

Tracts on the nature of animals and vegetables.

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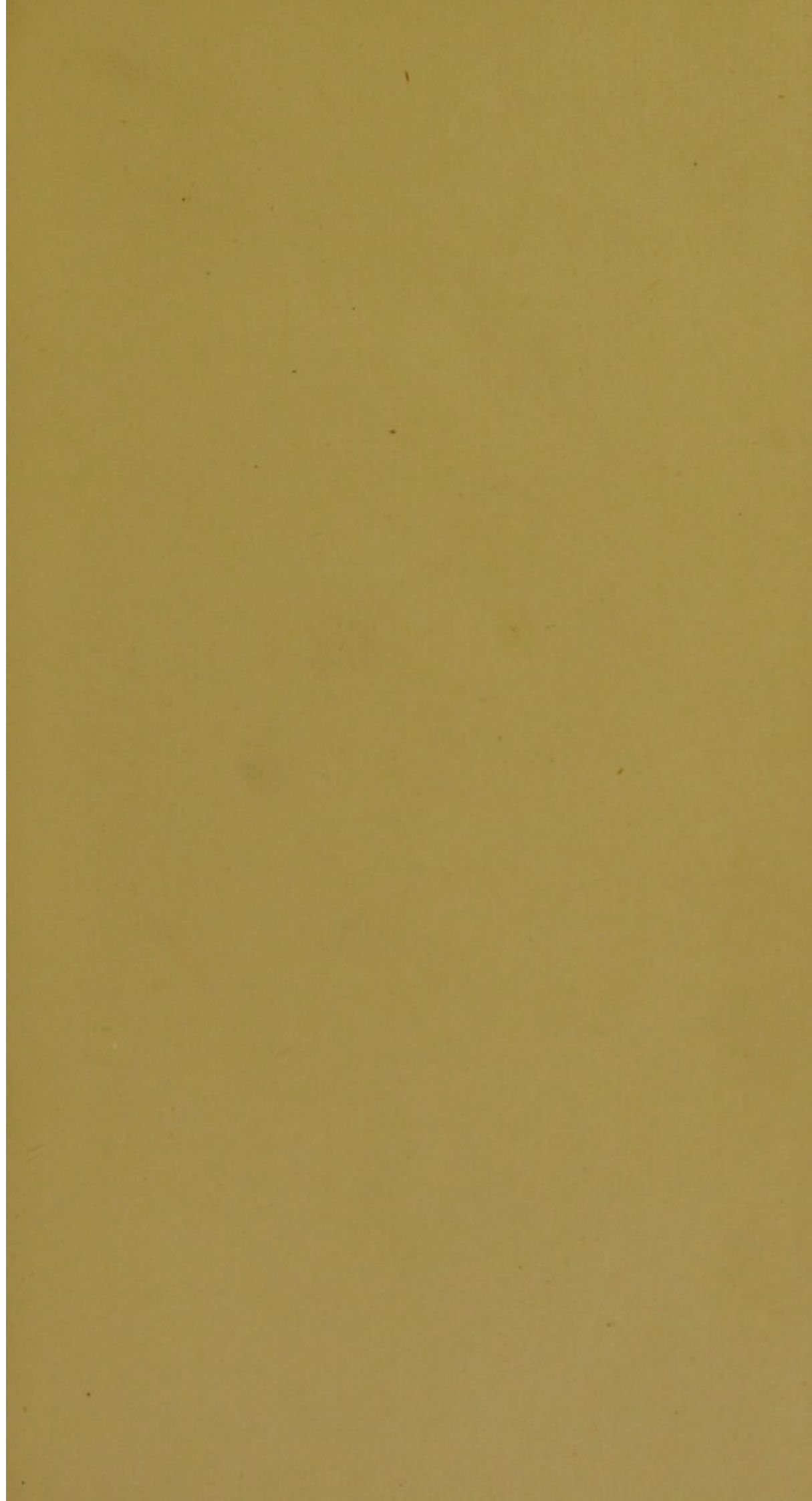
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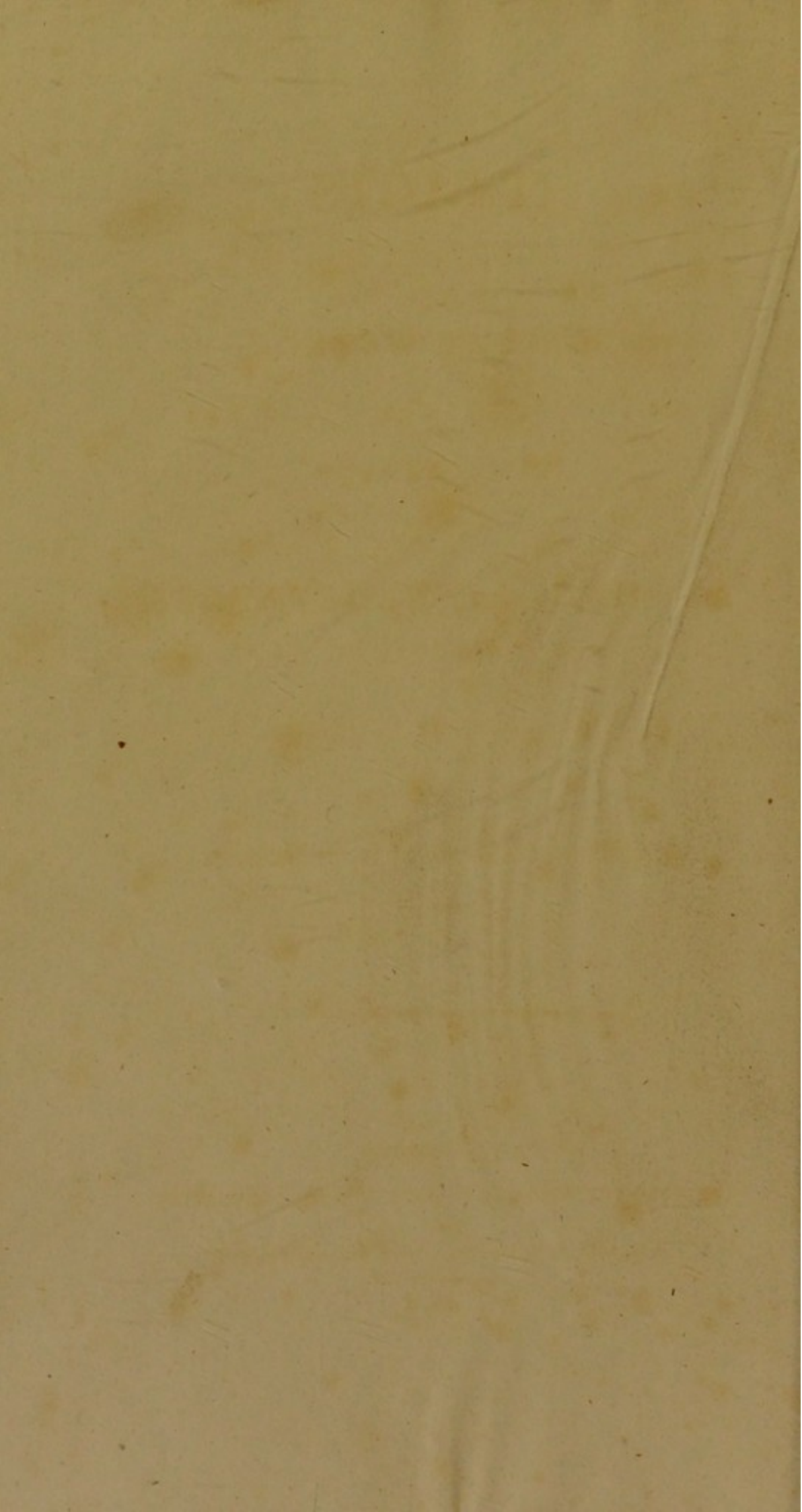












M Baillie

TRACTS

ON THE NATURE

OF

ANIMALS AND VEGETABLES.

BY

LAZARO SPALLANZANI, R. P. U. P.

EDINBURGH,

PRINTED FOR WILLIAM CREECH, AND AR. CONSTABLE,

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1799.

PLATE

THE TEMPLE

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THE TEMPLE

TRANSLATION OF

TO

JAMES EARL OF HOPETOUN,

VISCOUNT AITHRY, BARON HOPE,

LORD LIEUTENANT OF LINLITHGOWSHIRE,

ETC. ETC. ETC.

THIS TRANSLATION IS INSCRIBED.

TRANSLATOR'S PREFACE

The celebrity of Simon Stevin's name
has not failed to attract to his
work, which is now present
in the hands of English readers with
interest and attention, that
which is due to its author, and
which is due to the scientific reader.
The author of this translation has
endeavored to give it the same
character as the original, and to
make it as useful as possible.
The author of the original has
not only given the names of the
instruments and the names of the
experiments, but he has also
given the names of the persons
to whom they are due. All the
experiments and the names of the
experiments are given in the
original. The degrees of heat are
reduced from Reaumur to Fahrenheit's ther-
mometer. If the difference appear, and I can
not say there are none, probably they arise
either from the author using both thermome-

TRANSLATOR'S PREFACE.

THE celebrity of Signor Spallanzani occasions our regret that we are so little acquainted with his works. This, which is now presented in our national language, is replete with matter so new, so singular and amusing, that I anticipate the pleasure every Philosophic reader must receive.

The treatise upon Animalcula I have ventured to abbreviate. Perhaps it will be esteemed a sufficient reason, that only the detailed method of executing some experiments, some letters corroborating the author's discoveries, and controverfy with Mr Needham concerning the animation of animalcula, are omitted. All the experiments and the other treatises, are given as in the original. The degrees of heat are reduced from Reaumur's to Fahrenheit's thermometer. If inconsistencies appear, and I cannot say there are none, probably they arise either from the author using both thermometers,

ters, and neglecting to remark it in his work, or to typographical error.

Animal Reproductions are not so generally known in Britain as they ought to be. M. Bonnet's treatise upon the reproduction of the head of the snail, is more concise than any I have hitherto seen. Signor Spallanzani (besides his *Prodromo*) has published two memoirs, *Sopra la riproduzione della testa nelle lumache terrestri*, which would with more propriety have formed a part of this volume: but I am not so entirely master of my own time, as the superintendence of publishing two or three hundred pages additional would require. Both memoirs are highly worthy of perusal.

Upon the merits of this translation, it becomes me to be silent. An anatomist will see where the words *axilla*, *cubitus*, and *radius*, might have been successfully used. Perhaps, there is not a greater impediment to the diffusion of science, than the multiplicity of technical terms, and the variety of nomenclature: yet all are of infinite use, when acquired. In some passages, I confess, I have not been able to ascertain the authors' meaning with

with precision. The language of experiment is dull and uniform ; and, unless a total change of arrangement, even of expression, is made, it is often impossible to translate with any semblance to elegance. Frequently, such a change cannot properly be made.—When all the requisites of the skilful translator of a scientific work are considered, it will not appear surprising, if there are errors here. The favourable, I may say the flattering reception, which my first juvenile performance has experienced in the world, induces me to hope that there are likewise some persons who may derive amusement from this.

BINNS-HOUSE,

March 1799.

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OBSERVATIONS AND EXPERIMENTS

UPON THE

ANIMALCULA OF INFUSIONS.

CHAP. I.

To discover, whether long ebullition would injure or destroy the production of animalcula in vegetable infusions, I infused in distilled water, kidney beans, vetches, buckwheat, barley, maize, the seeds of mallows and beets; also the yolk of an egg. I took care that each species of seeds was from the same plant. The water used, was that in which seeds had been boiled, observing, that the water which had boiled half an hour, was taken to infuse seeds which had boiled half an hour. The vessels containing the infusions, were loosely stopped with corks.

Upon the 15th of September, I made thirty-two infusions, and, upon the 23d, examined them. There were animalcula in all; but the number and species differed in each. In the

B

maize

maize infusions, the animalcula were proportionally smaller, and more rare, according to the duration of the boiling.

In the infusion of kidney-beans, boiled two hours, were three kinds of animalcula; very large; middle sized; and very small. The figure of the first partly elliptical, partly umbelated, attached to long filaments; the second almost cylindrical; the third nearly spherical; all incredibly numerous.

In the infusion boiled an hour and a half, were animalcula of the largest and smallest class, but few in number: still fewer in that boiled an hour; and very few in that boiled half an hour.

The infusion of mallow seeds, boiled two hours, produced middle-sized circular animalcula, and others very large, the head extremity hooked. In two infusions, boiled an hour and a half, the number and species of animalcula were the same: when boiled two hours, there were more, and as many as when boiled half an hour.

An immense number was in infusions of vetches, boiled two hours. When boiled an hour and a half, they were very small and rare, all semicircular, or bell-shaped. Some may be seen when the infusion has boiled an hour; but it gives the eye pain to discover those in an infusion boiled half an hour.

In the infusion of barley, boiled two hours, were very large and numerous elliptical and oblong animalcula. Boiled an hour and a half, there was a moderate quantity of very minute animalcula. A few were observed, when boiled half an hour.

There was no fixed rule with the other infusions. In the buckwheat, boiled an hour and a half, were more animalcula than in the rest of the same grain. This happened with the beets and the egg: when boiled an hour, there were more animalcula than in other infusions of the same kind, boiled various durations of time. But in two infusions, boiled half an hour, there were fewer than in the others.

Thus, it clearly results, that long boiling the seed infusions, does not prevent the production of animalcula. To explain why the infusions boiled for the shortest time have the fewest animalcula, I may observe: That animalcula should appear in infusions, it is necessary the substances infused, sensibly begin to dissolve; for, as this dissolution is effected, or at least for a certain time, the number of animalcula augments. Seeds of plants, boiled for a shorter time, are, for a shorter period, encompassed and penetrated by the dissolving power of the fire; consequently, when put to macerate, will not be so soon decomposed. This is doubt-

less the cause why animalcula will sooner be seen in infusions of boiled, than unboiled feeds. A short ebullition is not sufficient to decompose the feeds of vegetables ; for, decomposition operates by long and gradual maceration.

Some days after making these experiments, the number of animalcula increased in the shortest boiled infusions ; and, towards the middle of October, the whole thirty-two infusions were equally crowded. The only difference was in the figure, size, and motion of the animalcula.

Soon after, I made experiments, in the same manner, with pease, lentils, beans, and hemp-feed. The animalcula in all but the infusion of beans, were more readily produced in the shorter boiled infusions. To know whether augmenting the intensity of the heat would obstruct the production of animalcula, I slightly warmed, in a coffee roaster, the eleven species of feeds, and then formed eleven infusions. Neither the production nor the number was affected. They increased, as usual, from a small number ; and, in the middle of October, twenty days after the infusion was made, they swarmed so much, that the fluid seemed perfectly animated.

I attempted to extend this experiment. I roasted the feeds, and ground them as we do coffee : the appearance was like foot. I then
made

made as many infusions as there were kinds of feed; also infusing the yolk of an egg, which had been exposed to 279 of heat. Animalcula were equally produced from this powder; only the time was a little longer before the number was so great: but the weather was colder: and animalcula multiply much more quickly in warm weather.

Further, I subjected vegetable feeds to the most intense heat, the heat of burning coals, and the flame from a blowpipe. I exposed the feeds in an iron plate upon coals. When converted to cinders, I reduced them to powder, and made as many infusions as there were kinds of feed. The cinders from the blowpipe were extremely dry and hard. I could scarcely believe my eyes, when I saw animalcula in these infusions.

CHAP. II.

I HERMETICALLY sealed vessels with the eleven kinds of feeds mentioned before. To prevent the rarefaction of the internal air, I diminished the thickness of the necks of the vessels, till they terminated in tubes almost capillary, and, putting the smallest part to the blowpipe, sealed it instantaneously, so that the internal air underwent no alteration. It was necessary to

know whether the seeds might suffer by this inclusion, which might be an obstacle to the production of animalcula. Other experiments had shewn me, 1. vessels hermetically sealed have no animalcula, unless they are very capacious: 2. animalcula are not always produced: 3. when they are produced, the number is never so great as in open vessels. Although I used pretty large vessels, two substances, pease and beans, had not a single animalcule. The other nine afforded a sufficient number; and to these I limited my experiments. I took nine vessels with seeds, hermetically sealed. I immersed them in boiling water for half a minute. I immersed other nine for a whole minute, nine more for a minute and a half, and nine for two minutes. Thus, I had thirty-six infusions. That I might know the proper time to examine them, I made similar infusions in open vessels, and, when these swarmed with animalcula, I opened those hermetically sealed. Upon breaking the seal of the first, I found the elasticity of the air encreased. Seeds contain much air: a great quantity should escape in their dissolution, by heat or maceration, which must, of necessity, render the portion of included air denser and more elastic. However, this elasticity may originate partly from the elastic fluid discovered in vegetables, the nature of which is apparently different from the atmospheric

mospheric fluid. I examined the infusions, and was surpris'd to find some of them an absolute desert; others reduced to such a solitude, that but a few animalcula, like points, were seen, and their existence could be discovered only with the greatest difficulty. The action of heat for one minute, was as injurious to the production of the animalcula, as of two. The seeds producing the inconceivably small animalcula, were, beans, vetches, buckwheat, mallows, maize, and lentils. I could never discover the least animation in the other three infusions. I thence concluded, that the heat of boiling water for half a minute, was fatal to all animalcula of the largest kind; even to the middle-sized, and the smallest, of those which I shall term animalcula of the higher class, to use the energetic expression of M. Bonnet; while the heat of two minutes did not affect those I shall place in the lower class.

