remainder of the cord, which measured about a yard and a half, I divided it into four portions: One was left untouched, the second was

was moistened with the seed remaining in the vesicles of the male; the third with juice expressed from the testicles; and the last with two drops of the seed with which the male was impregnating the cord as it was excluded, which I had intercepted in a watch-glass. These four pieces were set in separate vessels of water, and in a fifth was placed that piece of cord which had been already fecundated. The result was what I imagined it would be, from former observations. Except the untouched piece of cord, all the rest were animated with a number of tadpoles; whence we may draw two inferences, first, that in this fresh species fecundation is also external; and secondly, that it may be effected by art, either by means of the feminal fluid, or the juice of the testicles. Four other couples were afterwards brought to me, two of which served to confirm the foregoing observations, and the two others for experiments, which I shall relate in another place.

CXXVII. I shall finish the present chapter, with a short digression concerning the mode of propagation in this new species. The cord, both in the oviducts and uterus, is full of black globules, which whether examined internally or externally, exhibit characters perfectly resembling those of the tadpoles of
other

other species, when they lie in the oviducts or uterus. If the impregnated globules be compared with the unimpregnated, they will likewise be found to bear an exact resemblance both internal and external. But in time the latter become white, the surface grows wrinkled, and chopped in various places; the water dissolves them, and they at last fall to pieces, and are quite decomposed. The appearance of the fecundated globules changes also in about a day, but in a manner totally different. Upon the surface may be perceived two furrows, which meet to form an angle. Above the angle a prominent filament is seen, and at the same time the globule increases in bulk and length, while the end grows finer. During the following hours, the furrows become deeper, the filament projects further, and the point grows longer; on each side of the furrows arise two small tumours, which before were not visible. In about a day longer, the mystery concealed under these parts, which are gradually unfolded, is laid open. We find that the two furrows are the marks of the mouth, that the prominent filament is the spine, that the pointed end is the tail, and the two tumours the gills. All these parts become more distinct, when the young animals begin to move about the water. It appears

pears therefore, from these observations, that the same law which presides over the birth and evolution of the species mentioned in the preceding dissertation, extends likewise to this, that the fetus belongs to the female, and that the asperision of the seed of the male is a condition necessary to the animation and evolution of the fetus.

CHAP.

C H A P. II.

ARTIFICIAL FECUNDATION OF THE WATER-NEWT, AND THE FETID TERRESTRIAL TOAD.

CXXVIII. **A**S the fecundation of the water-newt does not take place without the body (CLXXX, CLXXXI, CLXXXIV), as in frogs and toads, I could not try to impregnate the fetuses, after they have been discharged by the female, but was obliged to seek some other means of accomplishing my intention. The female newt has no uterus; hence the fetuses, after quitting the ovarium, pass along the oviducts into the rectum, and are discharged at the vent. The seed of the male insinuates itself into this aperture, and fecundates the fetuses that lie nearest to it. I have moreover shewn in the eighty-fourth paragraph, that the more remote fetuses, those which are situated higher in the oviducts, are not then impregnated; and that before this can happen to them, they must come down lower, and take the place of those which have been impregnated and discharged. Those

therefore which occupy the oviducts, appeared to be the proper subjects of my experiments; but my attempts to fecundate them did not succeed. I proceeded in this manner; having laid open the oviducts of a female, and the *vasa deferentia* of a male, I took out the seed, which in whiteness resembles milk, and moistened the fetuses lying high in the ducts; I then put them in water without delay. Like those which are brought forth naturally, they sunk to the bottom, where they were held by the tenacious gluten that invests them, but not one grew. The gluten gradually quitted them, they burst, and were dissolved. I shall not relate the many frequent repetitions and variations of this experiment, how I altered the dose of seed, sometimes letting fall a few drops upon the fetuses, sometimes bathing them sparingly, and at others immerging them in it, and as it were saturating them, but still to no purpose. Their situation in the oviducts made me apprehend, that they perished for want of a sufficient quantity of gluten, as is the case with tadpoles taken out of the upper part of the oviduct (CXXIII), and at the same time fearing on the other hand, lest, if I took them from a lower situation, they should be impregnated by the male, I had recourse to an expedient which I thought would

would determine whether my apprehensions were just. I was then (it was in April) in possession of seven female salamanders, which I had kept all the winter in water, apart from any males; notwithstanding this, they were beginning to discharge their fetuses. I thought, therefore, of trying to fecundate these fetuses, being certain that on the one hand they were invested with a sufficient quantity of gluten, and that on the other they had not been impregnated. I have before remarked, that the males during the season of their amours, will emit their seed, if the belly be gently pressed, as also the females their young (LXXX, LXXXIV). By this easy method, I got these female newts to bring forth their young, and as they came away, took care to moisten them with seed. But notwithstanding this expedient, I never saw one of them grow, though I carefully kept them in water.

cxxix. Mortified at so many disappointments, I was about to relinquish the attempt, under a persuasion that artificial fecundation would not succeed on newts, when a doubt suggested itself, whether I had not neglected a precaution essential to the success of these experiments. In another place I have remarked, that the male fecundates the female,

not by injecting his seed immediately into her, but by throwing it into the water, whence it passes into the vent of the female, and impregnates all the fetuses that lie near the rectum (LXXX, LXXXI). It is therefore evident that the seed, when it produces its effect, is not pure, but diluted in water. Hence I was led to suspect that pure seed, which I had used in my experiments, was not fit for fecundation, (perhaps on account of its too great density) but that it must be diluted in water; I tried this method, and obtained results very different from those which have been just related. By gentle pressure upon the bellies of the seven females (CXXVIII) that had been kept by themselves, I procured the discharge of twenty-seven fetuses. I bathed them all with water in which a small portion of seed had been diffused. Seventeen perished, the remaining ten did well, and during their evolution presented the appearances that are consequent upon natural fecundation (LXXXVII, LXXXVIII, LXXXIX). By imitating Nature therefore in this circumstance, I succeeded in fecundating this amphibious animal by art, as well on the present as on other occasions; I lost indeed many fetuses, but this could not surprize me, since
many

many perish also after the natural impregnation (LXXXIX).

The juice expressed out of the testicles diluted in water, answers the purpose of fecundation, though in this case too, about one-third only of the fetuses is evolved. This failure, induced me to desist from the prosecution of my experiments on this animal for the present. The fetid terrestrial toad next engaged my attention, and indeed fully answered my expectation.

cxxx. We have seen, that in the terrestrial toad with red eyes and dorsal tubercles, artificial fecundation succeeds not only on the tadpoles that have been discharged, but also in those which lie in the uterus, and at the inferior extremity of the oviducts. We have moreover seen, that it may be effected either by the seed, or the juice of the testicles. I can assure the reader, that the result of these experiments repeated on the fetid toad, was exactly the same.

But the great quantity of this species with which the fishermen supplied me, the abundance of tadpoles contained in the uterus, and the facility with which they may be fecundated, led me to attempt something besides the bare confirmation of my preceding observations. The discovery of the means of

calling forth these animals into existence, without the co-operation of the male, was an experiment pregnant with many others, which I was the more tempted to undertake, since the harvest had not yet been touched by the sickle of philosophy.

One of my principal views was, to ascertain what would be the consequence of trying to force Nature out of her ordinary path, either by adulterating the seed, and the juice of the testicles, or by attempting to produce changes in the tadpoles. Such an investigation promised to afford much instruction.

In the preceding experiments, I had used the seed, and the juice of the testicles, immediately after it was taken from the male. The animal was fixed on his back, by means of nails driven through his hands and feet; after opening the abdomen, I sought for the seminal vesicles, which, when the intestines are removed, appear behind the bladder, under the shape of two tumours; if the bladder be evacuated, a better view of the vesicles is obtained, at the same time the testicles come into sight; in this species, they are of a livid brown colour, and lie upon the kidneys. After having made this disposition, I took, as my design led me, either the seed, or the juice of the testicles, and bathed the tadpoles,

poles. I now thought of trying these fluids some time after the death of the animal. In another publication I have observed, that frogs, toads, and other animals very tenacious of life, may be instantaneously killed by insinuating a sharp instrument between two of the cervical vertebræ, and destroying a little of the spinal marrow. They fall instantly into convulsions, and cease to live; an event that does not follow either decapitation, or any other treatment. Three hours after I had killed a fetid toad in this manner, I opened the abdomen, and searched for the seminal vesicles, which were a little collapsed, not now containing so much seed as when the animal is alive and coupled. With this seed I bathed some tadpoles taken out of the uterus, whence all that were used in the experiments related in this chapter were taken. The event shewed, that the seed had not suffered by a continuance of three hours in the dead body. The same observation is applicable to another toad which had been killed five hours and a half; the seed indeed being in very small quantity, could not be spread over many tadpoles. In the course of seven hours after death, the vesicles were so much shrivelled, that I could scarce get a drop of seed, which however retained its efficacy.

CXXXI. Two immediate consequences may be deduced from these experiments; it appears in the first place, that after death the quantity of seed in the seminal vesicles gradually diminishes till it quite disappears, agreeably to the laws of the animal œconomy; and in the second, that this fluid, notwithstanding its waste, loses nothing of its virtue, at least in seven hours. But does it retain its prolific power any longer after the cessation of life? As dead toads would not, for the reason already assigned, enable me to solve this problem, there remained no other expedient than to take the seed out of the vesicles, and keep it in a phial or tube. Having procured all that these toads would furnish, I put it in a little glass tube, and used it at different times. Six hours did not impair its efficacy, but after the seventh hour it seemed to be weaker, for only two-thirds of the tadpoles touched with it came to perfection. The eighth hour produced worse effects, and the ninth proved so hurtful, that out of sixty-five tadpoles, not one was evolved. The reader is not, however, to suppose, that this is always the exact measure of the time in which the seed becomes effete. By many experiments I have been taught, that this depends on the temperature of the atmosphere. In hot weather,
seed

seed taken out of its natural receptacles, becomes in six hours and a half unfit for fecundation. But in a cold season, it retained somewhat of its virtue eleven hours and upwards. The frequent alternations of heat and cold during last May, (1780) afforded me many opportunities of observing these different phænomena. Finding that cold favours the retention of the fecundating virtue, I thought of carrying the experiment further, by placing some seed in an ice-house. It now retained its efficacy twenty-five hours. When I reflect upon the nature of this liquor, it does not seem difficult to account for the effect of heat and cold. The seminal fluid, like other animal substances, is subject to putrefaction; now putrefaction is promoted by heat, and retarded by cold. In my second *Essay concerning the natural History of Animals and Vegetables*, I have spoken at large on the tendency of extravasated seed to become putrid, whence it loses the spermatic worms, its natural inhabitants, and acquires others of a very different kind, resembling the animalcules of infusions. The seed of toads runs sooner or later into the putrefactive fermentation, and on this account probably becomes effete, sooner when the atmosphere is warm, than when it is cold.

I have

I have said before, that in hot weather the seed in six hours and a half is rendered unfit for its natural use. I found that in this case, the thermometer stands $18\frac{1}{2}$ (*a*) deg. above the freezing point. This suggested to me the idea of trying what would happen to this fluid, if it should be kept for a short time in a higher temperature. From several trials it appeared, that the heat of 30 (*b*), and even 32 (*b*) deg. was not prejudicial, when the seed is subjected to it for two minutes only; but at the 35th (*b*) deg. it was quite spoiled.

CXXXII. But it is time to turn the reader's attention to the experiments on the juice of the testicles, the second head of the first enquiry proposed in the cxxxth paragraph; as before, I left the testicles in the dead animal; but the event was very different. In the vesicles the quantity of seed was so much diminished, that in seven hours there scarce remained enough for one experiment. The testicles on the other hand of the same individual preserved their juice sufficiently, and it was fit for fecundation. But that which was expressed from the testicles of another male left dead for a day, was in sufficient quantity,

(*a*) About 73 or 74 of Far. thermometer. (*b*) $30=99\frac{1}{2}$,
 $32=104$, $35=110\frac{3}{4}$, nearly of the same scale.

but

but altogether effete. In the prosecution of this enquiry, I found, that the continuance of the efficacy of this juice is connected with the temperature of the atmosphere, or, to speak more properly, with the time in which it began to grow putrid, as happens with respect to seed also (cxxxI). Hence in warm weather, the testicles afford nothing fit for impregnation after nine or ten hours have elapsed; whereas when kept in an ice-house, their juice is efficacious after thirty-four hours. It appears, however, from a comparison of this with the preceding paragraph, that in the same temperature this juice retains its virtue longer. Does this arise from its not becoming putrid so soon, at least when it remains in the testicles? I incline to this opinion, though I have not made any direct experiments with this view.

cxxxIII. The testicles of the toad are so small, that when exposed to the air, they become shrivelled and dry, sooner than they putrify. In the mean time they do not lose their virtue, but the juice, as long as they retain any, is fit for fecundation.

As they dry, they grow hard, and assume the appearance of thin leather; when put into water, they resume their former plumpness. When pressed, they appear to be full
of

of juice, but it will not cause the evolution of germs. And this is by no means matter of surprize, since the better and more active part of it, must evaporate during desiccation.

The same effect is produced by excessive heat, as when the testicles are kept a few minutes in air or water warmed, to about the thirty-fifth degree.

If we combine the facts related in this and the foregoing paragraph, with those of the cxxxth and cxxxist, it will appear, that the same causes are destructive of the qualities of the seed, and the juice of the testicles. Indeed it cannot be otherwise, since the latter fluid consists chiefly of the former; the same worms inhabit both. Hence it may be understood how it came to pass, that when I bathed tadpoles with the juice taken from the testicles of toads long kept by themselves, fecundation did not take place; this was not owing to a deficiency, but to the bad quality of the juice. It was to no purpose to try the juice of the testicles of young toads, before they are proper for generation. The failure arose from the want of seminal fluid, which, as is well known, is not produced either in men or animals before a certain age.

CXXXIV. In the artificial fecundation of both these species of toad, I had used seed or juice of the testicles not adulterated with any other substance. But having proposed in this chapter to enquire into the consequences that would follow the alteration of these liquors, it became me after the trials already instituted, to mix various matters with them. The slight difference between the seed and the juice of the testicles, rendered it probable, as in fact it happened, that their admixture would not prevent fecundation. Water too, though a fluid essentially different, did not seem likely to produce such an effect, since the seed is emitted into water, and necessarily mixed with it; and with respect to the newt, we have seen, that this is a requisite condition to impregnation (cxxxix). In the toad water heightened the prolific power of the seed; but I content myself at present with giving a simple intimation of this fact, and defer to the next chapter a particular account of it, as well as of the important consequences it affords. I here confine myself to other liquors. In opening the seminal vesicles, the seed which when unmixed has the purity and transparency of water, is always adulterated with blood that issues from the incision made in the abdomen: the mixture is then not un-
like

like the washings of flesh; I have used it for bathing a considerable quantity of tadpoles, and have found it just as good as pure feed. Further, blood, after being thus mixed, soon coagulates, and if taken away, leaves the seminal liquor as pure as ever. I have taken up some of the clots, and after rubbing the tadpoles with it, have found that it animates them, on account, no doubt, of the feed that adheres to it. The same remark may be applied to the other fluids of both male and female toads; the bile, the saliva, the juices expressed from the liver, lungs and kidneys, and in short the urine itself, do not prevent the feed from producing its proper effect, notwithstanding the last is thought by many to be poisonous, or at least corrosive and acrimonious. Human urine is not prejudicial, provided it is used in equal quantity. But the number of tadpoles became less when the portion of urine was increased, insomuch that when it was used in double quantity, none were evolved. Vinegar agrees very nearly with urine. Human saliva is innoxious in whatever proportion it is employed. Here I may possibly be asked, whether a real incorporation of these several liquors with the feed, took place: or rather, whether the feed formed a separate and distinct body, as water
poured

poured into oil; in which case it would not be surprizing, though fecundation should ensue? I reply, with great confidence, that the mixture took place in the first of these ways, and never in the second. When I dropped these different liquors into the seed, I was careful to observe what changes happened, and that I might be better enabled to judge I used the microscope. The mixture seemed always to be attended with a real incorporation of the two fluids. When water was used, it appeared as limped and homogeneous as if it had been water alone. When the seed was mixed with black vinegar, the mixture had a blackish colour, and when with white vinegar, it was somewhat less opake. In both cases the acid lost somewhat of its sourness, a certain proof that the seminal fluid, which of itself is insipid, had been diffused through the whole of the vinegar. The same thing took place when urine was tried.

The juice of the testicles after being mixed with the same fluids, lost nothing of its efficacy. And as it tinged these fluids with its own colour, it was reasonable to infer that it was incorporated with them as well as the seed. It appears therefore, that both these prolific liquors preserve their qualities some time after the death of the animal, both when
 pure,

pure, and when by being mixed among other matters they have sustained a considerable division of their integrant parts. But I shall speak more particularly concerning fecundation after a great, nay, almost infinite separation of the particles.

cxxxv. I now come to describe the various changes I have endeavoured to effect on tadpoles before fecundation, which was the other principal object of enquiry. Having killed several females in the way described in the cxxxth paragraph, I left the tadpoles for some time in the uterus. I wished to know how long after the death of the female, the young are capable of being impregnated; eight hours did not prove hurtful to them, nor twelve; but when bathed with seed, after thirteen or fourteen hours had elapsed, the difference became evident, for out of about two hundred, more than eighty perished. To the tadpoles of three toads, sixteen hours continuance in the uterus was fatal, but several taken out of that of a fourth came to perfection. This instance may serve to confirm a maxim adopted by the most able philosophers, according to whom a great multiplicity of trials is not merely expedient, but necessary, when we would establish any proposition. The induration of the tadpoles

poles of the three first toads, the thickening of the mucus and its incipient putrefaction, were indications sufficient to make me apprehensive, lest the application of seed should not be attended with any effect. But I supposed, that in a lower degree of heat the tadpoles might be kept longer, for these experiments were made in June, when the thermometer in the shade stood at 20° --- 22° ·(a). It readily occurred to me, that recourse should be had to an ice-house, in which I had before kept the prolific liquors good for a considerable time (CXXXI, CXXXII).

My supposition was confirmed by experiment. Tadpoles left in the uterus of two females for forty-one hours in this degree of cold, and afterwards moistened with seed, came most of them to perfection. I cannot inform the reader what effect is produced by immersing tadpoles in various liquors, not having had time for making such experiments. The only one which I could try was water; and I should not have imagined, that it would be so detrimental as it really proved to be, considering that the tadpoles are always deposited and live in water. After tadpoles extracted from the uterus of a living toad, have

(a) $20 = 77$, $22 = 81\frac{1}{2}$.

been left four hours in water, the application both of the feminal fluid and the juice of the testicles was quite ineffectual. They all turned putrid in a few days. The same event took place on other occasions, after the tadpoles had lain for three, two, and even one hour in water. This unexpected phænomenon made me desirous of ascertaining the precise time, during which tadpoles may be kept in water without being rendered unfit for fecundation. I found it to be exceedingly short, about thirteen minutes; many kept immersed longer, never came to perfection, and after a continuance of fifteen minutes not one was evolved. This at least was what I observed concerning several hundreds.

But how does water so soon damage these embryos? It is quite contrary to what happens when they are kept in the dry, whether they are left in the uterus, or placed in a vessel; for it is certain, that a continuance of several hours will not under this circumstance prevent the effects produced by fecundation. The best method of investigating the cause of the difference seemed to be, to observe carefully what happens to the tadpoles, and the mucus in which they are imbedded, while they continue in water. It has been already several times observed, that the mucus and
tadpoles

tadpoles constitute two long cords, which the female slowly discharges. The cords at first fall to the bottom, but if the weather be warm they soon rise to the top, and continue to float. This ascension arises from an increase of bulk, which renders the strings specifically lighter than water. The dilatation is obvious to the senses, and if the diameter be taken before they are put into water, and after they have been immersed some time, it will be found to be enlarged. This change must, I think, be imputed to water insinuating itself between the particles of the mucus. And my opinion is confirmed by the two following reasons; the strings, when left in the dry, do not increase in bulk; and if those which have been immersed be squeezed, water will ooze out of them. From these observations, I think we may account for the hindrance water occasions to fecundation. It is obvious, that the seed must traverse the mucus, in order to reach and impregnate the tadpoles. Now it must either insinuate itself into the pores of that substance, or pass along certain ducts provided on purpose by Nature. When these openings, of whatever kind they may be, are free, as in the case when the cords are kept in the dry, the passage of the seed through the mucus will not be interrupted;

errupted; but should they be closed, its ingress will be prevented; this must be the consequence of immersion in water, and thus we come to understand why in a few minutes they are rendered incapable of being fecundated.

CXXXVII. The insuperable obstacle arising from the insinuation of water, has been guarded against by Nature, when fecundation is left to the animals themselves. The male toad, as well as the male frog, eject their seed upon the strings, when they have been only a very short time in water. And I have found, that the male always impregnates those which have been just brought forth, and which consequently have been just subjected to the action of water. Thus as if they could foresee that fecundation would not take place after the tadpoles have lain some time in water, they never besprinkle such with seed. I have put some females in water by themselves during the act of parturition, and after about a yard has been excluded, have admitted an equal number of males. The males assumed their proper station without delay, and began to impregnate the tadpoles lying close to the anus, without paying any regard to the others. I said above, *as if they could foresee*; for it is certain, that these animals

mals have not sagacity to distinguish between those that are fit, and those that are unfit for impregnation. The instinct allotted to them by Nature, prompts them to mount upon the female, and in this situation to impregnate the young. In such a posture, they neither do nor can impregnate any but those situated near the parts of generation; and thus they unwittingly fulfil the great purpose of Nature, the propagation of the species. Hence it is evident, that although fecundation is external both in toads and frogs, as it is commonly supposed to be in fishes, it is not effected when the fetuses are separated from the female, and lie at the bottom of the water. Of this truth, I have had the most ample proof in every species of frog and toad mentioned in these two dissertations; for when many pieces of cord have been put in vessels of water containing males, of which the seminal vesicles were quite turgid, not one fetus out of so large a number ever came to perfection. We must therefore conclude, that not a drop of seed was ever cast upon them, since we have seen that a single drop is sufficient to impregnate great numbers.

C H A P. III.

ARTIFICIAL FECUNDATION OF THE TREE-FROG, AND THE GREEN AQUATIC FROG.

CXXXVIII. I WAS obliged to confine myself to a few experiments on the tree-frog, as well as on the water-newt, not on account of the difficulty of bringing the fetuses to perfection, as was the case with respect to that diminutive quadruped, but on account of the few I possessed when I was engaged in these experiments. All the information therefore which I am able to give the reader, consists in assuring him, that my attempts to produce artificial fecundation were successful with four couples of the tree-frog, both when the seed and the juice of the testicles was employed.

I shall therefore proceed to the green aquatic frog, a species with which its prodigious abundance in the plains about Pavia has enabled me to make as many trials as I could wish. But in the first place, let me dwell a moment upon the description of the parts of generation belonging to the male; those of
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the female, have been already described in the first chapter of the foregoing dissertation.

After the abdomen has been opened, and the intestines removed, the bladder, the seminal vesicles, and the testicles appear in sight. The first of these parts lies very near the anus, and is apparently divided into two lobes, though it has in reality one cavity, for it may be evacuated by opening either lobe. When the bladder is full, it partly covers the seminal vesicles; but when the urine is evacuated, they are seen very distinctly. They lie a little higher than the bladder, and consist of a thin membrane, through which the seed is seen; it has the transparency of water. Each of the vesicles is provided towards the top with a long appendix, which is inserted in the epididymis, and may be called the *vas deferens*. Four testicles have been sometimes observed in the newt, an observation which I have had occasion to verify. But frogs and toads are never so richly provided. In the present species there are two testicles, which have the colour of the yolk of an egg, and are externally granulated like those of toads; they are very full of juice at the season of amours, as are also the seminal vesicles (cxx).

cxxxix. I used both the seed and the juice of the testicles for artificial fecundation. My

first trials consisted in impregnating tadpoles, recently taken out of the uterus of living females, with the recent prolific fluids of the male. I afterwards used these liquors, after they had been kept and were approaching towards putrefaction, upon tadpoles in like circumstances. Lastly, I tried the effects of mixture with different substances. These heads comprehend the experiments related in the foregoing chapters. The event was the same, with the exception of a few slight variations. The prolific liquors of the green aquatic frog preserved their qualities longer than those of toads; but they do not so well resist the action of sudden heat. The seed of toads, exposed to the temperature denoted by 32° for a few minutes, did not lose its virtue (CXXXI), but that of this frog did in my experiments. The prolific liquors of the frog preserve their efficacy when mixed with more human urine than those of the toad will bear. The tadpoles of the frog sooner lose the capability of being fecundated in the uterus of the dead female than those of the toad. Such are the principal differences.

CXL. I now proceed to an account of some new experiments, which I hope will be acceptable to the reader. I had on all occasions either completely covered the mucus
which

which furrounds the fetuses with the prolific liquor, by means of a pencil, or immersed them in a quantity contained in a watch-glass, so that every part was moistened. But is it to be supposed that they must be so exactly covered? The question was of importance, and experiment alone could decide it. The mucus is not drawn out into long cords as in toads (XLV, LXVIII), but is moulded into balls or globules, of which each contains a tadpole. These balls, when taken out of the uterus, may be easily separated. I bathed some all round, others only over one hemisphere, and others over one-third of their surface; and immediately put them into water. The animation of all these tadpoles proved, that it is not necessary to cover more than a part of the mucous spheres with seed. The tadpoles of this species are round; one hemisphere is white, the other black (XIV). Does bathing the mucus which is opposite to the black or the white hemisphere produce fecundation most certainly? Or is this an indifferent circumstance? Experiment shewed, that which ever part is moistened with seed, the tadpoles are evolved alike.

In my last trials I had not bathed a smaller portion than one-third of the surface. But I did not stop here. I reduced the space more
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and more, till there remained not above the point of a pin, and even of a needle, which I dipped into seed, and then brought once into contact with a ball; and yet fecundation as readily followed, as when the globules were covered all over. I was next curious to know whether a quantity of seed so inconsiderable would fecundate the tadpoles lying contiguous to the sphere that I touched. I placed twenty-four globules in twelve watch-glasses full of water; the two globules in each glass lay close to one another, and the viscidty of the gluten kept them in this situation. I then touched, with the point of a needle dipped in seed, one ball in each glass. But as twenty-two tadpoles came to perfection, it appeared that every drop of seed had impregnated two. I repeated this curious experiment with the same quantity of seed, and also with a small addition. In the former case the number of tadpoles that grew to perfection was double of that of the balls that were touched: in the latter it always was; nay, when two globules were brought into contact with that which was touched, the tadpoles of both were very often evolved. The seed therefore of the great aquatic frog is so powerful, that the smallest drop is sufficient to impregnate several tadpoles.

CXLI. Considering that in this surprizing experiment the small quantity of seed must have passed through the mucus, before it could fecundate the tadpoles, I thought it worth while to try what would be the consequence of doubling the thickness of this substance, and using the same portion of seed. Having put with this view some single globules in watch-glasses, I laid hold of the mucus with a small pair of forceps, and stretched it about an inch. After fixing it in this situation, I touched the extremity once with the point of a needle dipped in seed. The tadpoles sometimes were spoiled, but they, not infrequently, came to perfection, an evident proof that the drop of seed had traversed the whole thickness of the gluten, for as it was extended in an horizontal direction, it could not be suspected that it had run down the outside.

The next experiment bears a close relation to this. About fifty globules were put into a glass-tube hermetically sealed at the bottom. The tube was placed in a vertical position; and a stratum about an inch thick of gluten was placed at the top; the gluten was so fixed, that it rose towards the circumference, and was depressed in the center, and thus it formed a kind of funnel. Upon this
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was dropped a little seed, and as soon as it was lost (which soon happened), the gluten was removed, and the tubes were placed in water. When the quantity of seed was not exceedingly small, almost all the tadpoles were animated, at other times several spoiled.

I repeated the experiment, substituting the white of an egg in the place of the mucus. But then not one tadpole was evolved. When I put a little upon the globules, it grew putrid and communicated the same fermentation both to gluten and tadpole.

The two first experiments related in this paragraph, shew the wonderful efficacy of the feminal liquor of the frog, though in such small quantity in penetrating into the mucus, and passing through it without losing its virtue. This phenomenon must be either owing to the pores of this substance, or to capillary tubes wrought by Nature, in order to imbibe the seed and transmit it to the body of the tadpole. The white of the egg being designed for the first nourishment of the chicken, an animal widely different from the tadpole, must necessarily be constructed differently from the mucus, which affords nourishment to the tadpole. Hence it is easy to comprehend why the passage of the prolific liquor

liquor through the white is impossible, while it is so easy through the gluten.

CXLII. But if we are struck with surprize by the small dose of seed which is capable of effecting impregnation, the following experiments will very much increase this emotion. In the cxxxivth paragraph I intimated that this fluid, when mixed with water preserves its efficacy, but I postponed the particular discussion of this matter: I shall now enter upon it. In my first trial, equal quantities of the seminal liquor and water were used. Finding that this mixture was fit for my purpose, I doubled the proportion of water. In every experiment I employed what was afforded by the vesicles of one frog, which never exceeded three grains. But the effect was heightened very much by this increase of water, insomuch, that twice the number of tadpoles was evolved: Not that the water concurred in producing this difference by any virtue of its own, but because the seed was so much attenuated, that it might be spread over twice the number of tadpoles. For the same reason, when the water was in the proportion of four to one, three hundred came to perfection, whereas when these fluids were in equal quantities, not more than one hundred were evolved.

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This fortunate result gave me courage to try the mixture of a pound of water with three grains of seed. And here my success was greater than I could have conceived. To my astonishment almost all the tadpoles furnished by the uterus of two females, were brought to life by being immersed in this mixture.

Convinced that admiration sometimes takes such strong hold of an object as to hinder reason from considering it properly, I sought to lessen it by a reflection. When I put the tadpoles into water, they sunk, as usual to the bottom, where they were held by the mucus. Now did it happen that the sperm, heavier perhaps than water, fell to the bottom, exactly upon the spot on which the tadpoles lay? If so, fecundation was not produced by seed diffused through so much water, but confined to a much narrower compass. By this supposition, should it prove true, the wonder would be very much lessened. The way I took to determine this question was, to mix the seed of another frog with a fresh pint of water. Now supposing the specific gravity of the former liquid to be greater, it would go to the bottom in the space of an hour. At the expiration of this time I fixed tadpoles at various heights in the water. If we suppose that the seed fell to the bottom, it is obvious
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that those only which lay there would be impregnated. They were indeed impregnated, but at the same time all those which occupied different stations were likewise impregnated, not excepting those which were at the surface. Nor could I perceive that the lowest had the advantage of being evolved in greater proportion. The consequence is obvious, the seed did not fall to the bottom, but was diffused equally through the mass. The first surprize therefore returns. Three grains of seed may be diluted in a pint of water, without losing their stimulating power!

CXLIII. The reader will easily guess, that after having carried the experiment so far, I should still urge it onwards, and employ quantities of water successively larger, till the fecundating virtue should be diminished or destroyed, for it is natural to imagine that this must happen at last. I therefore next employed eighteen ounces instead of twelve, and the tadpoles placed at different heights did as well, and were nearly as numerous as before. But the product was less in two pounds of water, and about one-third less in three, four pounds were still more prejudicial. But notwithstanding this diminution of effect, I could not help wondering when I saw that even
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upon the addition of twenty-two pounds of water, a few tadpoles came to life.

CXLIV. We have already seen how small a quantity of pure sperm suffices, a drop taken up by the point of a needle! (CXL). This truth appears with stronger evidence, when three grains are diffused in twelve, and in eighteen ounces of water (CXLII, CXLIII). But the facts which I am now to adduce, prove yet more forcibly than a quantity infinitely smaller will produce the same effect. How diminutive must be the particles of seed comprehended in a drop of water scarce visible, and taken from a mass of water of eighteen ounces, in which three grains of the feminal fluid have been diluted, the reader may imagine. They are, notwithstanding, capable of producing fecundation. I dipped the point of a needle into this mixture, and touched several globules with it. The diameter of the drop taken up was about one-fiftieth of a line. To my great surprize the tadpoles frequently grew to perfection. And what added strength to this emotion was, to find that those imbedded in globules touched at one point, were evolved just as well, and as speedily, as those of others immersed in pure seed.

CXLV. Having several pounds of water, containing each three grains of seed, I was
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unwilling to lay them aside before I had made some further experiments. I first tried, whether the water is rendered sterile by the fecundation of a number of tadpoles: As many globules as a pound of water would contain were immersed in it; they amounted to several thousands. After they were taken out, others were thrown in; and these came to life just as well as the former; and so did others afterwards immersed. I was weary of repeating the experiment before the water lost its efficacy. I kept an account of the tadpoles of fifty frogs, which were successively put into this water without impairing its prolific power.

I next tried, whether the fetuses of frogs are brought to life sooner by a long continuance in the water. A pound was divided into equal portions, and in one some globules were kept immersed for a second, and then put into pure water; at the same time, other globules were put into the other portion and left in it; but there did not appear to be any difference in the evolution of the tadpoles.

Lastly, I wished to know how long water, containing so small a quantity of sperm, preserves its qualities. It preserves them longer than pure seed. It would impregnate tadpoles thirty-five hours after the mixture, when the

thermometer stood between 17° and 19° (a) in my apartment. In an ice-house, in which the thermometer stood (b) $3^{\circ}\frac{1}{2}$ above the freezing point. I have before observed, that putrefaction probably destroys the prolific power of the seed (cxxxI). And since it does not so soon run into this degree of fermentation, when divided by a large quantity of water, we easily understand how it comes to retain its virtue longer.

cXLVI. From the cXLth to the cXLVth paragraph, I have related experiments made with the sperm of frogs, without mentioning the juice of the testicles. But it must not be supposed that I have neglected to make use of it; the singularity and beauty of the enquiry in which I was engaged, required that it should not be overlooked. I employed this juice in every trial mentioned in the preceding paragraphs; whenever I opened the seminal vesicles of a frog, I at the same time took out its testicles. I was thus easily enabled to remark the results afforded by each of these fluids, and if there was any remarkable difference, it could not fail to strike me; but no such difference was ever perceptible. I

(a) $17 = 70\frac{1}{4}$, $19 = 74\frac{3}{4}$ of F.

(b) $3^{\circ}\frac{1}{2} = 39$ or 40 .

may

may here clear up a doubt suggested in the cxxvth paragraph. I there observed, that the juice of the testicles of the terrestrial toad with red eyes and dorsal tubercles, impregnated in like circumstances a smaller number of tadpoles than the seed. I was speaking of it when mixed with other substances; and I subjoined, that it was not in my power to determine, whether the defect was to be ascribed to inferiority of efficacy, or to its thickness, a quality which prevents it from being spread over so large a surface. The addition of other substances; but especially of water, shews, that the latter is the true cause. I have found on many trials; that the juice of the testicles, as well of the frog as the toad; imparts to water as great efficacy as an equal quantity of the seed.

CXLVII. I must not omit to mention another circumstance of some importance. The great abundance of each species of toad mentioned in the two preceding chapters, which has fallen into my hands in the course of the present year, has enabled me to make as many experiments with them as with the green aquatic frog; and I have observed no essential difference in the results. Frogs and toads constitute but one genus; they copulate and propagate the species in the same manner;

the resemblance of the organs of generation in both sexes is very close; the fetuses are impregnated in the same way; they are alike imbedded in mucus: so many marks of affinity easily enable us to comprehend, why experiments so numerous and various afford corresponding results.

Before I conclude my account of these attempts to effect artificial fecundation, I must make two observations. Every experiment is not crowned with success alike. Suppose, for instance, five hundred tadpoles to be bathed with the seed of a frog or toad, and other five hundred with that of another. Let the quantity of this fluid be the same in both cases, and imagine the fetuses to be taken out of the uterus. The effect will certainly follow; we may be sure that the tadpoles will be evolved; but we cannot be equally sure that the same number, or nearly the same number, will be evolved; for it sometimes happens, that the whole of one five hundred come to perfection, while half of the other or more fail. So it falls out likewise when they are left to nature, as I have frequently witnessed when I kept these animals in vessels, in order to discover the secret of impregnation, and any follower of such pursuits may observe the same thing in the open fields.

fields. In May we may see in the stagnant waters, in which frogs celebrate their amours, some tadpoles that have lately quitted their mucus and are swimming about, while others are still involved in it; but they shew signs of animation, and are seemingly eager to break loose from their confinement. If we fix our attention upon the latter, we shall perceive in the heap globules not clear and transparent like those which contain living tadpoles, but turbid, and of a muddy white colour; they have no motion, nor are they elongated like the others, but remain at rest and preserve their original round shape. The fissures upon the surface are evident signs, that the fetuses will never grow to perfection. The same observation may be made upon the long strings of toads. This partial failure may be traced to two sources: it may arise either from the seed or the tadpoles. If every part of this liquor should not be equally prolific, the whole will not produce fecundation. Part also of the tadpoles may be vitiated, and incapable of being impregnated.

My second remark relates to the difference of time requisite for evolution. Though they should all be taken from the same situation, and all equally covered with seed, yet they will not all be equally forward in their growth.

There will be sometimes the difference of a few hours, and sometimes of a whole day. It may be observed, that of the latest more fail than of the others. This circumstance guided me in my enquiry, whether a smaller quantity of seed did not produce such speedy effects. This did indeed now and then take place, but my often repeated trials convinced me that it was but seldom. Upon the whole, those which were impregnated with a larger quantity of seed were not more advanced, and in a few instances they were not so forward. Upon weighing the several facts which I observed, I was obliged to conclude, that a larger or smaller quantity of seed is an indifferent circumstance in this respect. The tardiness of evolution seemed to be owing to the causes specified above.

CXLVIII. I shall conclude the chapter, with an answer to a question often asked concerning artificial fecundation. Many lovers of experimental investigations have enquired, whether the tadpoles and newts which I brought to life, exactly resembled, both externally and internally, those which are the production of pure nature; and whether I was certain, that they lose the shape of a worm in order to acquire the form of the species. When first I was thus interrogated,
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I was acquainted only with the few results of my first experiments made in 1777, and consequently could not give a satisfactory answer. The lineaments appeared to be very much alike, both in the products of art and those of nature; but I had not then conceived any idea of making a particular comparison between them. After I had obtained my purpose of bringing these animals to life, and they had grown to maturity, I was willing to enquire further, though I saw no reason for supposing that artificial fecundation should alter the laws of Nature. The comparison was however made in the present year (1780), and I can confidently affirm, that upon the most minute examination I have never discovered the smallest difference, either in the external or internal parts; both the one and the other have undergone the accustomed change at the time appointed by nature, the tadpoles becoming frogs and toads, and the young newts losing their gills and producing their legs. It is therefore evident, that artificial fecundation does not produce the smallest alteration in the œconomy of these animals, but that every thing proceeds in the same regular tenour as in natural fecundation.

C H A P. IV.

R E F L E C T I O N S.

CXLIX. **H**ITHERTO I have seldom departed from the duty of the mere historian; a duty which I have discharged with the greater pleasure, since the facts I had to relate had the recommendation of novelty. But it is now fit that I should assume the character of the philosopher; that I should analyze and compare these facts, and employ them not only for the further illustration of some truths which I have established relative to generation, but also for the explanation of different phænomena accompanying this admirable function. The following reflections will, I hope, contribute to this purpose.

I. That the females of several amphibious animals contain in their uterus fetuses completely generated before the approaches of the male, is one of the principal propositions shewn in the preceding dissertation. It is proved at length in paragraphs XVIII, XIX, XXX. LIV, LV, LVI, LVII. LXXII. XCI. It is confirmed in the cxxviiith paragraph of

of this dissertation. Hence by paragraphs cx, cxi. are destroyed the two famous systems of the *Epigenesists* and the *Vermiculists*. The facts related in the present dissertation furnish a new proof, or rather a demonstration of their falshood. Let us take a short view of the question. Those who consider the spermatic worms as the immediate authors of generation, must of necessity suppose, that they exist in the seed whenever fecundation takes place. It is therefore obvious, that according to this system the seminal fluid will be unprolific, when it is totally destitute of these inhabitants. And indeed its advocates admit this. But it does not agree with facts. Though in the seed of the animals so often mentioned vermiculæ may be generally observed, yet in that of two toads they were entirely wanting; but it impregnated tadpoles, just as well as the seminal fluid of other individuals, however well peopled that might be (cxxxii). In the next place, when the seed of the frog or the toad is mixed in equal quantity with human urine or vinegar, the worms are all destroyed. The seed, however, as I observed in the proper place (cxxxiv), did not lose its prolific power. It retained this power when a few grains were mixed with twelve ounces of water,

ter, and even with eighteen, though I could not distinguish a single spermatic worm, so thinly were they dispersed through that large body of water. It ought likewise to be observed, that in seed long kept, which yet answered the end just as well, I often found the worms dead, and floating upon the fluid of the vesicles, and likewise upon the juice of the testicles. Lastly, I could easily bathe tadpoles with feminal liquor abounding in worms, and yet prevent them from having the least concern in fecundation. When a drop is put upon the object-glass of the microscope, every part of it is full of worms. But when a little evaporation has taken place, they begin to quit the circumference, and assemble towards the center, as the animalcules of infusions usually do. As evaporation advances, they continue to fly towards the center, though many are caught, and died in the dried part of the drop. At this time I could take up with the point of a needle, at the edge of the liquor, many drops that were entirely destitute of worms. That I might be quite certain, I placed what I took upon another object-glass; and when I found that it was quite free from worms, I touched several tadpoles, and found that they came to perfection. My long experience in the world
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of microscopical animals, whether belonging to man or animals, will, I hope, vouch for me, that I was not deceived in this delicate investigation. These facts prove then irrefragably, that the system of Lewenhoeck and his followers is false.

CL. This system, it is true, had been attacked by many naturalists, among whom Vallisneri, my illustrious fellow-citizen, is entitled to particular praise. But if we consider their arguments we shall find that they are rather alluring, if I may use the expression of the great Verulam, than convincing. Haller alone, the immortal Haller, has, in my opinion, produced an insuperable objection against the spermatick worms, considered as the authors of generation. It is deduced from his celebrated discovery of the continuation of the membrane of the yolk with the intestines of the chicken. Now as the yolk existed in the hen before she received the cock, so did the chicken likewise before fecundation. Whence he drew this corollary, that the chicken had no dependence upon the spermatic worms, which pass from the cock to the hen. This inference, however just and direct it may appear, has not escaped the subtle cavils of some, who have objected the possibility of an inoculation between

tween the membrane of the yolk, and the spermatic worm coming from the male, whence they derive the continuation between that membrane and the intestines, observed by the physiologist of Berne. He, however, in his great work, and, after him, the acute Bonnet, have shewn how unphilosophical such a supposition is. But it is certain, that no such objection can be made to my observations on tadpoles, both because they exist before fecundation, and because in experiments, in which artificial fecundation was produced (CXLIX), the spermatic worms were not present.

CLI. This singular mode of impregnation, equally demonstrates the falshood of Epigenesis, or of that system, which has been raised from the dead, protected and carested by Buffon; who, by means of his *organic molecules*, has created an imaginary organic world, as his countryman Descartes had before constructed the whole mass of existences, both organic and inorganic, with his subtle matter. The spermatic worms having first struck his senses, and being thence transferred to his fervid and creative fancy, lost their former name of animals, and acquired the new title of organic molecules. What violence has been offered to nature by this
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unintelligible metamorphosis, I need not now attempt to shew, having treated the subject at length in my second *Essay on the Natural History of Animals and Vegetables*. But if I admit, for a moment, the reality of this extravagant opinion, it will follow, that whenever the seminal liquor of animals is destitute of worms, it must, according to this renowned naturalist, be also destitute of organic molecules. And as fecundation and generation depend upon the various combinations of these molecules, seed that does not contain any of them must be unfit for such purposes. In the instances mentioned in the CXLIXth paragraph, the seed of frogs and toads must therefore have been unprolific, which is directly repugnant to fact.

CLII. II. The confutation of these systems confirms more and more this important truth, that the young belong originally to the female, while the male only furnishes a fluid, which determines them to assume motion and life. I would not indeed assert, that these little organized bodies are without motion before they experience the action of the masculine liquor. From the time at which the tadpoles in the ovaria fall under the cognizance of the senses, till they are about to be impregnated, they grow considerably,

siderably, insomuch that they are many times larger at the latter than at the former period. Now growth implies nutrition, nutrition the circulation of the fluids, and circulation depends on the pulsation of the heart. I therefore conceive, that, previously to the influence of the seed, there was a beginning of motion and life, but in a degree exceedingly dull and languid, from the extreme slowness of the movement of the fluids; but this idea occurred to Haller and Bonnet before me. Hence tadpoles would never be so rapidly evolved, or attain that sensible animation, which we denominate life, if they were not subjected to the influence of the seminal fluid. This raises them from a state of apparent shapelessness and immobility, and produces a due unfolding of the limbs, and evident motion, and active life.

But the seed, in order to exert its power upon tadpoles, must penetrate into them; for it is not likely that it can produce animation by mere impression upon the skin. Are the paths by which it enters perceptible to the eye? I have bestowed much attention upon this question, and the repeated instances of Mr. Bonnet have still further engaged me in so difficult an enquiry. I therefore examined some mucilaginous globules
discharged

discharged by frogs, with a weak microscope. The mucous cover was so transparent, that I might have supposed the tadpoles to be naked, if I had not been sure of the presence of the gluten. But I could not perceive any pore or aperture either on it or the tadpoles. Neither did a glass of more highly magnifying power shew any such appearance. I next stripped the fetuses of their mucus, but discovering nothing in those globules that were full, I determined to try whether evacuating them would contribute to the success of my enquiry. Making, therefore, an incision with a small scalpel, I pressed out all the semifluid matter lying within, so that nothing remained but the empty skin or membrane. This was examined first with a weak and then with a powerful lens, when there appeared an immense quantity of lucid points, which may reasonably be supposed to be so many pores. I saw the same apertures upon the tadpoles of toads. This supposition, therefore, shews how the seminal fluid enters into the body of the tadpole through innumerable mouths. The presence of these apertures upon every part of the skin, explains a phenomenon before mentioned, why fecundation takes place whatever part of the globule, whether that which corresponds to the belly,

or

or the head, or the tail, be touched with feed; for when this fluid has traversed the gluten, it is sure to meet with paths prepared for its entrance.

CLIII. III. Those, who have supposed that the fetuses are not generated during the act of fecundation, but that they pre-exist long before, and are contained within the female, have explained their animation, by imagining, that the seed of the male enters into the fetus, and reaches the heart. By gently irritating the cavities of this organ, it excites alternate dilatation and contraction, and thus the fluids are forcibly impelled into their respective vessels. Hence follow, as so many necessary consequences, the dilatation of the vessels, an increase of the quantity of fluids, a great irritability of the heart, an universal expansion of the solids, and consequently the growth of the animals both in bulk and mass. Such is the doctrine delivered among others by Haller and Bonnet in their useful productions, and long before them Vallisneri, who deserves the greater praise, since he was unacquainted with the facts that directly prove the pre-existence of the germ, and was guided only by the twilight of conjecture. When I consider the evident pulsation of the heart, which is pre-
vious

vious to the least motion in any of the limbs, the evolution of the vessels and of the whole animal, which is consequent upon the action of this muscle, its irritability,--that property, by virtue of which it resumes motion, when stimulated by any mechanical agent, at a time when there are no remains of irritability in the other muscles, all these circumstances induce me to adopt the opinion of these writers. I shall, therefore, with them consider the seminal fluid as a stimulus, which penetrating to the heart, and powerfully irritating the internal parts; excites more frequent and stronger pulsations; whence arise that manifest extension of parts, and that animation which ever follow impregnation.

Both these effects are influenced by the temperature of the atmosphere. The sagacious reader easily conjectures, that they will sooner take place in warm weather, and later in cold. Tadpoles fecundated early in the spring, as those of the terrestrial toad with red eyes and dorsal tubercles, require several days to appear; and if the heat should be only a few degrees above congelation, ten or eleven are necessary. On the contrary, the evolution of the tadpoles of the fetid toad and of frogs is very quick, for they are fecundated in May and June. I once ob-

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served,

ferred, that when the thermometer stood at 21° , they became animated in less than twenty-four hours; whereas those of the very same species (viz. of the green aquatic frog) did not arrive at this state till the fifth day, when the temperature of the air was about 13° (a). The same variation is observable in the fetuses of the water-newt, of scaly fishes, the tortoise, the crocodile, and in general of oviparous insects: and what is truly surprising and scarce credible, the difference of temperature has the same effect upon the eggs of birds, of which it might be imagined, that they could not be hatched; but in a determinate heat, such as that of the female, which is about 32° (b). In this temperature the eggs of hens are hatched very regularly in twenty-one days. But by a diminution of the heat, Mr. Villers, a sensible naturalist of Lyons, has retarded the appearance of the chickens to the 25th; and Mr. D'Arcet, by increasing the warmth, has hatched them on the 13th day.

CLIV. IV. I have had occasion to make a singular observation upon the seed of the frog and toad, the proper stimulant of the heart of the tadpole. It is distinguished by two properties from the seminal liquor of other ani-

(a) $61\frac{1}{4}$ of F.

(b) $32 = 104$ of F.

mals;

nials. It is as transparent as water, and has not the least viscosity. It evaporates nearly in the same time as water, and hence would seem to have no spirituous parts; as is moreover evident from its not taking fire, when brought to the flame of a candle, and extinguishing burning coals when it is thrown upon them. To the taste it is insipid, and as it does not effervesce either with acids or alkalies, it must be considered as neutral. These qualities would appear to agree but ill with the nature of a stimulant, if we understand by this term a pungent, caustic, or spirituous substance. But this is not the sense in which Haller and his followers consider those bodies which stimulate the heart. They mean such as are capable of irritating the fibres of this muscle, by which means it is excited to motion if at rest, or if otherwise to an increase of action. To this class of stimulants belong the blood, water, air, and the seminal fluid, which will appear to be more powerful than the fluids previously circulating in the tadpole, if we consider the slowness of their motion.

CLV. v. A quantity of seed far more inconsiderable than we should ever have imagined, is sufficient to animate a tadpole. We have seen, that it is not necessary to cover the

fetus completely with this prolific fluid. A drop will suffice (CXL). Further, three grains mixed with twelve, and even with eighteen ounces of water, communicate to every part of it the power of fecundation, since tadpoles placed in any part of the mixture are impregnated (CXLII, CXLIII). The three grains of seed must therefore have been diffused through the whole mass of water! But what an enormous division of its particles must such a diffusion occasion? How small a portion of the prolific liquor will fall to the share of each tadpole? Yet there are facts which prove, that the semen still retains its virtue after this excessive division; for I have found a globule one-fiftieth of a line in diameter taken out of a mixture of three grains of seed, with eighteen ounces of water, was often capable of fecundating a tadpole (CXLIV). Desirous of knowing the proportion which the tadpole (that of a frog is two-thirds of a line in diameter) bears to the particles of seed diffused in a drop of this dimension, I have found, on calculation, that it is as 1064777777: 1. How infinitely small, therefore, is the quantity of seed in comparison of the bulk of the fetus which it fecundates! This deduction led me to calculate the weight of the particles of
semen

femen dispersed in this drop of water: it is $\frac{1}{1994687500}$ of a grain. That I might view these particles under every possible aspect, I reduced their bulk to cubic lines, when it appeared to be about equal to $\frac{1}{3002120420}$ of a cubic line (a).

(a) This note will shew how I obtained these numbers. Assuming with Metzius, that the circumference of a sphere is to its diameter as 355 : 113, the solid contents of a sphere $\frac{2}{3}$ of a cubic line in diameter will be $= \frac{8}{27} \times \frac{355}{113 \times 6} = \frac{1420}{9153}$ of a cubic line, and this will be the bulk of a tadpole to be fecundated.

The solid contents of a sphere $\frac{1}{50}$ of a line in diameter are $= \frac{1}{125000} \times \frac{355}{113 \times 6} = \frac{355}{84750000}$, and this will be the mass of the drop of water mixed with seed.

The solid contents of a cylinder which has 37 lines for the diameter of its basis, and is 35 lines high, are $= \frac{37 \times 37 \times 35 \times 355}{113 \times 4}$; and this is exactly the volume of water weighing 18 ounces, in which three grains of seed have been dropped.

The solid contents of a cylinder of which the base is one line in diameter, and the height 10 lines, is $= \frac{10 \times 355}{113 \times 4} = \frac{3550}{452}$; and this is the volume of the three grains of seed.

Now supposing that the seed is equally diffused in the water, the volume of water with the seed, and the volume of the drop of water mixed with seed, will be proportional to the weight of all the seed, and to the weight of the seed mixed with the drop of water. Hence, as the weight of all the seed is three grains, it will appear from the rule of three, that the weight of the seed diffused in the drop of water is $= \frac{3}{2994687500}$ of a grain.

As in homogeneous bodies the weight is directly as the bulk, having the weight of the whole seed and that of the seed mixed with the drop of water, we shall find by the rule of three, that the bulk of the seed contained in the volume of water is nearly $\frac{1}{3002120420}$ of a cubic line.

If we now compare the bulk of the tadpole with that of the seed contained in the drop of water, it will appear to be as 1064777777 : 1.

CLVI. VI. Here I may be asked, how a portion of semen so infinitely small can make such an impression upon the heart of a tadpole as to accelerate its pulsations and increase their strength, and thus to quicken the motion of the fluids and animate the whole system. I reply, that there are not wanting examples in the animal kingdom to give countenance to such a supposition. A drop of the poison of the viper, introduced into a wound, is sufficient to destroy the irritability of the muscular and the sensibility of the nervous system, and to cause the death of any animal whether great or small. A man, a dog, an horse, or an ox, are destroyed just as certainly as a sparrow, a goldfinch, or a mouse. If we consider for a moment, the proportion between the bulk of the venom and that of an horse or ox, we shall not be less surpris'd, that so small a quantity of poison should be fatal to these great animals, than that so minute a portion of feed should animate one infinitely smaller (*b*). But we have facts that
more

(*b*) I cannot but remark, that the author has been somewhat unfortunate in the example which he has chosen for the purpose of illustration. Fontana has proved, by numerous experiments, that the effects of the venom of the viper are directly as its quantity, and inversely as the bulk of the poisoned animal. This is very striking in his trials on dogs, of which the smaller species were always destroyed by the same quantity that some-
times

more directly prove, how very small a quantity of seed may produce a considerable irritation of the heart. A grain of storax thrown upon the fire will fill a whole room with its odour. The particles therefore must be dispersed, through every part of it, and notwithstanding their extreme minuteness, are capable of vellicating the nerves of the nose, and exciting the idea of a particular smell. This vellication is so strong, as to occasion sneezing in some persons of the more delicate sex. If then the inconceivably minute exhalations of certain bodies make such impressions upon our organs, why should we wonder that a few particles of seed prove a strong stimulus to the heart of the tadpole, an organ so much more susceptible of external impressions, as it is more finely framed than ours?

CLVII. VII. But the quantity of seed requisite for the animation of a tadpole has its limits; it is not infinitely small. I have expressly observed, that the quantity above specified, was *frequently* sufficient for the purpose (CXLIV, CLV). Whence it is to be inferred, that it sometimes failed. The diminution of the dose of seed afforded a more times killed the middle-sized, and scarce ever the large ones. From the data afforded by his experiments he calculates, that the poison of several vipers is requisite to destroy the life of a man, and a still larger quantity to kill an ox.

decisive proof. I put a drop of this fluid upon a smooth pane of glass, and drew it out with the point of a needle, into a fine and almost invisible filament, sometimes an inch long. I then took several mucous spheres and rolled them sometimes along half, sometimes along a third, at others along a quarter of this filament. This experiment is analogous to another related in paragraph CXLIII, where I observe, that if three grains of seed be diluted in two or three pounds of water instead of a pound and a half, fewer tadpoles will be evolved, and that the number continues to decrease in proportion to the increase of the quantity of water. The portion of seed then necessary to produce fecundation, must amount to a determinate quantity.

I am notwithstanding of opinion, that the quantity of seed which effects impregnation is small beyond conception and nearly approaching to what is specified in the CLVth paragraph. Hence I am inclined to believe that the surplus does not contribute at all to fecundation. My opinion is founded upon the following considerations. As fecundation is an indivisible operation, if the superabundance contribute to promote it, it can only be by hastening the animation and
growth

growth of the tadpoles. This must be in consequence of a greater number of particles exciting the heart to stronger vibrations. But such a notion is repugnant to facts; there is no difference either in the time or the degree of evolution, whether the fetuses be entirely covered with pure seed, or only touched with an infinitely small drop (CLXIV). I cannot therefore imagine, what purpose the surplus of seed can serve, and am obliged to consider it as useless. Nor is it difficult to comprehend, how an invisible particle only may be capable of producing fecundation if it be considered how inexpressibly small is the area of the vessels which it enters, for canals so narrow must refuse to admit above a very minute portion.

CLVIII. VIII. But a very obvious question occurs here. As these amphibious animals are animated by such a trifling quantity of seed, may this discovery be extended to fishes, birds, quadrupeds and man? Or may it be asserted in terms still more comprehensive, that nature employs only so inconsiderable a portion of seed for the fecundation of animals in general? The question is curious and interesting, but cannot I think yet be solved for want of data. I am aware, that this opinion is possible, nay, that the striking instance

instance before us renders it in some measure probable. I cannot, however be certain, that it is really so, when I reflect upon the multiplicity of means employed by Nature to attain the same end. Perhaps, therefore, this sovereign Artist may allot various quantities of seed for fecundation, according to various qualities of animals. But in order to discover whether she really admits such variety, I can imagine no other way than that of comparison. Let us, for instance, compare what happens in the amphibious animals which are the subject of these Dissertations, with what takes place in other classes, as in fishes, insects, birds and quadrupeds. The knowledge derived from such different instances, may enable us to fix our ideas with some degree of safety. We have, I believe, but two discoveries on artificial fecundation, that of Mr. Jacobi, inserted Berlin Memoirs (*a*), and mine. But without any design to lessen the merit of that naturalist, I may be allowed to say, that from his experiments we can only conclude, that artificial fecundation took place in the two species of fish of which he speaks. These were the salmon and trout. After putting their mature, but unimpregnated eggs in clear water, he let fall upon

(*a*) Tom. 20.

them

them feed from the milt of the male till the water began to grow white. In about five weeks the young fishes made their appearance. This is the amount of the whole paper of Mr. Jacobi. It affords no information concerning the present question, since the German experimenter has not ascertained, with any degree of precision, the quantity of seed necessary to produce fecundation. Instead of doing this, he has stopped at the first step, and has finished his trials just where an inquisitive naturalist would have begun them.

But if experiments on artificial fecundation are not yet sufficiently numerous to afford the necessary data, we cannot expect them from natural impregnation. A small share of information concerning the generation of man and animals will convince us of this truth. It remains, therefore, that we prosecute the experiments upon artificial fecundation in a rational manner. Besides the light which a series of well-devised experiments would cast upon the problem proposed, it would certainly clear up many obscurities in the history of animals. My enquiries relate to an order of beings, in which we are certain that fecundation is external. But I invite naturalists to bestow part of their attention and sagacity upon that incomparably more numerous

rous

rous class of which it is equally certain, that this function is performed within them. Of these, some, as is well known, are oviparous, others viviparous. It will be proper to keep some females apart from the males, and to observe the time at which they lay their eggs. We may then make upon the eggs such experiments as are described in this Dissertation. I do not think it will be difficult to adapt them to viviparous animals, if we use different means, and such as will act internally, nor do I see any reason to despair of success. Attempts, apparently more arduous, have not disappointed the hopes of the adventurous enquirer, and such unexpected instances of success have occasioned revolutions in natural philosophy.

CLIX. IX. The seed, agreeable to the principles of Mr. Bonnet, is not only the stimulant, but the nutriment of the fetus. The proofs of his opinion are these. The organ of the voice in that mule, which is the product of the mare impregnated by the ass, very closely resembles that of the male parent. If the germ exist in the female before impregnation, it must have been a horse in miniature, and not a mule or an ass; the organ therefore of the voice ought to correspond to that of the horse and not of the ass. The
seed

feed must then have modified the organ of the voice in the germ according to that of the male parent. In conformity with this model, the parts of this organ grow and are unfolded; yet we know the afs contributed only a fluid. It is therefore necessary to admit, that this fluid nourishes the component parts of the larynx, and that it is incorporated with their substance, since growth is the natural and immediate effect of nutrition. The sperm then is not a simple stimulant, it is moreover a nutritive liquor. The naturalist of Geneva strengthens his deduction by the instances of the growth of the beard in man, of the crest of the cock, and the horns of the stag, which are certainly owing to the efficacy of the feed.

Haller, in his great work, agrees only in part with the before-mentioned philosopher. He allows that these phænomena depend upon the feed, not indeed as a nutritive liquor, but as an impelling or stimulative principle. The evolution of the hair, the horns, and the crest, is owing to the sperm, which, being received again into the blood, excites the heart, by its alcallescent acrimony, to attenuate by stronger pulsations the viscid juices, and to impel them into the small vessels, by which means the preexisting germs
of

of the hair and the horns pullulate, and the crest grows to a larger size (*a*). With regard to the organ of the voice in the mule, its evolution arises from the greater power in the seed of the ass than the horse, to unfold and enlarge the parts (*b*). It appears, therefore, that according to the Swiss physiologist, a stimulating quality alone is sufficient, not only for the animation of the fetus, but also to explain those phenomena, which, according to Mr. Bonnet, require, that the seed should be considered as nutritive. I shall not presume to decide between these great philosophers. I venerate their merits, and am better qualified to admire than to judge them. I will only observe, with the utmost deference, that in the frog and toad the prolific fluid cannot be supposed with any probability to perform the office of a nutritive principle. I intreat the reader to call to mind what is said in paragraph CLV, about the proportion between the bulk of the seed and that of the tadpole; it is nearly as 1 : 1064777777. Supposing, therefore, the tadpole to be nourished by the feminal fluid, its growth would not exceed the bulk of the seed, that is to say, it would be next to none. It may be added, that if the nutrition of the tadpole, at the commencement of evolution,

(*a*) *El. Phys.* l. xxvii, Sect. 3. (*b*) *L. c. lib.* xxix. Sect. 2.

came from the feed, those which were bedewed with the greatest quantity of this fluid, ought to grow fastest and be evolved soonest. Yet we know, that a tadpole touched with the most inconsiderable drop, and one covered compleatly with this fluid, do not differ in this respect (CXLIV). I therefore, from my experiments, cannot assign to the seminal liquor any other quality, besides that of a stimulant.

Though the preceding observations afford the strongest reasons for believing, that the feed is a real stimulus to the heart of the tadpole; yet, as this is a mere hypothetical opinion, I was desirous of submitting it to the test of experiment. Reflecting, that the stimulating power of any substance must be enhanced by increasing the activity of its parts, and that this end may be attained by heat, I determined to warm the seminal fluid, and then to moisten some tadpoles with it. With this view I mixed three grains of the feed of the frog with half an ounce of water, and having heated it to 30° (a), I immersed twenty tadpoles in it, and immediately afterwards removed them into pure water, in which the thermometer stood at 17° . That I might have a term of comparison, I fecun-

(a) $30^{\circ} = 99\frac{1}{2}$, and $17 = 70\frac{1}{3}$ of F.

dated

dated other twenty tadpoles with the same mixture, after it had acquired the temperature of the atmosphere. The device was not ineffectual. The tadpoles fecundated in the heated mixture, were ten hours more forward than the others, a phenomenon which I cannot but attribute to the greater energy and stronger stimulating power arising from the action of heat.

CLX. x. The stimulating power, which animates the embryo, is not lost, though the seed should have been kept some time after it has been taken out of the body. This happens alike, whether it be kept unadulterated or mixed with other liquors (CXXXI, CXLV). It is truly wonderful that a single drop should not be lost, when mixed with so large a body of water (CXLII, CXLIII). Will the seminal liquor of other animals preserve in like manner its efficacy? For want of facts we must have recourse to conjecture. The present instance may be an exception to a general rule and confined to a particular class, in which fecundation is external and takes place in water, and on this account the seed is exposed to circumstances, never experienced by that of other animals. But, on the other hand, it is possible that those properties may belong to other seminal fluids. In my *second Essay on the Natural*

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tural History of Animals and Vegetables I observe, that the seed of man, and various quadrupeds, does not become corrupt for several hours after it has been taken from its natural receptacles. I confirm this observation by shewing, that the worms continue to swim about briskly for some time, provided it is kept properly heated. If, therefore, the seed of man and quadrupeds preserve its natural qualities under these circumstances, why may it not also retain its stimulating power, which is the cause of fecundation? And if so, is the famous story of Averroes, concerning that unfortunate queen, who became pregnant after bathing, without any commerce with man, so very ridiculous?

C H A P. V.

WHETHER FECUNDATION IS AN-EFFECT OF THE AURA SEMINALIS. WHETHER OTHER LIQUORS ARE CAPABLE OF PRODUCING FECUNDATION. TRIALS TO PROCURE ARTIFICIAL MULES IN THE AMPHIBIOUS ANIMALS IN QUESTION. ARTIFICIAL FECUNDATION OF THE SILK-WORM. ATTEMPT TO IMPREGNATE A BITCH ARTIFICIALLY.

CLXI. **W**HETHER the gross visible part of the seed be necessary to the fecundation of man and animals, or, whether the invisible attenuated part usually called the *seminal vapour* or *aura*, be destined to this purpose, is a very ancient question, and still continues to be debated every day. Those physicians and philosophers who favour the latter opinion, are obliged to maintain it from a sort of apparent necessity, rather than by any direct reasons or experiments. They reflect upon observations made by diligent anatomists, who have found the *vagina* of some pregnant women either very narrow or entirely closed. They attend also two other numerous observations, from which
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it would appear, that at the time of impregnation, the seed does not reach the uterus. Lastly they reflect, that the orifice of the oviducts, or Fallopian tubes, is so narrow, that it will scarce admit air, much less a probe. They conclude, that so many obstacles must prevent the seed injected into the female organs from arriving at the ovarium, where the embryos are lodged. Hence it is supposed, that fecundation must be effected by the volatile part of that liquor, which either reaches the ovarium by the way of circulation, or by rising through the mouth of the uterus and along the tubes. Notwithstanding these arguments, many other authors adopt the opposite opinion; they think, that the gross part of the seed operates impregnation, since the narrow passages are enlarged during the ardour of enjoyment, and since there are not wanting instances where seed has been found in the uterus, in the tubes, and even to have ascended as high as the ovaria. That the uterus, should often, after coition, be found without the fluid of the male, they consider as an objection of no weight, because the observation was probably made after too long an interval, when the seminal liquor had passed out of this cavity, or because so small a quantity had entered

into it as to escape the notice of the inspector.

Such nearly are the arguments on each side, but they are, in my opinion, insufficient to decide the dispute; the former, because they do not irrefragably prove that the volatile vapour alone reaches the ovaria; the latter, because though the gross part should arrive there, it still remains doubtful whether the animation of the embryo is effected by this, or the attenuated part. To cut short all controversy, it will be necessary to separate the aura, and to subject the fetus to its influence for then it will either be impregnated, a manifest proof in favour of the feminal exhalations, or it will not, and we may conclude, with equal certainty, that the sensible part is necessary to fecundation. But this mode of deciding the question has not, I believe, occurred to any one, or at least I do not know that any one has reduced it to practice. But from the facility of making experiments with the seed of the animals concerning which I have been treating, this appeared easy to be done.

CLXII. The reader will recollect what has been before said of the efficacy of seed, after it has been almost infinitely divided. A drop of water, one-fiftieth of a line in diameter, taken

taken from eighteen ounces of water, with which three grains of seed are mixed, is capable of impregnating a tadpole (CXLIV). This experiment is apparently favourable to the feminal aura, which, in the general opinion, is nothing but the vapour of the seed exceedingly rarefied. The facts, however, which I shall adduce, clearly prove the contrary. That I might moisten tadpoles abundantly with this exhalation, I put a quantity of seed, amounting to eleven grains, taken from several fetid toads, into a watch-glass. In another somewhat smaller, I placed twenty-six tadpoles, which were fixed very firmly to the bottom by the tenacity of the gluten. I then inverted it over the former, and in this situation both glasses were left five hours in my apartment, where the liquor of the thermometer stood at 18° . The semen lay exactly under the tadpoles, and they could not but be involved in the rising vapours, for the distance was little more than a line. Upon inspecting the tadpoles at the expiration of the fifth hour, I found them so much covered with moisture, as to wet my finger when I touched them; the moisture was the evaporated part of the seed, which had lost one grain and half. The tadpoles, therefore, had been bedewed with one grain and half of the

feminal aura, for it cannot be supposed to have escaped out of the glasses, they fitted so closely. The tadpoles, notwithstanding they were placed immediately in water, and left there several days, all perished.

CLXIII. Though this experiment is unfavourable to the aura, yet it stood alone, and I could not avoid further enquiry. One grain and an half ought indeed to fecundate many thousands, much more twenty-six. I however chose to increase the quantity of vapour, which could be done by only increasing the heat. Placing every thing exactly as in the former experiment, I set the glasses in the window, where the heat of the sun, being moderated by the glass through which it shone, amounted to 25° , and could not therefore be prejudicial to fecundation. In four hours the spheres were so covered by the vapour, that drops were seen hanging from them. But the effect produced was the same as before.

I repeated this experiment once more, not so much with a view of confirming the former results, as to see whether the seed, after part had been resolved into vapour, retained its efficacy. After the tadpoles were moistened with the exhalation, half of them were put, as before, into water, all which came

to nothing. The other half were bedewed with a little of the residuum, and then put into water: they almost all came to perfection. These experiments shew, that the vapour of the seed of the fetid toad is incapable of impregnating the young, and that the seed, after a considerable evaporation has taken place, is still efficacious.

CLXIV. Both these consequences were confirmed by subsequent experiments. The space between the tadpoles and the sperm was about a line: that the vapour might be more efficacious, I reduced it to one-third of a line; but still to no purpose.

I have already observed, that by the aura feminalis the vapour of the seed is generally understood. Some physiologists think that this exhalation consists of the odoriferous particles of that fluid; others that it is the most attenuated part, and others again that it is a very subtil spirit. Whatever it be, it is certainly incapable of producing fecundation. Yet as so subtil a spirit might be thought to escape at the meeting of the glasses, I determined to obviate this suspicion by cementing the edge of the upper glass to the inside of the other. I moreover substituted a little glass-funnel in the room of the upper watch-glass, and cemented it as before; the smaller

orifice was hermetically sealed. The tadpoles were fastened to the neck, and the surface of the feed was enlarged that evaporation might be the quicker. I thought that the conical form of the funnel would collect the aura into the point where the tadpoles lay. This new apparatus was kept six hours in a heat of 26° , and the fetuses were surrounded continually by the vapour, but the event was still the same, and the residuum was also efficacious (CLXIII.)

When I tried the effects of the aura in open vessels, where the air had free access, I found it just as ineffectual as ever.

CLXV. My last experiment made with this view was to collect several grains of the evaporated fluid. I then immersed twelve tadpoles in it, and left them several minutes. Twelve more were touched with the residuum, which did not exceed half a grain, but eleven of these grew to perfection, but not one of the others.

These various facts concur to prove, that fecundation in the fetid toad is not the effect of the aura feminalis, but of the sensible part of the feed. But my enquiries have been extended further; the abundance of the toad with red eyes and dorsal tubercles, and of the green aquatic frog, has afforded me ample opportunities of repeating upon them the
experiments:

experiments described in paragraphs CLXII, CLXIII, CLXIV, CLXV. But the aura appeared on all occasions incapable of producing fecundation. My few trials upon the tree-frog agree exactly with the others..

CLXVI. As I had found the juice of the testicles equally powerful with the seed, it was proper to make trial of its aura. I had used this juice with equal advantage, when pure and when diluted with water. I have evaporated it under both these circumstances in the same manner as seed; but always to no purpose: whence it was necessary to conclude, that this aura is just as inert as that of the seed.

My experiments on these two species of aura were made in the spring and summer of 1777, and I then gave intimation of them to Mr. Bonnet, as may be seen by some extracts of my letters, inserted in the useful annotations on the third volume of the new edition of his works. I repeated them in 1780, with the same result.

Thus then it appears, that in two species of the toad, and as many of the frog, fecundation is not the effect of the aura, but of the gross part of the seed. But are we to suppose that Nature observes this rule in animals and man? Sound logic does not allow

us to deduce from so few facts a conclusion so general. But these facts lead us to think the supposition probable. It is at least certain, that we may venture to admit it, till contrary facts shall be adduced. And thus the great question, whether fecundation is the effect of the *aura feminalis*, is clearly decided in the negative, with respect to some animals, and with probability with respect to the rest.

CLXVII. I may here observe, that it appears from this, as well as the preceding chapters, that the part of the seed which is capable of effecting impregnation is not, as many suppose, a spirituous liquor, of great volatility, and liable to lose its virtue when exposed for some time to the air, after which there remains nothing but an effete matter, a kind of *caput mortuum*, as is incident to many liquors, produced both by art and nature, which, on this account, are kept carefully shut up in vessels. But nothing like this happens to the seminal fluid, though it be exposed to the air for several hours; and after half had been driven off in vapour, the remainder was just as fit for fecundation as that which had been recently taken from the vesicles. The prolific liquor, to which these observations are applicable, may be compared in some measure to water. Whether this
element

element evaporates, either from agitation of the air, or the action of heat, the residuum retains the nature and properties of the whole mass. But we may, on the other hand, remark a great difference. The vapour of water, when collected, possesses every property of water never submitted to this process, whereas the vapour of seed, though it may be condensed into a liquid, does not retain its fecundating power. It must therefore be supposed, that the particles, when separated from the whole mass, acquire some bad quality, which disqualifies them for occasioning such an irritation of the heart of the embryo, as will produce animation; though the minuteness of the particles does not allow us to investigate the origin and nature of this bad quality.

CLXVIII. I now proceed to the discussion of another problem, which will appear both new and extraordinary. It was proposed by Mr. Bonnet, in a letter concerning artificial fecundation. And although he was aware of its extravagance, he did not hesitate to communicate it, in order that it might be put to trial. I cannot explain his idea better, than by laying before the reader an extract from his letter of the 15th of August, 1778. “ I will not conceal from you a vision that has
found

found its way into my head. Mr. Senebier must have informed you of the fine experiment of Mr. Achard, who has substituted electricity, in the room of heat, for hatching chickens, and has succeeded at least in part. If the electrical fluid be capable of causing the evolution of the chicken in the egg, it must be owing to an acceleration of the velocity of the fluids, or, what amounts to the same thing, to an increase of the irritability of the heart. Now I think I have given ample proof, that the seminal liquor fecundates the germ, by exciting the irritability of the heart. I could wish then, my dear Malpighi, that you would substitute the electrical fluid in the stead of the semen. Should an experiment so original succeed, the fecundation would be far more artificial than that which you have so happily effected. You may well imagine that I will not answer for your success: it is probable, that the electrical fluid cannot perform the office of the seminal liquor; but we have seen so many unexpected events happen in the organized kingdom that we cannot pronounce with too much caution concerning the impossibility of any experiment, more especially of such as relate to this subject.--Should we have suspected, that the polype possesses such surprising properties?

ties? And after the discovery of the polype, who would have suspected the reproduction of the head of the snail?"

Such were the expressions of this profound contemplator of Nature. But various occupations of another nature having prevented me from continuing my observations on artificial fecundation, it was not in my power to attempt this strange experiment, till the summer of the present year (1780). To confess the truth, I doubted exceedingly of its success, not so much for the reasons adduced by my celebrated friend, as on account of the striking difference between Mr. Achard's eggs, which had received the influence of the male beforehand, and the tadpoles which had not been subjected to it.

Having placed upon the conductor a metallic vessel, containing several fetuses both of the frog and toad, I excited the cylinder, and simply electrified them without sparks or shocks. In the first trial the tadpoles were electrified three hours a day for two days successively. As these became all putrid, I continued the process in another experiment for four hours on three successive days, but with no greater success. I then electrified nine tadpoles for thirty-three hours in the space of two days and a half, but the event was still the same. Instead

Instead of placing the tadpoles in the situation already described, I fixed them to the point of a metallic rod arising from the conductor, knowing that the electrical fluid would now be more concentrated and energetic. But I was not more successful, though they were electrified nineteen hours in two days. All these experiments were made in hot weather, and the tadpoles shewed signs of decomposition in three days.

At the same time I electrified other impregnated tadpoles: they grew faster than those which were not electrified, an event that exactly corresponds with the acceleration of vegetation, occasioned by the electric fluid. From these experiments we may conclude, that electricity increases the velocity of circulation in fecundated tadpoles, but not in the unimpregnated, probably because its action is not so mild and gentle as is necessary at this early period, and as that of the seminal liquor.

CLXIX. Before Mr. Bonnet imparted to me his ingenious thought, and indeed at the time of my first observations concerning artificial fecundation, I conceived hopes of being one day the fortunate discoverer of some fluid, equally efficacious with the semen, though of a different nature. When

I was

I was employed about the impregnation of the toad with red eyes and dorsal tubercles, and had discovered the efficacy of the seed taken out of the vesicles, I was struck, I know not how, by the thought of trying some of the other fluids of the same animal. I therefore moistened some tadpoles with blood, others with gall, others with juice expressed from the different viscera, as the heart, the liver, the lungs; they were put into separate vessels, and some of those that had been touched with blood and the juice of the heart, came to perfection. The surprize, which an event so unexpected occasioned, may be easily conceived; but it may likewise be supposed that I should repeat this experiment. But the result was now very different; not a tadpole was evolved. The same ill success attended many subsequent repetitions; and I was obliged to conclude, that neither the blood, nor the expressed juice of the heart, has any prolific power. But whence, I shall doubtless be asked, the success of the first experiment? It arose from an inadvertency which I afterwards easily detected. The tadpoles were taken from an uterus, which I had touched with forceps, that had been used for taking some drops of seed out of the vesicles. Not being at that time aware
of

of the efficacy of an exceedingly minute quantity of seed I had not wiped the instrument very carefully. On this account, it must have left an imperceptible portion upon some of the tadpoles, that were afterwards moistened with blood and the juice of the heart. This explanation became more probable, when I found, that of tadpoles taken out of the uterus with forceps, carelessly wiped after they had been dipped in seed, a certain number was always impregnated; whereas this never happened, when the forceps had not touched this fluid, or when they had been diligently cleaned. This negligence, and the detection of its origin, were excellent preservatives against the commission of like mistakes in future. They may also serve as warnings to those who shall be desirous of repeating my experiments on artificial fecundation, or of attempting others of the same nature.

CLXX. Convinced of the inefficacy of these liquors, I laid aside at that time all thoughts of trying others. My unsuccessful experiments with the electrical fluid, renewed the same train of ideas; but it was rather least I should have reason to reproach myself for having too soon quitted a pursuit entirely new, than with any hopes of success, that I re-
fumed

sumed these experiments. Guided by the great principle, that fecundation is the consequence of irritating the heart by the seminal liquor, I was led to try acrid and stimulating fluids, such as vinegar, diluted spirit of wine and urine; but these substances hastened the corruption of the tadpoles, instead of fecundating them. The juice of the lemon and citron produced the same effects, though their acidity was weakened by the admixture of water. In the rind of these fruits there is a stimulating spirit, which upon squeezing, spirts out in little jets, and takes fire when thrown into the flame of a candle. But this fluid, as well as many others, which I shall forbear to enumerate, was totally inefficacious.