Having hermetically sealed six vessels, containing six kinds of seeds producing animalcula of the lower class, I immersed them in boiling water for two minutes and a half, three, three and a half, and four minutes. The seals of twenty-four vessels being broken at a suitable time, there were no animalcula of the higher class seen, but more or fewer of the lower. The air was almost always condensed, both in this and in the other experiments.

In vessels immersed seven minutes, I found animalcula of the lower class. They appeared in vessels immersed twelve minutes.

The minuteness of animalcula of the lower class, does not prevent our distinguishing the difference of their figure and proportions.

Boiling half an hour, was no obstacle to the production of animalcula of the lower class; but boiling for three quarters, or even less, deprived all the six infusions of animalcula.

We know, that the heat of boiling water is about 212° . These infusions were of this heat at least, as appeared by the marks they exhibited of ebullition, the whole time the surrounding water boiled. Philosophers know, that water, boiled in a close vessel, acquires a greater degree of heat, than when boiled in an open. To know how much less than half a minute the boiling might be abridged, and animalcula of the higher class yet exist, I made use of a second-pendulum, and immersed the vessels in boiling water for a given number of seconds, beginning with 29. In a word, boiling for a single second prevented their existence; and I could only employ a degree of heat less than that of boiling water; for example, 209, 207, 205, 203° , descending to a degree which would not injure their production.

At a medium of 111° , I descended from 200, to 189, 178, 167° . Thus, I had four classes of

of experiments, corresponding to the numbers 200, 189, 178, 167. In each class, were nine species of feeds, which made thirty-six vessels. After the time necessary for the production of animalcula, I broke the hermetical seals, but saw none of the higher class at 167°. I continued this retrograde motion by 11°, and came to 155 and 110°; so that I had five classes of infusions, and forty-five vessels to examine. Not a single animalcule was seen of the higher class, in vessels hermetically sealed, and exposed to the moderate heat of 113°. This was during the middle of April: the thermometer in the shade stood at 88°. I took eighteen vessels; nine had been exposed to 99° of heat, and nine to 88°. No animalcula of the higher class were produced in the former; but I found them in the latter. In each vessel, the quantity and kind of animalcula, as in vessels not subjected to heat. The degree of heat fatal to them, was 92°.

Animalcula of the lower class, exist in sealed vessels exposed to the heat of 212°; while those of the higher class hardly appear at 92°: But, when produced, the same intensity of heat that is fatal to the one, also deprives the other of life; and animalcula of the higher, as well as of the lower class, perish at 106°, or at most at 108°.

Two

Two important consequences thence arise. The first evinces the extreme efficacy of heat to deprive infusions in close vessels of a multitude of animated beings; for, in open vessels, are always seen a vast concourse of animalcula. The second consequence, concerns the constancy of animalcula of the lower class appearing in infusions boiled in close vessels; and the heat of 212° , protracted an hour, has been no obstacle to their existence.

I have twice found animalcula produced in metal vessels sealed with metal, and kept half an hour in boiling water.

We are therefore induced to believe, that those animalcula originate from germs there included, which, for a certain time, withstand the effects of heat, but at length yield under it; and, since animalcula of the higher classes only exist when the heat is less intense, we must imagine they are much sooner affected by it, than those of the lower classes. Whence we should conclude, that this multitude of the superior animalcula, seen in the infusions of open vessels, exposed not only to the heat of boiling water, but to the flame of a blowpipe, appears there, not because their germs have withstood so great a degree of heat, but because new germs come to the infusions, after cessation of the heat.

CHAP. III.

IN the month of May, I took the eggs of frogs from a ditch where they had been deposited by the mother a few hours before. I divided the quantity into equal portions, and exposed each to a different intensity of heat. The eggs were completely immersed in the water of a vessel, where I had put the ball of a thermometer. I then placed the vessel upon a slow fire, and, when the thermometer ascended to the requisite height, I took the eggs from the warm vessel, and put each portion in cold water. I had ten vessels, because I had ten portions exposed to different degrees of heat; 110, 122, 133, 144, 155, 167, 178, 189, 200, 212°. The eggs exposed to 110, 122, 133°, produced young. At 110°, almost all were fertile; there were fewer at 122°; and at 133°, the number was very small. At a greater heat, all the rest became putrid.

The heat neither accelerated nor retarded the production. Tadpoles were hatched in the same time as from those not exposed. The tadpoles all expired at 110°. The frogs which had afforded the eggs, all perished at 110°. They were middle-sized green-backed frogs, frequenting the ditches of plains and meadows.

I know that there are frogs which live in warm baths, although subject to heat greater than 110° . For instance, my friend Signior Cocchi relates, that frogs are not injured in the baths of Pifa at 115° ; but they may perhaps be of a different species. Perhaps, after being long accustomed to a degree that would at first have been fatal, they no longer experience any bad effect. At least we know, that men, who, the first time, can hardly endure the steam-bath six minutes, and perspire violently whenever they enter; in a certain time support it during fifteen minutes, without any sensible inconvenience.

The heat of 110° destroyed the nymphs and larvæ of muskitoes; and 106° rat-tailed worms and water fleas. Water newts and leeches died at 110° ; the eels of vinegar at 113 . In my experiments upon the caterpillars of the elm-butterfly, and the larvæ of large blue flesh-flies, the animals became restless at 93 and 95° : at 97° , they were much affected; and all died at 108 . The eggs of these insects long withstood the impression of heat: at 88° , they produced the greatest possible number of worms: at 99° , they still produced many. The number decreased with the heat, and none were produced at 144 . The eggs of silk-worms had the same fate. The eggs of the blue flesh-fly were very fertile at 124° ; a small
number

number appeared at 135 and 137; none at 140°. The worms from the eggs became restless at 88°, and died at 108. Full grown worms of the same kind all died at 108. The flies of the same worms perished at 99. Some flies came from nymphs exposed to 104 or 106°; none from those to 110.

I exposed grey pease, lentils, wheat, lintseed, and the seed of trefoil, each to a different degree of heat, 167, 178, 189, 200, 212°. I sowed each kind of these seeds separately, in small distinct spaces of earth, so prepared, that each space might contain an equal number of seeds: 167° did not injure them; 178 begun to affect them, and very few were fertile. At 189°, there were produced only eleven plants of trefoil, and ten at 200°. Only three of those at 212° germinated; some trefoil seeds could withstand the heat of boiling water. The seeds were exposed to heat in a sand-bed: in a second experiment, I put them in water, which I slowly heated, until it acquired the requisite degree. By this means, the heat had greater influence upon the seeds. Pease, in this way, at 167°, germinated as abundantly as trefoil; but the lintseed, lentils, and wheat, did not. At 189°, seven stalks of trefoil, and one of flax; at 200°, only six of trefoil; at 212°, none.

For thirteen days, I exposed the plants to

167, 178, 189, 200, 212° of heat, by dipping the branches in water gradually warmed. They were instantly replanted; but all died. I saw that 167° was a heat injurious to young plants: 155 and 144° did not hurt them, as, upon being replanted, all continued to vegetate.

I heated in sand, as above, the following feeds: Beans, barley, white and black kidney beans, maize, vetches, parsley, spinage, beets, radishes, mallows. All exposed to 167°, germinated; some at 178° begun to perish; very few succeeded at 189 and 200°; and, at 212°, there appeared only one plant of white kidney beans, and three of beans. I repeated the experiment at 200, and 212°: not one germinated.

It is demonstrated, by these experiments, that the eggs of the animals upon which they were made, withstand heat better than the animals themselves. Tadpoles and frogs perished at 110°; their eggs only at 133°; and some supported even a greater degree. Silk-worms and the elm-butterfly died at 108°: their eggs became sterile only at 133. The large flesh-flies perished at 99°; their nymphs at 110; their larvæ at 108; and their eggs at 140°. We remark almost the same relation between plants and their feeds, as between animals and their eggs. Some feeds, as trefoil, kidney beans, and beans, are fertile, though exposed to 212°; while

while their plants cannot support 167°. The seeds of plants are more able to endure heat, than the eggs of animals. All the seeds upon which I made experiments by dry-warming, have been fertile, though exposed to 167°, and some to 212°; but no egg hatched after 144°.

Heat is much more noxious acting along with water.

The life of an animal concealed and concentrated in the integuments of an egg, is very faint, compared with that of an animal produced. During the first hours of incubation, the pulsation of the heart is the only indication of life in the chicken. Before the egg is hatched, its life is still more faint; it is a *lesser life*. Undoubtedly it is the same with that of the germs in the eggs of insects, which have not the degree of heat necessary to hatch them. May this feebleness of the life of the embryo in the egg, be a reason why it is better able to support heat in this state, than when it is more expanded? It is certain, that minute animals, which have in this state a life so feeble, which so little merits the name of life, resist, with greater impunity, external accidents, as the intemperance of the air, than when they are more vivacious. If we cut off the head, take out the heart, or deprive a frog, toad, newt, snake, or viper, during winter, of some members, while they are torpid by cold, and seem more
dead

dead than alive, they live much longer after such operations, than if they were to undergo them in the vigour of life : and I have observed, that insects immersed in water during winter, live longer than if immersed during summer.

The reason why seeds withstand heat better than eggs, rather seems to be, from the greater abundance of fluids in an egg, which, being expanded and put in motion by the heat, will violently operate against the subtile filaments of the germ, and occasion their rupture, and its destruction. Should the instance, of seeds resisting the influence of boiling water, induce us to think the germs of animalcula have the same faculty, the idea is corroborated by direct proofs, taken from the animals themselves. M. Duhamel observed a cockchafer live in a heat equal to that of boiling water ; and, Schæffer saw one species of caterpillar live at the same degree.

Carolina, and the Cape of Good Hope, swarm with animals of all figures and sizes, where the thermometer ascends in the shade to 110 and 122°. The direct heat of the sun is double, sometimes triple, in the warmest regions : that of the shade, at the Cape of Good Hope, is at least 189°, and in Carolina 212. If there are animals which sustain this heat, and their eggs preserve their fecundity ; and,
if

if there are animals in our climate, which can support this degree of heat; why shall we deny, that there may be animalcula of the same constitution? M. Sonnerat saw fishes swimming in some waters in the Philippine Islands, where the thermometer stood at 187° .

The germs of the higher class of animalcula, however, cannot support so great a heat as the animalcula themselves. The animalcula die at 108° : the germs are not developed after 95° . We must thence conclude, that the nature of the germs of the higher class is very different from that of the lower, with regard to the power of resisting heat.

The difference in the eggs of other animals is more marked. The eggs deposited by some butterflies upon the under side of leaves, like those which certain insects deposit towards a northern aspect, perish at 79° ; although this is 20° less than is necessary to hatch the eggs of other insects; as of the asili, which deposit them in the skin of oxen or of cows; and those flies, which insinuate them into the frontal sinus of sheep, goats, and deer; or those which deposit them in the rectum of the horse. We may say the same of the eggs of several which multiply in the human body, and in that of calves, where the heat is about 98° .

CHAP. IV.

I TRANSPORTED animalcula from the heat of the atmosphere to the cold of an icehouse. In the month of August, the thermometer in the open air stood at 83° , and in the icehouse at 36 . The only change I could observe, was, a slight relaxation in the motion of the animalcula; and they did not seem to suffer further, although exposed to this degree of cold for several days. I then covered the vessels with ice. At the beginning of the fourth day, a number of the animalcula died. Of twenty-two infusions put in ice, those of seven only remained alive. In eleven days, the animalcula in two of the seven had perished. After two months, those in the other five were alive: one species appeared more numerous. Along with those seven, I put in ice other two infusions, yet sterile, because lately made; but, in a few days, they were filled with an army of the most minute animalcula.

During winter, I subjected the animalcula to new experiments. While the infusions preserved their fluidity, there was no particle of ice observed; which was occasioned by the quantity of vegetable oil the infusions contained, which secured them against freezing. Notwithstanding

withstanding the cold killed the animalcula of several infusions, there were some more robust species that supported it. I took the advantage of a very cold day; and, although the thermometer fell to 13° under the freezing point, and the infusions were covered with ice; upon breaking it, and presenting some minute portions to the microscope, I found living animalcula in the parts not completely congealed. They were immured in little grottoes of ice. In those portions absolutely frozen, the animalcula were dead. They did not revive upon melting the ice. The rest retained their vivacity in the parts of the fluid not yet congealed^a. I could not decide whether the animalcula perish, because the cold has destroyed them, or because the infusions have lost their fluidity. It is a fact acknowledged by philosophers, that water does not lose its fluidity at 20 , or even 22° below freezing, when at perfect rest; which may be attained by inclusion in a close vessel, and removal from external motion. In this way, the animalcula survived, although they suffered cold almost 20° below zero, in water not frozen. They swam about, but with a slower motion than usual, while the thermo-

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meter

^a Mr Muller of Copenhagen has discovered some species of animalcula which survive congelation. I regret that his work did not come to my knowledge until the transcription of my manuscript was finished.

meter stood at 18° below the freezing point. This was the greatest degree they could support, as they died at 20° , although the water was not frozen, but was beginning to be covered by a thin crust. Two species survived; and I may perhaps, or even without perhaps, say, they would have supported a more intense cold, had I been able to keep the water longer fluid.

I made infusions similar to the preceding, sealed hermetically, and exposed to cold, produced by a mixture of fine pounded sea-salt, with snow. The thermometer descended to 34° below zero; and the infusions were so much frozen, that they took half an hour to melt. But the germs of the animalcula were not injured, since the sealed infusions, at the ordinary time, afforded a number of animalcula. This fact well corresponds with what has been observed in those insects that have the greatest analogy to animalcula. Some can support 42° under freezing, others die at 22, or at most at 24° : a very great number cannot support the cold of freezing, and others die at a degree far inferior. There is this difference between animalcula, and insects exposed to cold, that the former retain sufficient life to use their members, whereas the latter immediately lose their vivacity, and become motionless like dead bodies. Some insects we may compare

to animalcula ; for, besides the podura of Linnaeus, which inhabits the snows of Sweden, I have observed the eels of vinegar preserve the motion of their members when exposed to a very intense degree of cold ; for, although this fluid does not freeze so soon as water, the eels swim constantly while it is not frozen. Some kinds remain fluid 15° below freezing ; other kinds remain fluid at 24° : but, when the cold is thus increased, the eels become like animalcula insensibly motionless ; but they move, although there is a thin crust of ice. As the congelation increases, they continue motionless, extended in a straight line, or a little curved. If they receive sudden aid, by melting the vinegar, they will almost certainly be brought to life. If the ice is allowed to harden, it is impossible to revive them. These analogies between insects and animalcula, exist in the originating principle.

The winter of 1709 is celebrated for its cold, and the fatal effects it had upon plants and animals. The thermometer fell 33° below the freezing point. Who can believe, exclaims Boerhaave, that the eggs of insects were not entirely destroyed by this rigorous winter ? After the spring began to temperate the air, insects were at the ordinary time produced, the same as after the mildest winter. For five hours, I kept various eggs of insects, among

which were those of the elm butterfly, and silk-worms, enclosed in a glass vessel, in a mixture of ice and sal-gem, where the thermometer fell 38° below zero. All the eggs produced worms at the ordinary time.

The next year, by mixing ice and sal-gem with spirit of nitre, I obtained a degree of cold 56° under freezing. This did not injure the eggs of insects subjected to it. The result of all this is, that cold is less noxious to germs and eggs, than to animalcula and insects. Germs can, in general, support a degree of cold 33° under the freezing point. Some animalcula perish at freezing; others about 18° under it. The eggs of several insects sustained 56° ; the insects they produced, only 16 or 18: which I have remarked in silk-worms, and those of the elm butterfly. And, although there are caterpillars and chrysalids which withstand a very great degree of cold, their eggs will support a degree much more considerable. As animalcula and insects can less sustain heat than their eggs, so can they less sustain cold. What I said before, will apply here. The bodies of insects killed at 16 or 18° below freezing, are so indurated and frozen, that their members will not yield to the pressure of the finger, and seem perfect ice under the edge of the knife. This never happens to eggs exposed to a much more intense degree of cold.

Their

Their humours preserve their fluidity. They perhaps owe this advantage to the spirituous or oleaginous nature of their component parts, which may diminish the influence of the cold.

CHAP. V.

MAN, like other animals, being subject to physical laws, is consequently liable to perish by an excess of heat or cold. However, he can sustain both, when extended to a degree one would imagine insupportable. It is commonly believed, with Boerhaave, that man cannot exist in air as hot as the blood. This rule had been established by that philosopher, from his observing, that certain birds and quadrupeds died in air heated to 149° , which is 53° greater than that of the human blood. But, such an opinion appears ill-founded, since we know there are regions, inhabited, where the heat is greater than of the human blood. At the Cape of Good Hope, the thermometer stands in the shade at 113° . The heat of Carolina is greater than that of the human blood, since the thermometer falls, when taken from the shade, and put in a person's mouth. The heat of a warm bath sometimes equals that of the hottest climates. There are waters which raise the

thermometer to 113° , even to 122° ^a. Boerhaave thought the greatest degree of natural cold was zero in Fahrenheit's thermometer. He observed, that men, animals, and vegetables, perished at this degree. But we experience a much greater intensity of cold. For several winters, at Petersburg, the thermometer fell to 60° below freezing, and once to 67° . The cold at Quebec was 72° below freezing: that at Torneao, observed by Maupertuis, 83° . But those high degrees of cold, although very intense when compared with that of our climates, are incommensurable with those that chill many places in Siberia. At Tomsk, Kirenga, Jeniseik, the thermometer falls 120° , 149 , and even 157° under zero.