CLXXI. We learn, both from ancient and modern history, that the seed of one species of animal, frequently impregnates the embryos of another, when there is a near resemblance between them. A third species, usually denominated mules, is the fruit of this impregnation. Thus the male goldfinch produces with the female Canary bird, an intermediate species; as also does the white peacock with the common sort; and the pheasant with the hen. Among quadrupeds, the mules, which are the offsprings of the

as and the mare, or the horse and the she-
 as, are the most common. And the cele-
 brated Bourgelat has now established, be-
 yond a doubt, that the wolf propagates
 with the bitch. Nor is it always necessary,
 that the two species should be very nearly
 allied. For wherein consists the close resem-
 blance between the as and the cow or be-
 tween the bull, the she-as, and the mare?
 Yet the recent and certain observations of
 the same French Naturalist, have proved,
 that the singular species of animal called the
Jumart (*a*), is produced by the copulation
 of these widely differing quadrupeds, after
 its existence had been questioned by modern
 writers, and expressly denied by Buffon (*b*).

These instances led me to entertain hopes,
 that I should obtain intermediate productions
 by impregnating the embryos of one species
 with the prolific liquor of another. In my
 Prospectus I proposed to enquire, what would
 be the event of attempting such experiments
 on aquatic and amphibious animals, bearing
 little resemblance to each other (*c*). If they
 should happen to be successful, it is obvious,
 how much such irregular productions would
 contribute to illustrate the obscure function
 of generation. I have

(*a*) Œuvres des Charles Bonnet, Tom. V. (*b*) Hist. Nat.
 Tom. XIV. (*c*) P. 58.

I have prosecuted this inquiry, but with small success. Having repeatedly moistened the embryos of frogs and toads with the seminal fluid of the newt, and reciprocally, evolution has not taken place in any one instance. My hopes of impregnating neighbouring species with effect were much more sanguine. I need not observe, that the frog and toad are each amphibious, and that they nearly resemble one another, as well in external appearance as in internal structure. Besides this correspondence of organization, their manners, actions, and ways of propagation are alike. Having, therefore, fixed upon the fetid terrestrial toad, as its season of generation coincides with that of frogs, I bathed with the seed of this toad, the tadpoles of the green aquatic and the tree frog, and in return with their seed, I moistened the tadpoles of the fetid toad. I did the same with the frogs just mentioned, and the toad described in the cxxvith paragraph. The seed, as well as the juice of the testicles, was employed sometimes pure, and sometimes diluted with water. But notwithstanding so many and such striking marks of resemblance, one species could not be fecundated by another. If I did not wonder, that liquors essentially differing from seed were not prolific (CLXIX,

CLXX), I must own my surprize at finding that spermatic liquors, which must needs resemble each other very closely, were inefficacious. These experiments, however, though ineffectual, are not uninstruative; they teach us, that from analogy we cannot learn when we may procure intermediate productions. In general we observe, that they owe their origin to animals, in many respects, analogous to each other; but such analogy is not a certain sign of their being capable of producing together, as manifestly appears from experiment. In this branch of physics, as in numberless others, we must not generalize our ideas, but are under the necessity of consulting the oracles of Nature, and receiving her answers with respect and attention, as we proceed from one species to another. If we would obtain illegitimate productions, we must of necessity employ the seed of a different species. It must, at the same time, be capable of penetrating the embryo, and animating it by a bland and kindly impulse. But we can learn from the effects alone, whether any given seed is endowed with these essential properties, and we must abide strictly by the result of experiments.

And as if these amphibious animals knew the inefficacy of their respective seminal fluids,
I have

I have never, in the whole course of my inquiries, seen them coupled. At the season of their amorous ardour, I have placed a male toad along with a female of his own species, and a female frog; when disregarding the latter, he has flown to embrace the former. I have then separated them, and taken away the female: but the male shewed no desire of approaching the female frog, and seemed to be wholly bent on escaping. And though I kept them together for several days, copulation did not take place, not even when the female began to discharge her young, a time at which the males are more than commonly ardent, in order to fecundate the fetuses as they are brought forth. I witnessed the same inattention in the male of the fetid toad, to the female of the green aquatic and tree-frog, and in the male of the green and tree-frog, for the female of the fetid toad. I know that male toads are commonly supposed to copulate with frogs, and that many, for this reason abstain from eating the latter during their amours. I, however, have never met with such conjunctions in the whole course of observations; nor did Roefel ever see an instance in Germany. I must, therefore, reckon this opinion among those numberless prejudices, which have credulity and ill

grounded popular tradition for their only support.

CLXXII. After having finished the foregoing paragraph, with which I intended to conclude the Dissertation, I read over the whole from the beginning, in order that I might correct such passages as required correction. The CLVIIIth paragraph induced me to attempt another experiment. I there invite naturalists to try to fecundate some of those animals, in which it is certain, that impregnation takes place internally. It was now July, the season destined for the amours of the silk-worm (*a*), in the Modanese, and indeed in most other parts of Italy. I therefore determined to try, whether any portion of my success, with different amphibious animals would attend experiments on this insect. I recollect, indeed, the failure of the great Malpighi; nor can it be doubted, that such an instance may justly create diffidence in the boldest experimenter; more especially as the celebrated Bibiena was not more fortunate, when he undertook to repeat and vary Malpighi's experiment. As I did not, however, perceive, that it was impossible to succeed, I did not think the attempt would argue any blameable presumption in me. The un-

(*a*) *Phalæna Mori*. Linn. Syst. Nat.

impregnated

impregnated eggs of the phalæna originating from the silk-worm, may be obtained in two ways. If the female be kept apart from the male, they may be collected after they are discharged, or they may be taken from the matrix. The seed is to be taken from the genital organs of the male. Having procured some eggs in both these ways, I moistened some of them with a large quantity of seed, and others very sparingly, but to no purpose. The fecundated eggs of this insect assume a violet colour; the others remain yellow. My eggs, before they were moistened with seed, were of this colour, and retained it afterwards; they moreover became soft, and an incavation appeared upon the surface, signs that never fail to attend sterility. These experiments were made with that species, of which the eggs are hatched only once a year, viz. in the spring. And I believe, it was upon this species, that the great naturalist of Bologna made his unsuccessful experiment.

I afterwards tried another species, which is much cultivated in the cities of Lombardy, on account of its producing three generations in a year: one towards the end of the spring, another in summer, and the third in autumn, My labour was not unprofitably bestowed upon the eggs of this species. Many which

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I moistened

I moistened with seed, produced worms in proper season; nor is there any cause to apprehend, least I should have committed some inadvertence in my experiments. As soon as the last change of form had taken place, I put the females under the receiver of an air-pump; the males were on the outside; and either the sight or the smell of the females allured them to the vessel, for they were constantly fluttering about it. By these means, I supposed that the male organs of generation would be filled with seed. As soon as the eggs were discharged, I bathed them with seminal fluid. Many of them, which were at first yellow, began in a few days to turn brown, and at length assumed a brownish violet hue; in about a week they produced little worms; those which had not been moistened with seed remained yellow, grew flaccid, and spoiled. I procured fifty-seven at two trials.

CLXXIII. This unexpected event gave me courage to attempt another experiment. It shewed that oviparous animals may be artificially fecundated. Of viviparous animals I had before observed with surprize, the artificial impregnation in some species in which this function is external. It remained therefore, as the reader will easily guess, to try
whether

whether such experiments will succeed with those viviparous animals in which fecundation takes place in the body of the female; an animal of some size, such as the cat, the dog, or sheep, seemed fittest for my purpose. This idea had been for some time fermenting in my head, and I could not refrain from disclosing it in an article written by me for the *Prospectus of the new Italian Encyclopædia*, and entitled, *Artificial Fecundation*. Having proposed some views respecting the artificial fecundation as well of plants as animals, I employed towards the end the following expressions. “Hitherto I have spoken only of animals usually called oviparous; artificial fecundation may perhaps, by different means and such as would act within the body, be extended to viviparous animals. The reader understands my meaning.” In the CLVIIIth paragraph of this Dissertation I threw out the same idea, and exhorted Naturalists to put it in practice; for after I had succeeded so easily in impregnating different animals of another kind by art, I could not consider my project as very unpromising. The event of my experiments on silk-worms, in which impregnation is internal, rendered my expectations still more sanguine, and I immediately set about to bring them to an
issue.

issue. I chose a bitch spaniel of moderate size (*a*) which had before had whelps. Suspecting, from certain appearances, that she would soon be in heat, I confined her in an apartment, where she continued a long time, as will be seen below. For greater security, that she might never be let lose, I fed her myself, and kept the key the whole time. On the thirteenth day she began to shew evident signs of being in heat; the external parts of generation were tumid, and a thin stream of blood flowed from them. On the twenty-third day she seemed fit for the admission of the male, and I attempted to fecundate her artificially in the following manner. A young dog of the same breed furnished me, by a spontaneous emission, with nineteen grains of seed, which were immediately injected into the matrix, by means of a small syringe introduced into the vagina. As the natural heat of the seed in animals of warm blood may be a condition necessary to render fecundation efficacious, I had taken care to give the syringe the degree of heat which man and dogs are found to possess, which is about 30° (*b*). Two days after the injection, the bitch went off her

(*a*) *Canis aquaticus*, pilo longo crispo, instar ovis. Linn. Syst. Nat.

(*b*) 99 or 100 of F.

heat,

heat, and in twenty days her belly appeared swoln, which induced me to fet her at liberty on the twenty-sixth. Meanwhile the swelling of the belly increased; and sixty two days after the injection of the seed, the bitch brought forth three lively whelps, two male and one female, resembling in colour and shape not the bitch only, but the dog also from which the seed had been taken. Thus did I succeed in fecundating this quadruped; and I can truly say, that I have never received greater pleasure upon any occasion, since I have cultivated experimental philosophy.

CLXXIV. I have before observed, that nineteen grains of seed were injected; but it is proper to add, that the whole quantity did not pass into the uterus. Thirteen at most arrived there, since six at least adhere to the sides of the syringe. But we are not to conclude, that impregnation was effected by all of them. It seems beyond dispute, that a much smaller quantity is sufficient. As the effect is produced in the ovarium, the seed must pass through the tubes and who does not perceive, that some must be retained by their sides, as also by those of the uterus? A very small portion must therefore, in this case, have produced fecundation. Combining this experiment with those made upon amphibious animals, in which we have seen
how

how small a quantity of seed suffices, it may with great probability be inferred, that the dose of that vital liquid, by which nature is renewed, is always exceedingly minute, as well in the large as small animals. This inference is still further confirmed by observations relative to birds; from which we learn, that a cock fecundates at once all the eggs laid by the hen in twenty days. Hence, as one is sufficient for twelve or even twenty hens, he may every day be the father of thirty chickens (a).

I conclude with a brief reflection. Considering my last discovery, I have no difficulty in believing, that we shall be able to give birth to some large animals, without the concurrence of the two sexes, provided we have recourse to the simple mechanical device employed by me; and, at the same time, take advantage of such favourable circumstances as may promote the experiment, and use those judicious precautions which are indispensable. Meanwhile, reflecting upon the phænomena which I have related, I am inclined to exclaim with Pliny, *Mibi intuenti sæpe persuasit rerum Natura nihil incredibile existimare de eâ.*

(a) Buffon, histoire des oiseaux.

END OF DISSERTATION II.

TWO

T W O

L E T T E R S

F R O M

MR. BONNET, OF GENEVA,

RESPECTING THE FOREGOING

D I S S E R T A T I O N S.

A R G U M E N T.

A LITTLE before I delivered to the Printer the two Dissertations on the Generation and artificial Fecundation of different animals, I transmitted a French translation of the analytical index to my illustrious friend Mr. Bonnet; for my numerous engagements did not allow me to send him an abridgement of the Dissertations, notwithstanding I, as well as he, very much wished it. The great brevity of my communication produced effects, equally advantageous to the public and myself, but such as I did not at all expect. From its very nature, the index
could

could only serve to convey a summary of the contents, and was merely calculated to induce the reader to consult the work itself. When I considered that the Dissertations would soon appear, I supposed that my friend would be satisfied with the imperfect account I sent to him. But I was mistaken. The perusal of the index excited a desire of fuller information concerning several particulars: and he wrote a long and obliging letter, which drew from me an answer, in which I endeavoured to reply to the several pertinent questions proposed by Mr. Bonnet. My answer was followed by a second letter, not less polite and interesting than the former. Such was the occasion of these two letters. I consider it as a singular advantage, that I am authorized to publish them; not merely on account of their close connection with the Dissertations, but also on account of their good sense, the philosophical views, and profound reflections which they contain; and because they are recommended by that elegance (if I may be allowed to judge concerning what is written in a foreign language) which ever characterises the productions of this writer.

In the first, those passages of the analytical index, which require explanation, are frequently transcribed. It would have been superfluous

perfluous to publish my reply, containing these explanations, as I took it from my book: there the reader will find them, by having recourse to the numbers of the letter, which correspond to those of the Dissertation. With this help, he will also enter better into the reasons and reflections of Mr. Bonnet upon those passages of the index, of which he did not ask for any explanation.

The second letter, which relates to my explanations, is in general very perspicuous. In those places, both of this and the first, which seemed to require some previous information, I have placed a few notes at the bottom of the page.

L E T T E R I.

Genthod, Nov. 29th, 1780.

My dear Friend,

I WAS going to write to you at the time I received your interesting letter of the 7th, for which I return you many thanks. My health, it is true, has been very much disordered this year. In the summer, I had two tedious catarrhal fevers, one immediately succeeding the other; they harrassed me exceedingly, and very much affected my weak eyes. The violent and repeated efforts of coughing, drove the blood into them in too great abundance. I could do nothing all July and August and part of September. When I was first attacked, I was very busy about my numerous annotations on the *Contemplation*. I had gone as far as the tenth part. I could not resume this employment immediately upon my recovery; it required too much application for my weak state. I therefore set about the revival of the different memoirs I had published in Rozier's *Journal*. Hence I was led to compose new ones on bees, snails, and newts. They will compose the Fifth Volume of the edition in Quarto; and

and that the press may not remain unemployed, I have allowed my editors to print them as they are sent. The first volume of the *Contemplation* was printed, when they began the fifth volume. The *Contemplation* will take up the fourth volume. It will be enlarged about one-third. Hence it became necessary to divide the volume into two parts. It will occupy three volumes in octavo. You will easily believe, that your name frequently occurs, and I am always highly gratified, when I have occasion to mention your fine discoveries. But I have great reason to regret, that I did not sooner receive the synopsis of the new experiments you have obligingly communicated to me. As I treat in the seventh and eighth parts of the reproduction of animated beings, I cannot, as those parts are already printed, introduce many curious particulars noticed in your index. But I foresee, it will not be difficult to introduce many of your observations into the chapters not yet printed. I will, therefore, go over the principal articles, observing the order of your numbers.

1. I perceive that you have found, by many experiments, that the fetus exists before fecundation in the green aquatic frog, the tree-frog, the great terrestrial toad, with red eyes

S

and

and dorsal tubercles, the fetid terrestrial toad, and in two species of the water-newts (*a*). This short list will doubtless be enlarged, as soon as your curious experiments are extended to other species; as you will always have the distinguished merit, of having opened so rich and beautiful a career. All the direct means of proving the pre-existence of the embryo in birds and quadrupeds, have by no means been employed. I, you know, have never doubted of this pre-existence: all my reflections upon generation, even in my youth, led me to consider it as the most universal law of Nature. To contrive, therefore, expedients for exposing the germ, which is undoubtedly contained in the vesicles of the ovarium of large viviparous animals before the access of the male, is the first requisite. Its transparency probably contributes as much as its minuteness, to render the germ invisible before fecundation. Whatever would tend to lessen its transparency, and to coagulate the embryo, would bring it within the power of glasses; such experiments, so likely to illustrate the important and obscure subject of generation, have been little practised; and I foresee, that if you will but consent to

(*a*) N^o 1. of this and the second letter, are the only Numbers which have none to correspond with them in the Dissertations.

descend

descend into this of mine, you will bring away unexpected wealth; for Nature treats you as a favourite child, because you understand how to interrogate her properly. But though we should, not be able to obtain a distinct view of the germ in large quadrupeds and birds before fecundation, we may very justly presume, from what we most certainly know concerning this interesting subject, that the germ exists before impregnation, or that its formation is not owing to the concurrence of the male and female, but bears date *à primordio*. Hallar's demonstration of the pre-existence of the chicken is not strictly direct: it does not produce the germ itself before fecundation, but establishes only the continuity of its membranes with the yolk, which incontestibly exists before fecundation. This inquiry has, I think, been abandoned too soon; it has too hastily been determined to be beyond the reach of human abilities. I am persuaded, that if it had been conducted with the same perseverance, pains, and sagacity, as you have shewn in your profound researches concerning the animalcules of infusions and amphibious animals, far more direct proofs of the pre-existence of the germ in the females of oviparous and large viviparous animals, would have been obtained.

There is some contrivance, not yet conceived by physiologists, which, the moment they shall discover it, and put it in practice, will afford the demonstration we so much desire. Men should never be in haste to conclude that any inquiry is impracticable, merely because there appears no relation between the present means, and the fact to be ascertained; for have all the combinations of these means been tried? If not, who can be certain, that he is acquainted with all the relations between these means and the fact, which is presumed to exist, and proposed to be ascertained? This reflection is strengthened by numberless instances, recorded in every page of the history of natural philosophy, and more especially of physiology. Consider how many inventions, for which the ancients would scarce have dared to hope, even if they had had a distant glimpse of them, have enriched these sciences! Could they have imagined, that the artificial fecundation of the germs of various animals would one day be accomplished, and that chickens would be hatched without the aid of animal heat or of a stove? Need I mention those admirable chirurgical operations, the miracles of the healing art, which are scarce credible even to those who behold them? We are as yet incompetent

competent judges of what is impossible in philosophy; for we always decide from our present acquisitions of knowledge, which bear no proportion to Nature. Nature is immense; the possible combinations of beings have no bounds, and the human understanding is almost always too hasty in its decisions. The conviction of our ignorance or moderate attainments, ought to preserve us from despairing of any thing in physics; we should say to ourselves, what I cannot discover, another will lay open to every eye. But enough upon a subject capable of affording a small volume of practical reflections; it is time to come to the interesting sketch of the work you are about to publish. I shall be guided by the numbers of your paragraphs.

XIII. I find here, that you have imagined a curious contrivance to prevent the fecundation of the green aquatic frog. You do not particularize the experiment, and it excites my curiosity. I must therefore take advantage of your friendly offer, to answer the principal questions suggested to me by the perusal of your sketch. You may be sure, that I shall make use of your answers. You shall be my oracle, and I am certain that its replies will be more intelligible and instructive than those of the Delphian. How have you con-

trived to prevent fecundation in this frog? Probably the experiment has suggested new information concerning the mystery of fecundation; for nothing here stands unconnected.

xxvi. I cannot here understand the sense of your expressions, “the fecundation of the eggs, though effected out of the body, penetrates however a little way into it.” Is the action of the seed propagated by the gelatinous matter enveloping the eggs? But I ought not to try to find your meaning by conjecture.

LIX. I am glad that you have distinctly seen the circulation of the blood in tadpoles, before they yet shewed any signs of motion. Many other intestine movements doubtless take place in germs, before they are sufficiently developed to move their little limbs. If germs are all originally enclosed one within another, many intestine motions must have happened in them since the creation. But this admirable spectacle is reserved for those superior intelligences, whose piercing view penetrates into the most hidden springs of the machine of this world. Much has been said of the involution (*emboîtement*) of germs; the term is improper: germs are not little boxes enclosed one within another; they must have
been

been integrant parts of the first organized bodies that came from the hand of the Creator. I have insisted on this point in one of my new notes on the *Contemplation*. It is of consequence to fix the meaning of terms precisely.

LXXV, LXXVI. You are then convinced, that the eggs of scaly fishes lose by desiccation the power of being productive. You have seen the same thing happen to the supposed eggs of tadpoles; and you conclude, that the assigned explanation of the re-peopleing of dried ponds is false. I was therefore mistaken, when in *Art. cccxviii. of the Considerations on organized bodies*, I thought of applying to the eggs of fishes what my illustrious friend Trembley had observed with respect to those of the *tufted polype*, which may be kept several months in the dry without losing the power of producing. I however threw out an invitation to Naturalists on the subject of fishes eggs: I said, “ that it would be a curious experiment to keep different fishes eggs for some time in the dry, and then distribute them in proper places. This simple trial would ascertain, whether in such a state they can serve to perpetuate the species. Nature is not subjected to undeviating exactness: In her operations there is a latitude

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which

which the Naturalist ought to study, and which experiment alone can detect." You have accepted my invitation, and your experiments have not been favourable to my conjecture. As you have not pointed out your manner of proceeding, I am unable to judge whether it excludes the most common natural circumstances in ponds and pools. Many trivial particulars may produce a variation in the results. You easily guess what I have in view; but I am not less inclined to acquiesce in the falsehood of my conjecture.

LXXX. *Strange way in which the male newt impregnates the female without copulation.* I have never seen the fecundation of the newt; but my curiosity is much raised: I beg you will inform me in what this strangeness consists! My brain has long been disposed, by frequently taking cognizance of prodigies, for the admission of the strangest things; for such surely is the reproduction of the limbs in the newt, which has engaged my attention for some years, and still engages it. I should probably have seen the act of fecundation, if I had kept individuals of different sexes in the same vessel: but my only aim was to observe with my own eyes these beautiful organic productions.

LXXXIV, LXXXV. You treat in these articles of the natural fecundation of the eggs of the newt. I am totally unacquainted with it, and should be obliged by a brief explanation.

XCVI. *What foresight do amphibious animals shew in the multiplication of the species?* Nothing in the study of animals interests my curiosity so much as their *foresight*. We frequently commit mistakes on this subject, and are apt to form very unphilosophical ideas; but of this I have said enough in the *Contemplation*, and elsewhere. We impute to animals, with wonderful facility, our own foresight and our reasoning faculty. Could they judge concerning us, they would reduce us to their standard; for they would attribute to us their sensations and actions. Authors have run into the most opposite extremes in considering this topic; I have sought the medium, and hope that I have found it. You have a good right to decide.

XCVII, XCVIII, XCIX. I find here something relative to the industry of animals, and I dwell upon it with pleasure, having devoted one whole chapter of the *Contemplation* to the amours of the toad which you consider here. You enquire, *Why the male frog and toad*

toad embrace the female so closely and so long? I had presumed, that such a long continuance had some secret final cause; but I was unacquainted with any observation which could guide me to that cause. I expect from your friendship the instruction which I want. I should suspect, that the embraces tend to facilitate the descent of the eggs or the tadpoles into the ducts, and thence into the uterus, and moreover, perhaps to aid the expulsion of the embryos. I meet in every part of your sketch with enigmas, and hazard my conjecture without hoping that it is the true explanation.

CII. Has the interesting observation of Mr. Demours, concerning the amours of the toad, of which I made use, given occasion to any critical reflection? Was the observer deceived? He certainly stood in a good situation for observing.

CV, CVI. Here you do not seem inclined to abide by the current accounts of the fecundation of scaly fishes, You speak of *uncertainties*, what are they? Do you doubt whether the male emits his *milt* upon the eggs? Or whether he repeatedly rubs the belly of the female? Or of both these accounts? My own faith was very unsettled upon these points. I could no where find decisive observations.

vations. It is astonishing sometimes to see Naturalists remain so long and so contentedly in doubt, about the most interesting questions, and make no attempt to solve them by the easiest observations and experiments. The mind, naturally so active, is yet often very indolent. To contrive an insignificant experiment, or a small apparatus, requires as great an effort as to undertake a journey. Such observers only as you are always in breath; such only have their minds always in action and forming combinations. You have made more discoveries in five or six years, than whole Academies in half a century. I, however, who know the labourer, and the richness of the harvest, am not at all surprized. When, in 1765, you obligingly asked my opinion on the department of Natural History to which you should apply, I easily foresaw how much that science would one day be indebted to you. Your first production on the animalcules of infusions, fulfilled my prediction, and your interesting letters upon the wonderful reproductions of the earth-worm, the snail, and the newt, were new confirmations of it. I have contemplated, from the silence of my retreat, your gigantic progress in that fine career.

CVIII. From this article I learn, that the fecundation of the newt differs from that of other animals; but I am not informed in what the difference consists. The modifications of the great laws of Nature chiefly excite the attention of the philosophical Naturalist. They strike him with the greater force, as they shew the endless variety of the CREATOR'S resources, and of those subordinate means, by which his WISDOM prepares the first developement of animated beings. It was not consistent with the œconomy of the world, that all generations should exist together in a state of complete evolution. The earth could neither have contained them, nor supplied them with food. They were, therefore, placed one within another, in a series continually decreasing, and lost at last in the abyss of infinite littleness. The generations are therefore developed, one by means of another, and their growth is proportional to the order of degradations. Thus they are gradually advancing out of impenetrable darkness, and at length arrive at the term, which separates the visible from the invisible, and from which, by the aid of fecundation, they insensibly arise to the degree of perfection competent to the species. But as animated beings are greatly diversified, so
likewise

likewise are the laws which preside over their developement. Hence result many varieties in the forms which they successively assume, and in the mode and effects of fecundation. The delineation of these phases and varieties, constitutes the most engaging part of the history of generation.

CXXIII, CXXIV. From your numerous experiments on artificial fecundation it appears, that attempts of this kind upon germs, while yet in the ovarium, or at the upper end of the ducts, will fail. I can, I think, assign the reason. The seed acts on these fetuses as a simple stimulant. Now there is an original relation between the latent power, which causes the irritability or the contraction of the muscular fibre, and the present state of that fibre. If it has not yet attained the necessary degree of consistence, it will not be in proportion to the mode of action of that power, and its impression will therefore produce no effect. The germ must have arrived at a determinate growth before it can be susceptible of irritation. Such was the reasoning of the great Haller.

CXXVIII, CXXIX. I find here a particular which embarrasses me. *Artificial fecundation*, you say, *does not succeed in the newt with pure seed, and it is necessary to dilute it with water.*

water. But *pure seed* is the efficient cause of natural fecundation: how then comes it to fail in the artificial? does the air inspissate it too much, or is it mixed with some thinner fluid at the instant of its emission from the male. I know not what to think. You will instruct me.

CXXXIV. *The seed does not lose its prolific virtue, though incorporated with other liquors.* I have great pleasure, my dear philosopher, in seeing you found Nature, by means of combinations which she never made. Preceding physiologists did not conceive the idea of such combinations. But what liquors did you mix with the semen? It does not, it seems, mix intimately with them; there is no chemical solution, since the sperm retains its prolific virtue---it is at least very certain, that it is not decomposed by these different liquors, for decomposition would destroy its stimulating power. How do you reason on these facts?

CXLIII. *Three grains of seed incorporated with eighteen ounces of water, retain perfectly their fecundating virtue in the green aquatic and in the tree-frog. Three grains diluted with two hundred and sixty-four ounces of water, do not altogether lose their efficacy. This would never have been suspected; what wonderful energy*

energy must this singular stimulant possess ! since it is not sensibly weakened, when three grains are mixed with eighteen ounces of water, and is even perceptible, when they are diluted in two hundred and sixty-four ounces ! these three grains of seed apparently are diffused through this body of water, as three grains of musk are diffused through the air of a large apartment ; they nevertheless act upon the smell, and irritate the nervous fibres ; this irritation illustrates the effect produced by seed diluted in a large quantity of water, upon the muscular fibres of the heart of the embryo.

CXLIV, CXLV. *Other remarkable experiments upon a little seed, mixed with a great quantity of water.* Experiments so new and instructive, could not be too much varied ; and it is with great satisfaction I observe, that you have taken advantage of Nature's complaisance. I beg you will give me further instruction on this head.

CLII. *The seed fecundates tadpoles by penetrating into their diminutive bodies. Small openings for its admission.* Such circumstances have always been more interesting to me, than any thing in the history of fecundation. I have, always, you know, presumed that fecundation was effected *from without* ; and
you

you may recollect, that I once desired you to search for the little pores, which I imagined Nature had wrought in the covers of the germ, for the introduction of the seed: You have now discovered them, and I congratulate you most sincerely. The discovery is of high importance, and I am quite impatient for the particulars, I have observed in a new note, Chap. x. part. 7. of the *Contemplation*, “ What happens openly during the fecundation of the eggs of amphibious animals, takes place in other classes in the obscurity of the ovarium. The egg is always fecundated externally, both in oviparous and viviparous animals. The supposition followed very naturally, after the embryo was allowed to exist entire in the egg: the obvious inference was, that the seed acted only as a nutritious and stimulating principle. But this simple and philosophical notion of fecundation could not be conceived by those who reject all organic preformation, and imagine, that the embryo is mechanically formed by certain powers of affinity (*forces de rapport*) or by the successive juxtaposition of certain *molecules* issuing from the male and female, which had been moulded within them.

CLVIII. *Whether the small portion of seed, employed by Nature for the impregnation of amphibious animals, affords reason to suppose that large animals also are impregnated by so inconsiderable a quantity.* I am very desirous of knowing your thoughts on a subject, which has never been properly discussed, because, before it could be properly discussed, it was necessary to make those fine experiments, which you have so well executed on amphibious animals, and which yet were not supposed to be possible. If the ovaria of an ewe, bitch or cow, could be laid bare without endangering life, your question might be decided by experiments like those on amphibious animals, which suggested to you so many new truths. You might touch with the end of a pencil, dipped in the seed of the male, one or more of the *vesiculæ* of the ovarium; and if the wound could be healed without destroying the effect of fecundation, you would find the quantity of seed necessary for this purpose in large animals. This singular experiment would perhaps succeed better on the greater oviparous species. If it is to succeed at all, it must be in your hands. You have accustomed me to expect much from your dexterity and perseverance. Should you only be fortunate enough to perceive an

incipient evolution of the germ, it would be sufficient for our instruction. There are observations which directly prove, that the fetus may be developed in the ovarium. You might also try to inject different quantities into the matrix. Should the smallest portion prove as efficacious as the greatest, it would become very probable, that the fecundation of large animals differs but little in this respect from the amphibious class.

CLIX. *The seed of amphibious animals seems to be a simple stimulant, and not a nutritious fluid.* I think I perceive the foundation of your conclusion. You cannot reconcile the small quantity of seed sufficient for fecundation, with the idea of a fluid, destined to supply all the parts of the embryo with their first nutriment. You calculate (CLV), that the bulk of this portion of seed, is to that of the embryo, as 1 to 1064777777. There can, therefore, be no sort of proportion between the nutritious fluid, and the embryo to be nourished. I shall not contest the justness of your inference, as far as it regards amphibious animals; but I do not think you can extend it to large animals. The mules produced by them furnish facts, which render it impossible to doubt, that the seed modifies various parts of the embryo: I have given
several

several instances; the seed is therefore carried to these parts, and how can it be imagined to change their forms and proportions without penetrating into them (a)? Consider, in particular, the larynx of the *great mule*. I am brief; but you, who understand my principles as well as I do myself, will easily comprehend my meaning.

CLXVII. *The seed is not a spirituous nor very volatile liquor, as some philosophers have imagined.* I am glad you have ascertained this; but after having shewn what the seed is not, have you discovered what it is? We well know, that it is one of the most highly wrought fluids in the animal body. Have you made any chemical experiments upon it? I have

(a) When the experiments related in this work, shall be attempted upon large animals, as there is every reason to hope that they soon will, with success, I do not think there is any great temerity in predicting, that this favourite hypothesis of Mr. Bonnet will be proved to be false. It was long since rejected by Haller, the most candid and acute examiner of physiological hypotheses; and Spallanzani has furnished a very powerful additional argument, derived indeed from analogy, but more cogent than any proof in favour of it. There are numberless examples, of parts of the system being deeply affected by substances not in contact with them. Nervous sympathy is called in to explain such phænomena, and to what else is it possible to impute them? Physiologists will, in like manner, I suppose, deduce the changes produced by the seed of one species upon the embryo of another, from the same cause. T.

always entertained a fufpicion, that it very much refembles the nervous fluid. I have proved, that irritability muft depend on a very active fluid, diffeminated among the muscular fibres. *Contemp. Part X. Ch. 33.* The feed of amphibious animals, which may have a certain affinity (*rapport*) with the fluid difperfed in the fibres of this impelling organ. Here we find depths that cannot yet be fathomed.

CLXVIII. *The electric fluid accelerates the evolution of fecundated tadpoles, but does not animate thofe which are impregnated.* I thank you for having tried, as I propofed, whether the electric fluid might be fubftituted inftead of the feminal liquor, in the artificial fecundation of tadpoles. It did not answer, and you may imagine, that I do not wonder at the failure. It was proper to make the trial. *Que fait-on* is a queftion that frequently recurs in natural philofophy and natural hiftory. You have at leaft found, that the electrical fluid forwards the evolution of fecundated tadpoles, a new obfervation, to be added to thofe recorded in the hiftory of electricity.

CLXIX, CLXX. *Various other liquors are incapable of effecting artificial impregnation.* That talent of combination which you poffefs,

sefs, and which characterizes the true naturalist, gave me reason to expect these various experiments. Nature, by her negative responses, has afforded you no less information, than when she has answered in the affirmative..

CLXXI. *The seed of the newt is incapable of fecundating the embryos of frogs and toads, and reciprocally. This is also true of the seed of toads and frogs respecting one another.* Thus though Nature produces mules between large animals, and even between insects and plants, she refuses them to the amphibious tribe. You very properly questioned her on a subject so important; from her answers we learn, that she has not here allowed herself any latitude. This is very remarkable; for at first sight, the toad appears to differ much less from the frog, than the horse from the ass. If the contrary had happened, and you had obtained *mules* by the artificial impregnation of amphibious animals, you would not have had such good reason for concluding, that the seed of these animals acts but as a stimulant. It would remain to inquire, why Nature has here set herself such narrow limits. If it should be said, that in species apparently so nearly allied, one degree more of approximation would have destroyed the specific character,

rafter, this would be to assign the final, but not the efficient cause.

CLXXII. *The eggs of the winged insect produced from the silk-worm, artificially fecundated by the author.* An Italian, the famous Malphigi, first imagined this experiment: another Italian, the second Malphigi, first succeeded in it. Above eighteen years ago, I exhorted naturalists to repeat this interesting experiment; and in this long interval, the philosopher of Reggio alone has undertaken it. What satisfaction must he receive on this account! But he will not confine himself to the eggs of this species; he will proceed to those of other phalænæ. He will try to fecundate the eggs of one species with the seed of another: he will desire to know, whether butterflies and moths resemble amphibious animals in this respect; and he will not, I am sure, neglect making the same trials upon other different insects. Reasoning will not illustrate this subject: experiment alone can supply the information we desire.

From all your experiments, you deduce a conclusion highly pleasing to me; all the dreams of modern epigenesists are disowned by Nature. You know how averse I have been all my life to this system; I have com-
bated

bated it in all my writings, and when my late illustrious friend HALLER was inclining to this hypothesis, I had the courage to resist him, notwithstanding the impresson his authority made on me. The chicken came to my assistance, and the great physiologist declared against epigenesis.

I have gone through the principal articles of your table: many things are left behind, but the revival of my works does not permit me to attend to you as much as I could wish. I pass to your kind letter of the thirteenth of March, which I have not yet answered.

I knew not that the celebrated Merian had observed the Pipa before Ruysch. I have made use of the passage from that heroine, which you obligingly transcribed for me (*a*). I have inserted it in an addition to my Memoir. I have made the same use of that from Vallisneri, which was also quite un-

(*a*) In a letter, written March 13, 1780, to Mr. Bonnet, about his Memoir on the Pipa, inserted in Rozier's Journal for 1779, and of which I speak incidentally in Sect. CIV, I informed him that Merian, and not Ruysch, as he supposed, first spoke of this singular animal; and I moreover communicated to him the erroneous opinions of Vallisneri, who affirmed, that the kind of uterus containing the young toads, is upon the back of the male, and not of the female. To this paragraph of my letter Mr. Bonnet alludes.

known to me. Your censure of his negligence, in not ascertaining the sex of his two pipas, is just. I have dissected, or rather flead the pipa left in our bottle; but the fatigue my eyes underwent, together with the quantity of spirits which had penetrated into the inside, hindered me from observing distinctly every thing that lay before me. I made my incision on the belly, and after turning aside the skin, perceived the muscles and internal teguments. Having removed them, I discovered the intestines. I was first struck by a number of bright yellow grains that were dispersed over the viscera. In order to learn whether their shape was regular, I observed them with a glass, but it appeared to vary a good deal; some being round, others oblong, and others again like quadrilateral plates. The stomach was oval, muscular, and thick; it was filled with many small brown fragments, very brittle, some a line or two in length, and pretty much resembling fragments of the leaves of plants.--- But to come to our chief object, I could not perceive any vestige of an ovarium; I sought for the testicles, and found, near the bladder, an oblong fleshy body, which might perhaps be a testicle, but I could not find that on the other side. I could not, indeed, perform
this

this dissection according to my wishes; I saw just enough to form an idea of the great apparatus of viscera, which this singular toad offers to the attention of the observer; every one of which might employ him whole months, if he had the inclination or the ability to study it as Lyonet studied his *eruca*. To refute Vallisneri's opinion, it is not absolutely necessary to have recourse to dissection. Mr. Fermin's observation, of which I have given the substance, Sect. cccxxvii, *Corps organ. n. 1.* leaves no room to doubt that the female's back is furnished with the cells, since he dissected her, saw the ovarium, and was an eye-witness of the union of the sexes. I am glad my Memoir afforded you satisfaction. I wish the questions at the conclusion may induce some Naturalist to examine more particularly an animal so remarkable, and as yet without parallel.

I thank you for the Italian extract from your researches on the movement of the blood, which I could not read in your excellent work. Mr. Saladin translated it, and I shall borrow from it some curious particulars for a note on Part x. of the *Contemplation*. I ask nothing further on this subject, for my editors press me to finish as soon as possible. The subscribers grow impatient, and threaten

to withdraw their contributions. I have been obliged to write a short letter in excuse of my involuntary delay, and to satisfy them about the delivery of the subsequent parts.

Your conclusions, relative to the blood, have entertained me highly. You first have discovered an important truth; the impulse given by the heart is perceptible to the commencement of the veins. That the motion of the blood does not, as physiologists supposed, become slower at the extremities of the arteries, is another new observation of equal importance. They were mistaken about the power or powers of circulation. They assigned to the heart auxiliary forces; and you have shewn, that the heart is the sole impelling power. You have also proved, by an ingenious contrivance, that the changes of the colour of the blood, from yellow to reddish, and then to red, are mere illusive appearances. Haller warmly contested this point, but your mode of proceeding, I think, carries conviction. The vascular membrane, separated from the yolk of the egg, and observed upon a plate of glass, exhibits red vessels on the first days of incubation. You almost every where destroy error, and establish

blish truth. I most sincerely bid you farewell. You say nothing of the fecundation of plants: I have not been able to repeat your experiments this year.

END OF LETTER I.

LET-

L E T T E R II.

Genthod, January 13, 1781.

I PERCEIVE by your interesting reply, my dear and celebrated friend, that our opinions on several points coincide; this coincidence affords me the greater pleasure, because it shews, that I have reasoned justly on several of your experiments. But such a coincidence is no new thing between you and me, for how often have we *converged* in like manner on several topics of Natural History? It may be said, that my soul sometimes passes into your brain, and your's into mine. I owe you many acknowledgements for having interrupted the composition of your work on the generation of plants, in order to write that long and excellent letter, which you call upon me to answer. I am surpris'd that you have been able to do it in two days. I am not so happy as you in this respect, being able to allot a few hours only in a day to composition, so that when I write letters of eighteen or twenty pages, you may be sure that they have taken up at least twelve days. I must therefore now, in my turn, suspend my own labours, for the sake of answering the principal

cipal passages of your letter of the 12th of December. I shall follow the order of your numbers, or rather of my own in my last letter, which you yourself followed, and to which you replied.