We cannot deny, that cold so piercing was pernicious, and indeed fatal, during 60° felt at Petersburg: the face could not be kept uncovered above half a minute. At Torneao, where the thermometer fell to 83° , the breast felt as if lacerated. Some of the inhabitants of the cold climates lose their members in winter,

^a Dr Fordyce supported 150° for twenty minutes without inconvenience, 198° for ten minutes, and 262° for eight. His respiration for seven minutes was not affected; the eighth, it became more frequent. He supported 220° a long time without inconvenience. A dog did not suffer from being exposed to 360° , in a basket, for thirty-two minutes.

ter, an arm or a leg. The cold experienced by Captain Middleton in Hudson's Bay, froze all the liquors except brandy; and the beds were covered with a coat of ice three inches thick, although the walls of the houses were of stone, and two feet thick; the windows very narrow, closed up with strong boards; and although great fires were kept continually burning. The Dutch experienced a similar cold in Nova Zembla. One, by keeping in motion, may support a greater degree of cold. The savages of the most northern countries, hunt during the greatest cold; and they know so well that motion alone can preserve their lives, if any accident, during their expedition, should threaten them with death, they accelerate it by rest. From the narrative of some Dutchmen, who wintered at Spitzbergen in 78° of north latitude, we learn, that those who shut themselves up in wooden huts, one after another died with cold before the fire; whereas, those who lived in the open air, and employed themselves in hunting, carrying wood, or other exercises, preserved perfect health.

We must conclude, that man is in a condition to support a number of variations of heat and cold, between a degree far greater than that of freezing, and heat equal to, or even more intense, than of boiling water, which shows that he is not, by nature, meant to inhabit

habit certain determinate parts of the globe, but to live, multiply, and exercise his dominion in all, without experiencing an obstacle in climate. It is not so with quadrupeds. They have been distributed upon the earth; some framed for warm, others for temperate, and and others for cold climates. We have not hitherto found any species that can accommodate itself to all indifferently. The lion, elephant, tiger, panther, and leopard, dwell only in the warmer regions: when transported to temperate climates, they become incapable of generating, and quickly perish in cold. Our domestic animals do not suffer in the warmest regions: they cannot, however, exist in the coldest; as, the horse, the ox, the sheep. The elk, rein-deer, and ermine, inhabitants of the North, are never found in southern countries; and, so far from being able to exist there, they do not survive in the temperate climates. This, at least, has been observed with respect to the rein-deer, the naturalization of which has often been attempted in France and Germany; but all, instead of multiplying, have perished. The law which forces animals to remain in their native countries, is under modifications. Some there are that multiply in temperate, though they are originally from warm climates; while some animals, which are ordinarily found in cold climates,

mates, live very well in warm. The rabbit and Guinea pig are examples of the former; the beaver and lynx, of the latter.

Birds may, in this respect, be regarded as divided into two classes. Some, like quadrupeds, do not wander far from their native place, or at least do not change their climate; others have no fixed abode, but change their climate according to the seasons, being apparently necessitated to make such changes, either from the scantiness of food, or from their inability to resist the rigour of winter, or even a slight degree of cold.

We have said, with Boerhaave, that heat 149° above freezing, soon killed certain quadrupeds and birds. Indeed, 53° above blood heat, is considerable; and several species of animals cannot support it: but we do not find that it is insupportable, or that there are no animals which suffer it under the Torrid Zone, and in very warm climates. It seems to me, that we should reason upon the heat that birds and other animals can sustain, as we reason in relation to the cold; and since those of the most northern climates sustain a great degree of cold, so should the animals of the southern countries be able to support an excessive heat.

It is easy to determine, that the greatest degree of heat squamous or cetaceous fishes experience, is equal to the heat of the water
where

where they swim, which is therefore less than that of freezing. Those living in salt water are exposed to cold a little more intense than freezing. It thus happens, that fishes are secured against the rigour of cold, to which all animals are so much subjected, by the element they inhabit: and, except those inhabiting shallow waters, they are sheltered from the burning heat of the atmosphere.

There are carps in warm springs which support blood heat. I took the river carp, and heated the water where they were to 106° , without their seeming uneasy: at 108 , they became restless, and died at 116 . I made similar experiments upon eels, tench and lampreys: they died at a less degree of heat.

But, of all known animals, reptiles and insects are those that stand in greatest dread of cold, and seek for heat the most. We may say, the heat of the sun is their soul. When exposed to it, they have more sensation and motion; and, as the heat of that luminary is more powerful, so are their vivacity, agility, and boldness greater. The venomous are then more terrible, their poison more dangerous. Cold produces an opposite effect. A number of insects would, at the approach of winter, be exposed to hazard, did they not seek a safe retreat in the rents of walls, the midst of stones, the holes or clefts in the trunks of trees. Some seek for safety in the caverns of mountains

tains and subterraneous abodes, or in dung-hills, where, during the severity of winter, they always experience a gentle heat. But the bottom of waters, and the bowels of the earth, are in particular two certain retreats for the greater part of reptiles and insects. The lethargic slumbers they are in during winter, are the effects of the cold.

It is very possible, that there may be among quadrupeds and birds, perhaps even among fishes, some which experience a sort of lethargy like reptiles and insects. With respect to quadrupeds, I shall say nothing of toads, frogs, green lizards, small lizards, which pass almost the whole winter in the water or in the earth, in a constant torpor; but we observe the same facts in hedgehogs, land-tortoises, and also in several species of rats, marmots, and dormice. Those animals conceal themselves in the hollows of trees, or in the earth; some in solitude, and some in society. Bats in winter are found stiff and motionless in the holes of trees, the rents of walls, and suspended to the vaults of subterraneous caverns.

There are birds subject to the same torpidity. At the end of the fine weather, we see hundreds collect, crowd together in clusters, and plunge into the water, where they pass the winter in heaps, contracted within themselves. The intelligent reader already anticipates that

I here speak of swallows. The fact is too well ascertained, too well authenticated, to leave any room for doubt. Many respectable and credible persons declare, that they have not only seen numbers of swallows collect, and throw themselves into pools at the approach of winter; but that they have several times seen clusters of swallows taken out of water, even from beneath ice. The question is, therefore, whether the swallows spoken of are the same with ours; that is, with those that construct a nest of earth in our houses, and remain with us during summer? or whether they are stranger swallows, by which I mean a bird resembling our swallow, in figure, colour, and size, but is, at the same time, of a very different species? I have, for several years, endeavoured to solve this. By experience I know, that the animals, which are in a sort of lethargy during winter, are, when exposed to a degree of cold, subject to the same effect: so that, if one exposes a dormouse, a frog, or a lizard, to the cold of freezing during summer, when most vivacious, they become motionless, and continue so while the cold remains. I thought of exposing some of our swallows to the air of an icehouse, making them gradually pass from atmospheres less warm, as of a cave or a chamber adjoining to the icehouse, as a sudden change might be too powerful. In the month of August, they all perished

perished in the adjoining chamber in three hours. I could not observe, whether they had first become lethargic, although the thermometer stood at 43° above zero, a degree of cold rather too small, than too great, to produce such an effect. I repeated the experiment ; but all had the same fate. Whence I conclude these to be specifically different from the swallows found in water, and below ice.

There are certainly some fishes, upon which the influence of cold has the same effect. If we may credit Peclin, quoted by Haller, tench are of this kind : he says, he has seen them bury themselves at the bottom of the vessel upon the approach of winter, as we see many reptiles and insects then enter the earth. But, in general, fishes are a class of animals, which has the privilege of preserving vivacity and action, however cold the atmosphere may be.

Whence does it happen, that all insects and reptiles, at a certain degree of cold, lose their strength, their action, and appear as if dead ; while man, the greater part of quadrupeds and birds, retain their powers and liveliness, when cold is at this degree, and even beyond it ? What is the immediate cause of the death of the former, and the preservation of the latter, in similar circumstances ? M. De Buffon is the first who has seriously investigated this phenomenon. He observes, that the animals that
become

become torpid are cold blooded, such as marmots, dormice, hedgehogs, and bats ; that they have not of themselves any internal heat, but only the heat of the atmosphere ; so that, upon the approach of winter, their blood refrigerates with the atmosphere. This refrigeration is the cause of their torpidity ; and the use of the senses and members is lost ; for it then circulates, it is probable, in the larger vessels only. I could have wished, that this explanation, so plausible, was equally true. Haller, who has dissected several hedgehogs, assures us he has found their blood warm ; and he observes, that Lister, Robinson, and Lancisi had before him made the same remark. I have made experiments upon three hedgehogs, and found their blood warm. I have found the same in bats, introducing, by the mouth, a small thermometer into the body of the animal. M. De Buffon never saw the liquid rise ; on the contrary, it fell sometimes half, and sometimes whole degrees. But, by my experiments, the thermometer, introduced by the mouth of hedgehogs and bats, always ascended to 99 and 101° above zero, while kept there eight or ten minutes.

It was impossible for me to procure marmots, but I requested a friend to make the same experiments. The consequence was, that marmots had not cold blood, as M. De Buffon imagined.

imagined. The heat of one marmot raised the thermometer from 50° to 90° above zero. The heat of another raised it in five minutes to 92° . Some time afterwards I had two marmots. My experiments upon them, perfectly agreed with those of my friend. In the open air the thermometer stood at 65° ; when introduced by the animal's mouth, it rose to 101° .

There is a method of conciliating the opposite facts. It must be, that the French naturalist has made his experiments in winter, when the animals were devoid of sensation and motion. Then they would actually resemble cold-blooded animals; the rigour of the season had exhausted the principle of internal heat. Reason, and my experiments prove, that those animals do not become lethargic while the internal heat which animates them remains undiminished.

It is at the same time certain, that the blood refrigerates in all animals that become lethargic. But may we conclude, that this lethargy is the immediate effect of the refrigeration of the blood?

Frogs, toads, tree-frogs, water-newts, I have in my experiments observed, after having all the blood discharged from the heart, or the aorta cut, leap, run, dive, swim; have the use of sight and feeling; in a word, perform every corporal function, for several hours, the

same as before. I resolved to make new experiments, and begun with frogs. I buried in snow several that were extremely, but equally lively : one number was untouched, another deprived of the whole blood. I even endeavoured to empty the heart and principal vessels completely. In eight or ten minutes I took some from the snow. Those which were, and were not deprived of blood, appeared precisely in the same state, that is, half dead, and not attempting to escape although at liberty. Fifteen minutes afterwards, I took others from the snow : all seemed contracted, motionless, and almost frozen. I returned them to the snow ; and in some hours transported them to a warm situation, carefully observing what happened. By little and little, they stretched themselves, opened their eyes, and prepared to escape. This I observed in all, without any difference. I had the curiosity still to bury them in the snow. I saw anew the same phenomena ; and I constantly found the results the same, when the experiments were repeated at different seasons of the year. All the tree-frogs, toads, water-newts, whether deprived of blood or not, equally experienced the lethargic slumber, when exposed to cold, but revived with a sufficient degree of heat.

The coincidence of these facts constrains me to say, that the privation of sensation and motion

tion is not effected by the refrigeration of the blood, nor can we ascribe it to the more languid circulation: whence, it is consequent, the lethargic torpidity must arise from the solids alone; which, being severely affected by the cold, are in a situation very different from the natural state. What is this situation? It seems that we recognise it in the phenomena torpid animals exhibit: they are contracted; the muscles have lost their usual flexibility; they become extended and rigid. It is therefore demonstrated, that when the muscular fibre becomes very rigid, this rigidity injures the irritability of the muscles: how much must it be affected by a rigidity so great! Of this I was convinced, by irritating the fibres by different stimulants; but I scarcely occasioned the slightest indication of rugosity or contraction. While irritability is commonly viewed as the principle and source of life, when that is greatly diminished by exposition to cold, it is not surprising a lethargy similar to death ensues. If this is the real and immediate cause of torpidity in the animals I have named, I find no reason to prevent its application to all animals, that in the same way become torpid. It is true, that it is impossible to deprive warm-blooded animals of what M. De Buffon imagines the cause of their torpidity, because nature does not admit that they shall live without blood;

but it is always certain, that their muscular fibre becomes rigid, and insensible to every stimulus, when in this lethargic slumber. This is what I have observed in bats, which I sprinkled with salt, immersed in hot water, pricked with pointed instruments, and divided the pectoral muscle with a knife. All these methods were ineffectual to awaken the irritability. The electric shock was equally fruitless. If cold suspends the irritability of warm as well as of cold-blooded animals, and if the cessation of this power is such, as seems to me the only and immediate cause of the lethargy of the latter, I do not see why it may not be the same with the former.

All animals that become torpid, do not so at the same degree of cold. That heat marked *temperate* upon the thermometer, so mild to mankind, makes dormice torpid. A degree of cold a little greater, affects bees, snakes, vipers, and some species of bats. That degree which affects toads, frogs, newts, &c. approaches freezing; but this is so far from affecting marmots, that 11° under zero is required to make them torpid. This variation can only arise from the different nature of the muscular fibres, which should render certain animals more susceptible of cold than others, by sooner producing the rigidity of the muscles. I am well persuaded, that impediments to the circulation, may

may occasion the death of many animals : but, whence comes it, that there is a great number which cease not to live, although the circulation of the fluids is suspended for a time, or even when absolutely stopped ? In another work, I have proved that several animals live a long time after losing all their blood ; that they live when the circulation is suspended, by tying up the trunk of the aorta. I have remarked, that reptiles are subject to the same laws as vipers, snakes, eels, &c. The death of animals killed by cold, must then ensue from some other cause than from suspending the circulation.

To discover the immediate cause of the death of animals destroyed by cold, I made observations upon some rendered lethargic by a degree of cold not intense. The phenomena attendant upon death, are these. The muscular rigidity encreases more and more, until the flesh hardens and freezes. The freezing first appears at the extremities, and gradually extends, until it reaches the centre of the animal. If the animal is then removed into a warmer atmosphere, that the parts may relax, they will resume their pristine flexibility ; but the animal will not revive. Its death is truly the consequence of having been frozen. But we cannot say, that it happens from the freezing of the blood only : *first*, from all the reasons I have

given ; *secondly*, because having exposed, to the cold of freezing, animals entire and deprived of blood, all died in the same time. Whence I conclude, that death is occasioned by the freezing of the solids. The muscles, at a certain degree, become rigid ; and, when frozen, cease to be irritable. Such is the apparent cause of the death of animals exposed to cold. Cold contracts the muscular fibre, by hardening it, and consolidating the lubricating fluid. Congelation further contributes to the destruction of the fibres, by turning the liquid to a number of little icicles, which, with their points and edges, lacerate the finest and most delicate parts. If the muscular flesh is then examined, it appears full of those minute icicles ; and when one attempts to twist or to bend it, it breaks like a friable substance.

CHAP. VI.

CERTAIN odours are to insects the most virulent poison. Reaumur has tried upon them the effect of turpentine, and the smoke of tobacco. The odour of camphire, according to Menghini, and its vapour, are still more efficacious when heated. I exposed animalcula to the odour of camphire ; and the result was precisely

cisely the same with what has been observed of insects. The vapour occasioned inquietude and uneasiness, among the animalcula exposed. They endeavoured to retreat from the malignant odour, by retiring deep in the infusions. If the vapour was rare, they were much longer of dying than if it was dense. The odour of the oil of turpentine killed them; but the effect was less sudden than that of camphire. The smoke of tobacco became mortal in a few hours: that of sulphur killed them immediately.

Oleaginous liquids were mortal to animalcula. Corrosive and spirituous liquors killed them in a moment. Urine not only kills animalcula, but it has the property of reducing them to very small particles. As Hartsoeker observes, we could not have believed that urine, left at rest for some days, produces animalcula, if we had not remarked the same phænomenon in vinegar; although this liquid kills animalcula as readily as urine, notwithstanding that, it is full of microscopic eels. The urine, after standing some time, is covered with a pellicle, of an obscure cinereous colour, in which the animalcula are found: they are of a roundish figure, in minuteness like animated points. Urine kept for some months, has always about the same number of animalcula, but there never appears any new species: and from the ef-

fect of this urine upon other animalcula, that species must be of a nature essentially different.