I. I doubted not but the experiments which I proposed to you, in order to detect the germ in the ovarium before fecundation, would likewise suggest themselves to your consideration (*a*). You seem not to expect much

(*a*) In order to elucidate this paragraph, I shall transcribe the passage of my letter to which it refers. Mr. Bonnet, in his first letter, observed, that searching for the embryo in the ovaria of quadrupeds and birds, before the access of the male, would certainly illustrate Natural History: He was of opinion, that I might make some advances in this dark enquiry. I reply, "That the idea of turning my attention towards the ovaria of quadrupeds and birds, had often arisen in my mind; but that I had never enjoyed sufficient leisure to put it in practice. The urgency with which you recommend those researches, is a powerful incentive to me to attempt them, and perhaps I may do this ere long; but I confess that my expectations are not very sanguine. I fear, lest the extreme minuteness and transparency of the germ should prevent me from discerning it. It is, however, better to undertake unpromising experiments on a subject so interesting, than to leave them unattempted.

"Having lately had occasion to read some of Reaumur's memoirs on insects, I met with an observation of an Italian Naturalist, very analogous to mine, upon the pre-existence of the fetuses of the frog and toad. It is in Vol. III. P. 1. Mem. 7. Reaumur, speaking of the caterpillar of the little beetle (*scarabeus*) of the lily, after the fine dissertation of Patarol, says, "Mr. Patarol thinks, that the grubs cannot be said to come from the eggs, but that every egg appears to be

much from them: You presume that the extreme minuteness, as well as the transparency of the germ, would conceal it from all your researches. You do not however despair; and you give me room to suppose, that my exhortations contribute to encourage you: but the great success which has crowned your researches on other occasions, might serve to persuade you much more powerfully, that you will not on the present labour in vain. A lucky accident, some unexpected and unforeseen circumstance, may afford you the greatest assistance. You will create such fortunate occurrences as do not happen in the ordinary course of Nature, and as will surprize you with a full view of that which Nature concealed from Malpighi and Haller. It seems to me, that the first step necessary is to find the means of diminishing the transparency of the germ without altering its structure; for in my opinion this, rather than its extreme minuteness, keeps it concealed from the most

be changed into a caterpillar. The reason that induced him to propose so singular a notion, was, because he could not find, with whatever care he examined, any empty shell." Observe, I beg of you, the conclusion of the French Pliny; he appears desirous of repeating the observation, though other occupations prevented him, it would seem, from executing his intention. I am resolved to repeat it next spring, and I think Reaumur did not set a sufficient value upon it."

piercing

piercing looks of the observer. A drop of vinegar, or spirit of nitre, poured on the cicatricula of the egg, by a little condensing the moisture which dilutes the solids of the germ, may perhaps render them perceptible. You might also try other liquors. Two other means suggest themselves to my mind: The first consists in endeavouring to spread a liquor coloured by some vegetable tincture over the yolk: how do we know but the vessels of the yolk would absorb this tincture, and carry it to the germ? Should it only colour the contiguous parts, it would at least shew its place or point. The action of the vessels should be aided by a gentle heat. The ingenious process employed by Mr. Beguelin, to shew the Prince Royal of Prussia, the successive progress of the chicken in the egg, would not be unserviceable to you in your attempts. Again, who knows but a certain degree of heat would contribute to render the germ apparent, by coagulating its lymph? To substitute the semen of the cock, or any other bird, would be another means of attaining the same end. As the semen is, beyond controversy, the liquor which has the greatest influence upon the germ, it seems to be best adapted to produce some sudden change,
such

such as might render it accessible to the microscope.

It would be, at the same time, an attempt to produce the artificial impregnation of the germ in the egg. Mr. Beguelin's apparatus would be very convenient for this purpose. Should a drop of seed render the embryo immediately perceptible, it could not be objected that the seed had conveyed it into the *cicatricula*, as it has been supposed that the *farina fecundans* does the plant into the grain; for Haller's observation, as well as your discovery of the existence of the tadpole before fecundation, would destroy the objection. Lastly, it would be proper to try to observe the *cicatricula* of unimpregnated eggs in the solar microscope, which enlarges objects which are in some measure transparent more than any other. Need I add, that you will not be seeking for something which does not exist, since you have the strongest reasons to suppose, that the germ does really exist in the *cicatricula*.

Curious experiments have shewn, that the perspiration of the egg, notwithstanding its crustaceous cover, is considerable. Various internal changes proceed from this. You will therefore observe the *cicatricula* in unimpregnated eggs kept for a longer or shorter time.

time. Perhaps drying the *cicatricula* a little upon a plate of glass, before it is observed by the microscope, may be of use. The approximation of the elements of the solids, may somewhat lessen the transparency of the germ.

It will be proper, my dear Malpighi, to observe the real or supposed eggs of the beetle of the lily. The passage you have quoted from my illustrious master shews clearly, that he saw the necessity of repeating Patarol's observation. It is to be regretted that he did not do it, or at least inform us whether he had or not; but we may reasonably doubt, whether this illustrious Naturalist imputed to this observation the same degree of importance as you and I. He had not meditated so much upon generation, and particularly upon the interesting question of the pre-existence of the germ. You may easily suppose, that I shall not be much surpris'd if you should one day inform me, that the supposed eggs of this beautiful little insect are not really eggs, but the caterpillar disguised under the appearance of an egg. If so, these imaginary eggs would agree with those of frogs, and we should have a new proof of the pre-existence in question. You are acquainted with the eggs of the queen bee; I

have some suspicion, that they are not real eggs, but the maggot itself, in a shape little different from that which it assumes after fecundation. But the viviparous flies, of which Reaumur has written the curious history in vol. iv. will surely afford new proofs of the pre-existence of the embryo in the female. The spiral matrix, described by the author, is a beautiful organ; the different convolutions he informs us, consist of more than 20,000 maggots, arranged lengthways by the side of each other.

XIII. I am obliged to Messrs. de Reaumur and Nollet, for those little breeches of waxed taffety, which they contrived for the male of a certain species of the frog, in order to discover the manner in which he impregnates the female, and I am not less obliged to you for the repetition of this ingenious experiment. The male, which you cloathed with these breeches, did not accomplish the impregnation of the female, because the semen was intercepted. Since this liquor impregnated artificially the tadpoles to which you applied it, there can be no doubt but that it was real semen.

XXVI. You are then of opinion, that the suspicion I intimated in this article is not without foundation. I learn from this paragraph

graph of your letter a new truth, “ that in the TREE-FROG, the tad-poles are sometimes found fecundated, though they remain as yet in the rectum ; whether this happens in consequence of the semen insinuating itself into the orifice of that intestine, or because the tadpoles, scarce out of the rectum, and already moistened by the semen, are perhaps drawn into it by the motions of the female at the instant she is surprized by the observer.” Both these explanations appear much more probable than mine.

LXXV, LXXVI. I am always a great gainer, when, according to your wishes, I point out to you new experiments to make. You have then made upon the fecundated eggs of fishes that which I proposed, (Art. 418. Corps organ.): in order to ascertain, whether these eggs might be kept in the dry, like those of the tufted polypus ; and you have found, that they do not possess this privilege. Your various ways of proceeding, permit me not to doubt of the truth of the result. You have carried this experiment still farther to the fecundated embryos of frogs and toads, and you have found, that they do not, any more than the eggs of fishes, possess the property of keeping in the dry. My hypothesis then, with respect to the re-peopling of dried ponds,

is insupportable: but may not this privilege, which has been refused to the eggs of fishes, have been granted to fishes themselves in the state of infancy, or at some other period of their life. I am very desirous of knowing the conjecture you substitute instead of mine, and which you intend to explain in your work. Reading this passage over again, I perceive a circumstance that had escaped me; you say, “that you left your fishes eggs to dry in the shade, *sometimes on the mud*, where you found them, and sometimes in vessels.” But may not the fecundated eggs of fishes require, like the *Rotifer*, to be covered with sand, in order to be preserved in the dry? I can scarce suppose, that you neglected covering them with the mud on which they were laid by the female, well knowing, that in your experiments you neglect nothing (a).

LXXX. Your account of the singular manner in which the male newt impregnates the female has afforded me great pleasure; the whole was entirely new to me. The newt

(a) Mr. Bonnet’s opinion of me is too favourable; for in truth I was not aware of this precaution. But were it employed, I do not think it very probable, that the eggs of fishes, and the fetuses of frogs and toads could be preserved. The thing is however possible, and the experiment ought to be repeated with this circumstance, which I propose to do when I have an opportunity.

is then very chaste in his amours; no true copulation takes place between the two individuals; only a few careffes on the part of the male, which prepare the female for fecundation. The male darts his semen into the water; it forms a little whitish cloud, which surrounds the open and swollen anus of the female, and she is fecundated. What pity, that the poets were unacquainted with the chaste amours of this animal---their fictions would have derived great advantage from the history. That of Zephyr and Flora bears a strong analogy to the fecundation of the palm; in the animal kingdom, I know nothing which resembles it more than the impregnation of the newt. That of marine plants approaches still nearer, the male does not emit a fine powder, but a liquor, which, in like manner, forms a slight cloud in the water.

Your citation of Bomare's Dictionary, has induced me to read the article *Newt*, which I had never before consulted. Mr. Demours, I see, seems to have made the same observations as you, respecting the singular mode of impregnation in this animal. But naturalists will rely more upon you, and it required to be verified by an observer of your eminence. Du Fay's remark also concerning the presence

of the gills, when newts are young, and their disappearing totally afterwards, drew my attention. I had observed the same thing with surprize, but never mentioned it, for I was desirous of observing it again, but never did. These gills are a great ornament to the young newt. Du Fay was aware of the organization of the epidermis, mentioned in my first Memoir; and if I had been acquainted with the passage of this academician, I would have taken notice of it.

Since the semen of the male is always mixed with water, I see the reason why artificial fecundation does not succeed with *pure semen*. The observer must imitate nature and dilute it with water. I suppose with you, that the very thick seed of the newt requires dilution, in order to effect both natural and artificial fecundation. In like manner, the wisdom of Nature has found the means of diluting the human semen by the lymph, which so many vessels pour into the testicles and the seminal vesicles. Physiologists tell us wonderful things on this subject.

Few spectacles are so engaging to the philosophic observer, as that presented by the amours of animals, and the various means by which the Author of Nature has ordained
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that they should preserve their species. Should some able physiologist ever undertake to compose a complete history of generation, he would undoubtedly begin by a delineation of the amours of animals and plants; and if he should be as great painter as the illustrious Buffon, he will be able to engage the understanding, without giving any alarm to modesty; he will produce, not a *physical Venus* (a). but a *physical Minerva*. There is room for supposing, that the different modes of fecundation, observable in different animals, are proportional to the degree of sensation accorded to each species, or what amounts to the same thing, to their capacity for enjoyment. What difference in this respect between the fish or newt, and the ape, the stag, or dog; and in the imperial race of man, how is the physical part modified by the moral!

It is certainly very remarkable, that amphibious animals, such as toads and the tree-frog, never deposit their embryos on dry ground, where they must infallibly perish, and that they always take care to deposit them in water, their natural element. You even give me to understand, that they do not

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(a) *Venus Physique*, the title of a book written by M. Maupertuis. T.

lay them in the first water they find, that they never make the discharge in running waters, which would carry the young away, and not supply them with proper food; but that they constantly deposite them in stagnant waters, where the little tadpoles are not exposed to concussion, and where they are always surrounded by proper food. This kind of instinct very nearly imitates foresight, and attains its end equally well. But since we cannot, in this case, admit real foresight, which belongs exclusively to reason or intelligence, it remains to be ascertained, how our amphibious animals are so unerringly determined to quit the ground, for the sake of laying their eggs in dormant waters. The female, I should imagine, pressed by the desire of laying, must feel a certain internal sensation, which renders her abode on dry ground painful, and inspires her with the desire of gaining the water; and since stagnant waters are not so cold as running waters, this may, perhaps, be the reason why she prefers the former, not for the sake of her young, of which she cannot have any knowledge, or foresee the wants: for it is thus that Nature hath, on all occasions, provided for the necessities of young animals; she has found means to connect their wants with those

those which the parents themselves feel in certain circumstances. Your memory must suggest so many instances, as to render it unnecessary for me to point them out. Besides I see you entirely agree with me, with respect to the foresight and intelligence, so gratuitously, and so unphilosophically attributed to brutes.

xcvii, xcviII, xcix. I knew not, that your illustrious compatriot Vallisneri had entertained the same idea as myself, concerning the effect of the long continued embraces of male frogs and toads. Nor did I recollect that Swammerdam, on the contrary, had supposed, that so far from facilitating the passage of the eggs into the tubes, they rather serve to hinder it. I should not then have known, which side to have taken between these two great authorities, if Nature herself had not pronounced her decision from your lips. You inform me, that the opinion of Swammerdam, that the females are not embraced by the males, until the eggs have already traversed the tubes, is not generally true; that it holds only in the *tree-frog*, and by no means in the aquatic frog and in toads, but that Vallisneri is right with respect to the green aquatic frog. In this case then, no general rule can be established, as you
very

very properly remark, and we must wait till new researches have increased the number of facts.

CII. Mr. Demours had raised our curiosity to a very high pitch, by his account of the address of the male toad in assisting the female in bringing forth. His details were so circumstantial, that the truth of the fact appeared to be unquestionable, and I hesitated not to make use of it in the *Contemplation*. But it is really very singular, that neither you, my worthy friend, nor Mr. Roesel, should have surpris'd any male toad in this interesting employment. This would appear to weaken the credit due to the recital of the French observer, if testimonies simply negative could impair the most positive affirmation. Mr. Demours ought, as you observe, to have so described his toad, that we might have known to what species it belonged.

CV, CVI. Your doubts, with respect to the manner in which the impregnation of scaly fish is effected, are well founded, and we have both reasoned properly upon this subject, by estimating the authorities on either side. We know at least, from the experiment of Mr. Jacobi, that simple dispersion in water is sufficient for the impregnation of the eggs. Your idea of employing the Chinese gold fishes

fishes to clear up the question, to me appears excellent, and I cannot press you too warmly to realize it.

You will see much more in one day, than all preceding naturalists have ever observed. How many interesting questions yet undetermined, might be decided by the most simple experiments? The mind has always a greater tendency to imagine and reason, than to observe and make experiments. To what a train of reasoning had Digestion given rise, before Reaumur and Spallanzani set this function in the clearest light by their fine experiments.

CXXIII, CXXIV. You adopt then with me the Hallerian doctrine of embryos lodged in the ovarium, or in the upper part of the tubes of our amphibia, which cannot be fecundated artificially. But you assign another cause of the fact, which I did not suspect, but which appears to me, not less than to you, to contribute to produce it; since the glairy matter is the first nutriment of the fecundated embryo, and since this matter does not envelop those contained in the ovarium, or the upper part of the tubes, it is evident, that even if the semen could impregnate them artificially, they would soon perish for want of nourishment. Your experiments on
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this subject leave nothing to be wished, since the embryos you have entirely stripped of their gluten could never be impregnated, while those which were only partially deprived of it, were almost all fecundated. I know not whether naturalists before you, were acquainted with the true use of this matter.

cxxxiv. The blood of amphibious animals, their saliva, the juice extracted from their liver, lungs, kidneys, their urine and ours, are then the different liquors with which you have conceived the idea of mixing the semen. To these you have added vinegar, and none of the mixtures have deprived the semen of its prolific virtue. You have only observed, that when the urine and the vinegar were in too great abundance, fecundation did not take place; I doubted not but that you would think with me, that the semen is not decomposed by these mixtures. But they admirably prove the astonishing energy of this fecundating liquor. They may further serve to guide you to discover, which of the animal liquors has the greatest analogy with the semen: for that, which in equal quantity should least impair the virtue of the semen, might justly be presumed to be most analogous to it; and this would not be without its use,
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in inquiries into the constituent parts of the semen.

CXLIII. It affords me great pleasure to find, that we have both had recourse to the same comparison, in order to illustrate the prolific power of semen incorporated in very small quantity, with a very large mass of water. Your example, taken from the poison of a viper, of which a very little drop often proves fatal to a great animal, is not either less appropriated or less instructive. Hence you have good reason for saying, that we cannot be surpris'd, that a very small portion of semen should be sufficient to animate the heart of the embryo.

The very sudden action of certain poisons, such as the venom of the viper, afford room for presuming, that it is chiefly the nervous fluid that is affected. You remember Reaumur's curious experiments with American poisoned arrows. A bear pricked with one of them died, I think, in half a minute.

CXLIV, CXLV. In this article, you furnish me with a detail of the manner in which you have proceeded in your artificial fecundations. I entirely approve of it. It is surely very surpris'ing, that an embryo touched with the point of a needle, which has been dipped in a mixture

mixture of three drops of semen, and eighteen ounces of water, and which takes up a drop, measuring 1-50th of a line, should have been developed as perfectly and speedily as other embryos which were immersed in semen. Your reflection on this occasion is very just; since so small a drop of semen, mixed with so large a quantity of water, is sufficient to animate the embryo, it is very natural to infer, that the surplus furnished by the male does not concur in the operation. But Nature is never sparing in what concerns the propagation of the species: she is determined not to miss her aim, and she would run the risk of missing it by too great œconomy. She, perhaps, also has an eye to the pleasure the male receives from fruition; for emission is without a doubt a pleasing sensation to him, and this kind mother is desirous that all her children should find enjoyment; otherwise too the male would want an incentive.

CLII. You easily conceive, my dear philosopher, all the attention I have paid to this interesting passage of your letter. I imagined, that I beheld with you the small pores in the cover of the embryo, contrived for the introduction of the semen. Your details on this point fully prove, that you have not suffered yourself to be imposed upon; and that
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these little mouths, of which I had suspected the existence, are certainly to be found: and since they are dispersed over the whole cover, and this cover is perforated like a sieve, it can no longer be matter of surprise, that fecundation succeeds equally well, wherever the embryo is touched with the needle, after it has been dipped in the semen. The question now is, whether such apertures exist in the covers of the embryos of every species; and how probable is this, after all that has been discovered concerning the mystery of fecundation: I do not then doubt, and I have indeed never doubted, that if the germ of the pullet, of the lamb, of the calf, were as perceptible as the tadpole, you would detect absorbent pores, similar or analogous to those in the embryo of your amphibia. I would ask, if we have not the strongest proofs that fecundation is effected *from without*, and if it be thus effected, is it not necessary, that there should be little pores prepared for the reception of the fecundating liquor? These absorbent pores and their dependencies contain, without doubt, anatomical peculiarities, which we should admire, if we were permitted to descend to the bottom of the abyss. Each pore is probably the orifice.

orifice of a vessel communicating with the heart, &c.

CLVIII. I now come to the most curious and important article of your excellent letter. I suspected not, I own, that you had already succeeded in the artificial impregnation of the female of a large animal, by means of a small syringe, a project I mentioned in my last letter. This is one of the most important and interesting discoveries, that have presented themselves to the notice of naturalists and philosophers since the creation of the world. Your mode of proceeding, and your scrupulous attention to establish, in the most rigorous manner, the truth of this artificial impregnation, put it beyond all controversy. I had no occasion for your second letter of Dec. 15, to be assured, that you had neglected nothing to obviate the most subtle objection. Your bitch was then closely penned up for twenty-three days before the operation: on the 13th she began to be in heat; on the 23d you injected the semen, and you kept her in close confinement twenty-five days longer; on the sixty-second after the injection, she brought forth three whelps well conditioned, very lively, and resembling both the dam and the dog which had supplied the fecundating liquor. Nothing can be more exact or better ascertained;

ascertained; nothing can be finer or more original than this experiment. I congratulate you sincerely on your success, and what adds greatly to it is, that it was obtained with less than thirteen grains of semen. This experiment agrees perfectly well with those which you have executed on amphibious animals, and we have good reason for inferring, that the dose of semen which produces fecundation in large animals, is very considerable. I even presume, if you could effect the fecundation of the embryos of a large animal in the ovarium, by the process I pointed out to you, you would obtain the same results as the amphibia afforded; and that a drop of semen 1-50th of a line in diameter, would be sufficient to animate the embryo.

You are now in possession of a sure and easy way of ascertaining what species can procreate together; and the experiments you propose attempting next spring, by putting your voluptuous spaniel in the company of cats and rabbits, promise not so fair as those which you will make, by introducing the semen of this spaniel into the uterus of a doe-rabbit and a she-cat, and on the other hand, by introducing the semen of the male rabbit and cat into the uterus of a bitch. You hold

in your hand a precious clue, which will guide you to the most important and unexpected discoveries. I know not, whether what you have now discovered, may not one day be applied in the human species to purpose we little think of, and of which the consequences will not be trivial. You conceive my meaning: however that may be, I consider the mystery of fecundation as nearly cleared up. What remains principally to be discovered, is the formation of the mule, and what occasions the different marks of resemblance between children and their parents; and this brings me to your CLIXth article.

CLIX. You do me great honour, my dear associate, by suspending your judgment between Haller and me, with respect to the manner in which the mule is formed. What! did not the authority of the great Haller, in your estimation, overbalance mine, which is so much less weighty? I would not have hesitated a single moment to admit with him, that the semen acts merely as a simple stimulant, could I have accounted for the conversion as it were of the horse into the mule. His hypothesis, from its greater simplicity, is more acceptable to the mind. But is it sufficient in all cases? In order to account for
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the formation of the *great mule*, is it enough to say, that the semen of the ass is a more powerful stimulant than that of the horse; and that hence it so much elongates the ears of the embryo contained in the ovarium of the mare; for how then comes a part of the embryo's tail to be obliterated? Why is its crupper so slender? and above all, how comes the larynx to be so different from that of the horse, and so nearly to resemble that of the ass? I cannot, I own, conceive that the instantaneous action of a drop of semen on the heart of the embryo can produce effects so great and so permanent; on the other hand, I have against me the complication of my own hypothesis, of which the exposition required a long series of propositions, which make it appear still more complex, and not to be comprehended, but by readers of great attention and much accustomed to analytic deductions. Hence many have committed strange mistakes, with respect to my principles and their application.

There is also another circumstance, which seems to militate against my hypothesis; this is the very trifling portion of semen which is sufficient for generation; it is not easy to comprehend how a drop of semen, so disproportionate to the whole body of the embryo,

can serve for its first aliment. But this difficulty presses on Haller as much as upon me; for it evidently implies, that one kind of semen acts with more force than another on certain parts, and occasions a more ample evolution; that the semen of the ass, for instance, *impels the blood with greater violence into the arteries of the ear*; these are his own terms: he admitted, therefore, that the semen of the ass arrives at the arteries of the ears of the embryo of the horse, how, otherwise, could the simple action of this semen, on the heart of the embryo, propagate its impression to the ears, and cause so excessive an evolution of them? Besides, how come the ears to be the only part of the head which grow to such a prodigious length, since all partake alike in the impulse of the heart. Further, Haller speaks of the power possessed by the semen, of occasioning the growth of the beard, and of lengthening the tusks of the elephant and the wild-boar: he adds, if it has this power of promoting a greater growth in certain parts of the body which prepares it, than in others, it may have the same effect on the body of the fetus which it animates. Would not this serve to shew, that our author tacitly supposes a dispersion of the semen through the whole body of the embryo?

embryo? I suppose the same thing; and you have no greater difficulty than myself, in conceiving the prodigious division of which a drop of semen is susceptible. What we know of the divisibility of matter smooths this difficulty. It is much to be regretted, that our great physiologist confined himself to mere generalities on this subject, and that he did not apply his hypothesis to the explanation of the principal peculiarities offered by the mule. “It is true, says he, my answer does not explain the mode or the mechanism by which the semen of the male excites the germ of the ear, and causes so large an evolution of it. But I am not obliged to explain this mechanism, provided my facts are well established. The influence of the semen on the growth of the beard and horns is demonstrated, though the manner in which it acts may be perhaps for ever unknown. It is sufficient to shew, that there is a certain power in the semen of the male, which determines the growth of the fetus, so that certain parts come to be more developed: It would not be more just to demand an explanation of the mechanism by which this is brought about, than the reason, why the absorption of the semen of the male produces the growth of the beard.”

I should have escaped much labour, if in imitation of my illustrious friend, I had contented myself with repeating, that the semen of the male has a certain power to cause a greater developement of certain parts. But so vague an explanation not satisfying me, I have endeavoured to analyse facts, and from this analysis I have sought a solution, which might be applicable to the most essential peculiarities of these facts. In a word, I have supposed that the strong traces of resemblance between the mule and ass, implied in the semen of the latter, something more than a simple stimulating power: am I deceived, think you, in this conclusion, and are you inclined to believe, that a simple stimulating power is sufficient for the whole? I cannot yet presume so much, but it is very possible, that a more satisfactory hypothesis than mine may be imagined, and I will be the first to adopt it.

CLXVII. You have done every thing you possibly could, in order to detect the real nature of the semen of your amphibious animals. It is not then either viscous or inflammable, acid or alkaline; and yet how wonderful is its energy! It evaporates like water, and it is very well worth remarking, that its most volatile part is precisely that which is
unfit

unfit for fecundation. This part, in all appearance, is only lymph, or rather simple serum, destined to prevent the too great viscosity of the fecundating part. It would be an interesting employment to carry on these researches to the semen of large animals: they have not been pushed so far as they ought. Nor would it be less interesting to know, whether the semen of large animals incorporated like that of your amphibia, with a great quantity of water or other liquors, would in like manner preserve its energy. The happy experiment made on your bitch, points out the path that should be followed in order to ascertain this point. The seed has been constituted in a secret relation to the nature of the power which causes irritability, since it is destined to increase the operation of this power; but we have not yet sufficient knowledge of the nature of irritability. I would not, however, venture to affirm, that there does not exist in nature some other liquor besides semen, capable of causing the evolution of the germ. Who knows, if the powder of the stamina of certain plants, may not make some impression on certain germs belonging to the animal kingdom? This is, if you please, a silly idea, but I lay before you every thing which passes through my brain.

I could wish, that the powder of the stamina of the barberry might be tried, of which the fetid and penetrating smell seems to announce great energy. Animals and vegetables compose but one family, and their analogies are very numerous. Inverted experiments of this nature ought to be attempted, for it is only by infinitely multiplying the combinations of beings that our knowledge increases. I am always a little mistrustful of our general conclusions, however apparently well founded, because our premises are always more or less particular.

Thus, my dear friend, I have gone over all the paragraphs of your excellent letter; and I wish my reflections may afford you satisfaction.

I began this long letter the 9th of December, and it has employed me till this day, the 10th of January. If you send me any account of your new experiments on the generation of plants, I shall be able to make use of it at the end of Part the xth, provided it does not come too late.

My wife was very sensible to the politeness of your obliging recollection of her. She desires me to present her compliments and congratulation on the success of your inquiries. Her health is rather better this
winter

winter than it was the last; but returns of the cholic, from time to time, make her suffer severely.

You and the university have lost a great protector: but I am certain, that her successor will patronize the arts and sciences. The Guardian Angel of Austrian Lombardy, Count Firmian, has sustained a great loss, and his feeling heart will be deeply sensible of it. Present to that Sage fresh assurances of my respect and unfeigned wishes for his welfare. Accept yourself the vows which I am incessantly offering for you, my dear and celebrated friend, and entertain as much esteem for me as I feel for you,

The Contemplator of Nature,

END OF THE LETTERS.

A DIS-

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DISSERTATION

CONCERNING THE

GENERATION

OF

CERTAIN PLANTS.

 CHAP. I.

GENERATION OF THE PLANTS, DENOMINATED BY LINNÆUS SPARTIUM JUNCEUM, VICIA FABA, PISUM SATIVUM, DOLICHOS UNGUICULATUS.

I. **W**HOEVER is acquainted with Natural History cannot be ignorant, that the three principal systems respecting the generation of animals, the system of the *ovarists*; that of the *vermiculists*, and that founded upon the *two liquors*, have been transferred, with the necessary modifications, to plants. Some think, that the embryos pre-exist

exist in the ovarium, others that they are transported thither in the impregnating powder, and others believe, that they are generated in the ovarium, by the combination of two fecundating principles, the one furnished by the pistil, the other by the stamina.

My chief purpose being to investigate the generation of certain plants, I conceive there can be no better way to arrive at truth, than to fix my attention chiefly upon the ovarium. That I might have the greater chance of surprising Nature in her operations, I determined to examine this organ at three different periods; before fecundation, at the time it takes place, and after it has been effected; or, what amounts to the same thing, after the aspersion of the pollen. I was therefore obliged to examine the flowers while they were yet closed, when they were in full blossom, for that is the season of impregnation, and after the petals had dropped. I begin with the species denominated by Linnæus and others, *Spartium Junceum*, Rushy-twiggèd broom.

II. From the nature of this plant, as well as of others without number, the flowers of the same branch are not all equally forward, some being in the state of a small bud,---
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these are situated highest on the branch; others already blown, or about to blow,---these occupy an intermediate situation; and others again fallen or falling,---these grow on the lowest part. The same branch therefore furnished matter for various observations. The smallest buds were first to be examined. They are perfectly compact, and form a solid body, scarce a line in length. If they be dexterously opened with fine instruments, the petals may be disentangled and brought into view. They are of a light green, without any tinge of yellow, which is the colour of the ripe flowers. When the petals are removed, the stamina and pistil, the organs of generation, come into view. The powder of the antheræ may be perceived; it is far from being ripe, as is evident, not only from the extreme minuteness of the granules of which it is composed, but from its want of volatility, a property it eminently possesses when mature. It is now fixed to the antheræ by means of a viscid matter. The pistil, extremely tender, arises from the middle of the flowers. If its base be freed from the surrounding teguments, and attentively examined with a glass, the pod may be seen formed about 1-10th of a line in length. If the pod be examined externally, several tumours may
be

be seen along the sides, which, when observed against the light, are found to be produced by granules lodged within the pod, or, as I shall call it, the ovarium. Upon opening the ovarium longitudinally, these granules are found to be seeds, of very small size and round shape; they are distributed in their natural order, in so many depressions or sockets, and attached by filaments (*appicature*) to the inside of the ovarium, just as the ripe seeds are in the ripe and dry pods of the plant. These seeds are not found, upon dissection, to consist of an external cover, and a nucleus composed of two lobes, with a germ or plantule, as in their mature state; but they exhibit an apparently homogeneous substance, spongy, and like a tender jelly. From the reasonable supposition which I formed, that these are the seeds, it follows, that they exist in the ovaria at least twenty days before the flower is in full bloom, or in other words, before fecundation. Flowers at least of equal size of another branch, growing from the green stem of the same plant, were not full-blown before the twenty-fifth day.

III. The dissection of several buds, larger by about one-half than the preceding, presented the following phenomena. The petals, which were beginning to assume a yellowish

lowish hue, were not now so close and compact; and the fecundating powder still adhered to the summits. The stamina were become longer and thicker, as also was the pistil. The ovarium was not so small, and the seeds had grown in proportion; their substance was not so gelatinous, though it continued still homogeneous and spongy.

In flowers a little further advanced, the only difference consisted in an increased size of the petals, stamina, pistil, and seeds. Nor was there any essential change when the petals were expanded, and the powder of the stamina, being now mature, might be shaken from the antheræ by the slightest agitation, and diffused itself in a cloud through the air. In the seeds I could not at this period discover either lobes or plantule, but they were of a greenish colour and uniform substance, which was spongy and full of moisture. Yet I could distinguish the lobes and plantule in the ripe seeds contained in those pods, which had acquired a black colour, and were grown dry. It was therefore necessary to infer, that the two lobes and the plantule are either generated, or rendered visible during the ripening of the seeds. Hence, in order to discover the generation or the appearance of these parts, I was obliged to continue my observations

observations till I had gained some satisfaction upon this curious and interesting point.

I began with the ovaria or pods, from which the flowers had fallen some time. In ten days afterwards there was no difference. On the eleventh some new appearances began to take place in the seeds. They were no longer round, but resembled an heart, of which the basis was attached to the pod by an appendix, and towards the apex, when gently compressed, there appeared a whitish point in motion. When the heart was cut open longitudinally and the inside inspected, this white point proved to be a small cavity, inclosing a drop of liquor, which had been made to move by the pressure of the fingers.

Twenty-one days after the pods were stripped of their flowers, the cavity, which at first appeared at the apex, was enlarged, and extended much farther towards the base; it was full of a transparent liquor, with which the spongy substance of the seeds was also moist. On the twenty-fifth day the cavity was more enlarged, and still full of liquor; it moreover contained a very small semi-transparent body, of a yellowish colour, gelatinous, and fixed by its two opposite ends to the sides of the cavity.

In a month the seed was much enlarged, and its shape is changed from a heart to a kidney; the little body contained in the cavity is increased in bulk, is become less transparent and gelatinous, but there appears no sign of organization.

On the fortieth day the cavity, now grown larger, is quite filled with the body, which deserves to be more particularly examined. It is surrounded by a thin membrane, somewhat viscid and tender; after this membrane is removed, the body appears bare, and without any other teguments; it is of a bright green colour, and may easily be divided by the point of a needle into two portions, in which are manifestly recognized the two lobes; within these we may easily perceive the plantule exceedingly small, and attached to the lower part. The lobes, together with the membrane, are afterwards defended by a husk or skin, which forms the outside of the seed.

The reader will easily guess what afterwards happened to the pods as they grew riper. The lobes and the plantule were only more and more developed, and by degrees acquired greater firmness. And in this manner did the seeds of the broom arrive at maturity.

v. The

v. The foregoing observations shew, 1. That the seeds of this species exist in the ovarium many days before fecundation. 2. That they remain for some time solid, and then a cavity, containing a liquid, is formed in them. 3. That after fecundation a body begins to appear within the cavity, fixed by two points to the sides; and when in process of time it has attained a larger size, it proves to be the two lobes inclosing the plantule. 4. That the ripe seed consists of two lobes adhering to the plantule, and surrounded by a thin membrane, which is itself covered with a husk or cuticle.

These deductions illustrate the generation of the plant in question. We learn, that the embryos do not appear till after the falling of the flowers, and consequently not till after fecundation, though the seeds, or, to speak more properly, the integuments of the seeds may be seen long before.

vi. Having analysed the fructification of this species of broom, I proceeded to that of the common bean, *Vicia Faba*. I began with those which had the smallest blossoms. They appear externally of a green colour, and likewise internally, if the tender petals be unravelled; here and there, indeed, the green approaches to a white. The stamina are vi-

fible; the antheræ, instead of pollen, exhibit a viscid gelatinous substance. The pistil is of a white and green colour; towards the apex it is villous; and within the base or ovarium, if it be inspected against the light, may be perceived the seeds. It may be remarked, that the ovarium of the bean more easily splits into two than that of the broom, and that the seeds of the former plant are in like circumstances larger than those of the latter. The seeds of the bean are roundish, but on one side there is an hooked and sharp beak. They are semi-transparent, gelatinous, and when opened, are found to be solid and without a cavity.

If blossoms not quite so small be examined, the powder of the stamina is perceptible; it is found to be imbedded in a glutinous substance. The ovarium having now attained a larger size, contains seeds proportionally increased in size; they are neither so gelatinous nor so transparent, but they contain no cavity. Other seeds further advanced, and belonging to blossoms about to open, or already expanded, are in the same state.

The cavity is not visible till the 3d or 4th day after the falling of the flowers; it is then found to be full of a transparent liquor. The
seeds

seeds and the cavity increase together, and fifteen or twenty days after the fall of the petals, the cavity contains a body, which, when magnified, seems to be split into two, though the observer cannot be certain of this till afterwards; when if he opens seeds nearer maturity, he will find them to divide very easily into two portions. These portions are manifestly the two lobes, within which is contained the tender nascent plantule. If the lobes and the plantule be treated with extreme care, a mucous filament is found attached to the plantule, which, after passing between the lobes, is implanted in the substance of the little seeds. The lobes and the plantule are merely developed in process of time. They are covered, as in the above-mentioned broom, with the integuments of the seed.

VII. I shall relate at once my observations upon pease and *kidney-beans*, the appearances they offered being perfectly alike. The seeds are found in the ovarium full formed, when the flowers are little advanced, and when the buds are yet scarce visible. The seeds now seem to consist of an homogeneous substance, and are without any internal cavity. The same may be said of them when
 Y 2 they

they are become many times larger, and when the powder of the antheræ is ripe. The cavity begins to be visible some days after the withering of the flowers, and in ten or twelve days more it is enlarged, and a white mucilaginous point connected by a slender filament with the seeds may be perceived in it. This point is the rudiment of the lobes, between which the plantule may be distinguished by the aid of the microscope. What follows may easily be imagined: the lobes and plantule increase in size, and their increase implies the growth of the seeds which contain the lobes.

If we compare these observations upon kidney-beans and pease with the foregoing upon broom and beans, we shall find, that they all contribute to prove that the seeds, or their integuments, exist before fecundation, but that the plantule and the lobes cannot be seen till afterwards.

It would therefore appear, that these results do not agree with my observations on amphibious animals; for in them I found the fetuses before impregnation. But are we immediately to conclude, that in the process of generation Nature follows two different methods in plants and in animals? The principles

ciples of sound reasoning do not permit me to make so hasty a declaration; they require, that I should continue my researches on plants. These researches I undertook, and a relation of part of them shall be the subject of the following chapter.

C H A P. II.

GENERATION OF THE PLANTS DENOMI-
 NATED BY LINNÆUS, RAPHANUS SA-
 TIVUS, CICER ARIETINUM, IXIA CHI-
 NENSIS, DELPHINIUM CONSOLIDA, CU-
 CURBITA PEPO, CUCUMIS SATIVUS,
 EXAMINATION OF THE POWDER OF THE
 STAMINA.

VIII. *R*aphanus sativus, common Radish.
 The seeds are visible in the ova-
 rium long before the powder of
 the antheræ is ripe. They are solid, and
 continue so till twelve or fifteen days after the
 falling of the flowers: the usual cavity then
 appears, of small dimensions at first, and
 full of liquor, but successively enlarging, in-
 somuch that about the 20th day, it occupies
 almost all the inside of the seeds. In three
 or four days more, the lobes are seen sur-
 rounded as usual by a transparent liquor; and
 we may soon afterwards perceive between
 them a mucilaginous filament, attached by
 one end to the plantule, and by the other to
 the internal cavity of the seeds. When the
 seeds

feeds are nearly ripe, the two lobes are considerably increased; they resemble a pear with its stalk; the stalk is implanted into the concave surface of the feeds. If the lobes be taken out and carefully examined, the stalk appears to be part of the plantule; and instead of two lobes, the number observed in the plants before mentioned, there are four; the two smaller are implanted in the two greater; they are all united, and so attached to the plantule, as with it to form one body.

IX. *Cicer arietinum*, Chick-pea. The pistil in this plant represents in miniature a gourd, of which the belly is the base of the pistil, or, as I call it, the ovarium. If this be opened longitudinally twelve or fifteen days before fecundation, two pointed feeds, solid and of a green colour, come into view. As the parts of fructification ripen, the ovarium swells and takes the form of a purse, to which the feeds are attached. About four or five days after fecundation, the cavity begins to form, and on one side of it may be perceived two very minute lobes; if they be carefully opened, the plantule is seen attached to the feeds by a mucilaginous filament. Nothing further takes place except a progressive evolution.

x. *Ixia chinensis*. Having opened longitudinally the ovarium of one of the smallest buds, which is oblong and prominent at the centre, we find the pyriform seeds disposed in their natural order, and perfectly solid within. The same thing may be observed when the petals are opened, nay, after they are fallen, and still nine or ten days more elapse before the cavity begins to open. This plant is distinguished by a peculiarity which must not be omitted. At first the cavity is full of a transparent liquor, which in time thickens, and at length is converted into a white gelatinous matter, not unlike coagulated milk. The consistence becomes gradually firmer, and before the seeds are ripe, resistance is felt on attempting to cut it with a knife. This substance, thus hardened, occupies the whole cavity of the seeds, nor can there be perceived the smallest vestige of the lobes. My observations were made upon seeds not yet arrived at perfect maturity, but I could not continue them for want of riper seeds. Are we to suppose that the substance, which is at first liquid, then gains some consistence, and at last becomes hard, performs the office of lobes, so that they are produced in this instance in an unusual manner; or is
it

it more probable, that the lobes appear later than in other feeds? The phænomena of another plant, of which I am now to speak, incline me rather to the latter opinion.

XI. *Delphinium consolida*, Larkspur, subdivided larkspur. The appearances noticed during the growth of the feeds of the *Ixia*, recurred on the present occasion. The cavity, which is usually formed after the falling of the petals, is soon filled with liquor, at first transparent, then whitish, and afterwards of the colour and consistence of milk. If the feeds be now boiled for a short time, the milky liquor is changed into a matter like pomatum. There appears, upon examination, no sign of organization. When the feeds are nearly ripe, the consistence of the white liquor is increased, and when they are arrived at maturity, its hardness is considerable. The plantule and the lobes may now be distinguished, but they are so buried in the midst of this indurated matter, that it is difficult at first to distinguish them certainly, as well on account of their minuteness as their white colour. This observation, which agrees so nearly with the preceding, leads me to suspect, that the milky liquor of the *Ixia*, which is first coagulated and then hardened, does not constitute the lobes, but only encloses

encloses and conceals them. They are invisible, either on account of their minuteness, or because the seeds are not yet ripe. In the present species, the late appearance of the lobes, which does not takes place in much less than a month after fecundation, is very remarkable (*a*).

XII. *Cucurbita Pepo*, Common Pumpkin or Pumpkin. Disregarding for the present the male flowers, as having no relation with the present inquiry, on account of their sterility, I directed my attention to the female flowers, beginning with the blossoms that were least advanced; small, however, as they are, the little pumpkin is already formed under the bud, and the seeds within it are very visible. In this plant there is a peculiarity in their structure. They are not composed of an homogeneous substance, as the unripe seeds of other plants, but consist of two matters,

(*a*) I found, by observations made a year afterwards, on more advanced seeds of the *Ixia*, that my suspicion was just. When a longitudinal section is made along such seeds, the plantule is very visible; it has the shape of a cone; the base lies at the bottom of the seed, and the apex reaches about as high as the middle. The whole plantule is imbedded, and hid in the milky matter, which at this period is very hard. I must moreover observe, that notwithstanding all my pains, I could not discover the lobes; perhaps the part I call *plantule* might serve in their stead. The appearance of the plantule is as late in the seeds of the *Ixia*, as in those of the larkspur.

a shell

a shell and a nucleus. When the seed is cut transversely, these two substances appear in the plane of section; if a portion of the divided seed be held by the flat sides between the finger and thumb, and pressure be made towards the cut, the nucleus quits the husk, and flies into the air, as the stone of a cherry does when pressed in like manner. If the nucleus be taken whole out of its cover, it appears in the shape of a pear with its stalk, and bears, moreover, a perfect resemblance to the lobes with the plantule. But are we to conclude, that the plantule is really visible here before fecundation? I confess, this first observation on the fructification of the pumpkin, was very near leading me to adopt this opinion. But one circumstance suggested a doubt. In other plants the lobes are easily separable into two, and sometimes into four (VIII); in the present case, the nucleus would not admit of such a separation, but formed a perfect whole. The best way of removing my perplexity, evidently was to prosecute my observations. From the examination of three larger buds I learned, that the nucleus yet consisted of one body, and that it adhered so firmly to the cover, that it could not at all, or at least not without great difficulty, be parted

parted from it. But the cover or rind appeared much more complex than I had at first conceived. It consists of three distinct membranes. The first or outermost is the most thin and delicate of all; it may be denominated the cuticle or epidermis. The second, which lies immediately under the first, is thicker, and of a whitish colour, and has a degree of hardness approaching to that of wood; there is some difficulty in separating it from the third, which invests the nucleus; this innermost skin or membrane is greenish and firm, but not so firm as the second.

The inspection of the seeds, twenty-five days after the falling of the flower, gave me an opportunity of observing other new circumstances. The nucleus, which so much resembled the lobes, had disappeared, and in its stead, there was a membranous sac, terminated by a beak, that was attached to the inside of the seeds. The sac was somewhat tumid, with a liquid stagnating within it, as became evident whenever it was perforated. By gently laying hold of the beak with a pair of forceps, the little sac may without difficulty be entirely removed. It is composed of two membranes, which, when opened, are found to contain a very tender mucilaginous

nous body full of moisture. This body arises from the inside of the apex of the seed, and extends about half its length; it is attached to the place at which it terminates. The slightest touch breaks and destroys it. When examined by the microscope, it appears reticulated, whence I conclude that it is organized.

In seeds further advanced, when the flowers have been withered above a month, the mucilaginous body enclosed in the membranous sac is much enlarged, and at the inside of the apex of the seed appears a white, solid, and conical particle, which, when attentively examined, appears to be the germ or the plantule in miniature. To the plantule are attached the two lobes like it, of a white colour, and exceeding it in size; the mucilaginous filament above-mentioned passes in the middle between them, and is inserted into the plantule. It is easy, from what has been said above, to guess how the seeds gradually advance towards maturity. The mucilaginous filament enlarges in its dimensions, the plantule increases, and the two lobes advancing in bulk and mass, come at last to occupy almost all the inside of the seed. It appears, therefore, that I should have been greatly deceived if I had taken the nucleus,
which

which is found within the seeds before the opening of the flowers, for the lobes and the plantule, since these parts do not make their appearance till a month after the withering of the petals.

XIII. *Cucumis sativus*, Cucumber. My observations on the seeds of this plant, so exactly resemble those on the seeds of the pumpkin; both with respect to the appearance of the nucleus before fecundation, the membranous sac, and the gelatinous substance, which are observed afterwards, and last of all the lobes and plantule, that I think it unnecessary to enter into a particular account of them. I shall only say, that in the cucumber the plantule and the lobes seem to me to become visible still later: they at least were very small in one instance, when the fruit was nearly ripe, and beginning of course to turn yellow.

I afterwards proceeded to examine other plants, *Hibiscus Syriacus*, the Syrian shrubby Mallow, *Alcea rosea*, rose-flowered, or common Hollyhock, *Acanthus mollis*, soft or smooth-leaved Acanthus, *Convolvulus purpureus*, purple Bindweed, to which may be added *Ocimum Basilicum*, sweet Basil, *Cucurbita citrullus*, *Cannabis sativa*, Hemp, *Mercurialis annua*, annual Mercury. All these

plants

plants perfectly agree in the appearance of the seeds long before fecundation, and of the plantule and lobes, some time after the ripening and diffusion of the pollen. This general observation perfectly coincides with the account of the great naturalist Duhamel, given long since in his *Physique des Arbres*. Speaking of the formation of the seeds of trees bearing fruit with stones, such as the almond, the peach, plum, cherry, &c. he adduces many facts to prove, that when a stone, which has attained its full size, is opened before the fruit is ripened, (and we may make the experiment a long time after fecundation), it will be found full of a viscid liquor; a white body, he says, begins to appear within the point of the kernel, enclosed in a small transparent bladder: this body is the rudiment of the lobes with the plantule; they grow till they fill the whole capacity of the stone. From the coincidence of so many facts, I think myself authorised to infer, that this law is observed by Nature, if not in all plants, yet in a great number.

XIV. But with which of the three theories respecting the generation of plants (I), do my numerous observation bests agree? My principal aim was, as I have already remarked, to find, if possible, data sufficient for the
 solution

solution of this difficult problem. Should we be content with first appearances, we might perhaps think that system preferable, according to which, the embryos of plants are conveyed, at the time of fecundation, by the powder of the antheræ into the ovarium. The embryos have never been found in this organ before the aspersión of the powder. They always make their appearance after the pollen has been observed upon the antheræ. It should therefore seem, that the existence of the embryos directly depends upon this powder; and it may be inferred, with some degree of probability, that the embryos pre-exist in the pollen; and that when it falls upon the stigmata, they are conveyed through particular ducts into the ovarium. A learned naturalist (*a*), has accordingly been led to adopt this opinion, by the observations of some person, who pretended to have seen the embryos in the ovarium, immediately after the summits had discharged their fecundating dust. But whoever considers, with a little attention, the force of the argument, will find, that it is not very convincing. It is in the first place not true, that the embryos may be perceived as soon as the powder has fallen from the stamina. They do not come into

(*a*) Needham : Nouv. decouvertes faites avec le Microscope.
 view

view for more than a week, and sometimes not till a month after the dispersion of the powder, as is evident from all the observations I have hitherto related, and from others which I shall hereafter relate. In the second place, this mode of reasoning is, as Mr. Bonnet has very well remarked, illogical; when we infer, that the embryo does not exist in the ovarium before fecundation, because it is not visible before that time, we argue from invisibility to non-existence, a fallacious way of reasoning, as many instances may be brought to shew: in the impregnated egg, for example, the best microscopes will not bring the chicken into view, though we are sure that it is present. Notwithstanding the inconclusiveness of the argument, there seems no absurdity in supposing the embryo of the plant to pre-exist in the powder, and thence to pass into the ovarium. To remove these uncertainties, I resolved to give my inquiries a different direction.

xv. If the embryos of plants be really lodged in the pollen, why cannot we discern them by microscopes of highly magnifying powers? The thing did not seem improbable, and I thought it would be proper to turn my researches to this object. I accordingly examined the ripe pollen of several plants, and especially of those upon which the foregoing

observations were made. In the flowers of these vegetables, I found the powder to resemble very nearly that of others examined by botanists, and in particular by Duhamel; it formed an aggregate of particles, differing in figure in different plants; in some the particles are round, in others elliptical, in others pyramidal or conical, in others of an irregular form. Sometimes they are smooth, sometimes prickly, and at others covered with knobs. They are in different plants transparent, opaque, yellow, white, blue, or carnation. The size varies according to the difference of the flowers. But these corpuscles are not, according to the common consent of Naturalists, immediately concerned in the generation of plants. They are vesicles, sheaths, cases, ---, or by whatever name we may chuse to distinguish them, for the most part full of a thin liquor, which, when they are moistened, they emit with force. In this liquor floats a multitude of very small globules, which, at the time of explosion, are thrown in different directions. Those who adopt the theory we are considering, suppose the rudiments or embryos of the plants to be lodged in this liquor. It was therefore proper to fix my attention upon it, and to try whether I could perceive any thing resembling an embryo. When the
embryos

embryos are first seen in the seeds, they represent a small pointed body, to which two larger bodies are affixed, like the closed wings of a butterfly; these are the lobes. I examined the liquor with great attention, in order to see whether it contained any such bodies, or any thing like them. I used glasses of various powers, from the lowest to the highest; but I could discover nothing except the liquor, which in some cases is like oil or melted fat, and the globules that usually float in it. As the liquor, a substance without organization, cannot be what we term embryos, I had only to examine the globules; but I could perceive nothing in them, which bore any resemblance to lobes or embryos. These, as the word *globule* imports, are round bodies, or such as have nearly that figure; they constantly retain it with whatever microscopes they are examined. Nor did their form change, when I used the solar microscope, which so prodigiously enlarges objects. Of these facts, it is the direct consequence, that we cannot assert, with any probability, that the embryos of plants are lodged in the pollen.

It may, I am well aware, be objected, that the globules are the real embryos, and retain their round shape as long as they con-

tinue in the powder; but that as soon as they pass into the ovarium, and find a proper supply of nourishment, they unfold the parts which were before concentrated in a minute globe, and assume the shape of the lobes and plantule; just as the tadpoles of the frog and toad, according to my observations, represent so many globules as long as they remain in the body of the female, but when they come to be nourished with the liquor of the amnion (*a*) put on the real form of the tadpole. Such an objection may be started, but it amounts to no more than a bare possibility, that the globules floating in the liquor of the pollen may be the embryos. But I am not seeking for possibilities; I am enquiring whether the supposition is probable; whether there is any just motive for supposing the embryo to pre-exist in the powder of the stamina: and my observations shew not any such probability, or any such just motive. Not having, by these means, obtained the information I sought, there remained one other chance of solving the problem, by interrogating Nature in a different manner. This was to try, whether the embryos would appear and grow to maturity, when the powder of the antheræ was hindered from acting upon

(*a*) Dissert. I.

the pistils. Should the event be such, it would be evident, that the embryos do not belong to the pollen, but the ovarium. The experiment might be made in three ways, either by cutting away the antheræ of hermaphrodite flowers, before they shed their powder upon the pistil; or by removing the male flowers of those plants, in which they grow separately on the same individual before they are ripe; or lastly, by keeping the male plants at such a distance from the female, that there can be no suspicion of their dust reaching them; this experiment relates to plants producing two sorts of individuals; one provided with stamina only, the male, and the other with pistils only, the female. These experiments, together with some observations, will furnish matter for the two following chapters.

C H A P. I I I .

GENERATION OF SOME HERMAPHRODITE AND MONOICIOUS PLANTS, UPON WHICH THE POWDER OF THE STAMINA WAS PREVENTED FROM ACTING, CALLED BY LINNÆUS OCYMUM BASILICUM, HIBISCUS SYRIACUS, CUCURBITA MELOPEPO FRUCTU CLYPEIFORMI, CUCURBITA CITRULLUS.

XVI. I BEGAN with an hermaphrodite plant, viz. that species of Basil which is often raised in pots, and called by botanists *Ocimum Basilicum*, sweet basil. I happened, at the time I conceived the design of entering upon this inquiry, to have some in blow, and others of which the flowers were not yet expanded. That I might form a juster judgment concerning my experiments, I resolved to compare flowers, in which the pollen had acted with others, in which its influence had never been exerted. My observations upon the former were the following: every blossom of basil had four stamina, in the midst of which rises the pistil;

til; the stamina, as well as the pistil, are distinctly visible twelve days and more before the opening of the flower, when the petals have been cautiously removed. The seeds appear at the same time in the ovarium; they are four in number, and have an oval shape. They continue solid for several days after the falling of the flowers, but soon afterwards, while they are increasing in bulk, a cavity, like that which I have observed in the seeds of other plants, begins to form. Within this cavity, we may in a short time discern a solid point, at first shapeless, but soon putting on the form of the lobes and plantule. Meanwhile the cavity becomes more capacious, as the plantule and lobes grow, and at length these bodies occupy the whole inside of the seeds. Such is the process of Nature in those flowers of basil, in which the powder of the stamina is left to produce its effects: we have seen that the same phenomena occur in other plants.

xvii. It is now proper to proceed to the other more interesting part of our comparative investigation, and to observe what becomes of the embryo, when the influence of the pollen is withdrawn. At the time when the flowers were about to open, I cut away the four antheræ, which now were full of

ripe pollen. But this operation alone did not satisfy me. The excessive tenuity of that halitus, which bursts from the pollen (xv), and which, in the general opinion of botanists and naturalists, has an immediate effect in generation, (in whatever manner it may produce this effect) rendered it necessary not only to remove the pollen, by cutting away the antheræ of those flowers upon which the experiment was made, but also to keep at a distance the pollen of other flowers growing on the same plant. This end was easily obtained, by plucking off all the flowers as they were about to expand, except those which were the subject of my experiment. The pot was placed on a window, and there grew near it no other plants of the same kind, either in pots or in the ground. I cannot relate what happened without astonishment. The seeds, some time after the falling of the flowers, became hollow within, and in the cavity appeared the lobes and the plantule, which grew till they filled the whole capacity of the seeds. In short, the various phases assumed by seeds that have been subjected to the influence of the pollen, were observed without any variation in others, which may reasonably be supposed never to have felt that influence.

XVIII. I could not however acquiesce in this result: but was rendered uneasy by a suspicion, that so delicate an experiment had not been executed with all the necessary precautions. According to the common opinion of philosophers, the powder of the stamina exerts its action while the flowers are opening, or soon after they are blown. But on opening the flowers of basil two or three days before their time, I thought that in two or three instances part of the pollen was ripe, and that one or two grains had fallen upon the pistil, which lies very close to the antheræ, and indeed almost touches them. I therefore suspected, that I had cut away the antheræ too late, and after the pollen had produced its effects. This consideration led me to perform the operation upon flowers less forward, and not liable to such a suspicion. And now the event was different. I made the experiment upon eighty-four blossoms. The seeds of about one-third of this number fell before they were ripe. In the ovaria of the rest appearances were exceedingly various. Some seeds, notwithstanding it was the season of their maturity, were as small as when the antheræ were cut away. In some ovaria they were larger, but withered, dry, or quite spoiled. In twenty-five instances, they

they were as well looking and as large as those which had been subjected to the influence of the pollen. I was not however to be satisfied with external appearances. The principal question was, whether they each contained an embryo, or a plantule attached to lobes? They actually contained these parts, nor could I, by analysis or comparison, perceive any circumstance in which they differed from the plantule and the lobes of seeds, upon which the influence of the pollen had been exerted. Of these twenty-five seeds, certainly never acted upon by the powder of the antheræ, twelve were analysed in this manner, and the remaining thirteen were put into the ground. But they did not germinate, while thirteen seeds of the same plant, left to the action of the pollen, and sown in the same place, all came up. The immediate and most striking inference to be deduced from these observations is, that the fecundation, and sometimes the evolution of the embryos of basil, depend upon the pollen, but not their existence.

XIX. Whilst I was thus employed upon basil, I made observations and experiments of the same nature upon Syrian shrubby mallow, *Hibiscus Syriacus*. The antheræ were cut away from a great number of blossoms
before

before they expanded. I could not however, I must own, be certain, that a grain or two of powder had not fallen upon the pistil; for I oftener than once observed, that it was quite ripe. Some seeds fell soon after the operation, but the greater part remained in their proper situation, and grew considerably; they would perhaps have arrived at maturity, if the coolness of the atmosphere at the setting in of autumn, had not prevented them, as it prevented other seeds of the same species, which were left to themselves, and must be supposed to have been impregnated by the pollen. Some of these seeds, which had grown a good deal, but were yet unripe, were black within and spoiled, others were white, in all appearance sound, and, what is of chief consequence, they contained a plantule attached to its lobes, which were convoluted like cut lettuce. From the plantule arose, in some cases, an almost imperceptible gelatinous filament, of which the other end was inserted into the seeds.

When I cut off the antheræ from the blown flowers, I performed the same operation upon others several days before they were to expand. To effect my purpose it was necessary to force open the bud, which could not be done without sometimes injuring a
petal.

petal. At this time it seemed certain, that the pollen could not have produced any effect, as it was viscid, and adhered tenaciously to the summits of the stamina. The results exactly corresponded with the foregoing observations. The seeds of several flowers dropped prematurely, while in others they continued firm, and grew considerably, but did not ripen: some were vitiated internally; others contained lobes and a plantule completely developed and sound.

It is obvious, that the consequences of the experiments on shrubby mallow and basil perfectly agree; they equally concur to prove, that although the due evolution of the embryos depends on the action of the pollen, this powder neither conveys nor produces them.

xx. Having sufficiently satisfied my curiosity, with respect to these hermaphrodite plants, I proceeded, according to the plan laid down in Sect. xv. to plants bearing male and female flowers separately on the same individual. My observations were made upon two of this class, the pumpkin with shield-form fruit, *Cucurbita Melopepo fructu Clypeiformi*, and the citron pumpkin, *Cucurbita Citrullus*. I had the seeds of the former species from the public botanic garden at Pavia;

Pavia; they were sown in the spring at Scandiano, a well-known and very pleasant fort belonging to the state of Modena, and the place of my nativity: here, during the summer and autumnal vacations, I employ myself in those experimental enquiries, which are the subject and the foundation of my unimportant literary productions. At my arrival there in the beginning of June 1777, two individuals, for I had ordered two only to be raised, were just beginning to put forth a few flower-buds towards the bottom of the stalk. At this early period the male flowers may easily be distinguished from the female. The former, denominated also *barren* by botanists, have a slender stalk; while the stalk of the latter, where it joins the calyx, forms a tumour, consisting of the immature fruit, as is intimated in Sect. XII. I paid daily visits to these two individuals, and very carefully watched the progress of both sorts of flowers. That there might be no suspicion of the pollen exerting any influence upon the females, the males were destroyed at their first appearance. As fruit, when a small quantity only is left upon a plant, sooner ripens, and grows to a larger size, because it receives a greater share of nutritious juice, I left on each of my two individuals

duals two flowers only; the buds that made their appearance afterwards were taken away, along with the male flowers. Meanwhile my four gourds grew rapidly; finding that towards the middle of September they had attained the usual full size, I gathered one, in order to inspect the internal parts. The flesh was too soft, because the fruit was not thoroughly ripe; but in colour, structure, and taste, it resembled fruit produced by plants which had their male flowers, and which I examined both before and afterwards at Pavia. The seeds were in great number, and, as well internally as externally, were perfectly formed; the lobes, indeed, to which the plantule was attached, did not occupy the whole capacity of the seeds, but they had not attained their full size. At the end of the month the other three gourds were quite ripe; I therefore gathered them, and put the seeds of each into a separate box, that I might be able to examine them at pleasure. The lobes filled the whole inside of the seeds, and had all the characters of perfect maturity.

xxi. Thus far there is a perfect agreement with the observations made on the seeds of basil, which seemed, notwithstanding they were deprived of the efficacy of the pollen,

to

to have acquired the same degree of perfection as those impregnated in the usual manner. But as they did not grow, however perfect they might be in appearance, because they had not been vivified by the pollen (XVIII), I imagined, that for the same reason the seeds of my three gourds would not grow. It was, however, proper to make the experiment. I therefore dried one hundred and fifty in the sun, and afterwards planted them in three pots, fifty in each, taken from separate gourds. But the lateness of the season, it being the 10th of October, the constant rain, and the coolness occasioned by it, circumstances unfavourable to vegetation, obliged me to place my pots in a stove, which, though it was not heated, was kept warm by a contiguous chimney. The event did not by any means correspond to my expectation; I took it for granted, that none of the seeds would germinate, and yet they almost all came up very well. On the 15th, ten days after they were planted, thirty-seven, out of one hundred and fifty, had germinated; the number increased daily, insomuch, that on the 8th of November, one hundred and thirty-three appeared above the mould in the pots. I was unwilling to leave the seventeen seeds that did not shoot unexamined;

unexamined; I found that they were either empty, or that the lobes and the plantules were vitiated; that is to say, crisp and withered, because they were unripe.

I reserved the remainder of the seeds for another experiment to be made the following spring. Before it can be asserted that fructification has been complete, it is necessary, according to the determination of botanists, not only that the seeds should grow, but that they should also be capable of bringing productive seeds, or, in other words, of perpetuating the species. That I might learn whether the seeds of my three gourds enjoyed this prerogative, I caused some of them to be planted in the same place in May 1778, and when they were grown to some size, they were, as in the foregoing experiment, carefully stripped of all their male flowers, one female flower only being left upon each individual. These flowers were furnished with small gourds, which grew ripe towards the beginning of autumn, and the seeds they produced grew just as well as the former; nor can I doubt that they would have brought forth fruit just as well, if I had chosen to try them.

From these facts it appears, that the gourd with shield-form fruit does not agree with
basil

basil in the circumstances attending generation. The seeds of basil, to be productive, must be impregnated by the pollen, but the seeds of this gourd propagate the species without such help. The things which I have related furnish the most certain and unequivocal proofs of this assertion. It is impossible to suspect, that the powder of the male flowers, growing upon the same plant with the female flowers, had any share in producing this effect, because I destroyed them long before it was ripe. Nor can it reasonably be presumed, that any pollen was carried by the air to the spot from places at a distance. I purposely chose Scandiano (xx), as there was not a gourd of this species any where in the neighbourhood. We must therefore of necessity conclude, that neither the embryos of this plant, nor its fructification, depend upon the powder of the stamina.

XXII. I now come to the other species of gourd, the second plant, with male and female flowers growing separately on the same individual of which I proposed to treat (xx). One root of this species was planted late in the spring of 1779, in the same garden which the two preceding years had produced the gourd with shield-form fruit. The pre-

sent plant was in like manner deprived of all its male buds, as soon as they began to appear, and a few female flowers only were left for fructification, supposing it should take place in these circumstances. That this plant might be insulated, and that no suspicion might arise concerning the influence of adventitious pollen, I took another precaution, which he who is aware of the delicacy requisite in such experiments will not regard as unnecessary. My precaution consisted in enclosing, in large bottles of glass, those branches of which the female flowers were about to open, and so to lute the necks with a thick cement, as to prevent the passage of the external air, which is thought by many to be the vehicle of the fecundating powder, into the bottles. This contrivance eased my mind of another apprehension. Some botanists pretend, that the fecundation of several plants is effected by means of insects. According to them, bees and other winged insects of the same kind, by creeping into flowers to suck the honey, or extract the wax, are apt to brush away the dust of the stamina, and to convey it on their bodies, from flower to flower; in this manner, say they, are plants frequently impregnated. The instances of artificial fecundation, which will be

be

be hereafter mentioned, incline me to admit this supposition. The disposition of the bottles in the manner above described, not only precludes all suspicion of pollen being conveyed to the female flowers by the air, but it likewise hinders the access of insects. Two female flowers about to expand, were guarded in this manner by bottles with wide bellies; but the blossoms, as well as the young gourds belonging to them, came to nothing. The cause of this failure was the perspirable matter issuing from the two enclosed branches: it collected at first in the form of dew upon the inside of the bottles, and afterwards ran down in little streams upon the flowers; hence they perished in three days. But the unsuccessful termination of this trial did not cause me to abandon my pursuit; it only taught me how to ensure success. I had only, as appeared from the event of this experiment, to prevent the flowers from resting upon the side of the bottle. I therefore contrived to suspend them in the middle, so that they might be only just as much moistened by the perspirable matter, as would be sufficient to keep them in a state of vigour. In this manner two female flowers were enclosed for eleven days; that is to say, from the time they were

about to expand, till they were withered. In the interval the fruit had grown a little. Having thus eased my mind of fear, lest any unperceived pollen should have been conveyed to them at this most critical period, I removed the bottles on the twelfth day, and exposed the fruit to the open air till it grew ripe, which happened about the eighth of September. They both seemed perfect; and all the inside of the seeds was occupied by the lobes and plantule. The growth of the seeds of the former species gave me some reason to hope, that those on which I was employed would produce plants; and so it really happened: I planted an hundred, fifty taken from each pumpkin, of which eighty-nine were productive, and consequently eleven failed: the cause of their sterility was the same as in a former experiment (in which seventeen seeds of the other pumpkin proved unproductive (xxi). The plantule or the lobes laboured under some vice of organization; a circumstance by no means surprizing, since it is also observed in the seeds of the same plants, when left to the care of Nature. I planted the seeds of the present species in 1780, as I had before sowed those of the former in 1778; in both cases the results relative

lative to fructification were exactly alike. It was therefore reasonable to conclude, that fructification in this plant does not, in the smallest degree, depend on the powder of the stamina,

C H A P. IV.

GENERATION OF CERTAIN PLANTS PRODUCING MALE AND FEMALE INDIVIDUALS, ON WHICH THE FECUNDATING DUST WAS PREVENTED FROM EXERTING ITS INFLUENCE, VIZ. CANNABIS SATIVA, HEMP, SPINACIA OLERACEA, SPINACH, MERCURIALIS ANNUA, ANNUAL MERCURY.

XXIII. *CANNABIS Sativa*, hemp,
 During my residence at Scandiano, in the summer of 1767, I observed two phenomena, which deeply engaged my attention. The first took place upon a stalk of hemp, that sprung up spontaneously in my garden. It was strong and tall, and on all sides surrounded by a wood of branches. It produced a great number of seeds, which, in all appearance were perfectly formed. I found, by experiment, that they were productive, notwithstanding it was the only plant in the garden which is very large, and no male plants grew within a considerable distance. Prejudiced as I was in
 favour

favour of the universally received sexual system, I felt some slight suspicions arise in my mind from this observation; I could not refrain from mentioning it as a singular occurrence to several friends, and among others to the celebrated naturalist Campi Somasco. I however at that time thought, that the phenomenon might be explained, from the supposition of pollen conveyed by the wind from the hemp-yards, with which the marquisate of Scandiano abounds---more especially as I was aware of the great plenty of this powder, which rises in a thick cloud, when the plant is shaken. I imagined also, that from the greater abundance of pollen afforded by the male hop (*a*), the favourers of the sexual system, might account for the fructification of female plants discovered in places where there are no males.

xxiv. The second phenomenon which I observed the same year, was the following. The inhabitants of the territory of Reggio and Modena, eradicate all the male individuals from the hemp-yards, about the second or third of August, and leave the females till the end of September. In a season favourable to vegetation, the females continue to grow above a month, and to produce new

(*a*) *Humulus Lupulus*. Linn.

branches. Upon these branches spring flower-buds, which in time expand and bear seeds. Of such female individuals I saw, for the first time, great numbers growing on the 8th of September, 1767; that is, thirty-six days after the male plants had been pulled, as I was assured by a person of credit. I fixed my attention chiefly upon the late branches, on which as yet the first rudiments of the closed flowers scarce began to appear: I moreover tied a thread loosely round them, that I might try whether the seeds, if any should be produced, would arrive at perfection. This actually happened, as their shooting soon after I sowed them in October irrefragably proved. This second phenomenon gave rise to serious reflections, as I could not possibly at first comprehend how the pollen could have fecundated these female flowers, which made their appearance upon the branches so long after the removal of the male plants. After a little consideration, I perceived that the advocates of the sexes might find somewhat to adduce in their defence. Notwithstanding the want of male flowers, it might be said, the pollen which had been shed in such abundance, may float in the air surrounding the individuals of the other sex.

xxv. Eight years afterwards, in 1775, a Memoir appeared in Rozier's Journal on the fecundation of plants, the tenour of which perfectly agrees with my observations on hemp. The substance of this memoir, as far as it relates to our present enquiry, is the following. The author reared a female stalk of hemp in a pot at Paris, which produced, at the usual time, seeds of the common size, consisting of two well nourished lobes with plantule or germ. When the seeds were planted, they all without exception germinated, and grew up in a short time. The plant had not been covered by any thing; it stood upon a window level with the ground, at a great distance from other plants of the same species. The maker of this experiment does not pretend, that it entirely excludes all suspicion of the fecundating dust, but only that the access of it is highly improbable; he exhorts naturalists to repeat and improve his experiment. The author of the Memoir does not make himself known any otherwise, than by the initials *M. F. de B.* which several have suspected to design the celebrated Mr. Fougereux de Bondaroi, of the Academy of Sciences, nephew to the illustrious Duhamel. However this may be, it appeared to me when I read the account, that

that this delicate experiment had not been conducted with so much attention and such precautions, as to produce conviction in the mind of the reader (a).

XXVI.

(a) Having communicated in a letter, written Sept. 18, 1777, to my illustrious friend Mr. Bonnet, some results relative to the fecundation of plants, I received a very polite reply, dated Nov. 29, the same year. Among other things he mentioned this Memoir, supposing I had never seen it. It was his opinion, that the observation of the French Naturalist, whom he presumes to be Mr. Fougereux, is not sufficiently decisive to exclude the fecundating powder. He was induced, by his respect to truth, to make some strictures on different passages of this Memoir; which, as they are connected with my observations on generation, it will be proper to transcribe. “ Various inadvertencies and mistakes have been committed by M. F. which might be overpassed in an author of inferior reputation. I shall point them out from a short account I gave to another correspondent. The propositions distinguished by Italics are the words of M. F.

“ *We are not sure, that the chicken exists before impregnation in the egg, and we must consequently be ignorant, whether the plant exists in the seed before fecundation.*

“ The learned naturalist ought, I think, to have expressed himself very differently after the fine discovery of Haller, of the pre-existence of the chicken, after your’s of the pre-existence of the tadpole, and Mr. Muller’s and my observations on the pods of pease. *Palingen*, T. 1. p. 416. In my essay on the fecundation of plants, printed in Rozier’s Journal, for Oct. 1774, I had brought to the reader’s recollection these several discoveries on vegetables and animals, as strongly tending to establish the great probability of the pre-existence of the germ; and M. F.’s Memoir evidently refers to mine.

“ *But is fecundation, by means of pollen, necessary in all plants? Have none their parts of generation concealed, like the lice of plants (puceons)?*

“ The

XXVI. In relating my subsequent experiments on the generation of plants, I shall give

“ The parts of generation in the lice of plants are not *concealed*. On the contrary they are quite obvious. I dwell pretty largely on the amours of these insects in my *Insectology* published in 1745. I there describe at length their genital organs.

“ *Laying aside all systematical ideas, I abide by facts and observations, and follow a celebrated guide, M. Duhamel, who, before me, has multiplied facts and observations, without venturing to decide.*

“ It is doubtless commendable to abide in physics by *facts and observations*. But sound logic more than barely allows us to deduce the most immediate and direct consequences, as I have endeavoured to do in my works, and in the essay which Mr. F. seems to have held in view. He expresses his intention to take Mr. Duhamel for his guide. But the fine discoveries of Haller, Spallanzani, &c. were unknown to Mr. Duhamel when he composed his excellent Natural History of Trees, which was published in 1758. Mr. F. who published his Memoir in 1775, was naturally called upon to weigh the consequences resulting from these discoveries, and to avail himself of the analogical deductions drawn by me respecting vegetables. This, however, he has not done, and I cannot but be surprized at it.

“ *Those who suppose the plant to exist in the seed before fecundation, consider the pollen as consisting of a number of sheaths and cases, of which each contains a number of seeds floating in a thin liquor.*

“ Some naturalists, as Needham, have considered the powder of the stamina in this light, but they do not allow *the plants existence in the seed before fecundation*. Mr. Needham who so well explained the composition of the pollen, believed the germ to be in the pollen itself. Mr. F. has therefore expressed himself very incorrectly; but when he immediately afterwards subjoins, *the plant, according to this opinion, exists before fecundation in the powder of the stamina, he falls into the most glaring contradiction.*

give an account of the means employed to prevent the access of the pollen. Six small stalks

tradition. *That the plant exists in the seeds before fecundation, is an hypothesis which excludes the opinion, that the plant exists before fecundation in the powder of the stamina.*

“ *But here only human intellect is bewildered. How can we, using our reason, imagine the germs of all plants to be inclosed in such a germ. What an abyss! Let us quit this clue as likely to mislead us.*

“ *What meaning has the word only in this place: It is precisely by the use of our reason which does not imagine, but conceive, that we persuade ourselves of the probability of that hypothesis, according to which germs are inclosed one within another. If M. F. had read my Memoir more attentively, and reflected longer on the subject, his mode of expression would have been more philosophical. Does it then belong to the imagination to decide concerning matters lying exclusively within the province of reason? Can the imagination represent to itself an animalcule many million of times smaller than a mite? Can it represent to itself a globule of light, several thousands of which fall at once on the eye of such an animal? No; never will reasoning or calculation overturn the hypothesis of involution. Great and small are merely relative terms; and we are acquainted with surprizing facts that lead us to this hypothesis.*

“ *According to Haller, irritability is the principle which constitutes animality and produces life. The powder of the stamina exciting the irritability, and occasioning the fluids to be impelled in the organized body, produces in the vegetable the same effects as the spermatic liquor in the animal germ.*

“ *Haller did not admit irritability in vegetables. He could not therefore attribute to the powder of the stamina that stimulating property which he acknowledges in the spermatic liquor of the animal: and yet, from M. F.'s expression, it would be supposed that he does. But it was I, who, in my essay on the fecundation of plants, sought to apply irritability to the fecundation*

stalks of hemp were transplanted from an hemp-yard into six pots. As soon as they had

dation of vegetables—not, however, till I had declared my ignorance of any fact which rigorously proves the existence of irritability in the vegetable kingdom. Upon this occasion, I threw out some logical reflections, calculated to cause the suspension of the reader's judgment. Let me be allowed to observe, that no author in natural history has been more careful to distinguish conjectures from facts: but I do not believe that conjectures are unwarrantable. I have therefore confined myself to shew how we may conceive the influence of irritability in vegetables, supposing them to possess this property. M. F. read me too rapidly, and without reflecting sufficiently upon my mode of arguing. Moreover, the irritability of plants which I did but conjecture to exist, seems to have been well observed by the celebrated Gmelin in some species; but I was unacquainted with his observations in September 1774, when I was composing my essay. That author, in his Dissertation on Irritability, shews, that the antheræ, or summits of the stamina, appeared to him *irritable, or at least endowed with a property nearly approaching to the property of animals called irritability*. He adduces many instances exceedingly remarkable, and, among others, that of the orchis. *The antheræ of this plant, he observes, recently gathered, and irritated in a warm place, appeared to contract, and then to relax, and to undergo a kind of tremulation*. He adds, that he has frequently made the experiment, and that *it always succeeded*.

“ According to Mr. Bonnet, the seminal fluid, in producing impregnation, effects only the evolution of what was already formed.

“ This certainly is my opinion: but to say so much is saying nothing; for such vague expressions do not serve to distinguish my notions from those of other naturalists who reject the doctrine of Epigenesis. I have reason to presume, that the worthy M. F. has not studied with any care the nature and the connection of my principles on generation. He was doubtless too
much

had attained a certain height, and the male plants could be distinguished from the females, the former, which were four in number, were eradicated, and the two females only were preserved. About twenty days before they began to flower they were carried into a room looking towards the south, where they stood forty-two days. There were two windows in the room, which were always kept shut, as also was the door, except when I went in to water the plants. They thrived, notwithstanding their confinement, on account of the rays of the sun shining through the windows upon them several hours a day. Although this precaution seemed preferable to that of the French anonymous writer, who had placed his female plant upon a

much prejudiced with the idea of generation being a mystery not yet penetrated by the ablest naturalists. God forbid that I should have the presumption to think that I have drawn aside the curtain that covers the mystery: I have only attempted to lift up a corner of this curtain.