We know, that the electric shock is fatal to many animals, and that it kills them the more easily as they are smaller. It requires a battery ten feet square to kill a cat or a small dog; a pigeon is killed by one only two or three feet square; and a smaller apparatus a goldfinch or a canary. I exposed animalcula to the discharge of Dr Bevis' battery. Upon this battery I put a small spot of pitch, open in the centre, and filled the hole with some drops of infusion: from this hole I drew the electric spark. Although the infusion was full of animalcula, not one survived the shock. I diminished the shock, by giving the battery a smaller charge; but the effect upon the animalcula was precisely as before. I augmented the quantity of liquid exposed to the shock, by drawing upon the spot a right line, proceeding from the central hole, two-thirds of an inch in length, and two lines in breadth. I then passed the spark across the whole fluid. This was a thunderbolt to the animalcula: all perished at the same instant. I increased the breadth of the line, without augmenting the length. So long as the breadth was only two lines, all the animalcula perished; when greater, the whole did not suffer, as they did not die till some time after. Those in the space between the two lines were stunned,

stunned, and revolved as if they had been carried round in a vortex : this revolution became gradually more faint, and in a quarter of an hour they remained motionless. Those more distant from the formidable position, survived longer : the most remote did not perish, and their vivacity and activity shewed they were not incommoded by the electric fluid.

I made experiments by a simple spark drawn from a conductor. I used the same spot of pitch, placing it upon the conductor ; and, drawing the spark through the central hole, it seemed more sonorous and brilliant. I filled with liquid, either the central hole, or the little channel formed upon the spot, and varied the length and breadth. All the animalcula in the central hole perished ; those in the channel only after drawing two or three sparks. I applied a drop of infusion to a point upon the conductor : the animalcula died if the electric fluid passed through it for some time. In short, I have observed, in many repeated experiments, that the weakest shock is always fatal to animalcula. Simple electricity, that is, operating silently, has no effect upon them. There is no kind of animalcula upon which I have not made experiments : but electricity has been alike fatal to all.

I filled several small glass tubes with different infusions. The tubes were close at one end, and
open

open at the other. I put them under the receiver of an air-pump, at the same time keeping tubes in the open air filled with the same infusions. For fourteen days they were deprived of air, without the animalcula suffering injury. Upon the twentieth they begun to die, and on the twenty-fourth all had perished. Those in the open air were still alive. I repeated the experiment, with other infusions made of different feeds. The animalcula of some lived a month, and even five-and-thirty days : those of others died in fourteen, eleven, and even in eight days : some animalcula lived only two days.

The nature of certain animals is truly wonderful : they can perform in vacuo all the animal functions they exercise in open air. Vipers and snakes will creep, leeches swim ; some insects will feed, others actually copulate there. Such is also the nature of animalcula. They preserve every motion ; they even for some days propagate in vacuo. After a longer or a shorter space, according as the animalcula are more able to support the vacuum, their motion relaxes, and ends with the life of the animal. It sometimes, but rarely, happens, that the animalcula will recover life upon being taken from the receiver.

I never found any animalcula in whatever animal or vegetable substance left to macerate
in

in vacuo : the reverse has uniformly happened, when there remained in the receiver a quantity of air sufficient to keep thirteen inches of mercury in equilibrio. I have observed the same phænomenon with respect to the eggs of insects. I have often put those of terrestrial and aquatic insects under the receiver of an air pump ; but none ever hatched, although in every other respect in a condition to do so. The animal concentrated in the egg, enjoys the beneficial influence of the air, by means of a multitude of minute pores by which the egg is penetrated. Beside a number of animals respiring by the mouth, many receive air by means of apertures in the sides of the body, by the extremity of the abdomen, or by other parts. This is accomplished by many minute channels, with openings at the surface of the body, which, by ramifications, reach the most internal parts. Our animalcula, notwithstanding their apparent simplicity, display an organ, which we must imagine to be that of respiration. There are animalcula, that perish the moment they are deprived of air ; and there are others, whose nature and state is such, that they support this privation for a longer or a shorter time. A sparrow, a nightingale, a chaffinch, and, in general, other birds, perish very soon in vacuo : a lizard, a frog, reptiles and insects, still longer. All the different species

cies of animalcula do not support it equally well. But the lowest rank of animalcula seems to be that, of all other animals upon which experiment has been made, which can live longest without air. None can support it beyond a month; and, although they support privation of air a long time, they yield under it at last. I well know there are instances cited, of different animals said to have existed without enjoying the benefit of this element; such as, the accounts of frogs found alive in the middle of the hardest substances; of living toads discovered in the centre of large stones, or of entire trees, where the smallest particle of air could not insinuate itself into their retreats. But, those histories are more the object of the admiration, than the belief, of persons who have made any progress in Experimental Philosophy. It is requisite they should be supported by authority; which is most essential, in a case so strange and paradoxical. Until we obtain facts better ascertained, we think ourselves entitled to assert, that there is in nature no known animated being, which can exist without enjoying the advantages presented by air.

CHAP. VII.

WHILE we observe animalcula of some species, if we see two united together, we immediately suspect they are occupied in reproducing themselves. We have this opinion, although the animals which excite it are infinitely small; because many cases shew, that animals in this situation labour absolutely, and the most frequently, to propagate their species. Thus it has been thought, that animalcula copulated, because they have been seen united in pairs. Such is the opinion of Mr Ellis, and of Father Beccaria.

The transverse division of animalcula, not only exists in the spherical, and elliptical, but also in others with pointed extremities, though they have neither beak nor hook. The better to observe what happened, I isolated the animal in a glass of liquid. If the weather is warm, the traces of a contraction are very soon perceived about the middle of each side; the contraction encreases, and the animalcule somewhat resembles an oblong blown bladder, tied tight across the middle. The contraction gradually becomes deeper, and the animal is at length changed into two minute equal spheres in contact in one point. Plate I. fig. 1. A B C. The connected spherules continue to move as
before

before the division, with this difference, that they frequently stop, but for a very short time. The anterior spherule seems heavier than the posterior, which it drags along, the latter having no spontaneous motions, excepting those necessary to separate it from its companion. The division is at last completed, and from a single animalcule there arise two. At first they seem motionless; but their rest or indolence is soon dissipated, and the two portions resume the agility of their parent whole. The size of the complete animal is soon acquired. The parts, when very near separation, are not constantly spherical, but more or less elliptic. They are not always inactive at the moment of separation, but often retain the vivacity of the whole from which they originate. What specially merits observation, is, that in some animalcula, when the portions are just about to separate, each is almost equal in size to the whole from which they originate. Of the animalcula multiplying in this way, I have counted fourteen species; but there are only two that merit description. We see, in infusions of red and bearded wheat, a circular animalcule above the mean size. Around the circumference of the body is a circle of minute protracted points, resembling very fine cones, and moving with great quickness. These points serve the animalcule to swim, the same as the limbs of so many aquatic animals. The animalcula

nimalcula multiply by a transverse division in two. The division operates slowly; and one singularity is, that it is not completed till each equals the size of the whole, and each has during that time acquired minute points like those of the old animalcule, excepting that they are shorter.

The other species is found in infusions of the marsh lentil. The animalcula are so large, as to be visible to the naked eye, by illuminating with the rays of the sun a very thin-sided glass tube full of the liquid. The observer may easily see the successive divisions. Other elliptic animalcula of this kind swim when the division is scarcely begun, some when it is considerably advanced, and others when it is almost completed. In a short time, a single animalcule will people a whole infusion.

There are likewise animalcula which multiply by a longitudinal division. If a drop of infusion is presented to the microscope, animalcula are seen among the particles; some are fixed to these by the filaments, and some wander at large in the drop. This filament proceeds from the posterior part of the animal; and although its natural position is a straight line, it often contracts and forms a spiral. Then the filament resumes its original position. If the filament is free, it encompasses the animalcule. This it frequently does, almost periodically. It is of a
pearl

pearl colour, and its slenderness extreme, at least when compared with the animalcule : the length is equal to that of the animalcule, or more. The figure of the animalcule resembles an onion ; therefore I have called it the bulb animalcule. From a hole in the fore part below, proceed a number of extreme slender fibres, circularly disposed ; Pl. 1. fig. 2. These fibrilli, by their constant vibration, occasion a vortex in the fluid, which draws in the smallest surrounding bodies, and even the most minute animalcula. When the larger substances enter the hole of the animalcule, they are rejected, but the smaller remain ; we have therefore reason to believe, that they penetrate the body of the animalcule by some invisible channel, for the purpose of nutriment and preservation.

I have said, the filament has certain periodical motions ; I may add, that the animalcule has other periodical motions, immediately succeeding those of the filament. Always, when this contracts, the animalcule likewise contracts, and instantly draws the fibres and the hole within its body. Then it assumes the figure of a spherule, D ; Pl. 1. fig. 2. It afterwards extends the filament, and becomes like a pear, E ; and then assumes its ordinary figure, F G. The fibres and the hole re-appear : their motions re-commence with the vortices.

I first saw a little cleft open at the fore part of the animalcule: it was the beginning of an opening, which divided the hole in two parts. The cleft encreased, the vortex became double, and each of the animalcula acquired, in division, the rude figure of one. The two portions separated more and more; their shape became more perfect; and, when upon the point of separation, they were metamorphosed into two complete animalcula. One remained attached to the filament, soon becoming as large as the whole, and, by new divisions, producing new beings. The other animalcule wanted the filament, rapidly traversed the liquid, contracting and extending itself; and we perceived an appendage proceeding from the superior part, which was the origin of the wanting filament. Fig. 3. pl. 1. shews the different degrees of division before separation.

The bulb animalcula not only inhabit boiled, but also unboiled infusions of kidney beans, and several other infusions of legumes, as lentils, beans, small pease, grey pease, &c. To see the multiplication with facility, it is sufficient to macerate two or three portions of each kind of seed in a watch-glass. If the experiment is made in summer, in a few days some animalcula are seen attached by the filaments to minute particles in the infusion. The animalcula will divide under the observer's eye. The

number fixed by the filaments will be proportional to the number of divisions about to take place.

Another species of animalcule, which also multiplies by a longitudinal division, is produced by the macerated seeds. The fibrilli are not situated under the hole, but upon the lips. The figure of the animalcule resembles a monopetalous flower.

In a larger species of animalcule, sometimes found in an infusion of beets, the multiplication is effected by means of a little fragment detaching itself from the rest of the body. The figure of the body does not change like that of the rest: a small portion of the body detaches itself commonly near the part where the filament originates; Pl. 1. fig. 4. H. This fragment is in continual motion: when detached, it swims with agility in the infusion; and, although not a twelfth part of the whole, equals it in size during the day. Then it begins to multiply by the same divisions.

The reader will doubtless be curious to know, how I could isolate the animalcula. With the point of a writing pen, I transport a small drop of the infusion into a watch-glass. I put a little drop of water two or three lines distant from the first. I then make the two drops communicate by a sort of common channel, which is a prolongation of one drop with the

the pen. The animals traverse the canal, and arrive one after another in the drop. As soon as I perceive one enter, I cut off the communication.

The volvox, like most animalcula, is very transparent, and the internal structure is accurately seen. Some observers have already discovered young in the womb of the mother, extending to the fifth generation. In my long observations upon infusions, I have found two abounding with the volvox; those of hemp-feed, and the tremella. They are also found in the putrid water of dunghills. Those animalcula are at first very small, but grow so large, as to be distinguished by the naked eye. They are of a greenish yellow colour, of a globular figure, and of a transparent membranaceous substance. In the middle, are included several very minute globes; Fig. 5. pl. 1. These minute globes, when examined with the most powerful magnifiers, appear so many smaller volvoces, which have each their diaphanous membrane, inclosing others still less. I have distinguished the third generation, but never the two others. It is possible they were not visible in those I examined, from their not being of the size or species examined by other naturalists. When all had quitted the mother, the common membrane burst, and begun to dissolve. Meanwhile, the new volvoces con-

tained others, burst, and then dissolved. By isolating them, I saw the thirteenth generation.

One of the strongest objections made to the system of germs, arises from the great difficulty in conceiving the successive envelopment of animals in animals, and plants in plants. Oftener than once, we have found one egg within another; and some osseous part of one foetus has been found within another foetus. The butterfly is included in the shell of the chrysalis; and the chrysalis in the skin of the caterpillar. In the seeds of vegetables, are seen the rudiments of plants; and in the root of the hyacinth, the fourth generation has been discovered. The volvox affords a new argument for inclusion. There, we see it to the thirteenth generation; and probably that is not the last.

CHAP. VIII.

MR BAKER, in his treatise, intitled, *The Microscope made easy*, speaks of innumerable animalcula inhabiting water. He mentions a species discovered by Lewenhoeck in the marsh lentil, remarkable for a long tail, which serves to moor it to the roots of this plant; likewise
for

for a cavity like a bell in the anterior part of the body, and an internal motion, which contracts and extends the animalcula and their tails at pleasure. These peculiarities are so analogous to those of my bulb animalcula, that I determined to search for the species, with which I was unacquainted.

I was observing the motions of some tadpoles about the roots of marsh lentils I had put in a vessel of water to feed them. The solar rays fell direct upon the surface of the water, shewing the roots distinctly. One was distinguished from the rest by a slight tint of shining colour surrounding it about the middle. I was surprised to see the tint disappear, and then appear, which seemed periodical. I cut away the root while the tint was visible; it immediately disappeared, and in a very short time appeared again. I examined it more narrowly, and discovered it to be a group of the tails of the animalcula I have mentioned. There were more than fifty, each fastened by the tail to a lentil root. They resembled the bulb animalcule in the faculty of extending and contracting the body and tail, in forming a vortex in the water, and in directing the swimming corpuscula to the hole or bell. The vortex is formed by fibres or minute points proceeding from the edge of the bell; Fig. 6. pl. 1. But as this species is larger, so are the

points and the vortex also proportionally larger. When the bell was well opened, and the animal extended, the funnel appeared to terminate in the body by a little central hole, I. They all died in a few days, without seeming to multiply.

Six days afterwards, I saw a new spot formed upon the roots : I say formed, for it certainly was not there before. This afforded new peculiarities. One portion represented a tree in miniature. From the trunk proceeded several branches, which divided into smaller branches, and these into others, the size always diminishing. Each of these last, at the top, bore a bell animalcule. No object could be more singular or more agreeable. Every three or four seconds, the trunk contracted towards the root of the marsh lentil where it was attached ; and in the twinkling of an eye, drew in all the branches, all the twigs, and all the animalcules : but an instant after, the tree re-appeared in its former state, with its branches and its animals.

I detached the shrub from the marsh lentil root, by cutting the trunk. The animals, the branches, the twigs, no longer approached the stem ; but the stem, the twigs, and the branches, suddenly surrounded the animals ; and, at this moment, the vortices disappeared. Amidst these alternatives, the animals, detached

tached from their trunk, swam slowly through the water, drawing along the plant and its branches.

I visited the plant the following day. Instead of seeing a single animalcule come from the end of the branch, I saw two smaller; Fig. 7. Pl. 1. K; and these, as yet alone, were longitudinally marked with a very fine furrow, L. Upon examination, I found this to be the indication of a begun division. They begun to divide into two animalcula; so that each was double. In half a day, they were separated, and of their full size. The multiplication was very great. Two new branches were perceived to proceed from each old branch, and the reproduced animals fixed upon their summits. These, when full grown, divided like their parents, and remained implanted upon new stems; so that the multiplication of branches was proportioned to that of animalcula; and this double multiplication was, in the same manner, continued for several days.

During this, the shrub had extended its branches in such a manner, that its circumference became triple. The stalk and the branches encreased in the same proportion: but the death of the animalcula occasioned that of the plant. They begun to separate from the branches, as fruit separates from the tree; and, as they separated, the motive faculty was

lost. No more contraction or extension were seen; the vibration of the fibres, nor the consequent vortex: all semblance of animation was gone. Soon after, the figure of each animalcule was destroyed. The tree lived until it had lost all its animalcules. We may say, that it then neither lived nor vegetated, nor exhibited any mark of spontaneous or internal motion.

Although the animalcula in general died where they were produced, that is, at the extremities of their branches, or, at least, till the moment they are thence detached; it is not uncommon to perceive some swimming in the water, but always adhering to their stem, since we so term it. If, by chance, they touch a lentil root with this stem, they fix there, and give existence to a tree, bearing as many bell animalcula as branches. The animal upon the root soon divides in two, then in four, in eight, sixteen, thirty-two parts, &c. While those divisions take place, or the animals multiply, the parts of the tree multiply along with them. The number of twigs and branches supporting animalcula at the extremities, encreases. They all proceed immediately or mediately from the stem attached to the lentil root. This original stem has already encreased in length and thickness, and is truly the trunk of the microscopic tree.

This

This species of animalcule, whose mode of production Lewenhoeck could never divine, and which was unknown to Baker, is a polypus, very analogous to those called by M. Trembley, *polypes à masse*. Bonnet calls them *polypes à pennaches*. But these animalcula differ from those of M. Trembley; for the last produce the vortex, not by means of points, which they have not, but by the motion of the edges of the bell. Before the division, they lose the figure of a bell, and assume that of a round corpusculum: they are different, because they are not endowed with this alternate contraction and extension, divide unequally in two, at the division the vortex is suspended; and because the extension and contraction is not natural and periodical as in our animalcula, but occasioned by accidental influence, and is formed by the agitation of the water.