“ When we read the opinions of most naturalists, do we not incline to believe, that each, out of affection to his own ideas, flatters himself that he has divined the secret of Nature ? ”

“ I must confess that there are physiologists and natural historians who deserve this reproach; but there are others who do not deserve it; and M. F. ought to have excepted them. You have read me, and, what is more, have studied me. You, therefore, know whether I have ever proposed my notions as a man, who flattered himself that he had divined the secret of Nature.”

window,

window, it did not satisfy me. The pollen indeed supposed to be disseminated through the air at the flowering of the hemp-yards, could not very readily find its way into an apartment kept so closely shut, yet it cannot be asserted that its ingress was totally prevented; and yet it was of the utmost importance to hinder it, at least, from coming in contact with the flowers. To attain this end effectually, when I brought the pots into the room I immediately introduced two branches of my two plants, upon which some buds were growing, into two glass bottles, with long necks and large bodies. The necks were perfectly closed with cement, without injuring the branches. By being perfectly closed I mean, that all communication between the external air and the inside of the bottles was cut off, as was evident from the following decisive proof. Along with the branch of hemp, I inserted, for about five inches, an open tube of glass, which was imbedded in the cement; the other end projected four feet from the neck of each bottle, and was immersed in a basin of water to the depth of several inches; but I had first taken care to suck a portion of air out of the bottles. The water ascended to the height of a foot and three quarters into the tube, at which it constantly

constantly stood, being only subject to such variations as are observed in barometers, for the tube was now actually changed into one of those instruments. The permanent elevation of the water in the tube was a certain sign, that no external air found its way into the bottles: had a stream of air made its way through the cement, or any part of the bottle, it is obvious, that the water in the tube must have descended to the level of the basin. I must own, indeed, that I was obliged to renew the experiment several times before I succeeded. With this apparatus I was certain that the external air, and consequently that the pollen, which may be supposed to be dispersed through it at the approaching flowering of the hemp-yards, could find no access to the confined branches.

xxvii. But all these precautions seemed insufficient to render the experiment quite unexceptionable. It was necessary, not only to remove all suspicion of external pollen getting into the bottles, but likewise to watch whether any of this powder was produced within. The botanical reader immediately comprehends the reason of my apprehension. It has been observed by Linnaeus, Haller, Duhamel, and others, that male
flowers

flowers are not very infrequently found upon female individuals: a root of spinach, of which I shall speak below, furnished me with a remarkable instance; and the species in question is subject to the same accident, as I was informed by Mr. Bonnet, in a letter dated August 15, 1778. The letter gave me notice of an experiment which he undertook upon hemp, after I had communicated mine to him. The paragraph to which I allude is the following: "I began this year some experiments upon hemp. I have followed the method which I employed for rearing the lice of plants in solitude. My plants were covered with large tubes of glass, hermetically sealed at the top, and with the bottom sunk in the earth. But fortune did favour me---instead of a female I had a male plant in one instance, and in another a plant of great expectation, after putting forth many flowers with pistils, produced some with stamina, close to the former, which totally disconcerted the experiment." It therefore became me to watch diligently, whether any male flower was produced among the females; I therefore carefully examined twice a day, both with my naked eye and a glass, the confined branches, which were as open to inspection, as if they had

been growing in the open air. In the mean time, I did not neglect the other parts of my two plants. That the examination might be more certain, I stripped off all the branches except those which were inserted into the glasses, and took care to inspect the stems as closely as the branches. But I can assure the reader, with the utmost confidence, that the flowers which successively made their appearance, both on the bare stems and branches, were all female without a single exception. For the sake of perspicuity, I will first describe the trunks, and then the branches. The former began to put forth their flowers towards the end of August, at which time the hemp-yards were also in bloom. Notwithstanding their constant confinement, they produced seeds, and ripened them by the end of September. When I compared these seeds with some which I gathered in the hemp-yards, I found that they were smaller. Besides the number of seeds produced by the stems in the hemp-yards, were in like circumstances considerably more numerous, than by the two stalks confined in my apartment. But notwithstanding these differences, the seeds grew nearly as well in the apartment as those which grew in the open air, as I found by sowing several
. hundreds

hundreds of both sorts: Such are the results afforded by the two stems.

XXVIII. I am now to relate what happened to the two branches. The flowers, which nearly kept pace with those of the trunks, were succeeded by feeds; towards the 20th of September they had acquired such a size, as inclined me to think them ripe; I therefore resolved to take the branches out of the bottles, and I found in fact, that more than an hundred were arrived at maturity; this was doubtless in some measure owing to their having received no molestation from the perspirable matter, which was collected in large quantities in the belly of the bottles. By dissecting some of the ripe feeds, and others that were more or less distant from this state, I was enabled to make the following observations: When the parts surrounding the least forward are removed, a greenish pyriform body presents itself with two points like small antennæ, which are the rudiments of the pistils. In the pyriform body is contained a solid nucleus, semi-gelatinous, and consisting of an homogeneous substance. The nucleus in feeds further advanced, exhibits an excavation in the center, full of a liquid which, in feeds yet nearer maturity, is thicker; and in this inspissated matter may

be seen a white point, which is easily known to be the lobes and plantule. If seeds still more advanced be analysed, in opening of the lobes, which are bent into the shape of an arch, a cavity or depression is observed, and in it are lodged two whitish dentated leaves, which are inserted in the plantule, or rather arise from it. These leaflets are larger in ripe seeds. Such were the appearances of the product of the branches; and seeds gathered in the hemp-yards furnished me with the same observations. Hence I was obliged to infer, that Nature, in the production, or to speak more properly, in the evolution of the seeds, had operated alike in the closed bottles, and in the open fields.

I said before, that the seeds of the two confined stems were smaller and in less number, than of those in the hemp-yards. I may now add, that the small size of the seeds that grew in the bottles was still more remarkable, and that equal branches usually produce twice the number. But I shall be asked, did seeds so much smaller and less numerous grow when they were sown. To this curious and important point all my enquiries tended. Of a hundred and sixteen seeds, which were all that seemed to be ripe,
 one

one half, viz. fifty-eight, were set in a pot, and they all, except five, came up.

XXIX. The result of another experiment corresponded perfectly with the foregoing. Though a large quantity of pollen arises from the hemp-yards into the atmosphere, at the season of flowering, and is transported to no inconsiderable distance, it is reasonable to suppose, that in the course of several months it must be lost; for the rain and snow cannot fail to carry it down to the ground, and free the air from it: should any continue to float in the atmosphere, it will probably in time lose its prolific virtue. This supposition, which seems well founded, as will be more particularly shewn in the succeeding chapter, induced me to make some female plants of hemp blow before the hemp-yards were in flower. With this view, I set at Pavia twelve hemp-seeds in twelve pots, before the middle of November; the seeds were some that remained of the product of the confined branches (XXVIII). Although the season was unfavourable to vegetation, eleven out of the twelve germinated, and continued to grow, though but slowly, the whole winter, the pots having been removed into a warm room. When spring arrived, the windows were thrown open, and left so till I had oc-

caſion for the plants. Meanwhile they continued to advance, and about the 20th of May, the male and female individuals could be diſtinguiſhed. The former were deſtroyed. The remaining females, four in number, were expoſed to the air and ſun on the outſide of a window. That I might examine with greater eaſe and certainty, whether any male bloſſoms were produced upon theſe female plants, I cut away all the branches, and preſerved only the ſtalks, as in the experiment related in the xxviiith paragraph. But fortunately no flowers of this ſort ever appeared. Towards the end of May, flowers with piſtils began to appear, ſix weeks before the flowering of the hemp-yards beyond the Po (for there is no hemp raiſed in the neighbourhood of Pavia) and conſequently before the diſſemination of the freſh pollen. About the 8th of June moſt of the ſeeds were ripe. They were quite as thick and large as upon the ſtems reared in the fields; I ſowed ſeveral hundreds, and moſt of them germinated. One immediate conſequence may be deduced from theſe variations of my experiment. The perfect fructification of hemp is entirely independent of the action of the fecundating duſt.

xxx. But

xxx. But enough of the experiments on hemp. I proceed to speak, but much more briefly, of spinach (*Spinacia Oleracea*). I made several different trials with this plant in the course of some years.

In the first place, I transplanted in May several roots into my garden at Pavia. As soon as the different individuals could be distinguished, I destroyed the males. The females, although they remained alone in the garden, produced plenty of seeds, which, when sown, germinated in great abundance. At the time of flowering, I watched with great care, whether any blossoms with stamina opened: my illustrious colleague and friend Counsellor Scopoli, also examined my spinaches; but neither could that expert botanist or myself perceive any.

I next covered a plant of spinach with the receiver of an air-pump; the lower end was pressed to some depth into the earth of the pot. Thus the communication between the external and the internal air was cut off, unless the former may be supposed to have found its way into the receiver, through pores in the mould. I made use of this apparatus a few days before the opening of the flowers; but was obliged, in thirteen days, to remove the receiver, for the leaves began to wither,

probably on account of their confinement. Several flowers with pistils had in the mean time set, and the appearance of the seeds promised that they would do well; nor was I deceived in my expectation, for they ripened perfectly, and when sown, came up in great abundance.

xxxI. In the Modanese and in the Marquisate of Scandiano, the spinach raised by the people of the country, is usually in flower from the 20th of May to the middle of June, so that the seeds are commonly gathered at the beginning of July. I took advantage of this practice to procure pistilliferous flowers, a long time after the blossoms with stamina had decayed. For this purpose, I sowed some spinach towards the end of May; the female plants produced flowers with pistils in September, and many of the seeds came to maturity; but many others were checked by the beginning coldness of the season: but those which grew ripe were sown, and they germinated. Notwithstanding, therefore, near six weeks had elapsed since the falling of the staminiferous flowers in the fields and gardens, my seeds were productive. It is difficult to imagine, that the pollen could have been the cause of this fecundity; and the rather, since in spinach it is far from being in such abundance,

abundance, as to be capable of diffemination, like that of hemp, far and wide through the air. My spinaches were, moreover, constantly kept in an apartment.

xxxii. I nevertheless undertook another trial, which I believed would be decisive. I had several young spinaches transplanted into pots in August at Pavia, one into each pot. Great attention being paid to them, they grew before winter to a considerable height. And being kept in a warm room, they made some further progress during the course of this season; and before the end of March, the female plants, which were three in number, began to put forth flower-buds with pistils. But here an accident, which is not very uncommon, and to which I have already adverted (xxvii), occurred. In one of my daily visits to my three plants, I perceived upon one individual an unexpected conjunction of male and female flowers, growing close together, and forming very elegant groups.

The blossoms with pistils were very conspicuous, but those with stamina were so little advanced, that they could not be distinguished by the naked eye. Both sorts appeared to be equally numerous, but the union extended only to two branches---all the rest bore female

male blossoms only. I may here incidentally remark, that the great abundance of the male flowers, in the present case, is a very singular phenomenon. I have read in botanical writers, that a few male flowers are sometimes found in company with females, but never that they amount to an equal number, a circumstance that excited my admiration with respect to this individual; for I counted two hundred and seventy-five male buds. This unforeseen accident determined me to extirpate the plant. I consider it as very fortunate, that I observed it in time, while the male blossoms were yet in a very immature state, and consequently before the pollen could yet have produced any effect. Having thus freed myself from the trouble of watching this extraordinary individual, I continued to observe the two others; they produced only blossoms with pistils. April was the season of the maturity of these blossoms, which is generally estimated by their aptitude to be fecundated: it was earlier by thirty-five days, than the usual flowering of this plant in the environs of Pavia. Before the end of May, the seeds of my two plants were almost all ripe. Every branch and every twig was loaded, and they were of equal size with the seeds produced in the
neighbouring

neighbouring gardens the preceding year. To complete the experiment, it remained to try whether they would germinate. Of a hundred and fifty, chosen among the whole product as the finest, a hundred and thirty-two came up; and of a hundred, which I afterwards sowed, ninety-three grew. These proofs obliged me to conclude, that the seeds, notwithstanding the privation of the pollen, were productive.

xxxiii. The next and last plant producing male and female individuals, which I mentioned, is annual mercury (*mercurialis annua*). Five very small plants were removed from a garden, on the 22d of August, into five pots. They were managed in the same manner as the spinach during the winter (xxxii), and were all so far advanced at the beginning of spring, that there was no difficulty in distinguishing the males from the females; of the latter there were three, and these alone were preserved. By the 24th of March blossoms with pistils appeared upon several branches, growing out of the axillæ of the leaves, and in a few days more the number was exceedingly increased. They were borne upon short flower-stalks, and, as usual, consisted of two small seeds resembling testicles. They were of a green colour and hairy.

hairy. But here the event was just contrary to what happened in hemp and spinach. The greater part of the blossoms dropped prematurely; of the few that remained the seeds grew for some time, but fell before they were ripe, and when sown, they did not spring. As this took place before the male plants in the gardens and the fields about Pavia were in flower, I began to suppose mercury to be one of those numerous vegetables, which cannot propagate the species without the powder of the stamina. Meanwhile my three plants continued to put forth new branches, and the old ones, instead of withering, vegetated with great vigour; but still the seeds dropped prematurely. This gradual evolution and production of fresh branches, was of such long continuance, that they shewed no tendency to decay, but were producing blossoms with pistils when the mercury in the fields was in flower. I therefore began to entertain hopes, that the seeds now put forth, and those which should follow, would succeed better than the earlier seeds, more especially as the pots were exposed to the open air upon a window, and looked into a garden, in which grew several male individuals of this species. But my expectations were disappointed: as long as the
the

the three plants continued to thrive, the seeds dropped almost as soon as they appeared; nor did one of those that were sown ever come up. I repeated the experiment two succeeding years with the same event.

xxxiv. It therefore became necessary to vary the mode of conducting it. Being more confirmed in my suspicion, that the sterility arose from want of pollen, which, though it was at no great distance, did not reach my plants, I determined to bring it nearer; without, however, setting individuals of the different sexes in the same place. Two male plants of mercury, reared the next year in two pots, were placed on the outside of a window, and two females growing likewise in pots, were set on the outside of another window. Both windows belonged to one room, and had the same aspect. The four roots of mercury were nearly of the same age, and of the same size. And I waited with great anxiety to see whether the females, on account of their vicinity, would be impregnated by the males. The seeds were constantly falling, but not in such abundance as in the former experiment, when the males were at a much greater distance (xxxiii). Those which adhered went on thriving, and seemed as if they would ripen; and they did accordingly

accordingly arrive at maturity, and, what is of more consequence, were productive; for soon after I had sown them in a pot, I had the pleasure of seeing them spring. It therefore appeared probable, that the vicinity of the males to the females had been instrumental in occasioning fecundation: their influence could scarce be derived from any source, besides the action of the contiguous pollen.

xxxv. This experiment obviously required another: it was proper to bring the different individuals nearer to each other; I accordingly placed two males and two females upon the same window. It now became manifest, how much influence the approximation of the two sexes has upon fecundation. The two females retained almost all the seeds which were produced at this time, exceeding an hundred. The seeds grew perfectly ripe, and when put into the ground, were unfolded into as many plants.

xxxvi. The females having several branches in vigour, and still continuing to vegetate, an experiment, the reverse of the two preceding, suggested itself to me. I removed the pots in which the male plants grew into another chamber. This separation served further to corroborate the necessity of the pollen

pollen in mercury; for in consequence of its entire removal, all the seeds afterwards produced, by the growing branches fell before their time, and were unproductive.

Moreover, it is by no means surprizing that females, at a distance from males, should be sterile. The pollen is in too small a quantity to be widely disseminated. The inconsiderable quantity which was afforded by several male plants, at a time when the antheræ, viewed by the microscope, appeared full, gave me ample proof of its scarcity.

I shall make upon mercury another observation, very necessary to be known. I have informed the reader, that most of the unimpregnated seeds wither and drop, while a few grow to some size, but are not productive (xxxiii). The scope of my researches required that I should examine, whether these seeds consist of lobes and a plantule. By analysing them repeatedly, I found that the seeds, which fall soon after the flowering, are full of an homogeneous and gelatinous substance, and afford no vestige of plantule or lobes. But impregnated seeds, examined at the same period, that is to say, a little after the season of flowering, exhibited the same appearances. Many days elapsed before the lobes and the plantule could be discerned,

cerned, an observation that is not confined to mercury, but extends to every plant hitherto examined by me. In those seeds which made a tolerable progress in their growth before they fell, appearances were very different. They first became full of an almost fluid whitish matter, which was gradually inspissated; and in the midst of this matter, after some time, might be seen a little body, so soft as to adhere to the finger when it was touched; but it afterwards made some resistance to the touch, and now evidently appeared to be the lobes and plantule; which parts grew more conspicuous and distinct as the seeds advanced. The plantule had the shape of a cone, dilated towards the base into two flat circular bodies, which were the lobes. The same was also the figure of the lobes and plantule contained in productive seeds.

C H A P . V .

RECAPITULATION OF THE PRINCIPAL
CONSEQUENCES OF THE FACTS RELAT-
ED IN THE FOUR PRECEDING CHAP-
TERS.

R E F L E C T I O N S .

XXXVII. **A**LL these facts convey, in my opinion, instruction sufficient to serve as a foundation for speculating on the mode observed by Nature in the generation of plants. If we inspect the ovaria before fecundation, we find the seeds externally solid: they continue in this state for some time after fecundation. They afterwards became hollow within; in the cavity appears a small body, not distinguishable at first, but afterwards known to be the embryo or the plantule with the lobes. The embryo sometimes seems detached from the seeds, and floating in a liquid, and sometimes is attached to them by one or more connecting threads (II, III, IV, V, VI, VII, VIII, IX, X, XI, XII, XIII, XXXVI).

As these facts seemed favourable to the opinion of those, who suppose the embryos of plants to pre-exist in the powder of the antheræ, and thence to pass into the seeds, which therefore do not contain an embryo before fecundation, I was led to turn my researches towards the powder. I examined it with the microscope, but could not by any means find that the embryos, or any beings analogous to them, are lodged in it (xv). Hence I proceeded to the expedient of interrogating Nature in another manner: I endeavoured to prevent the access of the impregnating dust to the ovaria, supposing that such means would furnish the clue to this abstruse mystery. Three descriptions of plants were submitted to experiment; hermaphrodites; those which bear separately upon the same individual, male and female flowers; and, lastly, those which produce both male and female individuals. The antheræ of the first were cut away before the ripening of the pollen; in the second the male flowers were extirpated, and the female individuals of the third were insulated. In the hermaphrodite plants it appeared, that the privation of the pollen does not prevent the appearance of the embryo in the seeds, though the seeds are incapable of germinating; in some plants the

the absence of the pollen neither hinders the embryos from appearing, nor the seeds from being productive; the same observation is to be applied to several plants with female individuals; but in other plants of the same class, the want of pollen renders the seeds sterile, though the embryo appears in them, (xviii, xix, xx, xxi, xxii. xxvi, xxvii, xxviii, xxix, xxx, xxxi, xxxii, xxxiii, xxxiv, xxxv, xxxvi).

The direct and immediate consequence of these results is obvious: for if the embryo appears without the co-operation of the pollen, and if besides, in many cases, the seeds grow, it is clear that their existence can have no dependance on the pollen; and upon those occasions on which the seeds do not grow for want of it, we can only say, that they stand in need of the condition requisite for their further evolution, as the fetuses of animals do not thrive for want of seed, although they pre-exist in the ovarium of the female. Another consequence not less evident is, that as the embryos do not belong to the pollen, they must necessarily appertain to the ovarium.

Some naturalists, of no vulgar reputation, have supposed, that the embryos of plants are formed by two principles, one contributed

by the powder of the stamina, the other by the pistils. But my observations, which prove that the embryo has no dependance upon the pollen, clearly prove the falsehood of this supposition.

xxxviii. But the embryo, which I have shewn to belong exclusively to the ovarium, may be conceived to belong to it in two ways. It may be formed of matter furnished by the parent plant, or it may pre-exist. Now, which of these ways are we to admit? An Epigenesist, a Count Buffon, accustomed to view Nature under an angle proportional to his favourite ideas, would not probably hesitate, if the facts observed by me had passed under his inspection, to embrace the former opinion. The embryo, not being at first visible in the cavity of the seeds, but appearing afterwards in the form of a gelatinous point, without any determinate shape, floating besides, in a liquid without any perceptible connection with the seeds, continuing to grow and becoming organized in it--- These arguments would no doubt be sufficient to lead that famous naturalist into the belief of the embryo's formation in the seed; he would perhaps also believe, that it is formed of the liquor in which it floats. But if we lay aside all prejudice in favour of any sect,
and

and apply the strict principles of logic, we shall find these arguments inadequate. In the first place, that a liquor, or any other inorganic substance, should nourish and promote the growth of an organized body, is a proposition easily conceived, and is indeed proved, by daily observations on ourselves, on animals, and plants. But it is not so easy to conceive, that an inorganic and shapeless body, whether liquid or solid, can be organized by mechanical laws only: and with all his eloquence, and by all imaginable efforts, the naturalist above-mentioned has never shewn, that one animal, or one vegetable, even among those that appear the least perfect, and the least organized, owes its origin to a mechanical opposition of parts. Secondly, though the embryo does not appear to be organized, this is by no means a sure proof that it is not. During the first hours of incubation, the organization of the chicken is not perceptible; and yet we cannot doubt that it is organized, if it can be proved to exist before fecundation. Now Haller has given the proof of this with respect to the chicken; and I have given it with respect to newts, frogs, and toads. It is therefore beyond all doubt, that these amphibious animals are organized before fecundation. But

their organization is not then apparent; mere appearances would lead us to suppose them formless bodies; for we see nothing but spherules, consisting of a cover or skin filled with a semifluid matter. The same seemingly inorganic structure continues for some time after fecundation. The embryos of plants, then, may also be organized, although they do not appear to be so; and I have certain proofs that this organization, though not at first apparent, is actually present. When I opened the seeds at the time the gelatinous point began to appear in the midst of the liquid, and examined it without using any method of preparation, I could discover nothing that had the appearance of being organized; I saw neither lobes nor plantule; nothing in short but a mucilaginous particle. But if I first boiled the seeds, this point oftener than once put on a different aspect. It was not only no longer gelatinous, but when I turned it about, and pressed it gently with the point of a needle, it would sometimes split into two exceedingly minute pieces, in the middle of which was implanted a pointed atom: it was not very difficult to discover that these parts were the lobes and plantule, as was still more manifest when a microscope of great magnifying power was employed.

This

This point, therefore, proved to be organized at a time when it was not suspected; nor is this the first occasion, on which the action of fire has been used as a convenient method of ascertaining the organization of a body which had no such appearance. It is well known to the cultivators of Natural History, that the worms generated by those winged insects, usually called *carrion flies*, after having acquired, by feeding on putrefied flesh, a proper size, are formed into an elongated bowl, in which the fly is gradually elaborated; or, to speak more properly, unfolded. If these bowls be opened a few days after they are formed, no lineaments of a fly present themselves to the sight; we find only a kind of pap, without any sign of organization. But the scene changes, if the bowls be boiled at different periods; on the third day the legs are visible, on the fourth the wings, soon afterwards the proboscis, and lastly the animal perfectly formed. These observations were first made by Reaumur, and in years past I have amused myself with repeating them.

But the application of heat made me acquainted with another truth. By boiling I discovered the point, or the embryo, some days before it usually appears. I have not

seen it indeed in feeds, containing a liquor in which the embryo floats, but in some of those in which the liquor coagulates and is inspissated. Several feeds were taken from the same ovarium, and some of them were boiled for a few instants, and the rest left to themselves. In the latter I could not discern the embryo, but it was plainly to be distinguished in the former. The difference arises from the consolidation of the particle, or the embryo, by the boiling water; in the other feeds it did not strike the sight, on account of its transparency and fluidity.

xxxix. I come to the objection derived from the insulation of the embryo in the feeds; from this circumstance it might be supposed to have no dependance upon them, and consequently to originate from the liquor in which it floats. The embryo certainly, in some cases, seemed to swim at liberty in the liquor, nor could I discover any connection with the feeds, but on other occasions I manifestly discovered the connecting medium, as in the common bean, the radish, and the Syrian shrubby mallow; it consists of a mucilaginous filament, of which one end insinuates itself between the lobes, and is inserted into the plantule, and the other end passes out of the lobes, and is attached to the
inside

inside of the seed (VI. VIII, XIX). This filament probably consists of a number of vessels, designed, like the umbilical cord in animals, to convey nutriment to the plantule and lobes, whether the nutriment be the liquor lying in the cavity of the seeds, filtered and rendered perfect in these vessels, or the substance itself of the seeds, at the expence of which the lobes and plantule grow; for it constantly happens, that as these parts are enlarged, the internal substance of the seed is destroyed, and at length reduced to a mere skin. But besides the manifest communication formed by this filament between the embryo and the seeds, I have found, in more instances than one, that it communicates at other parts; as is obvious in broom, pease, kidney beans, chick-pease (IV. VII. IX). If I have sometimes not been able to find any ligament passing from the embryo to the seed, it is not therefore to be concluded, that there exists none, but that it cannot be discerned, either on account of its minuteness or transparency. For as the embryos, in which the filament is visible, are unfolded within the cavity of the seeds, in the same invariable manner as is observed by Nature, in embryos appearing without this connecting medium, we have just reason to infer, that it exists in
the

the latter as much as in the former. That the minuteness, and much more the transparency of these cords, may hinder them from being seen; not to mention the proof afforded by the embryos themselves before they are subjected to the action of fire (xxxviii), is confirmed, by a striking instance, in the tadpoles of the fetid toad, while they as yet float in the liquor of the amnion: these tadpoles are provided with an umbilical cord, but which is not visible on account of its transparency.

As it is evident, therefore, that we have no foundation for supposing the embryo to be mechanically formed in the ovarium of the plant, and as it has been moreover proved, that it does not depend upon the powder of the male, it remains to be inferred, that it pre-exists in the ovarium. In favour of its pre-existence we have another very powerful argument, in the communication effected by the filaments between the embryo and the seeds; for hence it is clear, that the seed and the embryo compose but one organic whole. As then from all my observations it appears, that the seeds pre-exist in the ovarium long before the opening of the flowers, I see no reason why we should not assert the same thing of the embryos.

XL. The

XL. The celebrated Muller, a gentleman of Denmark, made at Geneva, several years ago, under the inspection of Mr. Bonnet, an observation, nearly approaching to those related in this Dissertation. He found the seeds of pease arranged in rows within the pods before the opening of the flowers, and consequently before the action of the pollen had been exerted; but he was not solicitous to push his observation any farther, by examining the inside of the seeds, or by tracing the progress of evolution. The philosopher of Geneva could not but deduce important consequences from this fact. He began to reflect upon the nature of the seeds, that they were bodies admirably organized, bodies now in miniature what they were soon to be at full length; that, like all the parts of the egg taken together, all the parts of these bodies conspire to form an organic whole. He justly concluded, that in the seeds of pease, shewn to him by the Danish naturalist, the plantule existed with its lobes, as in the egg, however diminutive, the embryo, with its covers, lies concealed. Hence he thought himself authorized to infer, that as the seeds of pease exist before the expansion of the blossoms, and, of course, before the aspersions of the fecundating dust, so the lobes

lobes and plantule pre-exist in like manner (a); the very same consequence which I have above deduced from my numerous observations (xxxix).

I may possibly be asked by some curious persons, whether, after having abundantly proved, that the embryos of my plants exist before fecundation, or, to use an expression equally applicable to those which require the fecundating dust, and those which do not, before the opening of the blossoms, I ever thought of having recourse to some contrivance for rendering them visible before this period. It is proper to inform such enquirers, that the idea has not only occurred to me, but that I have put it in practice, though indeed without effect. I have taken the seeds out of the close and compact blossoms, and dissected them under a powerful magnifier, but have never been able to discover any thing more than by a glass of inferior power, that is to say, an homogeneous gelatinous matter. I doubt whether the embryo might not be withheld from my view, more by its transparency, for the jelly was transparent, than its smallness. I boiled some seed for a little while, and steeped others in spirit of

(a) Palingénésie, T. I. page 420, 421.

wine. The jelly was inspissated in both cases, but the embryo was not rendered conspicuous. Coloured infusions have sometimes been employed by naturalists, with the utmost advantage, to bring into view those organs of plants, which are otherwise invisible. By these means Mr. Bonnet has discovered the small vessels which arise from the embryo, and pass in a serpentine direction through the substance of the lobes (*a*). I put several seeds into differently coloured infusions, and they acquired, both externally and internally, the colour of the infusion. But the embryos were not rendered in the smallest degree perceptible. Thus unsuccessful did these various trials, as well upon closed as expanded blossoms, prove, with respect to the embryos, nor were they more fortunate with respect to the lobes. The same thing happened after the falling of the flowers, and the formation of the cavity so often described. The whole advantage gained by my applications arose from spirit of wine, which inspissated the liquor of the cavity and the embryo at the same time, so that it might be discerned five or six days sooner, as I collected from the examination of other seeds of the same age left on the

(*a*) Recherches sur l'usage des feuilles.

plant. This result adds confirmation to the truth above established, that the embryos are not generated, but only unfolded in the seeds (xxxviii, xxxix). It moreover proves the extreme difficulty, I might say, the impossibility of obtaining a view of the plantule before the expansion of the blossoms. When spirit of wine enabled me to discern it, it was yet a most diminutive atom, notwithstanding fifteen days had elapsed since the opening of the blossom in which it grew. Now the plantule had, doubtless, in this interval, passed through several successive stages of growth. And how prodigiously rapid is the growth during this first period! How inconceivably small, therefore, must the plantule be before the opening of the flowers! Is it not then unreasonable to hope, that we shall be able to discern an object so excessively diminutive?

xli. The illustrious Haller was the first who shewed, that in birds the fetus exists before fecundation. I have extended this discovery to different species of amphibious animals. These facts led me to believe it probable, that the same pre-existence takes place in all animals. What has been discovered in various individuals of the animal kingdom,

kingdom, has been also observed in several sorts of plants. I have therefore an equal right to draw the same conclusion with respect to all plants. But if I may express my deliberate opinion on this important topic, I dare not affirm that the embryos, in all cases, exist before the opening of the blossoms. Such a decision would be precipitate; we may learn from a thousand examples the variety of Nature, even in those operations which are designed to accomplish the same end. I therefore only observe, that till the contrary shall be proved, we have just reason to continue in believing pre-existence to be universal. Hence we have a new and striking point of analogy between plants and animals, to be added to the many others long known; and hence the suspicion, that these two tribes of organized beings compose, perhaps, only one immense family, receives strong confirmation.

XLII. Basil and mercury are two plants, which unite with many others, to shew the necessity of pollen to fecundation. The latter of these is particularly deserving of the reflections of the philosopher. Female plants of mercury, at no great distance from the males, remain sterile, because the fecundating atmosphere, if I may so speak, of the latter,

latter, is not far diffeminated, on account of the small quantity of pollen produced by them (xxxiii. xxxvi). If the males be brought near the females, they bear some productive feeds (xxxiv); but if the males be placed almost in contact with the females, the greatest part of the feeds will adhere and ripen and germinate (xxxv). Should the males be again removed, the females return to their former barrenness (xxxvi). This experiment is exactly conformable to one related by Duhamel and Bernard Jussieu. Those botanists having observed, that a female turpentine-tree, growing in a garden at Paris, remained constantly barren for want of a male, notwithstanding it flowered every year, determined to bring one near it. The very year they put their design in execution, the female bore a plentiful crop of fruit and productive feeds. But the following year the male, which grew in a pot, being carried to its former station (*a*), the female relapsed into its accustomed sterility (*b*).

XLIII.

(*a*) Duhamel Physique des Arb. T. I.

(*b*) There is growing in the Bishop of London's garden, at Fulham, a male tree of this species, and in the botanic garden at Chelsea, a female. Some years since an attempt was made to fecundate the female, from which the feeds always drop prematurely, by carrying some branches cut from the male,
and

XLIII. The successful experiment of Mr. Gleditsch upon the palm, denominated *Chamaïrops*, by Linnaeus, and by Boerhave *Palma dactylifera major, spinosa, fœmina, folio flabelliformi*, has been much celebrated. This palm, which grew in the Royal Garden at Berlin, and is perhaps still growing there, had never borne fruit, notwithstanding it was eighty years old. The sagacious naturalist suspecting, that the failure arose from the want of a male plant in the garden, and having no opportunity of availing himself of the expedient employed by the two Academicians of Paris, had recourse to the only alternative remaining. He procured some pollen from the male of the same species, and sprinkled it upon some of the female blossoms. The event corresponded admirably with the views of the curious observer. All the flowers that were touched by the pollen

and fixing them upon the female. But the experiment, from whatever cause, proved unsuccessful: whether the antheræ had already shed their powder, or whether it was shaken off in carrying. Plants of the turpentine tree are scarce and valuable; it would therefore be an object, not only of curiosity, but of use, to succeed in the fecundation of the female; and surely the end may be attained, by a little patience and dexterity. Let some pollen, for instance, be gathered, and carefully sprinkled upon the female blossoms, according to the method successfully employed by Mr. Gleditsch, in impregnating the palm. T.

produced dates in proper season, and from the seeds which were planted there sprang an equal number of young palms; but the other flowers, upon which the influence of the pollen had not been exerted, lost the greatest part of their fruit, while it was yet small, and in the little that remained, there was no stone, nor did the seeds germinate (*a*). One circumstance attending this artificial fecundation deserves to be mentioned. The pollen employed by the botanist of Berlin was not fresh, but dry; it had been gathered nine days.

When I was making my experiments upon mercury, it did not occur to me to attempt artificial fecundation; but I have not the least hesitation in supposing, that my success would have been the same as that of the Prussian naturalist. And as such experiments, under the guidance of a philosophical mind, cannot but throw great light upon the obscure subject of the fecundation of plants, I exhort botanists to undertake them upon mercury, a plant, not confined, like the palm, to a few countries, but widely spread over the globe. I could wish, that, in the first place, if I may be allowed to express my slender

(*a*) Acad. de Pruss. for the year 1767.

conceptions,

conceptions, the pollen of the mercury might be carefully examined, in order to discover what part of it is the immediate cause of fecundation. I have already observed (xv), that it consists of vesicles. In these vesicles the industry of enquirers has detected a subtile liquor, containing a multitude of globules. This simple observation has given rise to several discordant opinions. Some have supposed the globules to be the immediate agents of fecundation; others have attributed this effect to the subtile liquor; nor have there been wanting others, who have imagined it to be produced by a fluid of excessive tenuity, a species of vital spirit, exhaling from the above-mentioned liquor, which serves only as a vehicle to the fluid (a): A few delicate experiments would perhaps determine which of these opinions is the true one. With respect to the last, we might probably put it to the proof, by occasioning the explosion of the vesicles by moisture, and exposing the liquor they yield for some time to the air. If it will not now fecundate the female blossoms of mercury, it would seem, that it is effected by such a spirit, or at least the most subtile, active, and volatile part of the liquor, which by this time has evaporated.

(a) Adanson familles des Plantes, T. 1.

But should fecundation nevertheless take place, instead of imputing it to the spirit, it must be ascribed either to the liquor or the globules, and then the result of my experiments upon animals would be verified with respect to plants: it would appear, that the efficient cause of fecundation is not the subtle, volatile part of the spermatic liquor, but the gross sensible portion. Next, in order to learn whether the globules, or the liquor independent of them, produces fecundation, it will be necessary to separate the one from the other, and touch different female blossoms with each; nor do I think, that a person accustomed to delicate experiments, would find it very difficult to effect this separation.

XLIV. Mr. Adanson lays it down as a certain truth, that the smallest particle imaginable of pollen falling upon the stigma, will produce fecundation (*a*). If this were really the case, the impregnation of plants would in this particular agree with that of several amphibious animals, for which I have shewn an infinitely small drop of seed to be sufficient. But this celebrated Academician must allow me to remark, that what he advances is a mere hypothesis, in support of which he

(*a*) L. c.

does

does not bring the smallest presumption, much less any proof: nor does he offer either presumption or proof of the existence of this vital fecundating spirit in plants, notwithstanding he asserts, that the embryos of animals are impregnated by a spirit of the same nature, issuing from the seed; an assertion which my experiments shew to be absolutely false. Nothing, however, can be easier than to bring this hypothesis to the test of proper experiments, by artificially fecundating mercury. And were such experiments diversified, like mine upon animals, I have no doubt but that they would furnish new and curious information.

XLV. The feminal liquors of animals and plants possess one property in common; they retain their virtue for some time after they have been taken from their natural reservoirs. The seed of frogs and toads preserves its prolific power for several days after it has been taken out of the animals; and the same quality was observed in the pollen of the palm, several days after it was separated from the plant (XLIII). But the seed of animals becomes in no very long time inert. Are we to suppose that the same thing happens to the pollen after the dropping of the stamina? If we consider the physical cause which, ac-

according to every appearance, destroys the virtue of the seed of animals, it would seem as if the inference was not very just. The seed of animals becomes unfit for fecundation exactly in proportion as it putrefies; and if it can be preserved from putrefaction, it continues to be prolific. This species of corruption does not seem so much to be apprehended in the pollen, or rather in the attenuated liquor which it contains. From the analysis given by able naturalists we learn, that the liquor is of an oily nature, and refuses to mix with water (c). It appears, therefore, not unreasonable to suppose, that, like other vegetable oils, it will keep for a long time; hence some may be induced to think that the prolific virtue of the pollen continues so much longer than is generally believed, that there is nothing extravagant in imagining it to last for many months, nay for a number of years. But notwithstanding its oily nature, which may seem likely to preserve it from putrefying, the following reasons lead me to suppose that it soon loses its prolific power. In order that this liquor may produce its proper effect, and the same observation is applicable to the globules and the spirit of Adanson (XLIII), it must at the

(c) Gleditsch. l. c.

time it acts on the stigma iffue from the vesicles, or have been lately discharged, for it is natural to suppose, that by exposure to the injuries of the air it must be dispersed, altered, and rendered unfit for fecundation. But this dispersion and alteration must unavoidably happen to pollen, that has been ripe for some time. According to Mr. Needham's observation, which has been verified as well by others as by me, that if the ripe pollen of any plant whatever be examined by the microscope, we shall always perceive some vesicles from which the liquor has escaped; those that still retain it will explode if they be wetted, like an eolipile suddenly exposed to a strong heat. It is equally agreeable and surprizing to see the successive explosion of the vesicles, and the projection of the liquor, a few instants after the application of water. From this experiment it appears, that the vesicles remaining after fecundation, whether they float in the atmosphere or fall to the ground, cannot continue long without exploding and dispelling their contents, because some vesicles will burst spontaneously, and the explosion of the rest will inevitably be occasioned by vapour, dew, or at least by rain. These agents will, I am persuaded, prevent the pollen from retaining its prolific

virtue long after the season of fecundation. That rain, in particular, is destructive of this quality evidently appears from those plants, which, during the time of flowering, are exposed to violent rains, being either barren or bearing only unproductive seeds. This failure is owing to the explosion of the vesicles before they reach the stigma, the situation in which they fecundate the embryos contained in the ovarium.

However this may be, to return for a moment to the continuance of the virtue of the pollen, after the stamina are withered.---The term ought certainly to be ascertained both in mercury and other plants. The subject is highly interesting.

XLVI. It has been usually admitted as a decided point, that fecundation is effected by some vesicles, or, according to the common expression, by some granules of pollen, which entering through the stigma into the style, advance into its longitudinal duct, till being stopped by the narrowness of the passage, and moreover moistened by the juice oozing out from the sides, they burst and send forth the spermatic vapour, which penetrates as far as the ovarium, and impregnates the seeds contained in it. But this explanation has not had the good fortune of meeting with Mr.

Adanson's

Adanson's approbation; not that he denies the pre-existence of the seed in the ovarium, nor that it is fecundated by the liquor of the pollen, or, as he calls it, by the vital spirit residing in it. But he pretends that the vital spirit does not pass along this duct to the seeds, because in many plants there is no such duct existing. He allows, indeed, that in some plants, as in several of the liliaceous tribe, the canal extends from the stigma to the ovarium, but he asserts that it is otherwise in the greater number of other plants, which have their pistils imperforated. In order, therefore, to explain the manner of fecundation, he imagines that his vital spirit, which in subtilty and activity is equal to the electrical fluid, passes along the tracheæ that terminate at the surface of the stigmata into placenta, whence it insinuates itself into the seed, and fecundates the embryo (a). The illustrious Bonnet, in a sensible and profound Memoir, entituled, *Idées sur la Fécondation des Plantes* (b), mentions the opinion of Adanson, but appears little satisfied with his hypothesis concerning the *tracheæ*, on account of its being altogether precarious, al-

(a) L. c.

(b) Inserted in Rozier's Journal for 1774, and reprinted in his works.

though.

though it is proposed by the author as a well-ascertained fact, and moreover finds great difficulty in admitting that any pistils are without a duct, and perfectly closed. Hence, prompted by his constant ardour for the investigation of truth, he takes occasion to exhort philosophical botanists to examine this matter afresh. Although it was not my purpose, when I was making enquiries concerning the generation of plants, to enter upon a complete examination of the internal structure of the pistil, I did, however, occasionally examine some. For this purpose I cut the pistil transversely into several pieces, and then placed them vertically under the microscope. Thus I could ascertain whether they were perforated or not. I found that some pistils were completely perforated from the stigma to the ovarium; in others, the canal did not reach above half the length, and sometimes not so far down. Lastly, in several other pistils there appeared no perforation at all. Upon the pistils of mercury I made no observation, which, however, it would be proper to do; and then the validity of that hypothesis, according to which fecundation is not effected by the passage of a subtile impregnating substance along the duct of the pistil, but along the tracheæ, which terminate

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nate on the surface of the stigmata, may be scrutinized. I could wish that some pollen might be laid upon their surface, while care is, at the same time taken, that none enters into the aperture of the duct: and that on other pistils the inverse experiment might be attempted, that is to say, that some pollen might be introduced into the opening, while the surface remains untouched, and that we might suspend our opinion till we know the issue of the trial.