The longitudinal divisions, of which I have treated in this chapter and in the preceding, all begun at the anterior part of the animalcule, that is, the part which is before when the animalcule advances, and where, in many, the mouth is observed. But, in other animalcula, the division begins in the opposite part. One species resembles a sea hedge-hog in miniature; the figure is spherical, the surface of the body spinous, with long and pointed prickles. We distinguish the anterior from the posterior
part

part of the body ; for it proceeds before, and forms the vortex, by darting out the spines, while the other is behind : the rest of the points are in constant agitation, at least while the animal advances, or while it pleases. Another species resembles the segment of a sphere, or rather an hemisphere, entirely covered with points. Those in the concave part serve for fins ; others form a vortex ; these proceed from the section or plane of the hemisphere, which is always the anterior part of the animalcule. Thus, both are separate, and their separation seems to smoothe the body of the animalcule, which can remove the points at pleasure : it even appears that its agility or slowness, and the magnitude of the vortex, is regulated by the number of points in motion. These two species inhabit the tremella, and are of a colossal size, compared with other animalcula ; divide longitudinally, but the division begins at the posterior part. As usual, a faint cleft was first perceived, which extended as the body of the animal encreased, until it separated into two equal parts, which, before the division was finished, were two complete animalcula, equalling in size the whole from which they came. During the division the vortex continues, and the points proceed from the cleft, soon appearing similar to the old. The completion of this division requires a considerable time.

In

In an infusion of tremella we often see two minute balls, attached by several continued points, swim rapidly through the infusion, in irregular directions. Fig. 8. pl. 1. M. These balls are two animalcula upon the point of division. In a moment the one ball will separate from the other, notwithstanding the apparent strong adhesion. When it is of the proper size, there is a faint constriction, and each gives birth to two minute balls, which in their turn divide again.

I have seen groups of different round corpuscula in vegetable infusions. The group is sometimes formed of four animalcula, sometimes of five or more. The corpuscula commonly differ in size, according to the diversity of the groups. Fig. 8. pl. 1. They separate one after another from the cluster, and then divide into as many portions as there were corpuscula.

I isolated one of those animated corpuscula; I confined it in a glass the moment it separated from the group. The solitary corpuscules rapidly increased, and, when of the size of the group from which they were taken, there were seen several furrows in the body, which gradually changed to a new group similar to the old. This new group decomposed into other corpuscules or animalcula, which soon equal-
ed

ed in size and number the old cluster decomposed.

But the most surprising and the most extraordinary multiplication I have seen, is that of some animated globes, which roll like pellets in the infusions of water lentil, and are visible without the microscope. They are externally covered with tumours, formed of several animalcula, situated upon each other, and attempting to escape; Fig. 8. Pl. 1. N. Figure a body almost spherical, formed of concentric strata, each of which is an aggregate of animals. The animalcula composing the exterior or first stratum, separate from this sort of sphere: then is the second stratum laid open, which is composed of animalcula, and, by its separating, discovers the third. There are even strata inferior; so that the whole globe is decomposed, from the circumference to the centre. The globe has no other than a rolling motion; but the composing animalcula have the greatest activity. Each globe produces more than an hundred.

While the strata of the globes decomposed, I seized some animalcula, and isolated them. At first, each did not equal one hundredth part of the globe in size; but in three or four days, every one was as large as the whole. Their motion became slower, in proportion as they increased. When full grown or complete, they rolled

rolled with only the précession common to those globes. The surface of the stratum was at first smooth; it became unequal, and loaded with tumours. These tumours were so many distinct animalcula, which, after the separation, swam in the fluid. The animalcula of the second stratum did the same, likewise those of the successive strata, until the globe was entirely decomposed.

Such are the modes of production among animalcula propagating by division. They are in reality so many polypi, which I shall term the Polypi of Infusions, or rather Microscopic Polypi. Their kingdom is not bounded by the limited confines of infusions. I have at various times examined the water of ditches, dung-hills, marshes, pools, ponds, fountains, melted snow, rain, bath and medicinal water, both of mountains and of plains, and I can affirm, that I have found them all full, more or less, and infinitely varied with minute polypi. The different classes have their appointed times to originate and be destroyed. When one species of animalcula becomes very numerous, the greater part of its individuals perish, either by disease or by a violent death. An infusion swarming with animalcula to-day, will, in a few days, have almost none. Signor Corti has observed, that some maintain a fierce war. We know the ingenious method practised by that cetaceous

cetaceous fish, called by the northern nations, the Great whale. Having closed up a multitude of herrings in a proper situation, it gives the water a blow with its tail, so as to occasion a vortex of wide extent, and great rapidity: The monster presents its vast mouth and deep throat, which are soon filled with the herrings, drawn in by the vertiginous current. The carnivorous animalcula also form a vortex in the fluid, by means of the vibrating fibres. When so full as to seem more corpulent, they become indolent; but if they are kept fasting in distilled water, they become active, and employ themselves only in devouring the small animalcula which are presented. The transparency of their bodies permits us to see the devoured animalcula still continue to move.

All the kinds of division I have described, may be seen at all seasons, even the most cold and stormy. We cannot deny, that heat promotes the multiplication of animalcula, and that cold retards it; so that we may say, the time necessary for division is nearly proportional to the heat of the atmosphere. During the rigour of winter, several hours are requisite; in spring and in autumn, it is sooner performed; and in the heat of summer, a quarter of an hour is sometimes enough for entire completion. Upon this account, the summer infusions

fusions are much more populous than the winter.

CHAP. IX.

SEVERAL kinds of animalcula are viviparous and oviparous. One of the oviparous kinds is found in rice infusions. The size is of the largest among animalcula. The figure resembles a kidney bean, except that one extremity is curved like a pointed hook or beak; Pl. 1. fig. 9. O. I isolated one of those animalcula in a watch-glass, along with a small portion of infusion, which, for security, had been boiled a long time. In seven hours, the animalcule had a companion, so like itself, that it was impossible to distinguish them. I had no reason to believe it came from without, or was produced by the infusion. Half an hour after, I visited the glass, and discovered something new; that is, two minute spheres at the bottom of the glass, one of which was oblong; Pl. 1. fig. 9. P. It moved itself several times, and changed its place. The alternate motion and rest continued an hour and a third; then became quicker, and entirely local. The sphere began to swim slowly in the fluid; and, in a short time, the quickness of the smallest equalled

equalled that of the other two animalcula. The other sphere, Q, included a spherule so small, as to be with difficulty perceived, and would not have been noticed, but for a slight revolving motion upon itself, while the including sphere was tranquil. In several days, the integument burst, and the confined spherule escaped, expanded, and became slender at one extremity, to form the curvated beak. These animalcula, therefore, seem to originate from an egg, represented by this integument. The following day, there were more than forty-five animalcula in the glass, similar to the first that was isolated. There were also, at the bottom, several minute balls, partly round, partly elliptic; the former less, the latter larger. Of the round, one after another burst, and as many inactive ill-shaped animalcula came out, which soon acquired figure and motion. To leave no doubt, I confined some animalcula in a very small portion of water. In scarcely a quarter of an hour, one was, in my view, delivered of a round corpuscle. I saw more eggs laid in this manner. I counted eleven, proceeding from the posterior part of the isolated animalcula. These gave birth to an equal number of animalcula.

Two species of viviparous animalcula are carnivorous. They swallow their prey by means of a great vortex drawing them to the mouth.

mouth. We see them distinctly pass down the œsophagus, and enter a little bag, in their way to one larger, which apparently is designed for a stomach. Each animalcule has a long tail, the extremity of which is divided in two, and serves to fix it to the surrounding bodies. Upon the sides of the tail, are seen two oval substances; above these are two smaller, resembling two little narrow leaves; Pl. 1. fig. 10. R. The parts resembling leaves, are parts of the animal; the two other, are real animalcula. If kept in view with a magnifier, we see them expand, quit the mother, and swim. The opacity of this kind of animalcule prevented my seeing the foetus before exclusion; but, when it came to maturity, two small ones were observed, where the tail originates from the abdomen. I have never discovered more than two in each animalcule, although I have examined many. I have indeed seen three attached in the same manner to other animalcula; but these I judged to be of a different species, S. These two species generally inhabit the tremella of ditches.

Having taken an egg laid by an animalcule, I put it alone in a watch glass: afterwards, there were as many animalcula seen, as eggs laid by the animalcule excluded from this egg.

I isolated viviparous animalcula, taking them before they were completely developed, and

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still

still adhering to the body of the mother. After the necessary time, each isolated animalcule of the one species became parent to other two, and those of the other species to three.

These two genera of oviparous and viviparous animalcula, are, then, hermaphrodites in all strictness. I should conclude, that hermaphroditism, which at first seems confined to a few species, will extend far in the animated world.

We must say, that the original inhabitants of infusions originate from some germ, or minute egg, which passes from the air into the infusion, and becomes the principle or source of this numerous colony. This is confirmed by facts. I took the liquid from a number of eggs, so as to leave them perfectly dry; and thus they remained for ten days. I then put them in their native liquid, where they were soon hatched.

Every liquid does not equally favour the expansion of the eggs of animalcula: pure water, for example, is most unfit for it. This is no more a mystery. We constantly observe, that no animalcule almost ever appears in pure, or, for the greater reason, in distilled water; but I have found no fluid more favourable to the production of animalcula, than water with seeds infused, especially when the seeds begin to become putrid.

In

In my observations, I have particularly inquired, whether animalcula specifically varied as the infusions of vegetable seeds were different, so that each might have peculiarly its own; but I have found nothing constant. It is true, I have often found certain species of animals only, in particular kinds of vegetables, but I have frequently seen the reverse. The animalcula of the same infusion were different, at different times and different places; and it even is not uncommon to see this variety in two infusions made of the seed of the same plant. All this well agrees with the vast variety of animalcular eggs, scattered in the air, and falling every where, without any law. Doubtless, the microscopic polypi also proceed from a pre-organised principle: but is that an egg, a germ, or other similar corpusculum? If facts are required to solve this question, I ingenuously acknowledge, we have no certainty. The polypi die when the fluid is taken away, nor do they revive when it is restored. It may happen, that the germs, or pre-organised principle, are too transparent, or too minute, to fall under the senses. The idea, that animalcula come from the air, appears to me to be confirmed by undoubted facts. I took sixteen large and equal glass vases: four I sealed hermetically; four were stopped with a wooden stopper, well fitted; four with cotton; and the

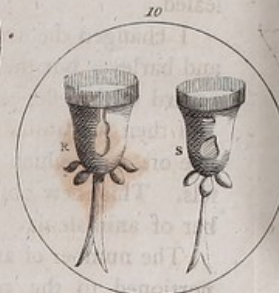
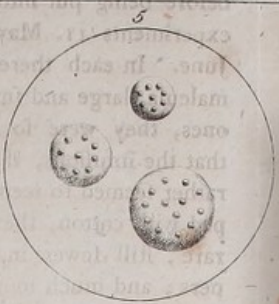
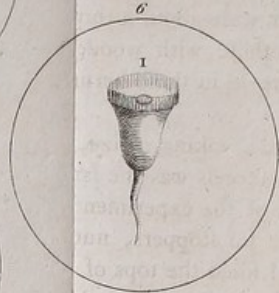
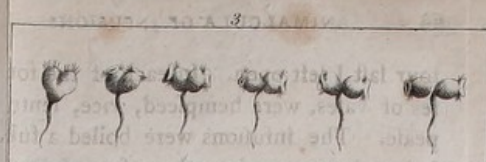
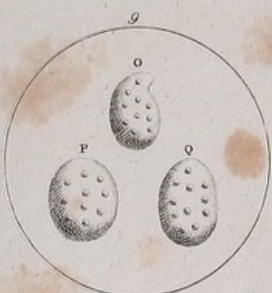
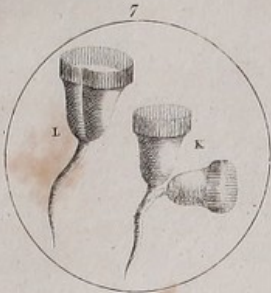
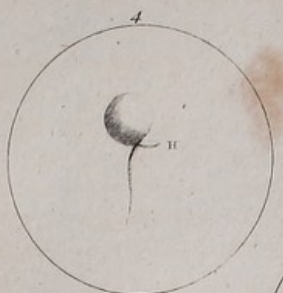
four last I left open. In each of the four classes of vases, were hempseed, rice, lentils, and pease. The infusions were boiled a full hour, before being put into the vases. I begun the experiments 11. May, and visited the vases 5. June. In each there were two kinds of animalcula, large and small; but in the four open ones, they were so numerous and confused, that the infusions, if I may use the expression, rather seemed to teem with life. In those stopped with cotton, they were about a third more rare; still fewer in those with wooden stoppers; and much more so in those hermetically sealed.

I changed the feeds, taking maize, wheat, and barley; but the success was the same with regard to the essence of the experiment.

I then substituted, for stoppers, nut and olive oil, with which I filled the tops of the vessels. This new obstacle diminished the number of animalcula.

The number of animalcula developed, is proportioned to the communication with the external air. The air either conveys the germs to the infusions, or assists the expansion of those already there.

Although the organisation of animalcula is so simple, that they appear but as granula invested by an integument, yet we discover many different parts, such as the fibrillæ for the vortex,
and



and the points for swimming. We see a mouth, an oesophagus, and a stomach, where there is observed a peristaltic motion, which puts the included aliments in motion. I should mention another organ which I have discovered in this new sort of obelisks, and which I supposed to be destined for respiration. It consists of two flaps, with a very minute globe in the centre. They are figured as the foot of elliptical animals above the mean line; Pl. I. fig. 11. I. The flaps move in motion, when the animal is at rest, but the motion is regular. Every three or four seconds they take a bladder becoming full of air, and then they fall, and the air is expelled as performed very slowly. The flaps are full, with this difference, that when they are full, the rays are empty, and when the rays are full, the flaps are empty. During the time the flaps are full, the globe is lean in the largest animals, and very long narrow ellipse U, figured on the right between the two flaps which is in common motion.

and the points for swimming. We see a mouth, an œsophagus, and a stomach, where there is observed a peristaltic motion, which puts the included aliments in motion. I should mention another organ which I have discovered in this new course of observations; and which I suspect to be destined for respiration. It consists of two stars, with a very minute globe in the centre. They are situated as the foci of elliptic animalcula above the mean size; Pl. 1. fig. 11. T. The stars are always in motion, whether the animalcula move or are at rest; but the motion is alternate and regular. Every three or four seconds they swell like a bladder, becoming three or four times larger; then they fall, and the inflation and efflation is performed very slowly. The same is seen in the rays, with this difference; when the globes are full, the rays are empty, or when the rays swell, the globes fall. During this, there is seen in the largest animalcula a very long narrow ellipse, U, situated on the side between the two stars, which is in continual motion.

OBSERVATIONS AND EXPERIMENTS
UPON THE
SEMINAL VERMICULI OF MAN AND OTHER ANIMALS,
WITH AN EXAMINATION OF THE CELEBRATED
THEORY OF ORGANIC MOLECULES.

INTRODUCTION.

THE subject I am about to treat, should form a chapter of the preceding treatise upon animalcula, from their analogy with spermatic vermiculi; but the sound arguments of M. Bonnet have changed my determination. I sent him the results of my experiments upon Animalcula, Spermatic Vermiculi, and Mould. He honoured them with his approbation, and advised me to separate the subjects; to treat of each in a separate dissertation. In this form he thought they would more readily fix the attention, and attract the curiosity of readers. I have found his advices salutary, and have profited by them; they have enabled me to extend the subject, and enlarge my researches upon Spermatic Vermiculi.

The

The reality of the existence of those animalcula, as well as the knowledge of their peculiar nature, is a subject as fit to engage the inquiries of a philosopher, as they seem to retreat from his penetrating examination. I may say, that, like the Proteus of fables, their figure and appearance change with the naturalists who attempt to study them. The seminal fluid of man, and of some animals, examined by Lewenhoeck, appeared to him full of animalcula; which he named vermes, from their resemblance in figure and motion. But they were soon considered by philosophers, as a phantom of the imagination, an illusion of the senses, or some imperfection of the microscope. They thought there was nothing real in what the Dutch philosopher had described.

By others he was judged with less severity. They agreed that there was a number of corpuscula in the seminal fluid; but they denied they were animals, and thought them unorganised particles, which, from their subtilty, were raised and evaporated sooner than the rest: thus, forming a fermentation and motion in the fluid, that created the idea of animation.

The celebrated Linnæus adopts this opinion nearly. He thinks the vermiculi are only inert molecules, swimming like oil in the seminal fluid, moving and darting in various directions,

as they are agitated or heated by the temperature of the fluid.

Messieurs Needham and Buffon, published their sentiments upon the question, which they have elevated to the subject of the animation of those microscopic beings ; and it would appear that their theories are directly opposite. The first thinks they originate from the vegetative power acting upon the seminal fluid, after it comes from the animal ; by which, it is necessitated to vegetate, to expand, to put itself in motion, and to change into beings not yet animated, but simply vital.

M. De Buffon, enamoured of his organic molecules, thinks he finds them in the vermiculi ; and, from a long detail of experiments and observations, endeavours to establish his theory upon the ruins of that of Lewenhoeck.

Who could imagine that so many disputes, and such opposition of sentiment, would arise upon a matter of fact ? I confess, this has singularly surprized me ; and I have often thought that the diversity of opinion arose less from essential difficulties, than from the fault of observers, who had not the proper methods of examination ; from a prejudice in favour of some theory, made their senses the cause of their errors ; or, finally, from want of sufficient practice in the difficult art of accurate observation. As I treated of a subject analogous to the history of
spermatic

spermatic vermiculi, I wished to study them, to discover, if possible, where the truth was. I applied to the enquiry with all the attention, care, and reflection, in my power; and for the greater certainty, endeavoured, first, to forget all that had been written upon the subject, acting as if I had been the original author of it. In controverted facts, I have always found this the safest method to avoid confounding the opinions of the philosopher with the responses of nature. After reaping a fruitful harvest of facts, I presumed to think, that I knew what had been seen by others. I then compared their results with my own, and allowed myself to give an opinion with respectful deference. I doubt not but my sincerity will be believed, when it is known that I have taken no side of the question, and that it was absolutely indifferent to me, whether my discoveries were corresponding, or contradictory to those of others.

The seminal fluids I used, were that of man and different quadrupeds. I did not neglect to examine that of the smallest animals. I employed the seminal fluid of man as recent as possible; that is, taking it from dead bodies while yet warm. I took the semen of animals the moment they were killed. I have frequently examined the seminal fluid of animals alive, and several times used that ejected during copulation.

pulation. The importance of these facts, in the illustration of this question, will be evident to the reader of the following chapters.

LETTER FROM M. BONNET.

“ In the Country, 16th October 1771.

“ I agree to your request, my esteemed
“ correspondent, and delay not to inform you,
“ that I have received the excellent letter I
“ owe to your friendship for the *Palingenesist*.
“ Excellent book, I should say: it is almost
“ such in size: and I shall add it to those of
“ the same kind with which you have already
“ enriched my library. I could not resolve to
“ engross to myself a work, almost every line
“ of which demands serious attention. I de-
“ termined to revise it with an observer wor-
“ thy to understand and to follow you: I
“ speak of my excellent friend, the illustrious
“ discoverer of the polypus. Yesterday, we
“ perused it together; and I am unable to tell
“ you how much we are enchanted with it.
“ When you give me time, I shall detail your
“ observations along with you. But I ought not
“ to avoid remarking, how desirous M. Trem-
“ bley and myself are, that you should publish
“ separately your observations and experiments
“ upon Infusions, Vermiculi, and Mouldiness,
“ and

“ and upon the other subjects of the same na-
“ ture, which you have treated with such
“ learning and success. They are too import-
“ ant not to merit and require a separate im-
“ pression. They would make an admirable
“ appearance in detached treatises, and would
“ thus more firmly fix the attention of ama-
“ teurs. Send me them when they come
“ from the press. M. Trembley and I will,
“ under our own inspection, cause them be
“ translated into French as soon as possible. I
“ doubt not that you will comply with our re-
“ quest. This new treatise upon infusions,
“ will form an appendix to the former. We
“ may even reprint the French translation of
“ the last, and place it at the beginning of the
“ new work. Think of all this, and let me
“ know. I mentioned it to M. Trembley.

“ Your tracts upon infusions, &c. will, in
“ my opinion, be excellent rhetoric for the
“ use of naturalists, which, I assure you, in
“ my view, is not the least merit of your learn-
“ ed researches.

“ You judged right, that I had associated
“ M. De Saussure in our amusement ; and he
“ was not once absent for fifteen days. He
“ is now absent at Lyons ; but we shall regale
“ him upon his return.

“ Still a word upon infusions.—Behold the
“ poor Epigenesist reduced to an impalpable
powder ;

“ powder ; and you have no less pulverized
 “ his friend De Buffon. I have read nothing
 “ upon spermatic vermiculi with so much satis-
 “ faction ; and I congratulate myself that I
 “ induced you to study them. Your observa-
 “ tions are most valuable, in my opinion : they
 “ are both new and accurate. I would reani-
 “ mate the worthy Lewenhoeck. What plea-
 “ sure would it afford him, to see the attack
 “ of M. De Buffon so well repelled ! I hope
 “ he will now be polite enough to acknow-
 “ ledge, his microscopes have not done him
 “ justice, and yield to your evidence.

“ Your mould is almost as new as your
 “ spermatic vermiculi - - - - But I do not ob-
 “ serve that I begin to answer, in detail, your
 “ interesting letter ; and, if I continued, you
 “ would not so soon know that I had received it.
 “ I therefore end, renewing the assurance of
 “ my inviolable regard.

“ BONNET.”

CHAP.

CHAP. I.

THE seminal fluid taken from a dead human body, resembles coagulated milk ^a. When examined with a magnifier, the cause of its opacity is not discovered; but, when it begins to dissolve, and assumes the colour of soapy water, if examined with a magnifier of small power, the irregular parts seem to be in an indistinct, slow agitation. I observed, that those parts were moved by corpuscula infinitely more minute, of a globular figure. Each had a sort of filament, or short appendage; Pl. 2. Fig. 1. A. It is evident, that the more gross part of the seminal fluid is put in motion by globular corpuscula; for there is no motion, unless they are in agitation. The corpuscula have two motions, one oscillatory, from right to left, and from left to right, curving the appendage from one side to another. The other is progressive: the vermiculus transports itself by oscillation. When we consider this mode of moving, we would imagine them blind. They strike against every obstacle, and, when amidst a number, make a thousand contortions to escape, at last taking that way where they feel least

^a Dec. 21. The thermometer 48° above 0.

least resistance. Thus they are in continual motion. In twenty-three minutes, the motions of oscillation and progression had diminished; and, in an hour and a half, it had lessened so much, that a very small number of corpuscula preserved any appearance of motion. In general, the progressive motion ceases before the oscillatory; so that, at last, the corpuscle merely bends from right to left, and reciprocally. They continue fixed to the same spot, until the oscillatory motion insensibly dies away.

After all motion is gone, the corpuscles remain entire in the fluid, and then they are better seen than even when the liquid is diluted with water. Each corpuscle is not properly globular, but elliptic; and the appendage is not only longer than it appears, but the breadth is not equal throughout like a thread, but increasing as it approaches the body; Fig. 1. B. It was impossible to discern where the filament terminated, being so much immersed in the fluid. When the corpuscula have ceased to move, the appendage remains extended in a straight line, or with little deviation.

When the seminal fluid has been kept for a day, or indeed for less, in a watch glass, it becomes transparent, although preserving its original viscosity. It deposits a sediment of whitish matter, resembling a mass of slender rags.

The

The feminal fluid having dissolved later than that of the preceding observation, I put some particles in rain water ^a. The shapeless corpuscula swimming in the liquid, with the double motion of oscillation and progression, became motionless when touched by the rain water. Other waters mixed with the feminal fluid, produced the same effect as that of dunghills, rivers, snow, ice, and even distilled water. I have found nothing but saliva preserve the motion of the corpuscula. It may be used indifferently, either from the mouth, or when it is cold. I have often employed it to continue my observations. The ovular corpuscula I then found; and the phenomena exhibited, were precisely the same with those of the preceding observation.

When the liquid drop dries up, all the corpuscula, without exception, become motionless. And as it dries first at the circumference, that is, where it is thinner, and advances to the centre, the corpuscula first becoming motionless, are those at the edge, then those of the interior part, and lastly those in the centre of the drop. If a drop of saliva, or of the feminal fluid, is put upon that which is dried, the corpuscula do not recover motion, though the humidity continues long. The corpuscula of this observation became sooner motionless.

In

^a January 11. The thermometer 36° above 0.

In fourteen minutes they were languid; in three quarters of an hour there was complete repose. At last they had no progressive motion, although a remnant of the oscillatory continued; so that when the two motions ceased, most of the appendages remained extended in a straight line.

The seminal fluid taken from a dead human body, was chiefly coagulated ^a. It was then peopled by the corpuscula I have described. In one of the preceding observations, it appeared to me, that there were some larger corpuscula among the rest. I apprehended I was deceived; that this difference of size might arise from minute portions of the semen attached to the corpuscula: but I was convinced this could not be the case, because, when the seminal fluid was completely dissolved, the corpuscula retained the same size, although I passed them through another fluid; Fig. 2. C. The motion ceased two hours after the semen was taken from the body.

The seminal fluid of man was like milk ready to coagulate. I took a small portion for examination ^b. It presented a singular phenomenon. I saw four corpuscula attached by the filaments to a clot, which dissolved. They seemed to me to make every effort to disengage

^a February 18. The thermometer 49° above 0.

^b March 8. The thermometer 51° above 0.

gage themselves from this incumbrance, by many motions and contortions; but they remained fixed. The filament sometimes described a curve, and was sometimes extended in a straight line. Amidst the struggles, one extricated itself from the clot, and begun to swim in the fluid. Like the rest, it had the motions of oscillation and progression. The other three corpuscula did the same, one after another; and, by degrees, detached themselves entirely from the clot.

The novelty of this phænomenon made me desirous to take other molecules of semen not completely dissolved, to see if I could find other ovular corpuscula in the same situations. In some, the corpuscula were free, and I saw them swim in the place of the clot dissolved; others I saw, attached by the appendage to that part of the clot yet entire, and endeavouring to disengage themselves by all the contortions I have just mentioned. I saw, that when entirely detached from the spermatic molecules, they liberated themselves, and swam about in the fluid. I found something more. One of the clots was partly in filaments; many ovular corpuscula were seen about the filaments, which, notwithstanding their endeavours, were unable to set themselves free. In this semen the corpuscula lived two hours and a half.

G

When

When I took the feminal fluid from a dead human body, most part seemed dissolved ^a. Many of the corpuscula surpassed the common size. In this experiment, my object was to search, with all possible care, for what I had seen in the course of the preceding observations. I saw all I had observed, excepting the phenomenon of the corpuscula attached to clots, which could not happen here, as the semen was perfectly coagulated. Some of the corpuscula continued to move for three hours.

In the examination of this feminal fluid ^b, which was at first a little thick, I discovered, by chance, a method of seeing, with more convenience and precision, the figure of the corpuscula and their appendages. I put a clot of semen upon a talc slider; as the thickness prevented my seeing it with accuracy, I swept it with a small pencil, but the talc was not so clean, that some little portion of the spermatic matter did not remain. I cannot tell what induced me to examine the talc again with the microscope; but I there found what I had no idea of discovering. I saw scores of the corpuscula very distinct, although motionless, because they were dry. They were separate, without any mixture with the spermatic matter: the appendage of some was straight, of others curved, and

^a March 27. The thermometer 54° above 0.

^b April 15. The thermometer 60° above 0.

and nearly of the same length in all; that is, about six times the length of the body, the extremity not much pointed, but thicker as it approached the body; and it was distinctly seen, that the corpuscle made but one whole with the appendage, which, in the thickest part, is thrice as small as the body, and even more. Each corpuscle is somewhat like the red globules of blood, but smaller. The appendage, as the corpuscle, seemed to be composed of a homogeneous matter; Fig. 2. D.

After this fortunate event, I frequently wiped the talc whereon I had put some drops of semen, and I constantly saw the corpuscula with the same precision. I may add, they remained dry upon the talc, without any alteration of figure, for several days. In this semen they became motionless in two hours and a half.

I wished to proceed rigorously with my researches, as it appeared the results I had hitherto obtained, were insufficient to bestow the character of real animals upon the ovular corpuscula. We have not effectually had that congress of characteristic marks to decide their animality. Doubtless, we may name them moving corpuscula, or possessing a spontaneous motion; for the testimony of our senses will not permit us to suppose, this double motion of oscillation and progression, the effect of any ex-

ternal cause. The sequel of the work will make us more perfectly acquainted with their nature.

These observations have likewise demonstrated another truth, that the duration of motion, after the corpuscula come from the body of the animal, depends, in a certain degree, upon the temperature of the atmosphere. At 36° above 0, all motion was gone in three quarters of an hour; at 47° , in an hour and a half; at 49° , in two hours; at 51° , in two hours and a half; at 54° , in three hours; and at 59° , only after the elapse of three hours and a half.

From this I remarked, that the motion of the corpuscula continued longer, as the temperature of the atmosphere encreased. Examining the human semen in the warmer months, to learn whether the phænomena I had already witnessed might be then observed, I had the pleasure of seeing them again in the same manner. I constantly observed, that as the heat was greater, the duration of motion encreased; so that in the middle of summer, when the thermometer had ascended to 81° , our corpuscula continued to move for seven and three quarters, and even for eight hours.

While this heat continued, I varied the experiments. I exposed a portion of the seminal fluid, taken from a man, to the air of an apartment where the thermometer stood at 82° ; I
put

put another portion in a cave, where the heat was 65° ; and a third portion in an icehouse, where the thermometer stood at 42° . Here the corpuscula continued to move half an hour; in the cave, four hours; and in the apartment, eight hours. Corpuscula abounded in each drop of the fluid, and, although the drop was small, it included an innumerable multitude.

After examining the feminal fluid of man, I examined that of the horse. No method of observing it could be more convenient, as I always obtained it at the moment of copulation. I made use of the semen of different horses. The first I observed, was without clots, very fluid, and of a light cinder colour ^a. The corpuscula, in motion or swimming, were not so numerous as those of human semen. There was no difference in the size of human corpuscula and those of the horse, only the latter seemed a little larger. The appendage is more visible, probably because it is thicker. It is distinctly and completely seen, although immersed in the feminal lymph; Fig. 3. Their oscillatory motion is not so great as that of the human corpuscula, which may be the reason they advance further in the same time. Their progressive motion is quicker, and sometimes saltatory. The size of all is not the same, nor do all die at the same time. Some

G 3

move

^a March 11. The thermometer at 43° above 0.

move more than a quarter of an hour after the rest. The greater part died in three quarters of an hour, and sometimes they did not live above half an hour. When the motion ceases, they remain entire, with the appendages extended in a straight line, or a little curved. The semen of the horse, is very glutinous and filamentous ^a. We see corpuscula moving about, attached by the body, and in particular by the filament, to various irregular substances mixed with the fluid; and being unable to disengage themselves, the substances are sensibly agitated by their motion. More than once corpuscula are seen attached together, which might induce us to think them larger than the rest; but, with attention, the two separate appendages are soon perceived, each oscillating by itself; and if the observation is continued, it is not uncommon to see the bodies divide, and thus form two distinct corpuscula. I am well assured it is not an optical illusion, with regard to the corpuscula of a different size, but a positive fact. I may say there were some a third larger than the rest. I made this observation before, upon the 11 of March.

I made many attempts to follow the diminution of their motion. It is in proportion to the time the seminal fluid has been exposed to the

^a March 22. The thermometer 5° above 0.

the air. Scarcely has it proceeded from the animal when the corpuscula are seen in great agitation ; darting forward with vast rapidity, and oscillating to both sides. This alacrity insensibly diminishes ; so that if they at first describe a certain given space in a second, in a quarter of an hour they do not traverse a third of it in the same time. The arcs of oscillation successively become smaller ; and at last the motion of the corpuscula is reduced to a languid vibration of the body and appendage, without any change of position. The tremor soon disappears, and the appendage remains extended in a straight line, after the manner of those beings.

Many aquatic animals of the apodal class transport themselves by the contortions of their members, which vibrate and oscillate from side to side ; and indeed one may positively affirm, that the anterior part of the body is pushed forward, and moves progressively by the contortions and oscillations of the posterior part. I paid the most strict attention to see whether the anterior part of the corpuscle was pushed forward by the oscillations of the appendage. When the motion is very rapid, it is impossible to observe it distinctly, from the quickness of the mutual vibrations of the body and appendage ; but when it relaxes, we perceive with facility the mode by which they advance. And

it is the same with that of the aquatic animals I have just mentioned. When the oscillation of the appendage ceases, the progressive motion of the body ceases; but the body begins to move when the oscillations of the appendage recommence. I made this important observation, not only upon the corpuscula in the seminal fluid of the horse, but upon those in the semen of all the animals that I shall afterwards name. The motion in this observation did not continue above an hour and a half.

The corpuscula in the two portions of the seminal fluid of the horse, mentioned before, were very numerous; they were rare in the last ^a. These were perfectly similar to the others both in figure and properties. Their motion continued two hours.

I examined the semen of other six horses. The corpuscula were similar to the preceding, without difference in figure or number; I therefore think it needless to stop and describe them. When the seminal fluid is mixed with water, or even with saliva, all the corpuscula instantaneously become motionless.

The seminal fluid of the bull, contains moving corpuscula, in numbers surpassing those in the seminal fluid of man ^b. The appendage is longer

^a May 2. The thermometer 64° above 0.

^b March 30. The thermometer 57° above 0.

longer than that of the human seminal corpuscula, and the body also seems a little larger; Fig. 4. The whole length, to the extremity of the appendage, is distinctly seen, though deeply immersed in the seminal matter, which is perfectly fluid, and of a whitish colour. The progressive motion is performed while the appendage oscillates. This motion is different from that of the human corpuscula; it is much more rapid, and suspended for short intervals; which is not to be seen in the corpuscula of human semen. The small quantity of the semen of the bull I had upon this occasion, prevented me from extending my researches farther.

I was more able to satisfy my curiosity upon another occasion, when I had a greater quantity^a. Beside the phænomena already described, I also remarked, 1. That the corpuscula not only swam horizontally, but rose and sunk in the semen, as fishes do in water: 2. When the semen dried, the motion of the corpuscula was irrecoverably gone: 3. In times of equal heat, the motion was quicker than of those of man, or the horse: 4. The mixture of every kind of water, and even of saliva, with this semen, was fatal to the corpuscula.