XLVII. But although I could discern no duct in the pistils of some plants, I am not, with Mr. Adanson, disposed to think that there are none. While I sincerely wish for knowledge extensive as his, I should be sorry to reason in the same manner. I have found that the vesicles or granules of pollen vary with the size of the plant, an observation which has been made and published many years since by other naturalists. Some are so large as to be perceptible by means of a single glass; while others are scarce discerned by the aid of the microscope. Hence I can believe, without reluctance, that there are some which no assistance can bring under the cognizance of the senses. If we suppose the duct in the pistil to be the canal of fecundation, its area will be proportional to the bulk
of

of the granules which are to enter it: if they be large, the passage must be wide; if they be very small or invisible, the passage will probably be so also. According to this supposition then, we shall not be able to see the duct, should it even exist. In order to determine whether my reflection be just, it will be only necessary to observe, whether the granules of pollen are invisible when the pistil appears to be without a duct. But should the granules be perceptible, and the pistils have no canal, as far as the eye can judge, it would hence follow, that this is really the case, for it may easily happen, that the pistil shall be examined at a time when the duct is either not yet open or has closed. Let me at once illustrate and prove this proposition. In the *Sponsalia Plantarum*, which is without doubt one of the best dissertations in the *Amœnitates Academicæ*, the celebrated Linnæus describing the amours of plants with great complacency, and in the same strain in which another naturalist would describe the amours of animals. He adduces, among other evident marks of the female's appetite for the male, the direction of the stigma towards the pistil and its dilation, like the gaping of a ravenous dragon, longing for the pollen of the male, with which, when it is

satiated,

fatiated, it closes. *Gratiola*, these are his expressions, *æstro venereo agitata pistillum stigmatate biat rapacis instar draconis nil nisi masculinum pulverem affectans; at satiata rictum claudit*. He applies terms of like import to various other plants. The reader must by this time perceive, that what I would observe is, that we may possibly miss the exact season of the female's ardour for the male; and then the aperture of the stigma will either be closed after satiety, or not yet opened. Hence we should falsely conclude, that these stigmas are without ducts. A circumstance of the same kind happens to various animals, particularly to some of the amphibious class, as frogs, toads, newts, of which the females have their oviducts quite free and open at the season of their amours; but at all other times they are so narrow, that air can scarce be introduced into them by means of a small syringe. If I should be asked, when the female is most ardent, and when, of course, the ducts are to be sought for, I would answer with Mr. Gleditsch, that at this period the delicate *papillæ* upon the stigma are slightly covered with moisture of the same kind as that which transudes from the vesicles of pollen, for then, according to that author, fecundation takes place.

XLVIII. I have before observed, that the surface only of the stigma without the orifice might be touched with the pollen of mercury (XLVI). But I should wish to know also, what would be the consequence of touching other parts, as for instance the surface of the stile. Fecundation might be also attempted upon the leaves and root of female mercury. This idea is not my own, but partly at least Mr. Bonnet's, who obligingly communicated it to me some time since, that I might put it to trial, but I have been hindered by my other pursuits. Let us hear him in his own words: "The author," (who is, as he supposes, M. Fougereux, see the long note at paragraph xxv), "finishes his Memoir with some ingenious views, which to him appear worthy to be followed up: I earnestly wish you could undertake this task: He would be glad, for instance, that attempts were made to fecundate female plants by the root. This idea certainly deserves to be brought to the test of experiment. But the root is at a great distance from the flower, and the impregnating spirit must travel far before it can reach the ovarium. It would be therefore proper to try the shorter road of the leaves, and above all of the petals."

XLIX. But it is time to proceed from mercury to other plants, not less worthy of the attention of the naturalist. In mercury, as well as in basil, we have seen the necessity of pollen to fecundation. The pumpkin or gourd with shield-form fruit, the citron pumpkin, hemp, spinach, exhibited a phenomenon directly contrary: they produced, without the influence of the male dust, seeds capable of perpetuating the species. And in order to be satisfied that the dust had no share in producing the effect, recourse was had to the most vigilant precautions, and the most decisive proofs. The pumpkin with shield-form fruit flowered in a place many miles distant from any plant of the same or a neighbouring species: moreover, the male flowers were all plucked off at their first appearance (xx, xxi). The same precautions were used with respect to the other species of pumpkin, with the addition of a third; the access of the external air to the female flowers, was prevented during all the time, in the course of which they must, in the opinion of botanists, be impregnated by the pollen; for this purpose the blossoms and branches were introduced into glass bottles perfectly close (xxxii). Various precautions were likewise taken with hemp. In the first place,

place, several female individuals were shut up in an apartment, from three weeks before their flowering to the time of the ripening of the seeds. Secondly, some branches were inclosed in bottles for the same space, as in the citron-pumpion. Thirdly, some female stalks were made to flower six weeks before those which grew in the hemp-yards. Lastly, I assured myself, that no male blossom ever grew upon the females (xxvi, xxvii, xxviii, xxix). Precautions of the same kind were observed with respect to spinach; some female plants were raised in a garden, in which there were no males; secondly the pot was completely covered with a glass receiver, when the flowers with pistils were in blossom; thirdly, blossoms with pistils were procured earlier than the usual appearance of blossoms with stamina in the gardens and fields: lastly, I took care to satisfy myself, that none of the females ever produced any male flower (xxx, xxxi, xxxii). One circumstance relative to hemp I must not omit to repeat; the seeds of individuals confined in an apartment, were less numerous and smaller than of those which grow in the open air. Both these defects were still more remarkable on those branches which were introduced into bottles (xxvii, xxviii).

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Are we to suppose, that these defects arose from want of pollen? I am little inclined to admit such a supposition; it is far more probable, that they were owing to confinement in an apartment, and what was still more unfavourable, in bottles: hence the plants, and consequently the seeds could not attain the same degree of perfection as those, which in the fields enjoyed the benefit of fresh and open air, and were subject to the direct influence of the solar rays. My opinion is strongly confirmed by those plants which flowered six weeks earlier than the others, and, notwithstanding the absence of the pollen, produced perfect seeds as large, and in as great abundance as those of the hemp-yards, because they were constantly exposed to the kindly influence of the air and sun (xxix). From these observations on hemp, pumpions, spinach, compared with others made on basil, mercury, palm, turpentine, &c. &c. it may in general be inferred, that if a great number of plants require the powder of the male, in order to fructify; in many others fructification has no dependence on this powder. And although we are as yet acquainted with a few only belonging to the latter class, it will doubtless increase in proportion to the number, application, and fa-

gacity of those who shall employ themselves in cultivating this branch of the Natural History of vegetables.

L. But by affirming, that there are plants which bear productive seeds independently of pollen, I am aware, that I shall incur the displeasure of all modern naturalists and botanists. They will exclaim against me with the utmost indignation, and reproach me with having advanced the most absurd of all possible propositions. I shall be told, that from the days of Cæsalpinus to the present hour, every naturalist of distinction, Grew, Ray, Camerarius, Morland, Geoffroy, Vailant, the Jussieu, Duhamel, Adanson, Bonnet, &c. &c. have admitted the two sexes of plants, and the necessity of both to fecundation---that the prince of modern botanists has consecrated an entire essay to the celebration of the amours of plants, and to the description of their sexual parts---that upon this distinction of sex, as upon a solid and immoveable basis, he has founded the great edifice of his famous *System of Nature*---that in conformity with this theory he defines the blossoms, *the organs of generation in plants, which are subservient to the impregnation of the seeds*--- that in the essay, to which I have referred, he shews, that the dust of the stamina

mina is not diffused, till the stigma of the pistil is in a fit condition to receive its influence---that the pistils are always disposed, with respect to the stamina, in a situation favourable for the reception of the pollen. They will bring to my recollection the proof of the necessity of the pistils and stamina to fecundation, deduced from experiment; for if the stamina or pistils of hermaphrodite flowers be cut away before the time of blowing, fructification will infallibly be prevented; and the same failure is observed whenever the stamina are converted in petals, or the pistils are expanded into little leaves. Finally, they will adduce, in full confirmation of these assertions, the various instances of artificial impregnation, successfully attempted by different naturalists upon various species of plants, by means of pollen. From all these considerations, it would seem no longer to be questioned, that the fecundation of plants, in consequence of the action of pollen, is one of the most universal laws of Nature.

LI. Such are the objections which I seem to hear urged against me by the learned and worthy maintainers of sexualism. I must, however, inform them, that I was not unacquainted with these arguments, when I

made my experiments upon plants. I knew, moreover, that some of them were directly opposite to experiments related by the northern Pliny; as that, for instance, described by him in the following terms. “ Cannabem flores masculos tantum ferentem, si ante divellit rustica quam cannabis feminifera flores pistilliferos non aperuerit, nullam aut sane exiguam portabit feminum copiam (a). I also had in view an assertion, nearly alike, of the illustrious Duhamel, in his *Physique des Arbres*. It has been observed, says he, that an insulated plant of spinach produces but a very little seed capable of germinating. When I first read this writer, I gave full credit to his assertion, for I had no reason to doubt of it; and even quoted it in a note on the *Contemplation de la Nature*, translated by me, and published at Modena, in the year 1770.

These respectable authorities served only to render me more diligent, cautious, and assiduous in my experiments. They had not such influence as to make me adopt an opinion, contrary to that to which facts pointed: nor shall they now hinder me from making, with proper deference, such strictures upon the instances just quoted, as are suggested by the dispassionate love of truth. To begin

(a) Spons Plant.

with the fact mentioned by Linnæus, it does not appear that he made any experiment upon hemp; he seems merely to have adopted the vulgar notion, according to which, if the male hemp be pulled before the flowering of the female, the latter will produce either no seeds at all, or only very few. To shew the falseness of this notion, I have only to advert to my experiments, in which every stalk of hemp proved fruitful, though they were all kept so close, as to be perfectly exempt from the influence of pollen---not to mention the common practice, in the territories of Reggio and Modena, of pulling the male plants at a period, after which the females grow vigorously for several weeks, and bear productive seeds. I intreat the learned reader to call to mind the twenty-fourth paragraph, in which this practice is described; and also to compare the force of my observations on hemp, with the few contrary expressions of the late Upsal Professor, and then to decide. I give the same answer to Duhamel's intimation concerning spinach. If that illustrious naturalist had deduced the barrenness of this plant when insulated, from experiment of his own, I would have discussed them, and have rendered them all the justice they would have deserved; but he barely mentions the com-

mon supposition, which is contradicted by the facts adduced by me.

With respect to the diffusion of the pollen, which was the first of the three arguments in favour of the sexual system, only at a time when the stigma is in a fit condition to receive it; and with respect to the favourable position of the stamina and pistil, the philosopher will immediately perceive, that these are by no means direct proofs of the sexual system, but mere considerations relative to convenience, calculated, if I may employ the expressions of the great Verulam, rather to allure, than to extort assent.

LII. Of the second argument, deduced from the failure of fructification, when the stamina or pistils are either cut away or degenerate, I must take the liberty to observe, that such instances do not prove that these parts are the organs of generation, but merely that they have some concern in this function, as well as many other parts, which yet have never been suspected to be appropriated to generation. The only facts which clearly evince the distinction of sexes in plants, are instances of the failure of fructification in females placed at a distance from males, and of artificial fecundation, which was the third argument adduced. But shall we conclude,
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with these worthy naturalists, that such facts, of which so few are known, in comparison with the immense multitude of plants, are sufficient to establish an universal distinction of sexes? Is it not, on the contrary, evident, that they have deduced a general conclusion from particular premises? They ought at most to have said no more than that, considering the close analogy which plants of different classes, genera, and species, bear to one another in their properties, and that as a distinction of sexes has been demonstrated in some individuals, it probably extends to others. Thus, from my discovery of the fetus's pre-existence in the female of different amphibious animals, and from a like observation made on birds by the great physiologist of Berne, I supposed that I might justly infer, that the same pre-existence extends to other animals. After I had found, that the embryos of several plants exist before the appearance of the flowers (XLI), I made the same inference with respect to all others. But the favourers of the sexual system are not warranted, by so inconsiderable a number of facts, to establish it as an universal law, as others have done, besides Linnæus, who, without limitation, defines flowers, *the organs of generation of plants, which are subservient*

to the fecundation of the seeds. Hence he attributes to plants as many males, or husbands, as there are stamina. But before he so decisively determined the number of husbands, he ought to have been sure that they perform the office. If this far-famed naturalist had sacrificed less to nomenclature, and more to the spirit of investigation, he would have been led to study the parts of flowers more philosophically and more profoundly; hence he would have been better enabled to ascertain the truth or falsehood of his system: but he was in too great haste to erect his own system upon the ruin of Tournefort's and his eagerness has not, perhaps, been highly advantageous to the philosophy of botany.

LIII. Among various particulars concerning the generation of plants, which I communicated to Mr. Bonnet, in a letter dated October the 18th, 1777, and alluded to in a note, at paragraph xxv, I mentioned some experiments, which were then indeed imperfect, but seemed to shew, that in some plants the fecundity of the seeds was totally independent of the powder of the stamina. Hence I concluded, that botanists in general had been guilty of a species of sophism. Although what I then wrote to my illustrious
friend

friend was but a sketch of those experiments which I afterwards undertook, yet he accepted it with complacency, and admitted my conclusion, notwithstanding his opinion was before different. In his answer, dated November the 29th, the same year, he set forth the reasons why he had adopted the sexual system, and subjoined the following acknowledgment, which amounts to a panegyric on his candour. “ I am, however, sufficiently convinced by your experiments, that all the great naturalists whom I have enumerated, as well as myself, were deceived. We had all formed a hasty decision, and drawn a general conclusion from particular premises. We had deduced the necessity of the pollen in fecundation, from experiments executed on different species of plants; whereas we ought only to have said, that it seems to follow, from these experiments, that in such species the influence of the pollen appears to be necessary to fecundation.”

LIV. Such indeed is the fatal rock on which the genius of system-makers usually splits.---They illogically deduce general propositions from particular proofs. Finding that certain parts are common to a number of plants, and that in them they perform
particular

particular functions, and have particular properties, they determine, with wonderful facility, that the same things take place in all other plants: and on these plants performing such functions, and endowed with such properties, they found their system of botany, without perceiving, that before the system can have such an unlimited generality as they pretend, they ought to be acquainted with all the plants of the earth. But how many are yet unknown? How many more than are known? With what propriety then can the whole vegetable kingdom be comprehended under one law? Does the organic world afford a single law, which can be called universal? Have not the laws, which hastily and imprudently were established as universal, been found not to be really such? The learned botanist, Necker, in his *Physiology of Mosses* (a), ably points out this defect of system-makers, in which he distinguishes them from observers; not indeed with great purity of style, yet with the utmost justness of sentiment. “Alterum systematicum, alterum observatorem distinguimus. Ille non nisi quibusdam plantarum speciebus universa stabilit systemata: à particulari ad universale

(a) *Physiologia Muscorum*, Manhemii, 1774.

concludit,

concludit, i. e. omnibus globi terraquei vegetabilibus easdem proprietates ac iis quæ experimentis explorata sunt, tribuit. Observator omnia theoretica rejicit systemata, solis observationibus necnon experimentis innixus, Naturam perscrutans: perfectio botanices ab individuorum singulorum inter se affinium eorumque identicorum characterum notitia essentialiter pendet. Ea proportione notitia hæc acquiretur, qua observatorum numerus, qui valde exiguus, augebitur systematicorumque cumulus minuetur. Certum indubitatumque quod systematicum ingenium præcipua causa sit cur de modico perfectu botanices dolemus. Systemata botanica cum tempore exolefcunt, quia Natura ac experientia potissimum non nituntur."

Lv. That Nature should follow a different process in different plants---that some should require the influence of pollen in order to multiply the species, while it is not necessary to others, is perfectly conformable to what we every day observe in animals to which they are so analogous. Numberless animals are incapable of multiplying, without the concurrence of both sexes, or, at least, without the intervention of that liquor, upon which depends the perpetuity of the species; as is evident in man, quadrupeds, birds, fishes,

fishes, reptiles, and insects. But a vast number, on the contrary, propagate without such means; as for instance, the POLYPES; under which denomination is included an immense variety of small animals, inhabiting the bottom both of the fresh water, of ditches, ponds, puddles, and of the salt water of the sea. To these may be added, the lice of plants, a most comprehensive class; whole armies dwelling on a single individual. The animalcules of infusions, moreover, agree with the lice of plants and polypes, in the mode of propagating the species. I have shewn, in my *first essay*, relative to the *natural history of vegetables and animals*, that many species are multiplied by a natural division of the body, some splitting into two portions, some into four, some into six, and others into eight, &c.---that some are oviparous, others viviparous, and that all are strictly hermaphrodites, each individual propagating its kind, without needing the concurrence of another. Thus it may, and really does come to pass in some plants, which multiply without the influence of the fecundating dust.

LVI. One objection, however, may be started, and it would be inconsistent with my impartiality

impartiality to suppress it. It may be urged, by the advocates of the sexual system, that although at the time I was making my experiments upon the pumpkin with shield-form fruit, the citron pumpkin, spinach, and hemp, I was perfectly certain that the pollen had no access, yet it might previously have impregnated the seeds. It may be said, that the female individuals might have been fecundated several years before, and that by this single act, not only the seeds of the current year, but those of several successive generations, might have been fecundated.

This objection is only a repetition of what was modestly suggested by the illustrious Trembley, to Mr. Bonnet, when he made his celebrated discovery of the hermaphroditism of the lice of plants. *Who knows, said that naturalist, so cautious and deliberate in his decisions, but that one copulation may serve for many generations (a)?* Although my excellent colleague acknowledges the question to be altogether gratuitous, yet, as it was put by a Trembley, he was anxious to repeat his observations; and he justly concludes, that the suspicion is entirely removed by the production of ten successive generations with-

(a) Bonnet Corps Organ. p. 311. Ed. in 4to.

out copulation (*b*). With respect to my plants, I must frankly acknowledge, that I did not obtain from any of them more than three generations without the intervention of pollen. Nor did the nature of the thing allow me to satisfy my curiosity so soon as Mr. Bonnet, for he, in the short space of three months, observed ten successive generations; whereas not less than ten years must have passed, before I could have obtained an equal number in the subjects of my experiments. I have not, therefore, been able to derive sufficient information from this source; yet I can adduce considerations weighty enough to overturn the objection. In the first place, all Nature does not afford a single instance, wherein one act of fecundation serves for several generations. The only case, concerning which any such suspicion has arisen, has been abundantly cleared from it. Secondly, we have instances of many successive generations, without copulation or fecundation, as in the polype with arms.

(*b*) Notwithstanding Mr. Bonnet has discovered and published this fact so many years ago, and notwithstanding it has met with no opposition from any observer, Linnæus advances, in his *Systema Naturæ*, and in the last edition of 1767 repeats, this scandalous proposition. “A copula parentum fecundas nasci filias, neptes, proneptes, abneptes asseverant entomologi.”

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From the body of this species sprout small polypes, which, during their evolution, produce other smaller polypes, and these again others still smaller and so on: hence we have at last a series of generations, growing one out of another, and all supported by the mother polype, of which we are certain, that she has never had any sort of commerce with her fellows; because she was torn, while yet very small, from the body of another, and ever afterwards kept in the most perfect solitude (*a*). Since, therefore, we observe so many successions in animals, independently of the action of any seminal principle, why may not, or rather why must not, the same thing take place in some kinds of plants? We have no fact which proves a number of successive generations in any organized body, to be the effect or the result of an antecedent act of fecundation.

LVII. Although the many facts which I have related, compel me to reject the action of the powder of the males in several plants, yet I dare not altogether deny the possibility of fecundation. What I mean to insinuate is, that in the course of my frequent meditations upon this important subject, a doubt

(*a*) Trembley, Polypes d'eau douce.

has arisen in my mind, whether the seeds in the ovarium may not be fecundated by some feminal principle residing in the pistil. I have frequently seen upon the stigma of certain plants, a kind of powder exceedingly like that of the stamina, though I had at the same time clear proofs, that the latter had not yet burst from the antheræ. But I did not, I must own, make any careful observations on this subject. My surmise was however strengthened by reading in Kölreuter, not without surprize, that the properties of the feminal powder belonging to the stamina, are exactly the same as those of the matter furnished by the pistil. Now I have sometimes doubted, whether the dust of the stigma may not produce the same effects on some plants, as that of the stamina on others. But this is a mere suspicion, which it would give me great pleasure to see confirmed or destroyed by some able observer. And as I am speaking of the stigma, let me add, that I wish this organ were more carefully examined, than it has ever yet been; great attention should be paid, in order to determine whether it is imperforated or not in some plants (XLVI, XLVII). Such researches, united with others relative to the stamina, would contribute to dissipate the mist which covers

covers the fecundation of plants. As I have derived great advantage in my attempts to illustrate this function in animals, from artificial fecundation. I cannot but again recommend the same means; nor do I doubt, but that they will throw as much light upon plants as upon animals.

LVIII. By these exhortations, suggested by an unfeigned desire of attaining truth, and extending the limits of botannical knowledge, I wish to induce naturalists to study in a more philosophical manner the natural history of plants. The ligneous species have, it is true, been much illustrated by the profound enquiries of Grew, Malpighi, Hales, Duhamel, and Bonnet; but the herbaceous kinds so much exceeding the others in number, and so deeply interesting to mankind, on account of the sustenance they afford, have been hitherto almost totally neglected. We have at most a bare nomenclature, in which their different parts are described according to the system adopted by the author. I do not on this account condemn nomenclatures; the necessity of them is evident, since it is impossible to enter upon a rational investigation of plants before we are acquainted with them; and we cannot be acquainted

with them without the science of names (a). I observe only, that nomenclature gliding along the surface of things, is incapable of furnishing such information, as will satisfy the curiosity of the profound enquirer, and advance our knowledge of the vegetable kingdom. The bodies of Nature are not simple, but exceedingly complex; Muschenbrock justly compares them to a clock, consisting of many wheels, and enclosed in a case, which prevents us from obtaining a view of the size and mutual dependency of the wheels, and the various actions of the springs. To see, therefore, the internal parts, and understand them properly, it becomes necessary to open the case: we must do the same with respect to natural bodies, and not be contented with barely inspecting the outside, but endeavour to penetrate further, and investigate the wonders lying with-

(a) Besides being obliged to learn the nomenclature, (whence it may be called the grammar of natural history, and those who teach it the grammarians) we are under the necessity of using it when we write, if we wish to be generally understood. Thus, in treating of plants and animals, unless they happen to be the most common of all, it would appear, that at present we cannot do without Linnæus. If, at least, we make use of the names in his *Systema Natura*, whatever glaring defects that work may contain, we are sure of being understood by all Europe. I said *plants* and *animals*; for *fossils* we use Cronstedt and Wallerius, as the classical authors.

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in. When the first mineralogists characterised the productions of the fossil kingdom, by difference in colour, in transparency, opacity, by roughness or smoothness, by their granulated or fibrous structure, &c. they held out only superficial, vague, and, for the most part, fallacious notions. Before they could be properly understood, it was necessary that chemistry should search these productions by its analysis. What this science has done for fossils, anatomy has performed for animals. We cannot but acknowledge the greatest obligations to Swammerdam, Redi, Vallisneri, Reaumur, Lyonet, Daubenton, and, above all, in the class of insects, to the immortal Malpighi, whose dissertation on the silk-worm is a tissue of discoveries, and may be justly considered as alone superior to all the nomenclatures of insects hitherto published. I could wish that the same attention, which so many able naturalists have bestowed upon researches concerning fossils and animals, was paid to herbaceous plants. Their œconomy, the principal object of natural history, ought, above all, to be the aim of our enquiries. But it will be impossible to fulfil such an intention, till a careful examination has been made, both of their internal and external struc-

ture (*a*). I am aware, that researches of this nature require quickness of apprehension, pertinacity of attention, fertility of resources, exactness of judgment, the utmost vigilance in noticing the phænomena, and no less sagacity in distinguishing them; these requisites do not seem indispensably necessary in the learned nomenclator, who is generally conspicuous chiefly for memory; and this is the true reason why, at a time when we are rich in nomenclators, we are poor, nay beggars, in observers. But it is only by such laudable exertions, and such valuable accomplishments, that we can hope to advance any farther in this noble science, and to increase the stores of useful knowledge. I cannot, therefore, but set an higher value upon the genius

(*a*) Those who, from a desire to serve the public, and to contribute to the progress of human knowledge, shall apply to the study of plants, and indeed of minerals and animals also, would do well, in my opinion, to follow the example of Erxleben, a late celebrated academician of Gottingen. Mr. Kœstner, President of that respectable academy, draws, in his elogy, the following exemplary character of him, and subjoins a very just reflection. “*Historiam Naturalem, quam vocant, judicabat (Erxleben) animalium, plantarum, fossilium, non catalogum esse, sed physicam. Itaque horum corporum structuram, analysim, proprietates, usus docuit, non neglectis tamen notis quarum ope velut in indices possent referri. Quos indices solos qui memoriæ mandant aut ad evolvendos illos manus habent exercitatas, frustra sibi persuadent, librum ipsum Naturæ se tenere.*”

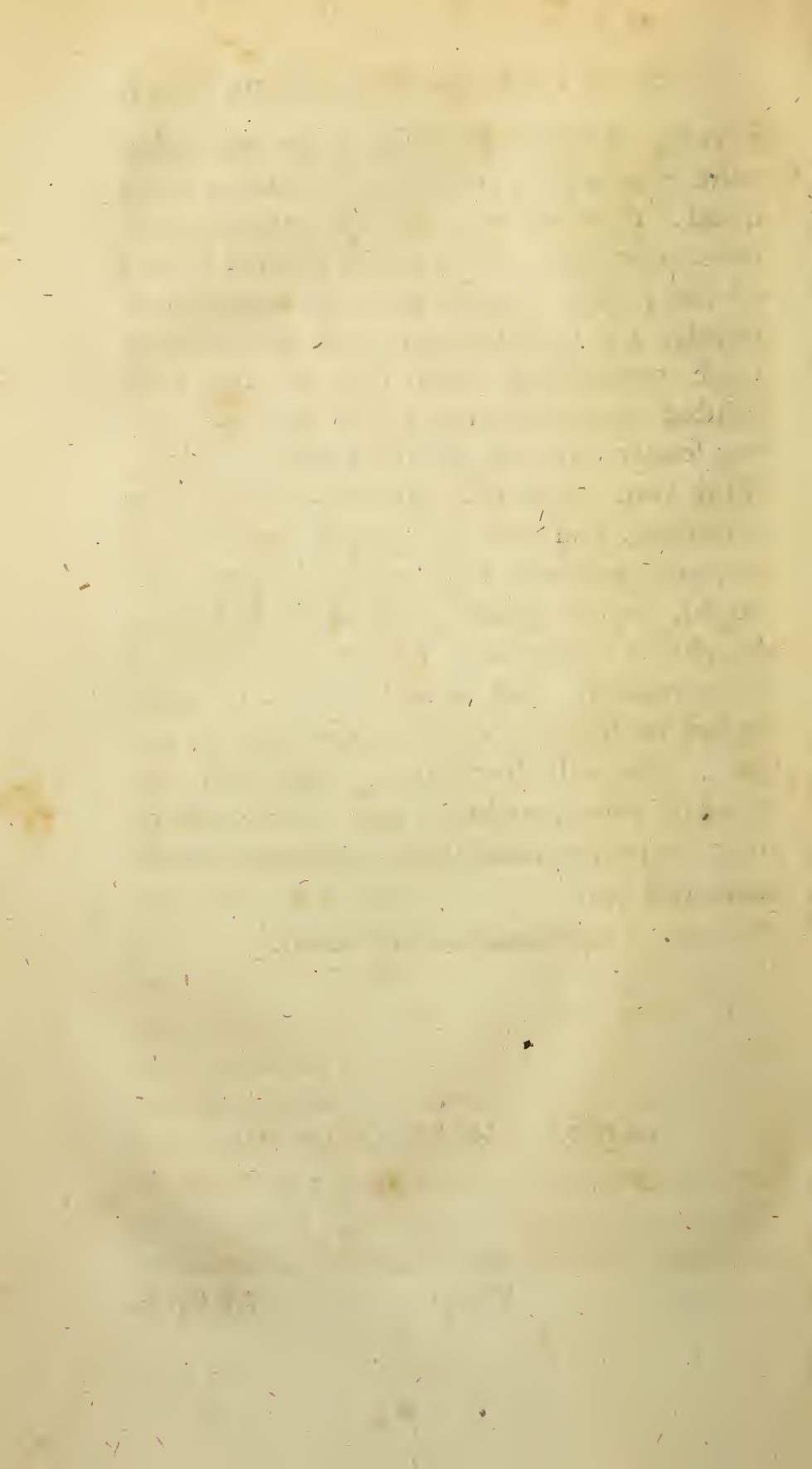
of those naturalists, who confine their attention to some part of natural history not yet sufficiently elucidated, than upon the art of those, who only employ themselves about nomenclatures; and I have the pleasure of agreeing in opinion with those great men, whose most earnest wish is the greatest possible improvement of science, and who, while they recommend it, with patriotic zeal, to others, advance it themselves in their immortal productions. Not that these illustrious writers despise nomenclature, which is so necessary, that it may be called the key of the three kingdoms; but they consider the pursuits of nomenclators as not of the most useful tendency, because the desire of comprehending, in voluminous nomenclatures, like so many Briareuses, the whole of Nature, as more than one have attempted, seems to them equally presumptuous and puerile. If Men would go about to discover things, and not words; if they would investigate Nature, and bring to light some part of her inexhaustible stores, they would render far greater service to the science which they cultivate, and deserve better of the republic of letters. Lest this representation should appear exaggerated, I will adduce, from among many respectable authorities which

are at hand to confirm it, those of the incomparable d'Alembert, and of the sublime contemplator of Nature. "We by no means wish to resemble that tribe of naturalists, who have been so justly censured by a modern philosopher; incessantly employed in dividing the productions of Nature into genera and species, they spend in such labours the time which would much better be dedicated to the study of their properties." Such are the expressions of the former in that most masterly production, the *Preliminary Dissertation to the Encyclopædia*. I shall conclude with the words of the latter. "What ought we to think of those pompous *nomenclatures*, which are arrogantly offered to us as the *System of Nature*? They immediately suggest the idea of a scholar, who attempts to form the index of a large folio, after having read only the title and a few of the first pages. And are we in possession of even the first pages of the book of Nature? How many passages do they contain which we do not understand, and of which the hidden meaning probably contains interesting truths? I wish not to extirpate nomenclators; they strive to give our knowledge some arrangement; but I will venture to say, that a mere nomenclator will never make any great discovery.

covery. I will add, that I set an higher value upon a good treatise concerning a single insect, than upon a whole entomological nomenclature.---Study the admirable history of the polype, peruse the fine memoirs on insects, and then compare the usefulness of these productions with that of the most boasted nomenclatures. Of which would you sooner chuse to be the author? Which, think you, imply most sagacity, genius, and invention, and have the greatest tendency to improve anatomy and natural history? We ought, in my opinion, not to be so anxious to make a catalogue of human knowledge, as to increase it. Let us collect more materials before we think of erecting a temple to Nature. She will else refuse to dwell in it. Instead of corresponding to her majesty, it will only be proportional to the meanness of the architect (a).”

(a) Contemplation de la Nature.

END OF DISSERTATION III.



A P P E N D I X.

AN ABRIDGEMENT OF MR. DEMOUR'S OBSERVATION IS TO BE FOUND IN THE FRENCH MEMOIRS FOR 1741, p. 28. IN THE MEM. FOR 1778, PRINTED IN 1781, p. 13, WE FIND THE ACCOUNT IN HIS OWN WORDS. THE TITLE IS, "OBSERVATION DE DEUX ANIMAUX, DONT LE MALE ACCOUCHE LA FEMELLE.

THE eulogies of Naturalists upon the industry of animals are boundless. It has been pretended, that man is indebted to them for the useful arts; and some are extravagant enough to affirm, that even the most abstracted sciences have been borrowed from brutes (*a*). According to Pliny and several other naturalists, the idea of bleeding was suggested to physicians by the hippopotamus; and that of injection by the stork, which, on account of the great services it rendered them, was formerly worshipped by

(*a*) *Giovanni Bonifacio*, l'arti liberali e mechaniche, come siano state dagli animali irrationali, agli Huomini dimostrate.

the Egyptians. The bear and the dog pointed out the advantage of emetics; the former by swallowing ants, the latter by eating the leaves of dog's grass. Happily for medicine, the fact recorded in this Memoir, was unknown to these naturalists, otherwise they would not have failed to ascribe the origin of the art of midwifery to the animal in question.

One evening during the dog-days, I observed near some steps by the great basin in the king's garden, in which I at that time occupied the office of demonstrator, and keeper of the cabinet of Natural History, two land toads of the smaller species (*a*) in the act of copulation. I saw that the male frequently moved his hind legs; a desire to discover the cause of these movements led me towards the place, where I was surprized by two phænomena, which I believe have not been hitherto noticed. The first was the difficulty with which the female brought forth her eggs, which was such that she absolutely required foreign aid, what no other animal with which we are acquainted does. The second phænomenon was the efforts made by the male to extract the eggs with his hind

(*a*) *Rubeta minor*. Gesneri.

feet, and his thus performing the office of an accoucheur.

I could not conceal my satisfaction at a spectacle so uncommon. My attention redoubled, and I seated myself quietly on the ground, in order to observe more accurately; and especially to watch whether the male impregnated the eggs as he extracted them.

To understand the operation completely, it must be remarked, first, that the fore as well as hind feet of these animals are divided into several toes, by the help of which the male draws the eggs out of the vent of the female.

Secondly, that these animals copulate like frogs, i. e. the male gets upon the female's back, and embraces her with his fore feet. In the frog the feet are long enough to clasp the female completely, whereas in the toad they reach only to the side of the breast, against which the animal presses with such force, that an ecchymosis often takes place before he quits his situation.

I must observe in the third place, that the eggs of this species of toad consist of a strong membranous cover, within which the embryo is contained. The eggs are about two lines long, and of an oblong shape; they are
connected

connected by a short and strong cord. A pretty just idea of these eggs may be conveyed, by comparing them to a necklace, of which the beads lie at about half their own length from each other. They are discharged at the vent, because the receptacle in which they were contained, opens into the lower part of the rectum.

There is reason to suppose, that the first egg is expelled by the exertions of the female, and that the male extracts the rest. He performs his office with an address that one would not expect in an animal naturally so awkward. When his motions drew my attention, he had extracted the second, and was endeavouring to extract the succeeding egg. The first was fixed between the toes of the right foot, by the filament which connects it with the second, and it was by stretching out the foot, that he drew them from the anus of the female, which continued motionless during the whole time. He tried to lay hold of the cord with his left foot, and made several attempts before he succeeded. My presence undoubtedly sometimes drew off his attention: for he would now and then stop suddenly, and look at me with an air expressive either of inquietude or fear:

fear: then he would resume his employment with fresh alacrity, and soon relax again so much in his ardour, as to seem irresolute whether he should proceed or not. The female also testified her disquiet by movements that disturbed the operations of the male, but she never seemed desirous of returning to the station under the stone which she had quitted.

At last, whether from my silence and immobility, or from the urgency of the case, the female became still, and the male resumed his employment: he soon laid hold of the cord with his left toes, and now drew it out with both feet, by extending them gently. When he had stretched them out as far as he could, he withdrew the left foot, without quitting the eggs that adhered to it, and continued to extract with the right alone. But here his difficulties recurred a second time. The portion of cord that lay between the middle fingers of the right foot, often prevented him from getting hold of a new part of the cord with that foot. He made several efforts without success, and stopped of a sudden oftener than once. I was afraid, that the movements of my head were the cause of these interruptions, and I then sat motionless,

tionless, and held in my breath. Sometimes I was inclined to attribute them to the difficulty of the operation itself; this tempted me to assist the animal, but I forbore, for fear of breaking off the process. My attention had hitherto been divided between two objects; while, on the one hand, I admired the dexterity of the male, I watched on the other, whether he bedewed the eggs with semen as he extracted them, but as yet I could not see any appearance like this, though it seemed absolutely necessary to the fecundation of the eggs, and was, at the same time, the chief object of my curiosity: I thought the want of light might prevent me from seeing it, and therefore, at all hazards, set them on my hand, that I might inspect them more closely.

This change of situation put a stop to the procedure of the male for a few moments; but the urgent necessity for delivering the female, doubtless made him collect courage to resume his employment. I now tried only to observe, whether the male impregnated the eggs as he drew them out; but notwithstanding all my solicitude to see a phænomenon, of which the water-newt had before furnished me with an example,

example (a), I could not perceive any thing like what I expected. The evening now closed in, and after observing them for about half an hour, I was forced to set them down in the place from which I had taken them.

My meeting with these animals was one of those fortunate occurrences, of which a naturalist alone can estimate the value. It afforded me the view of a fact, singular and hitherto I believe unnoticed. The assistance yielded by the male I saw distinctly; but it was quite otherwise with respect to the other part of the observation. I could not, with all my attention, perceive any thing like what Swammerdam saw in frogs, viz. that after forty days of coition the male impregnates the eggs as the female discharges them, by bedewing them with semen.

Though I could not perceive this irrotation, yet there is reason to suppose, not only that it is necessary for the fecundation of this species of fry, but also that it takes place in

(a) The female newt is impregnated without coition. The male keeps at about the distance of an inch above the female, and throws out the seminal liquor upon her sides, which renders the water a little turbid. This observation is to be found at the end of the first volume of my translation of the Edinburgh Essays.

the same manner as in the frog. The exact conformity between the structure of the internal parts of these animals, between their mode of coition and their embryos, which in each kind pass through the state of tadpole before they attain the form of frog or toad, strongly confirms this conjecture. I should therefore suppose, that the male performs this function at some other time. He probably, as well as the frog (*a*), bedews all the eggs at once, which he can not accomplish when he has them entangled about his feet. In truth, the waste would be too great if he impregnated them successively, and in this case a great quantity of semen would fall on the ground, and my hand, upon which the male operated for about a quarter of an hour, must have received some.

This observation apprized me of a mistake I had formerly committed. During the two last years of M. Duverney's life, I assisted him in his anatomical labours; and I had once occasion to seek by candle-light for newts, which the heat and drought of the season confined in their places of retreat in the day-time. Near a reservoir, consisting

(*a*) How far this supposition is to be admitted, the reader may learn from the first of the preceding dissertations. T.

of a barrel that projected eight or ten inches above the surface of the ground, I perceived a toad of the species above-mentioned, with a bundle of eggs on his back. The animal traversed round the barrel to no purpose; the edge was too high for him to discharge his burden into the water. I took it up with an intention to relieve its distress, by throwing it into the water; in an instant I felt something move in my hand, and upon bringing the light near I perceived several little tadpoles just come out of their envelope; they were very lively, and as full formed as tadpoles of frogs that have lost their tail, and which consequently have quitted their cover fifteen days. The cord of eggs was so entangled in the hind feet, that the animal could make but very short steps. At that time I supposed it to be the female, but the observation which I have now related proves, that it must have been the male which is charged with the cares that attend incubation. Nature, in the present instance, has divided the toil that relates to the progeny equally between the two sexes. The female has the burthen of the eggs before delivery, and likewise supports the male upon her back, probably for forty days, as

in the frog and water toad. The male, in his turn, delivers the female, and carries the eggs on his back till the time of the exclusion of the tadpoles, which happens sooner or later, according to the season, and he then deposits them in a proper place: He throws himself into the first water he meets, and is frequently the victim of paternal attention. If he gets into a place whence he cannot easily find his way out, he perishes in a few days, as I have seen in many instances. This species, which Ol. Jacobæus considers as the *Rana phrynoides* of Gesner, and which some authors call *Rubeta minor*, in order to distinguish it from the common toad, which they denominate *Rubeta major*, is incapable of living in water after he has once quitted it. He passes the former part of his life in water, and the latter upon dry land. This, according to Gesner, was known to the ancient naturalists, who assert the same thing of the common frog.