The same phænomena were seen in the seminal fluid of other three bulls. I had the semen

^a May 30. The thermometer 68° above 0.

men from those three at the moment of copulation.

Having opened the testicles of a dog, alive and in perfect health, the epididymis was full of semen ^a. It was a little viscous, very thick, and of a cinder colour. The thickness prevented my seeing the corpuscula accurately. I only perceived a confused agitation of the substance, which ceased upon mixing the semen with water: the corpuscula, become motionless, were distinctly seen. I then suspected, that they were the cause of the agitation, which the mixture with water had destroyed. My suspicions were confirmed upon mixing saliva with the semen; for the tumultuous motion continued. I saw it was produced by the corpuscula, the number of which was prodigious. The reader will not therefore be surprised, if I say nothing of their figure, size, motions, &c. since I would have to repeat all I have said of the corpuscula in the human semen; for the corpuscula in the semen of the dog perfectly resemble those of the human semen. They became motionless in three quarters of an hour.

The semen of a dog, I procured during the moment of copulation, was a little viscous, and like muddy water ^b. The moving corpuscula
were

^a Feb. 14. The thermometer 48° above 0.

^b April 27. The thermometer 61° above 0.

were not so much immerfed, as in that taken from the epididymis; and it was unnecessary to mix it with any other fluid; to fee the corpuscula. Every part was fufficiently vifible, and their motion very rapid; but this rapidity infenfibly diminished; and, two hours after the fluid came from the animal's body, all motion was gone; the corpuscula remaining with the appendages extended in a ftraight line.

I repeated thefe experiments upon the femen of other five dogs, and found the results perfectly alike.

Nine hours after a ram was killed, I opened the tefticles, and expreffed the feminal fluid into the glafs of a watch ^b. All the corpuscula were motionlefs; they were larger, and thus were more eafily feen than thofe of the dog, or of man.

Having taken the feminal fluid from the tefticles of a living ram, I found all the corpuscula in motion ^c; the oval part, or body of each corpuscle, fometimes immerfed itfelf in the fluid, and fometimes efaped the eye, then re-appearing on the furface. Their properties refembled thofe of other corpuscula, if we add a gentle vibration and a greater activity. When the corpuscle contracted itfelf, the appendage was lefs curved from right to left; but

^b May 10. The thermometer 66° above 0.

^c June. The thermometer 66° above 0.

but the total duration of motion was much shorter than in the others. Although the heat of the atmosphere raised the thermometer to 66° , they all ceased to move in half an hour.

I examined the epididymis of another ram still alive. The quantity of seminal fluid was so great, as to fill two thirds of a watch glass. Viewed with the eye, the liquid seemed in continual motion, although the glass was situated upon an immoveable plane. I examined a drop with a magnifier of small power; the whole appeared in motion, and the microscope proved this motion to be produced by the agitation of the corpuscula alone. Their motion ceased in an hour.

After examining the seminal fluid of all those warm-blooded animals, I thought of examining the semen of some whose blood is cold. I begun with fishes; and, for this purpose, delayed until they spawned. I took the milt of a living carp, and expressed the fluid into a vessel. The fluid was tenacious, thickish, and of a dull white. Many moving corpuscula were seen; but I could not obtain distinct vision, until the density of the semen was diminished by water. Then I had new objects: the corpuscula were no longer composed of two parts, a body and an appendage, like the corpuscula of other fluids; but were a united whole, resembling minute spheres, and apparently solid; Pl. 2. fig. 5. Those spherules,

spherules, of a darkish colour to the naked eye, swam through the liquid in every direction; advanced, retreated, mutually avoided each other, immersed themselves deep in the fluid, and ceased to move in a moment. In a word, they had many of the motions and properties of animalcula. The number was infinite, and they continued their course a quarter of an hour: then they stopped, and moved no more. I repeated the experiment five times, expressing anew the fluid of the milt, and the consequences were the same. If the liquid mixed with the semen was fresh, such as water, saliva, and the like, I was sure of putting the spherical corpuscula in motion, or rather of increasing their motion. But if the mixing liquid was ardent or corrosive, instead of being increased it was destroyed.

At that time, I had two water newts. I cut the testicles of a male into several pieces, and expressed the liquid, which was thick and glutinous. I exposed it to the microscope, and it changed to a mass of long slender corpuscula. Some were extended in a straight line, others curved; some solitary, others entangled like a skein of thread. I examined those alone, as the most easily distinguished. They are not throughout of equal thickness. Each corpuscle is composed of a body, and a very long appendage; Fig. 6. They moved with difficulty,

culty, the greater part of the body being immersed in this viscous substance. I resolved, likewise, to dilute the mass with common water; by which means, I saw the corpuscula traverse the whole liquid. As the fluid was at perfect rest, and as I saw no external cause to act upon the corpuscula, I was inclined to think this motion spontaneous, and peculiar to them. I then adopted an idea, that I had discovered the efficient cause of the motion; for, looking steadfastly, I saw two rows of minute points cover the whole appendage of each corpuscle, moving like the most minute oars; Fig. 7. During this motion, the situation of the corpuscula changed; but, when they ceased, the corpuscle also ceased to move.

When the mixture of semen and water dried up, the motion of the corpuscula was irrecoverably lost, although again wet with the liquid: but those in the mixture which did not dry, ceased to move in an hour.

I repeated similar experiments upon the fluid taken from the testicles of other newts, and I had the same results: but, upon diluting the fluid, I often saw the corpuscula collect in numbers, place themselves parallel to each other, and then bend into a circle. When all collected, they bent themselves so much, that the point of each appendage almost touched the opposite extremity of the body. In this position,

position, they begun to revolve around a common centre. The figure was somewhat like a funnel. They preserved this vortical motion for some time.

I found the corpuscula not only in the testicles of newts, but also in the *vasa deferentia*. The vessels resemble two very white pipes: they are situated about the middle of the vertebræ of the kidneys, one upon each side. One end is fixed near the head of the animal; the aperture of the other is in the part through which the excrement passes. The vessels are almost always full of seminal fluid, but in particular while the males fecundate the eggs of the females. The semen is white like milk: the number of corpuscula it contains is so great, that the fluid part is small, compared with the mass they form. The corpuscula are perfectly similar to those in the seminal fluid of the testicles: at the same time with this difference, that they need neither water nor any other liquid to encrease their motion. In the semen, they naturally move with the quickness of the corpuscula in the fluid from the testicles, diluted with water.

The corpuscula of the *vasa deferentia* retain motion longer than those of the testicles; and the duration of their motion equals that of the human corpuscula, those of the horse, &c. I have

have always found this kind of corpuscula in male newts at every season of the year.

By expressing the seminal fluid from the testicles of frogs, it may be seen, that it is full of corpuscula. They are shorter than those of the newt. They possess loco-motion as they advance. They vibrate, making gentle contortions of the body ; are of a long elliptical figure, Fig. 3. ; and very soon cease to move.



CHAP. II.

AFTER this course of observations upon the seminal fluid of man and different animals, I thought of reading and examining, what Lewenhoeck and De Buffon had said upon the subject, as they had made it their particular study. Several years had elapsed since I read their discoveries upon Seminal Vermiculi, so that only a general idea of their opinion remained. I even wished to proscribe that remembrance, and, in these researches, to have my mind as a pure tablet, the more fit to receive the real impressions of what my eyes might behold, without any danger of alteration, by adding any part of the observations of others.

I shall begin with Lewenhoeck : and, that the reader may have before him the real sentiments
of

of this naturalist, and that he may compare his observations with mine, I think it necessary to transcribe some of the chief passages, where he speaks of Spermatic Vermiculi. Buffon has, before me, employed part of the passages, to compare his own observations with those of this illustrious observer; and here I have the pleasure of following his example. This excellent Dutch philosopher wrote, 1677, to Lord Brouncker, President of the London Royal Society, to communicate his microscopical discoveries upon the Human Semen.

“ Postquam excellentissimus dominus pro-
 “ fessor Cranen me visitatione sua sæpius ho-
 “ norabat, litteris rogavit dominus Ham cog-
 “ nato suo, quasdam observationes mearum vi-
 “ dendas darem. Hic dominus Ham me se-
 “ cundo invisens secum in lagenula vitrea se-
 “ men viri gonorrhœa laborantis sponte distil-
 “ latum attulit, dicens se post paucissimas tem-
 “ poris minutias (cum materia illa jam in tantum
 “ esset resoluta, ut in fistula vitrea immitti posset)
 “ animalcula viva in eo observasse, quæ cauda-
 “ ta et ultra 24 horas non viventia judicabat.
 “ Idem referebat se animalcula observasse mor-
 “ tua post sumptam ab ægroto terebinthinam.
 “ Materiam prædicatam fistula vitrea immis-
 “ sam, præsentem domino Ham observavi quas-
 “ dam in ea creaturas viventes: et post decur-
 H “ sum

“ sum duarum aut trium horarum eandem so-
“ lus materiam observans, mortuas vidi.

“ Eandem materiam (semen virile) non æ-
“ groti alicujus non diuturna conservatione cor-
“ ruptam, vel post aliquot momenta fluidiorem
“ factam, sed sani viri statim post ejectionem, ne
“ interlabentibus quidem sex arteriæ pulsibus
“ sæpiuscule observavi, tantamque multitudin-
“ em in ea viventium animalculorum vidi, ut
“ interdum plura quam mille in magnitudine
“ arenæ sese moverent : non in toto semine, sed
“ in materia fluida crassiori adhærente ingentem
“ illam animalculorum multitudinem observa-
“ vi, in crassiori vero feminis materia quasi sine
“ motu jacebant, quod inde provenire mihi i-
“ maginabar, quod materia illa crassa ex tam
“ variis cohæreat partibus, ut animalcula in ea
“ sese movere nequirent : minora globulis san-
“ guini ruborem adferentibus hæc animalcula
“ erant, ut judicem millena millia arenam gran-
“ diorem magnitudine non æquatura. Cor-
“ pora eorum rotunda, anteriora obtusa, poste-
“ riora ferme in aculeum desinentia habebant :
“ cauda tenui, longitudine corporis quinquies,
“ sexiesve excedente et pellucida crassitiem ve-
“ ro ad vigesimam quinquam partem corporis
“ habente prædita erant : adeo ut ea quoad
“ figuram cum Cydaminis minoribus longam
“ caudam habentibus optime comparare que-
“ am : motu caudæ serpentino, aut ut anguil-

“ læ

“ læ in aqua natantis progrediebantur : mate-
 “ ria vero aliquantulum crassiori caudam octies
 “ deciesque quidem evibrabant antequam lati-
 “ tudinem capilli procederent. Interdum mi-
 “ hi imaginabar me internoscere posse adhuc
 “ varias in corpore horum animalculorum par-
 “ tes quia vero continuò eas videre nequibam,
 “ de iis tacebo ^a.”

Those observations were accompanied by others, written by Lewenhoeck to the Secretary of the Royal Society, 1678. He composed them, because some one had suggested to him to examine the feminal fluid of animals.

“ Si quando canes coeunt” (Lewenhoeck an-
 “ swers the Secretary) “ marem a fœmina statim
 “ seponas, materia quædam tenuis et æquosa
 “ lymp̃ha scilicet spermatica ex pene solet pau-
 “ latim extillare : hanc materiam numerosissi-
 “ mis animalculis repletam aliquoties vidi, e-
 “ orum magnitudine quæ in femine virili con-
 “ spiciuntur, quibus particulæ globulares ali-
 “ quot quinquagies majores permiscebantur.

“ A cuniculorum lymphæ spermaticæ gut-
 “ tulam unam et alteram e fœmella extillantem
 “ examini subjeci, ubi animalia prædictorum fi-
 “ milia sed longe pauciora comparuere.”

In the same year 1678, Lewenhoeck also communicated to the Royal Society, the animalcula he had found in the semen of the Dog.

H 2

“ Seminis

^a Philosophical Transactions, N^o. 141.

“ Seminis canini tantillum microscopio ap-
 “ plicatum iterum contemplatus sum in eoque
 “ antea descripta animalia numerosissima con-
 “ spexi. Aqua pluvialis pari quantitate adje-
 “ cta iisdem confestim mortem accersit. Ejus-
 “ dem feminis canini portiuncula in vitreo tu-
 “ bulo, unciae partem duodecimalem crasso
 “ servata, sex et triginta horarum spatio con-
 “ tenta animalia vita destituta pleraque, reli-
 “ qua moribunda videbantur.”

Lewenhoeck confirmed his discoveries during
 the following years, and made additions to
 them. In a letter to Mr Wren, he thus ex-
 presses himself respecting the spermatic vermi-
 culi of Frogs.

“ Hic animalculorum numerus erat tantus
 “ ut credere subiret ad quodvis femellæ ovu-
 “ lum a masculo emitti forte talium 10,000 ta-
 “ lium animalculorum, quæ in femine ejus
 “ continentur.”

In the year 1699, Lewenhoeck wrote to the
 Royal Society of London upon his theory of
 Seminal Vermiculi. He believed them to be
 male and female.

“ Si porro his addamus, quod me antehac
 “ in observationibus meis animadvertere cen-
 “ fui inter animalcula ex femine virili quædam
 “ apparuisse, quæ aliquantulum ex se mutuo
 “ differre videbantur, unde concludere non ve-
 “ rebar, alterum genus mares alterum vero
 “ fæmellas

“ fæmellas repræsentare, atque si cogitemus i-
 “ dem in omnibus feminibus masculinis locum
 “ habere, nullus video ” - - - -

There is another passage coinciding with this.

“ Sed jam ubi in feminibus masculinis ani-
 “ malium avium, piscium, imo insectorum, re-
 “ peri animalcula, multo certius esse statuo,
 “ quam antea, hominem non ex ovo, sed ex
 “ animalculo in femine virili oriri : ac præ-
 “ fertim cum reminiscor, me in femine mascu-
 “ lino hominis et etiam canis vidisse duorum
 “ generum animalcula. Hoc videns mihi ima-
 “ ginabar, alterum genus esse masculinum al-
 “ terum fæmininum.”

In 1701, Lewenhoeck wrote to the Royal Society in these words :

“ Die Julii vigesima septima, circa nonam
 “ horam vespertinam, accepi testiculos juvenis
 “ Arietis. Cum vero lanius hisce testiculis pri-
 “ mam detraxisset cutem seu membranam, ego
 “ vicissim quoque eos altera privavi membrana,
 “ ut hac ratione vasa feminifera nuda visui ex-
 “ posita jacerent. Primo ergo aperui vasa fe-
 “ minalia in testiculi parte exteriora sita, iis-
 “ que exemi semen masculinum (quod nudo
 “ observatum oculo album repræsentabat colo-
 “ rem) illudque microscopio apposui, atque hoc
 “ pacto oculo admovi, quando mihi animalcula
 “ feminalia tam stupendo apparuere numero,

“ ut vix fidem apud quemquam, nisi testem o-
 “ culatum, inventurus sit. Hæc animalcula
 “ nubium in morem, integris agminibus, inter
 “ se vagabantur, natabantque, quorum mul-
 “ tas eodem tendere natatu videbantur, ut mox
 “ aliquot millena sese ab uno agmine separantia
 “ alteri sese agmini adjungebant, illudque sequi
 “ videbantur.”

Lewenhoeck adds :

“ Hæc vero animalcula nuper a me obser-
 “ vata caudas habent juxta corpus crassiores,
 “ atque sensim fiunt tenuiores, adeo ut eorum
 “ extremitates ubi materia, cui animalcula in-
 “ sunt, atque innatant, paulo densior est, visum
 “ plane effugiant : atque sic horum animalcu-
 “ lorum caudæ fabrica plane convenit cum o-
 “ mnium piscium caudis.”

What respects the properties of Spermatic Vermiculi, is presented in these results by Lewenhoeck.

“ Quotiescumque animalcula in semine mas-
 “ culino animalium fuerim contemplatus, at-
 “ tamen illa se unquam ad quietem contulisse,
 “ me nunquam vidisse mihi dicendum est, si
 “ modo sat fluidæ superesset materiæ, in qua
 “ sese commode movere poterant : at eadem
 “ in continuo manent motu, et tempora quo
 “ ipsis moriendum, appropinquante motus ma-
 “ gis, magisque deficit usque dum nullus prorsus
 “ motus in illis agnoscendus sit.”

From

From these quotations it may easily be seen, that Lewenhoeek and myself have remarked the same facts in the Human semen. This observer, under the appellation of Animals, or Spermatic Vermiculi; and I, under that of Moving Corpuscula. We both agree, 1. upon the figure assigned to the corpuscula in the seminal fluid of man, the ram, the dog, and the rabbit. Describing the corpuscula, I have said, they seemed composed of two parts, a body and an appendage. Lewenhoeek also acknowledges the existence of those parts. 2. We agree concerning the size of the body, the length, the figure, and proportions of the appendage; which will further appear, from the designs he has given of the spermatic vermiculi of man, or the corpuscula of which I have spoken. 3. We have each discovered a prodigious number of beings in the semen: we have remarked the size of some different from that of the rest: we have allowed them the same properties: we have said, their motion in swimming was serpentine, like that of eels; that it was uninterrupted; that towards the end of their lives, they became languid; that the vermiculi in the grosser parts of the fluid met great opposition to their progress. But I have observed all this in the preceding chapter. We have both remarked, that rain water deprived the canine vermiculi of motion. I have also found, that

this effect was produced upon those of man, even by other kinds of water, as dunghill, ice, snow, and river water. I have constantly observed, that when motion ceases, the appendage never encircles the body, but always remains extended in a straight line, or in one very little curved. This had been remarked by Lewenhoeck, as appears from his engravings of the feminal vermiculi of the dog and the rabbit. When he means to represent them dead, he exhibits the appendage extended; if he means to represent them alive, it is with the appendage curved.