The species here mentioned quits his envelope in the form of a tadpole without a tail; at this time it has gills, and lives in water like fishes: there it continues till the gills begin to be effaced, which happens as soon

foon as the fore-feet burft through the membrane which contained them : when the animal can walk or jump it feeks a different element, and quits the water before its tail is entirely effaced.

DISCOVERIES ON THE SEX OF BEES, EXPLAINING THE MANNER IN WHICH THEIR SPECIES IS PROPAGATED; WITH AN ACCOUNT OF THE UTILITY THAT MAY BE DERIVED FROM THOSE DISCOVERIES BY THE ACTUAL APPLICATION OF THEM TO PRACTICE. BY MR. JOHN DEBRAW, APOTHECARY TO ADDENBROOK'S HOSPITAL AT CAMBRIDGE, AND MEMBER OF AN OECONOMICAL SOCIETY IN THE PRINCIPALITY OF LIEGE, IN WESTPHALIA. COMMUNICATED BY THE REV. NEVIL MASKELYNE, B. D. F. R. S. AND ASTRONOMER ROYAL.

READ NOV. 21, 1776.

THE republic of bees has at all times gained universal esteem and admiration: their culture, an object so worthy of our attention, has attracted and still does engage that of many of the learned, and has arrived at a considerable degree of improvement of late years; but their mode of propagating their species, seems to this day to have

have baffled the ingenuity of ages in their attempts to discover it. The most skilful naturalists have been strangely misled in their opinion, that the bees, as well as the other tribes of animals, are perpetuated by copulation; though they acknowledge that they have never been able to detect them in the act.

Pliny, who was likewise of the same opinion, that in this particular they do not differ from other animals, observes, “*Apium coitus visus est nunquam.*” Swammerdam, that sagacious observer, having never been able to discover it, entertained a notion, that the female or queen bee was fecundated without copulation; that it was sufficient for her to be near the males; that a vivifying *aura*, exhaling from the body of the males, and absorbed by the female, might impregnate her eggs. At last the incomparable Reaumur thought he had in a great measure removed the veil, and brought their manner of generating nearly to a proof. This part of physics has been the principal object of my researches for several years past, having been insensibly engaged in it by the pleasure I took in so curious an enquiry; and although this pursuit has been attended with more difficulties and embarrassments than can be well

imagined, I have not been discouraged, and have carefully avoided launching into conjectures. To introduce a new system in the doctrine of bees, which in a great measure contradicts all former received opinions, requires, previous to its appearance, every sanction the various experiments, successfully repeated, can possibly give it. The results of those experiments, made in all glass-hives, which carry with them an intire evidence, afford sufficient reasons to assert, that bees belong to that class of animals among which, although they have sexes, a true copulation cannot be proved; and that their *ova*, like the spawn of fishes, most probably owe their fecundation to an impregnation from the males, as will appear in the sequel of this narrative.

I am not a little pleased to find, that the celebrated Maraldi had such a notion, and I lament his neglecting to confirm it. He says, in his *Observations upon Bees*, in the History of the Academy of Sciences for the year 1712, p. 332: “ Nous n’avons pû découvrir jusqu’ à present de quelle maniere se faite cette fécondation, si c’est dans le corps de la femelle, ou bien si c’est à la maniere des poissons, après que la femelle, a posé ses œufs: la matiere blanchâtre dont l’œuf est environné

ronné au fond de l'alvéole peu de temps après sa naissance, semble conforme à la dernière opinion, aussi-bien que les remarques faites plusieurs fois d'un grand nombre d'œufs qui sont restés inféconds au fond de l'alvéole autour desquels nous n'avons point vû cette matiere."---" We never yet were able to discover in what manner this fecundation is performed; whether it is in the body of the female, or whether it is after the manner of fishes, after the female or queen-bee has deposited her eggs: that liquid whitish substance, with which each egg is surrounded at the bottom of the cell, a little while after its being laid, seemingly establishing this last opinion, as well as the frequent remarks made of a great number of eggs remaining barren in the cell, round which we could not see the above-mentioned whitish substance."

This ingenious naturalist, by a nice examination of the structure of the drones, had, as well as Swammerdam, discovered some resemblances to the male organs of generation; and from thence conjectured, they were the males of the bee-insect; but he owns, with the rest, that he never could discover them in the act of copulation.

Having stood the trials of so many prying eyes in every age, the bees, as has been observed by an ingenious author, had gained the character of an inviolable chastity, till Reaumur blasted their reputation. He makes the queen no better than a Messalina (a); though he could see no more than what would raise a mere jealousy or generate suspicions.

In order to be the better understood in the relation of my own experiments on the fecundation of bees, I here premise the outlines of the opinions adopted by the above-mentioned naturalists on that head. They assert, that the queen is the only female in the hive, and the mother of the next generation; that the drones are the males by which she is fecundated; and that the working bees, or bees that collect wax on the flowers, that knead it, and form from it the combs and cells which they afterwards fill with honey, are of neither sex.

But of late Mr. Schirach, a German naturalist, has given us a very different view of the classes that constitute the republic of bees, in an ingenious publication in his own language, under the title of *The Natural*

(a) Vid. Juvenal, Sat. vi. ver. 128.

History of the Queen of the Bees, which has been since translated into French; an account of which has been given in the Monthly Review, from which I beg leave to relate the author's doctrine with regard to the working-bees only; the quality and functions of the drones being points which do not appear to be yet settled by Mr. Schirach himself. He affirms, that all the common bees are females in disguise, in which the organs that distinguish the sex, and particularly the *ovaria*, are obliterated, or at least, through their excessive minuteness, have not yet been observed: that every one of those bees, in the earlier period of its existence, is capable of becoming a queen-bee, if the whole community should think proper to nurse it in a particular manner, and raise it to that rank. In short, that the queen-bee lays only two kinds of eggs, viz. those that are to produce the drones, and those from which the working-bees are to proceed.

The trials made by Mr. Schirach seem to evince the truth of his conclusions in the most satisfactory manner, singular as they appear to be at first sight; and indeed in my own judgment, from the constant happy result of numerous experiments, which I began near two years before Mr. Schirach's publication,

publication, and repeated every season since, I am enabled to pronounce on their reality.

Chance, I own, befriended me in that discovery, whilst I was most anxiously endeavouring to ascertain the use of drones. It was in the spring of the year 1770, that I, for the first, discovered what Maraldi had only conjectured, I mean the impregnation of the eggs by the males; and that I was made acquainted with the difference of size in the drones or males, observed by Maraldi in his *Observations upon Bees*, inserted in the History of the Royal Academy of Sciences for the year 1712, p. 333, in these words:

“ Nous avons trouvé depuis peu une grande quantité de bourdons, beaucoup plus petits que ceux que nous avons remarqué auparavant, & qui ne surpassent point la grandeur des petites abeilles; de sorte qu’il n’auroit pas été aisé de les distinguer dans cette ruche des abeilles ordinaires, sans le grand nombre que nous y en avons trouvé. Il se pourroit bien faire que dans les ruches où l’on n’a pas trouvé de gros bourdons, il y en eût de ces petits, & qu’ils y aient été confondus avec le reste des abeilles, lorsque nous ne savions pas encore qu’il y en eût de cette taille.”---“ We have of late found a great quantity of drones, much smaller than those

we

we had formerly observed, and which do not exceed in size the common bees; so that it would not have been easy to distinguish them in that hive from the common bees, had not the quantity of them been very considerable. It might certainly have happened, that in those hives, where we have not been able to discover large drones, there were a great number of those little ones, which may have been intermixed among common bees, when we were yet ignorant that any such small drones were existing."

Reaumur himself, p. 591, of his *Natural History of Insects*, says, "We have likewise found drones that were no bigger than the common bees."

They have notwithstanding escaped the observation of Mr. Schirach, and of his friend Mr. Hattorf, member of an academy in Lusatia, who, in a memoir he presented in the year 1769, annihilates entirely the use of drones in a hive; and advances this singular opinion, that the queen-bee of a hive lays eggs which produces young ones, without having any communication with the drones. For what purpose should wise Nature then have furnished the drones with that large quantity of seminal liquor? To what use so large

large an apparatus of fecundating organs, so well described by Reaumur and Maraldi?

But I beg leave to remark, that those gentlemen seem to have drawn too hasty conclusions from their experiments, in rejecting the drones as bearing no share in the propagation of those insects. Their observations, that hives are peopled at a time of the year when there are no drones in being, is no ways conclusive; as it is evident, that they had seen none but drones of a large size, their silence on the difference in the size of them justifying my remark. But to resume the narrative of my experiments: I had watched my glass-hives (*a*) with indefatigable attention from the moment the bees, among which I had taken care to leave a large number of drones, were put into them, to the time of the queen laying her eggs, which generally happens the fourth or fifth day. I observed the first or second day (always before the third) from the time the eggs are placed in the cells, that a great number of bees, fastening themselves to one another, hung down in the form of a curtain, from the top to the bottom of the hive, in a similar manner they had done before at the time the queen

(*a*) Glass-hives were used in preference to boxes, for a purpose too obvious to need explaining.

deposited

deposited her eggs; an operation which (if we may conjecture at the instincts of insects) seems contrived to hide what is transacting: be that as it will, it answered the purpose of informing me that something was going forward. In fact, I presently after perceived several bees, the size of which, through this thick veil, (if I may so express myself) I could not rightly distinguish, inserting the posterior part of their bodies each into a cell, and sinking into it, where they continued but a little while. After they had retired, I saw plainly with the naked eye a small quantity of a whitish liquor left in the angle of the basis of each cell, containing an egg: it was less liquid than honey, and had no sweet taste at all. Within a day after, I found this liquor absorbed into the embryo, which on the fourth day is converted into a small worm, to which the working-bees bring a little honey for nourishment, during the first eight or ten days after its birth. After that time they cease to feed them; for they shut up the cells, where these embryos continue inclosed for ten days more, during which time they undergo various changes too tedious here to describe.

To evince the reality of this observation, and to prove that the eggs are fecundated by
the

the males, and that their presence is necessary at the time of breeding, I proceeded to the next experiments. They consisted in leaving in a hive the queen with only the common bees, without any drones, to see whether the eggs she laid would be prolific. I accordingly took a swarm, shook all the bees into a tub of water, and left them in it till they were quite senseless, which gave me an opportunity to distinguish the drones without any danger of being stung. After I had recovered the working-bees and their queen from the state they were in, by spreading them on brown paper in the sun, I replaced them in a glass-hive, where they soon began to work as usual: the queen laid eggs, which I little suspected to be impregnated, as I thought I had separated all the drones or males, and therefore omitted watching the bees; but at the end of twenty days (the usual time of their hatching) I found to my surprize some of the eggs hatched into bees, others withered away, and several of them covered with honey. I immediately inferred that some of the males, having escaped my notice, had impregnated only part of the eggs; but, in order to convince myself of the truth of my supposition, I thought it necessary to take away all the brood-comb
that

that was in the hive, in order to oblige the bees to provide a fresh quantity, being fully determined to watch narrowly their motions after new eggs should be deposited in the cells. This was done accordingly, and at last the mystery was unravelled. On the second day after the eggs were placed in the cells, I perceived the same operation which I have related in a former experiment; I mean, the bees hung down in the form of a curtain, while others thrust the posterior part of their body into the cells: I then introduced my hand into the hive, broke off a piece of the comb containing two of those insects, and kept them for examination. I found in neither of them any sting, (a circumstance peculiar to drones only); and upon dissection, by the help a Dolland's microscope, discovered in them the four cylindrical bodies, which contain the glutinous liquor of a whitish colour, observed by Maraldi in the large drones.

Having till then never observed any difference in the size of drones, I immediately perused the memoirs on bees, published by Mess. Maraldi and Reaumur, and found that they had remarked it frequently. I have inserted, in a preceding page, the substance of their observations on that head, as taken from

from their writings. The reason of that difference must, I doubt, be placed amongst other *arcana* of nature. I found myself, therefore, under a necessity, in my next experiments, to be more particular in destroying the males, even those which might be suspected to be such.

I once more immersed all the same bees in water ; and, when they appeared to be in a senseless state, I gently pressed every one of them between my fingers, in order to distinguish those armed with stings from those which had none, which last I might suspect to be males. Of these I found fifty-seven, exactly of the size of common bees, yielding a little whitish liquor on being pressed between the fingers. I killed every one, and replaced the swarm in a glass-hive, where they immediately applied again to the work of making cells ; and on the fourth or fifth day, very early in the morning, I had the pleasure to see the queen-bee depositing her eggs in those cells ; which she did by placing the posterior part of her body in each of them. I continued on the watch most part of the ensuing days, but could discover nothing of what I had seen before.

The eggs, after the fourth day, instead of changing in the manner of caterpillars, were
found

found in the same state they were in the first day, except that some of them were covered with honey. But a very singular event happened the next day about noon: all the bees left their own hive, and were seen attempting to get into a neighbouring common hive, on the stool of which I found their queen dead, having, no doubt, been slain in the engagement. The manner in which I account for this event is as follows: the great desire of perpetuating their species, which is most observable in these insects, and to which end the concurrence of the males seems so absolutely necessary, made them desert their own habitation, where no males were left, in order to fix their residence in a new one, in which, there being a good stock of males, they might the better accomplish their purpose. If this does not yet establish the reader's faith of the necessity of the males bearing a share in the fecundation of the *ova*, the next experiment cannot, I presume, fail to convince him.

I took the brood comb which, as I observed before, had not been impregnated; I divided it into two parts; one I placed under a glass-bell, N° 1. with honey-comb for the bee's food; I took care to leave a queen, but no drones, among the common bees I con-

fined in it. The other piece of brood comb I placed under another glass-bell, N^o 2. with a few drones, a queen, and a number of common bees, proportioned to the size of the glass; the rest I disposed of as before. The result was, that in the glass, N^o 1. no impregnation happened: the eggs remained in the same state they were in when put into the glass; and, upon giving the bees their liberty on the seventh day, they all flew away, as was found to be the case in the former experiment: whereas, in the glass, N^o 2. I saw, the very day after the bees had been put under it, the impregnation of the eggs by the drones in every cell containing eggs; the bees did not leave their hive on receiving their liberty; and, in the course of twenty days, every egg underwent all the above-mentioned necessary changes, and formed a pretty numerous young colony, in which I was not a little startled to find two queens.

Fully satisfied concerning the impregnation of the eggs by the males, I desisted for the present from any further experiments on that head, being exceedingly anxious to endeavour to account for the presence of this new queen.

I conjectured, that either two queens, instead of one, must have been left among the bees

bees I had placed under that glass; or else, that the bees could, by some particular means of their own, transform a common subject into a queen.

In order to put this to the test, I repeated the experiment with some variation. I got four glass-hives blown flat, which I thought preferable to the bell-shaped ones I had used before, as I could with those better examine what was going forward. I took a large brood-comb from an old hive, and, after having divided it into several pieces, I put some of them, containing eggs, worms, and nymphs, with food, viz. honey, &c. under each of the glasses; and confined within each a sufficient number of common bees, among which I left some drones, but took care that there should be no queen.

The bees finding themselves without a queen, made a strange buzzing noise, which lasted near two days; at the end of which they settled and betook themselves to work: on the fourth day I perceived in each hive the beginning of a royal cell, *a certain indication that one of the enclosed worms would soon be converted into a queen.* The construction of the royal cell being nearly accomplished, I ventured to leave an opening for the bees to get out, and found that they returned as re-

gularly as they do in common hives, and shewed no inclination to desert their habitation. But, to be brief, at the end of twenty days I observed four young queens among the new progeny.

On relating the result of these experiments to a member of this university, well conversant in the natural history of bees, he deemed it necessary that they should be repeated, in order the better to establish the truth of a fact, seemingly so improbable, that the eggs, destined by Nature to produce neutral or common bees, should be transformed into females or queens. He started an objection to me, which, by the publication of Mr. Schirach appearing a little time after, seems to have been pointed out to that author also by Mr. Withelmi, his brother-in-law, namely, that the queen-bee of a hive, besides the eggs which she deposits in the royal cells, might also have laid royal or female eggs, either in the common cells, or indiscriminately throughout the different parts of the hive. He further supposed, that in the pieces of brood-comb, which had been successfully employed in the last experiments for the production of a queen, it had constantly happened, that one or more of these royal eggs,

or

or rather the worms proceeding from them, had been contained.

But the force of his objection was removed soon after, by the same success having attended a number of other experiments which I since made, an account of which would take up too much room here; and this Gentleman, together with Mr. Schirach's brother-in-law, was at last brought to admit, that the working-bees are invested with a power of raising a common subject to the throne, when the community stands in need of a queen; and that accordingly every worm of the hive is capable, under certain circumstances, of becoming the mother of a generation: that it owes its metamorphosis into a queen, partly to the extraordinary size of the cell, and its particular position in it; but principally to a certain nourishment appropriated to the occasion, and carefully administered to it by the working-bees while it is in the worm-state, by which, and possibly other means as yet unknown, the development and expansion of the germ of the female organs, previously existing in the embryos, is effected, and those differences in its form and size are produced, which afterwards so remarkably distinguish the queen from the common working-bees. And finally

it appears evident, from the experiments made by Mr. Schirach and myself, that the received opinion, that the queen lays a particular kind of eggs, appropriated to the production of other queens, is erroneous. I am not a little flattered with the similarity of my discoveries with those of the ingenious German naturalist, in proving the sex of the common bees, although we so widely differ in what relates to the use of the males, whom, as we have seen before, he imagines to be quite useless. I am also not a little pleased to find, that our experiments on the production of a queen from a common embryo agree so well.

I shall now beg leave to point out the advantage that may accrue to the public from these observations, which is that of forming artificial swarms or new colonies; or in other words, of furnishing the means to bring on a numerous increase of those useful insects: an object of some importance to this kingdom, as being the only means to prevent the annual exportation of considerable sums in the purchase of wax, a great quantity of which is lost every season for want of keeping up a sufficient stock of bees to collect it.

The practice of this new art, Mr. Schirach tells us, has already extended itself through

Upper

Upper Lusatia, the Palatinate, Bohemia, Bavaria, Silesia, and even in Poland. In some of those countries it has excited the attention and patronage of government; and even the empress of Russia has thought it of such importance, that she has sent a person to Klein Bautzen, to be instructed in the general principles, and learn all the *minutiæ* of this new art.

The narrow limits of this paper do not permit me here to give an account of Mr. Schirach's ingenious observations. I beg leave to refer the curious reader to the work itself, which, with the reviewers, I wish was translated into the English language, as it contains many particulars highly deserving the notice of the speculative naturalist, as well as of those who cultivate bees either for profit or amusement.

PART OF A
 L E T T E R
 FROM THE
 ABBE' SPALLANZANI,
 TO THE
 MARQUIS LUCCHESINI,
 CHAMBERLAIN TO THE KING OF PRUSSIA.

I WAS some time ago favoured, by your means, with a letter from the king. I had before been agreeably surprized by the repeated instances of esteem and kindness, with which that great monarch has condescended to honour me at different times: but I now feel much greater satisfaction than ever, on account of his gracious acceptance of my last publication (*a*), which I humbly

(*a*) His Dissertations.

offered

offered him by you. In consequence of his command you made an extract, which he had the goodness to read, and to consider as not unworthy of his royal approbation. Two particulars, you inform me, especially drew the attention of this acute philosopher: the first, my discoveries respecting the generation of several amphibious animals. By these observations he thinks, that the doctrine of *Epigeneſis* is fully confuted, and that of the pre-existence of germs established, an opinion to which he is much inclined. The other particular is, the artificial fecundation of various animals. This made the deepeſt impreſſion on his mind, as I collect from your following expreſſions: “ But no part of your book excited ſuch aſtoniſhment as that which treats of artificial fecundation. On reading the analyſis of it he thought of a thouſand experiments, worthy of the notice of the naturaliſt, as to extend the diſcovery from particulars to general, and to enlarge, if it be poſſible, the kingdom of animated nature by new colonies of various ſorts of mules.” Theſe two views are certainly the moſt intereſting of all which natural philoſophy affords; but I now conſider them as more intereſting than ever, ſince they have appeared ſo agreeable in the eyes of this philoſophic king. With reſpect

spect to the first, I rejoice exceedingly with the favourers of the pre-existence of germs, among whom are included the most judicious naturalists of the present age, that we are joined with so wise and great a monarch; and I cannot but be proud of his glorious approbation, so graciously accorded to my discovery.

With respect to artificial fecundation, the subject being, I presume, worthy of your attention, permit me to detain you a little while upon it.--- And in the first place, a few words concerning amphibious animals. Finding that I had reaped in a field which I may term my own, I was tempted to revisit it, by the hope of adding some new ears to what I have already gathered. After the publication of my work I therefore made fresh experiments, which proved fortunate beyond all belief. Words cannot express the abundance of the produce. I knew, by experience, that those topics of natural history, which we suppose to be exhausted by our industry and patience, when resumed, present themselves under new and unexpected aspects, whence result either new truths or useful consequences: all the works of the supreme Architect being stamped with the seal of his infinite perfections, cannot be exhausted by human industry. I will
not

not detain you with the particulars of my new observations on the artificial fecundation of my amphibious animals, for the abundance of matter is ill adapted to the limits of a letter. I will rather take the liberty of doing it, when I shall have sufficient leisure to extract from my journals the sum of my observations, in order to compose a supplement to my publication on that subject.

Meanwhile, I pass on to the artificial fecundation of quadrupeds, concerning which I will observe to you, that I feel the greatest satisfaction at finding that my sentiments do not differ from those of your sovereign. When I succeeded in the artificial fecundation of a bitch, meditating with surprize upon my discovery, I conceived that it might be an excellent way to procure, if the thing be possible, different sorts of strange mules, an idea in which my illustrious friend Mr. Bonnet, to whom I usually first communicate my experiments, concurred. Hence I resolved to provide myself, at my convenience, with a number of female quadrupeds, as cats, bitches, rabbits, and to try to fecundate them with the seed of some different species, at the season of their amours. I likewise communicated this idea to Dr. Rossi, a celebrated Professor in the University of Pisa, that he might
put

put it in practice: Dr. Roffi, as you perhaps know, is the naturalist, who last year repeated, with success, my experiment on the artificial fecundation of a bitch. Towards the middle of last November, when I returned to the university of Pavia, I procured two cats, one two years old, which had once brought young, the other eleven months old, and which had never produced. Both had the liberty of my chamber, but they could not get out, nor was any male ever permitted to enter: there was only in the same apartment a little spaniel three years and a half old, the same which had furnished the prolific liquor that fecundated the bitch. The older cat was first in season; this fell out on the 3d of December: incessant loud cries, sufficiently expressive of her wants, afforded a clear proof, that she now began to seek and to invite the male. Being unable to satisfy her desires by means of a male of her own species, and being by nature, like all other female cats, exceedingly salacious, she for the present forgot her antipathy to the dog-kind, and did not hesitate to approach the spaniel, and to invite him, by stroaking his belly, and reiterated careffes; but he, without either hurting the cat or flying, never consented to her wishes, though he was of a very voluptuous

tuous disposition; he would smell her, and then turn with indifference another way. The third day after the appearance of these signs, I tried to fecundate my cat artificially with twenty-two grains of seed, furnished by the same dog. The same means and precautions were employed as in the experiment on the bitch. But having observed that the females of this species receive the male many times, I was not satisfied with a single injection, but repeated it thrice more before the cat went off her heat, which happened on the 11th of December; I kept her confined along with the other, as in a former experiment.

You may conceive my anxious expectation of the result of this unattempted experiment. Should any one of my injections prove prolific, and should the young partake, both in form and manners, of the female which conceived them, and the male that furnished the seed, I fancied, that the most singular mules, and such as had never been before seen, would now be produced. With respect to manners, two most opposite natures would be kneaded together and be confounded; the one, that of an animal susceptible of education, full of courage, abilities, and sentiment, all ardour, all affection, all obedience to his
master;

master; the other, that of an animal in internal qualities, far inferior, by instinct intractable, abhorring all subjection, faithless to its owner, affectionate only through interest, and born with an irreconcilable enmity to the former. Nor would the nature of these two animals engrafted together, be less different in a physical point of view, whether we consider the external configuration, the proportion of the limbs, or the internal organization. But unhappily this was an occurrence not easy to be brought about by the experimenter, and in which his labour is not crowned with success. The cat, notwithstanding all my care, was not fecundated. I was not, however, discouraged by this failure from repeating my attempt with the seed of the same dog and the same precautions, upon the other cat, which began to be in heat on the 18th of January, and instead of four, I injected seed seven times; that is to say, once every day as long as the season of her amours lasted. At each injection I did not introduce less than eighteen grains of seed; but impregnation did not take place; for from the last injection to the date of this letter, thirty-two days have elapsed, and there does not appear the least intumescence of the cat's belly. This is also

also the case with the other, though the experiment was made so long before. We know, that these animals bring forth in about 55 days, and bitches in sixty-three at farthest.

I would not, notwithstanding, pronounce the attempt impracticable, for I think that, to warrant such an assertion, a greater number of trials is necessary. These two experiments, however, may justly render us mistrustful of any that shall be hereafter attempted; I shall not be surprized if they should prove unsuccessful, considering the widely different nature of these animals. But should this really happen, we ought not to be discontented, since Nature has thus replied to our interrogatories; and her responses, whatever they may be, should be held precious by us, as they serve to increase the stores of useful knowledge. Further, the failure of these attempts, ought not to prevent us from making others upon animals differing in their nature. It is true, that every kind of seed will not fecundate every species of animal. This liquor, on which depends the perpetuity of the species, must have a certain relation with the embryos to be fecundated; and it is natural to suppose, that such a relation does not belong to all kinds of seed. But it is also true,
that

that we can only learn from the effects, that is from experiment, when this relation does subsist. The very experiments which at first seemed contrary to the production of such and such mules, when repeated in a better manner, proved favourable to it. Buffon destroyed our hopes of procuring mules, by keeping rabbits and hares together, seeing that in the experiment he adduces, the one species never copulated with the other. But this conjunction has been effected by other hands (*a*), and hence *bare-rabbits* have been procured. A dog and a she-wolf, kept together for a long time by the same author, never shewed any sign of mutual attention. But the experiment, when repeated by others, had a very different degree of success. The mules that were by these means produced propagated their kind (*b*): Buffon failed in the same manner with respect to dogs and foxes: this experiment has not, as far as I know, been repeated by others. I should think, that in more expert hands, it would have been attended with a more fortunate result. But in the race of mules, there is nothing perhaps so curious and surprizing as the famous

(*a*) In those of the Abbé Amoretti.

(*b*) *Bonnet Œuvres*, T. 3.

Jumart. Three varieties, you know, are enumerated, the offspring of the bull and mare, the afs and cow, and the bull and sheafs. Leger and Shaw admit the existence of all without hesitation; but Buffon, in his history of animals, reckons them all imaginary. Yet in his supplement, he does not absolutely deny the possibility of their existence, though he doubts it much. But in truth, the French Pliny was mistaken. Mr. Bourgelat, formerly inspector-general of the *Ecole Veterinaire* at Lyons, in a letter to the illustrious Bonnet, expressly says, that he had been in possession of several of these Jumarts, and that one was dissected under his inspection in the school at Lyons; and he communicates the result in his letter to the philosopher of Geneva (*a*). The authority of this celebrated and ingenuous person, merits the utmost deference. Assuming then the existence of this singular sort of mule, I could wish that they should be multiplied much more than they have hitherto been, both because they are well adapted to throw fuller light upon the great function of generation, and because they may possibly prove highly advantageous to mankind, as they are

(*a*) l. c.

said to have been possessed of extraordinary strength. Natural fecundation, tho' studiously attempted, would not very fully accomplish this object, on account of the indifference, or rather the aversion of quadrupeds of various sorts to copulate together, especially when they happen to be placed at a great distance from each other. These illegitimate marriages take place only, when the ass or the bull cannot find the means of satisfying their desires upon their own species, and are moreover uncommonly ardent. Artificial fecundation, properly performed, would be most convenient in this case. I will add, most respectable Marquis, that I am well disposed to put it in practice; but my public and private engagements have hitherto prevented me, as also the expence, for I will openly acknowledge it, of keeping these animals for several months, to which the narrow income of a philosopher is not very adequate. Hence I made application to a rich person in Lombardy, to assist me in these experiments; but he was insensible to the proposal, a circumstance which did not surprize me, as it certainly will not surprize you, who know, that the nobility in many cities of Italy are not very friendly to science and literature. In order to satisfy my wishes,

I see

I see no better expedient, you will excuse my philosophical freedom, than that of applying to you. The high honour you enjoy, in possessing a station in the court of one of the greatest princes upon earth, suitable to your eminent virtues and valuable endowments, of a prince, who is at once the delight of his happy kingdom, and the great protector of letters and learned men: your ardour for natural philosophy, and for whatever has a tendency to ennoble and advance it; your close connection with our famous royal academy of sciences and polite literature at Berlin, which is so desirous of enlarging the limits of this noble branch of knowledge, by the sure guidance of accurate experiments: these favourable circumstances afford me hopes, that you will not refuse to second my wishes. I am willing to believe, that you will choose the most advantageous moment to speak to your sovereign of this curious project; nor do I despair of his encouragement, since it may, in some sort, be termed a thought of his own. But enough of this. I proceed to another subject.

[The remaining part of this letter, having no connection with any topic treated in the foregoing Dissertations, is omitted.]

SPALLANZANI having adverted, in more Passages than one, to the FECUNDATION OF FISHES, Mr. FERRIS was induced to send the following OBSERVATION to the Editor of the *Journal de Physique*.

AT the age of about fourteen or fifteen, he happened to be on the bank of a river abounding in fishes. The stream was rapid, but the water was so shallow and clear, that at about the depth of two feet, he observed two salmons stirring the sand with their tails, which were turned to each other. They made a hole in the shape of a funnel, over which the female placed the extremity of her tail, and discharged a quantity of red liquor: her place was then immediately taken by the male; who, in the same position, emitted a considerable jet of white liquor. They then in concert covered the hole with sand and parted.

This observation is succeeded by a paper, copied from Mr. Duhamel's work on fishes and fisheries, on the mode of producing salmons and trouts, as it is practised on the banks of the Weser. It is not surprising that an essay, written chiefly with œconomical views,

views, should not in every respect satisfy the curiosity of the philosopher. A box or case of wood is directed to be made with an aperture towards the bottom of two opposite sides. The aperture is to be secured with a grate of iron. The bottom is to be covered with sand and gravel, and a gentle stream is to be brought through the box. In November, when the salmon repairs to brooks and rivulets, in order to propagate the species, a female is to be held by the head over a bucket of clean water. If the eggs be quite mature, they will drop out of themselves; or if they should not, gentle pressure on the belly will bring them out. The male is to be treated in the same manner, and when the surface of the water appears white with milt, the operation is finished. The eggs are now to be carried to the box. Sometimes the young are formed in five weeks, and may be seen to move in the egg. They may be distinguished by their eyes, which are black, while the other parts are yet diaphanous: in eight days afterwards they break through the skin or membrane of the egg. This period, however, varies with the temperature of the water, and of the atmosphere, and is sometimes prolonged to ten weeks. While the young fry is growing in the egg,

a fine pellicle, distinct from the external membrane, may be observed; to this pellicle the little fish is attached; it forms a sac round it. The sac fills almost the whole capacity of the egg, and serves the fetus *for a stomach and bowels*. The first feeds upon the matter contained in it four or five weeks after it is hatched. During this time the mouth, which is at first shapeless, gradually elongates; afterwards the sac totally disappears, and the animal assumes its perfect figure.

The same method is to be observed with respect to trouts, but their eggs are not mature till December and January.

The author observes, that fishes do not copulate, and that fecundation is external. He procured from a trout some eggs perfectly mature, and took the utmost care of them, but he put no milt upon them, and they all spoiled without producing a single young fish.

Among the trouts produced according to the method above-described, there appeared many varieties of monsters; the author explains these phænomena from Lewenhœck's hypothesis concerning the spermatic worms.

But the most curious experiment in this paper is the following. The author took the mature eggs out of a trout, which had been dead four days, and which was now
very,

very putrid and offensive. He covered them with the milt of a living male, and the produce of fry was as abundant, as if the eggs had been furnished by a living female.

He next proposes to fecundate the eggs of the trout with the milt of the salmon, and reciprocally. He pretends, that certain waters, and a particular kind of food, will convert trouts into salmon-trouts.

He asserts, that the eggs of both these species infallibly perish, if any impurity adheres to them, or if they lie long upon the ground: hence he deduces the final cause of their depositing their offspring upon the gravelly bottom of rivulets, in places where the stream is continually freeing them from impurities.



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RECAPITULATION. REFLECTIONS.

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TRANS-

T R A N S L A T I O N
OF THE
LATIN, FRENCH, and ITALIAN
PASSAGES in this VOLUME.

Page 10. Neither fecundation or impregnation of the egg, takes place in any living body in Nature without the body of the female.

Page 50. *All things were created good.*

Page 114. The male frog, firmly seated on the female, waits for discharge of the eggs; he then emits his embryos, such as I observed them, they attach themselves to the eggs, and feed upon them for a few days, till they are capable of taking coarser food. These embryos retain the figure which they had in the vesicle of the male for about a month; they then change their shape like silkworms. Their hind feet are next developed and separated; the hind feet were at first united, and formed the tail of the tadpole.

Page 117. l. 14. He advances as serious truths, tales equally ridiculous with those told by old women, as they spin by the fire-side

Ib. l. 24. I will therefore venture to assert, not only that Gautier had made little proficiency in the anatomy of the frog, but that he was scarce acquainted with the external form of this animal.

Page 118. l. 4. Were it possible that Gautier should relate his discoveries to Pythagoras, I firmly believe, that he would enjoin him silence, not for two or for five years, but for ever.

Page 126. As often as the female discharges any eggs, the male bedews them with seed, as I have repeatedly observed at my house, with some admiration.

Page 133. I resign this Dr. Pirri to you. He is in good hands, and you will be sufficiently able to defend the good cause of Nature. It is always a mark of temerity to attack experiments by reasoning.

Page

Page 134. Lastly, we have a demonstration, which directly proves the existence of the young in the female, at least in birds. For the intestine of the young bird is a continuation of the membrane of the yolk, and the internal coat of the intestine is a continuation of the epidermis, the external of the skin: in short, it is the same as the membrane of the yolk.

Page 252. Nature, when I have been contemplating her, has often persuaded me to suppose nothing incredible which relates to her.

Page 420. Should the hemp, producing male flowers, be pulled before the seminiferous individuals have opened their pistiliferous flowers, there will either be no crop of seeds, or only a very small one.

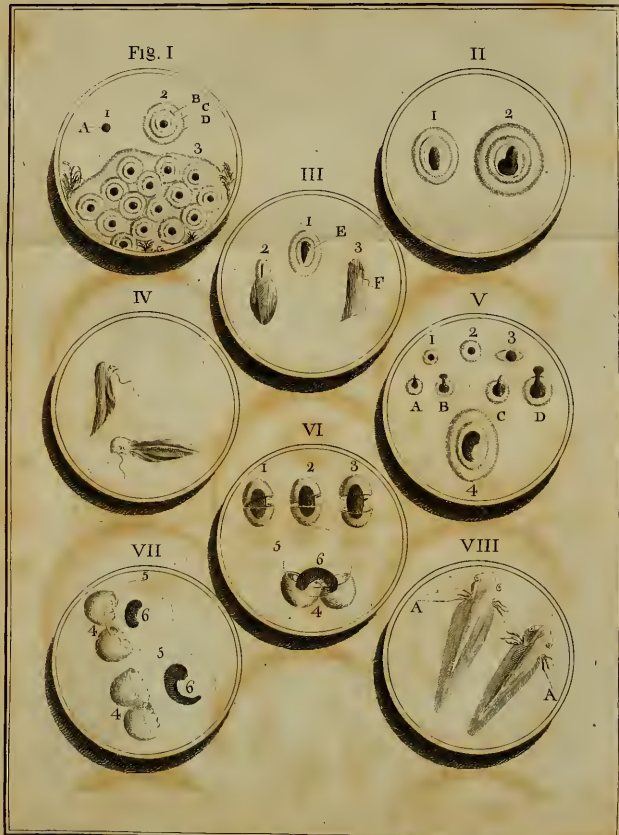
Page 426. We distinguish the observer from the adherent of system. The latter builds whole systems upon a certain number of species: he concludes from particulars to generals, that is, he assigns to all plants the same properties as are possessed by those upon which experiments have been made. The observer rejects all theories, and relies on observation and experiment alone. The perfection of botany essentially depends on the knowledge of individuals related to each other, and their characteristics. This knowledge will be acquired in proportion as the numbers of observers, which is very inconsiderable, shall increase, and the croud of system-makers shall be lessened. Systems of botany become obsolete in time, because they are not chiefly founded on Nature and experiment.

Page 436. He was of opinion, that Natural History does not consist in a catalogue of animals, plants, and fossils, but in the knowledge of their qualities. He therefore taught their structure, analysis, properties and use, without neglecting, however, such marks as may enable us to arrange them in an index. Those who commit to memory nothing but such indexes, or are dexterous in turning them over, in vain persuade themselves, that they are in possession of the book of Nature.

ERRATA.

In a Passage, which the Translator cannot now find, for *impregnated*, read *unimpregnated*. The Context will easily enable the Reader to make this Correction in the proper Place.

There are also a few Improperities in the Punctuation, which the Reader is also desired to correct.



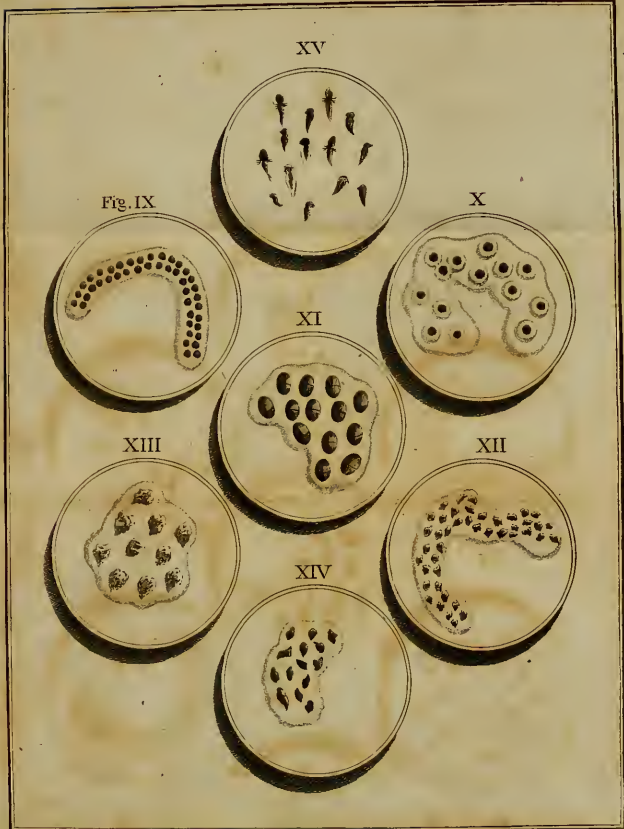


Fig. XVI



XVII



XVIII



XIX



XX