The moving corpuscula found by me in the semen of man, the horse, the bull, the dog, the rabbit, the ram, newts and frogs, are therefore precisely the beings Lewenhoeck terms worms, or spermatic animals. I shall likewise use this last appellation, not only to speak in the language of this naturalist, but because I esteem the facts I have related sufficient authority to bestow upon them the name of animals. The spontaneous motion, and the contortions of the body, by means of which they move from one place to another, are characteristics sufficiently decisive of their animality. Of this, we shall in the sequel have more conclusive evidence.

What I have said, is enough to shew how erroneous the opinion of Sir Charles Linnæus must

must be, when he maintains, that the spermat-
tic vermiculi are only particles of inert matter,
suspended in the fluid, and put in motion by
the heat. As the celebrity of the Swedish na-
turalist might induce us to suppose he does not
advance this without evidence, I should men-
tion the reasons which determine him to adopt
his sentiments ; and, that none of their force
may be lost, I shall give them, such as they have
been explained by the author himself, in a La-
tin thesis, while he was president in December
1759, under the title, *De Generatione Ambi-
gena*.

“ Vermiculos feminales Lewenhoekii vivos
“ esse vermes, in omni genitura prolifica ma-
“ ris præsentés, ad nostra tempora firmiter
“ satis credidit orbis eruditus. N. D. Præses
“ Lugduni Batavorum 1737, commoratus cu-
“ rioforum quorundam amicorum, et commi-
“ litonum utebatur consortio quales erant, J.
“ F. Gronovius Floræ Virginicæ auctor, ho-
“ die Consul Leidenfis : D. Van Svieten hodie
“ liber Baro, et Archiater Imperatoris : Isaac
“ Lawfon piæ memoriæ Scotus medic. exerci-
“ tus Angliæ : D. Lieberkuhn P. M. Beroti-
“ nensis : D. Kramer auctor libri artis Doci-
“ masticæ : Johan. Bartsch P. M. Regiomonte
“ Baruffus medic. Surinamensis : et D. Abrah.
“ Ens Pomerano, Petropolitanus. His igitur
“ quodam die congregatis. Ostendebat Do-
“ minus

“ minus Lieberkuhn præstantissima sua micro-
 “ scopia, quem rogabat N. D. Præses, ut ho-
 “ rum, ope, vermiculos in cane observandos
 “ præberet quod statim impetravit: contem-
 “ plabatur illos adcurate atque insectorum na-
 “ turæ gnarus statim vermiculos hosce Lewen-
 “ hoekianos, non esse corpora organis prædita
 “ et animata, atque adeo neque infecta neque
 “ vermes, sed particulas motas, quarum motus
 “ a calore dependeret liquoris rotundo ore ex-
 “ clamat. Præsentēs omnes attenti hos intue-
 “ bantur et oculis suis alii credere, alii vix qui-
 “ dem videbantur. Conclusionem hujus rei in
 “ dissertatione *de sponsaliis plantarum*, anno
 “ 1740, p. 24. edidit N. D. Præses his quidem
 “ verbis: *Vermiculi isti Lewenhoekiani minime*
 “ *sunt animalcula propria et voluntario motu*
 “ *gaudentia sed corpuscula inertia, quæ calidæ*
 “ *genituræ innatant; non secus ac particulæ*
 “ *oleosæ, quod selecta Lieberkuhnii microscopia*
 “ *nobis manifeste ostenderunt.* Hoc postea etiam
 “ vidit et confirmavit summus physiologus il-
 “ lustris D. V. Hallerus ut adeo auctoritas ver-
 “ mium seminalium jam prorsus fere in desue-
 “ tudinem venerit.”

This singular opinion of Linnæus was un-
 known to me, until it was communicated in a
 letter from M. Bonnet, dated 20th April 1771,
 who transcribed the words used by Linnæus:
 He, undoubtedly from politeness, added the
 following

following paragraph, or perhaps with a design to encourage me to prosecute my researches upon spermatic vermiculi. “ You perceive “ there is cited here, the eminent testimony “ of a Gronovius, a Van Swieten, a Lieber- “ kuhn, &c. and even that of Haller. At some “ future period I shall write to him about it. “ Nevertheless, all those authorities could not “ have weight with me; could not, in my view, “ balance your opinion: because I know you “ to be a much better judge of such things, “ than the philosophers the author names in “ his thesis. You have paid much more at- “ tention to the animalcula in question, and “ you have long studied the animalcula analo- “ gous to them. You have thus obtained a “ kind of touchstone, which experiment more “ and more improves, and which can never “ prove deceitful.”

Before considering what was advanced by Linnæus, I could not dissemble my extreme surprise, to see Haller cited as one of those who denied the existence of spermatic animals, whereas he had always been one of their most strenuous supporters. The notes he has made upon the Lectures of Boerhaave, his Elements of Physiology, his Physiology at large; in a word, all his works bear the most manifest evidence of it. In the sequel of this treatise, I shall

shall have occasion to use the authority of so great a naturalist.

We see how easily the opinion of the celebrated botanist of Upsal is established. Scarcely has he viewed Lewenhoeck's animalcula, when he decidedly pronounces them not to be animals. I leave it to the judgment of an impartial reader, whether a hasty glance of the vermiculi is sufficient absolutely to decide their nature; and to decide it better than Lewenhoeck, who had, during a number of years, examined so many species with so practised and attentive an eye. We know well the time and labour naturalists have bestowed, to be assured of the nature of certain organized bodies, doubted whether to be animals or plants. At the same time, those bodies were not, like seminal vermiculi, microscopic animals: their size admitted of their being completely manipulated, and easily seen with the eye. Linnæus should have been better convinced; he who has certainly characterised so many bodies in the three kingdoms, who has made many reiterated attempts, and made them with so much patience. If his laborious and useful occupations had left him sufficient leisure to penetrate the world of *invisibles*, where, as Muller observes, he might well be a stranger without a crime, and had he applied to this subject with that wisdom and penetration he has displayed
in

in the discoveries he has made upon the visible world, we cannot doubt that he would have omitted in his thesis the quotations we have just made from it. For, with an attentive view of the spermatic vermiculi, he would easily have seen, that they do not swim upon the liquid, like oleaginous particles; but that they swim at some depth. I have had eminent proofs of this. Penetrating the first stratum of the semen of the dog with the microscope, I pressed it downwards, so as to reach the lower parts: in each I saw an equal number of spermatic vermiculi.

I adopted the same method, with a considerable quantity in a watch glass. Wherever the focus of the magnifier penetrated, I saw the motion of vermiculi.

Finally, a third experiment has demonstrated their presence for a considerable thickness. I filled a thin-sided crystal tube with semen; the calibre was half a line, the length five inches. I held it perpendicular; and, in this position, applied the lens to the sides, the great transparency of which easily permitted me to see the included semen, when the tube was interposed between the sun and the eye. Wherever I applied the lens, whether to the top, the middle, or the bottom of the tube, I always saw the vermiculi. I repeated this experiment with a tube of much greater capacity; it was

a third of an inch in diameter within, and at least four inches long; but the opacity of the semen, occasioned by its great abundance, prevented me from seeing the vermiculi it contained. I examined the drops oozing from the lower part of the tube, which was insecurely stopped, whereby some drops escaped. All the drops were equally full of vermiculi, as the liquid in the higher part of the tube. When the experiments were repeated in this manner upon the semen of the dog, and that of other animals, the consequences were the same.

In the second place, had the Swedish naturalist bestowed upon spermatic vermiculi that attention they deserve, he must have perceived, they are not inert corpuscula; but that they have a spontaneous motion, well characterised; that this motion is regulated by the manner in which they advance; that they swim in the spermatic lymph, contorting and vibrating their members, like many other aquatic animals. He would not have ascribed the motion of the vermiculi to the heat of the semen, since, when this is gone, and there remains the heat of the atmosphere only, which happens a little after the semen comes from the animal, the vermiculi do not cease to move; but their motion continues for a given time, which is sometimes several days, when they are included in small tubes.

In

In short, the spermatic vermiculi of frogs, fishes, and newts, completely destroy the opinion of Linnæus, their seminal fluid being destitute of every sensible principle of internal heat. Since the blood of most of those animals is cold, heat cannot here occasion the motion of the vermiculi; but they should be absolutely motionless.

All this evinces how much two modern naturalists, Valmont de Bomare and Ernest Asch, are deceived in thinking, that spermatic vermiculi do not exist, or that they are only the most active parts of the semen; and this they maintain from their never being able to discover them, notwithstanding repeated observations. A similar objection had been started, after the discoveries of Lewenhoeck, who was content with remitting its authors to their studies, as unfit for observers. “*Dominos illos nondum tantum profecisse, ut eos recte observando contemplari valeant.*” I must be pardoned if I make the same answer to those new opponents; for we must say, their observations have been very unfortunate, or their vision very bad, or their microscopes good for nothing, or their unexactness and inexperience very great in the art of observation. It is true, M. De Bomare asserts his eyes are very good, and his microscopes excellent; and makes no hesitation to say, “*We have repeated all the experiments of natural-*
“ *istæ*

“ists upon semen; and although our eyes were
“very good, and our microscopes excellent,
“we have been able to discover nothing.” I
was almost induced to exclaim, May heaven
preserve such good eyes, and such excellent
microscopes, for with them we shall run no risk
of regarding as illusions the most beautiful discoveries
philosophers have hitherto made; and we need not fear
that we and our posterity will be forced to relinquish the
prospect of making new ones! With this acuteness of vision,
and this perfection of microscopes, we should be obliged
to revisit the ignorance of our ancestors in the world of
invisibles. But I would rather believe M. Bomare’s sight
is acute, and that his microscopes are perfect. What reason
can we assign for the unsuccess of his attempts to see any
thing in semen with all that assistance? The conclusion is
simple. We must not say he is unaccustomed to the art of
observation; I do not wish to lessen the esteem due to his
merit: he has acquired the name of a laborious and indefatigable
compiler. His Mineralogy and his Dictionary, which are an
assemblage of fragments copied here and there, will do him
this justice. But no one knows, that he has been, or that he
may be, a microscopical observer: and to make microscopical
observations with accuracy, many natural and acquired
qualities are requisite, and many more are necessary

fary to guard against being deceived in the subtle researches after those beings infinitely minute.

I have frequently, *viva voce*, confirmed the difference of sentiment, that books afford concerning the nature of spermatic vermiculi, by means of persons whom I caused observe the semen of man, and of other animals. Some, although all had distinguished merit, were certain they saw nothing, although they looked long at a time through the microscope, and even returned frequently to observation. The number of persons who could see nothing, was not great. Others, after a very long and painful examination, seemed to see a very indistinct and obscure fermentation in the fluid. A few could see the body, or corpusculum of the vermiculi, but were unable to distinguish the appendage. And a very few could perceive their form and all their motions. These last, indeed, were well accustomed to study microscopical objects, and might with reason be termed, professional observers. Let those who deny the existence of spermatic vermiculi, attempt to enter this last class of persons, and practise upon minute objects, and I assure them, if they will repeat their experiments upon the seminal fluid, they will see spermatic vermiculi, as I, and Lewenhoeck, with many other naturalists, have seen them long before me. Then, if they choose to

communicate their observations, they will have the advantage of knowing, if their publications already made excite the compassion of the learned, their future works will merit an eulogium.

CHAP. III.

WE descend from the observations of Lewenhoeck, to those of M. De Buffon, which, although very numerous, comprehensive, and specific, we shall but abridge. M. De Buffon, with the compound microscope, observed the fluid from the feminal vessels of a dead human body yet warm. It was full of filaments, moving about, and branching into many parts. The filaments swelling, burst, and many ovular corpuscula escaped, which still remained attached to the filament, as by a thread; then they oscillated like a pendulum, and during those oscillations the thread extended. The corpuscula, at length detached from the filaments, traversed the most fluid part of the semen, along with their filament, the extreme length of which impeded their motion, and they seemed to him to endeavour to free themselves of it. Having diluted the semen with rain water, the microscopic view was better defined. It clearly appeared, that
each

each ovular corpuscle had a double motion of oscillation, and of progression. In two or three hours, the seminal matter acquired greater fluidity; the filaments disappeared; the number of corpuscula encreased; the threads contracted; the oscillations relaxed; and the progressive motion encreased. In five or six hours, the ovular corpuscula having lost the threads, resembled animals more than ever; not only because their quickness in swimming was greater, but because they directed their course to every quarter. In twelve hours, the activity of the corpuscula was great; and some revolved upon their axis; others changed the ovular to a globular figure, under the observer's eye. Some divided afunder; so that one formed two. At the end of one day, the number diminished; and, upon the third, none were to be seen.

In other semen, which seemed entirely filamentous, the ovular corpuscula did not proceed from the filaments; but these, dividing in two, were metamorphosed into corpuscula. They were embarrassed by a thread. The longer it was, the more it impeded their motion; but it gradually contracted, and was at last completely destroyed. The figure of those ovular corpuscula resembled that of those of infusions. They swam with a progressive motion,

tion, though, at first, the thread occasioned a simple oscillation.

M. De Buffon having examined a new drop of semen, ten or twelve hours after it was taken from the animal, he saw the whole corpuscula proceed in a crowd from the same side, where there were filaments like a thread, to which many were fixed. They gradually disengaged themselves from it. The body of the thread then diminished; so that it appeared less by an half.

The author thought, in his first observation upon the human semen, that the corpuscula became gradually smaller. At the same time, he was not certain of it; but another observation convinced him.

He then observed the seminal fluid of a dog. He found it clear, and without filaments. The ovular corpuscula almost completely resembled those of the human semen; only, he found them more active, and less numerous. Upon the fourth day, very few of the corpuscula had threads.

In another portion of the seminal fluid of the same dog; beside this, he saw corpuscula proceeding from a mucilaginous substance in the semen, which, to him, seemed internally animated by an inflating motion, which induced him to think the mucus swelled in some parts to form little tumours. Those corpuscles were

were all without a thread. The figure of some changed. They extended, contracted, and inflated themselves; and, amidst those wonderful operations, divided afunder, and gave existence to two corpuscula, whose figure and properties were similar to those of the generating corpuscula.

The French naturalist extended his experiments to rabbits. From the seminal vessels, he took the fluid, and, mixing it with fresh water, observed the following phenomena: In three hours, the globular corpuscula became smaller; and thus constantly diminished until the eighth day, when they were scarcely visible. But, in proportion as their size diminished, their number and activity encreased: in the same manner their figure varied, they were ovular, spherical, and elliptical.

He repeated the experiment upon the semen of another rabbit, ejected at the moment of copulation. He found ovular corpuscula; some with the filament, others without it. The former greatly resembled those of the dog, and of man; excepting that they were less, more agile, and the thread much shorter. He could not be certain that they were real threads, but only little spirals, formed in the liquid by the course of the corpuscula.

The semen of the ram presented corpuscula moving in every direction. They were elliptical, without filaments, and equal in size.

M. De Buffon also examined the feminal fluid of some fishes; the carp, the pike, and the barbel; procuring it while the animal was alive. There, he found a number of corpuscula in motion, which were of a dark colour, almost black, and very small.

Such is an abbreviation of the observations made upon the semen of animals, by the illustrious author of Natural History. And he draws a general conclusion, that the corpuscula, examined and described by Lewenhoeck near a century ago, could not, by the Dutch observer, or by any one, be termed Spermatic Vermiculi, because they possess no characteristic of animality. The labour they experience in drawing along their tails; in divesting themselves of them; in changing their figure so often, to form anew under the observer's eye; and their division into parts, and diminution of size: seem, to him, peculiarities incompatible with animality. On the other hand, not being able to say that they were bodies completely inert, because he had really seen in them signs of animation; he inclines to constitute them into a particular class, giving them the appellation of *organic molecules*, which are particles disseminated through all matter, original, incorruptible, animated, and always active. Nor does he hesitate to confide the formation of the animated universe to those molecules.

molecules. I enter not, here, into a discussion upon organic molecules ; but, occupied solely with the facts M. De Buffon relates, I sincerely acknowledge, that the essential differences I found between his account, and what I have myself seen, have made a deep impression upon my mind. It is not, that I wish to flatter myself, my observations are of more value than his, from exactness of execution, or assiduity of continuance. If I may be allowed, I shall even say, that mine should be preferred, as they are much more numerous. At the same time, M. De Buffon's perfect conviction of the truth of his observations ; the great confidence he has, that his readers, who repeat them, will find them scrupulously exact ; the liberal and natural manner in which he opposes them to those of Lewenhoeck ; and the errors with which he reproaches him : all makes me judge it possible, that the Dutch naturalist and myself might be deceived. All those things were aided by a consideration, which, though foreign to the subject, is at the same time plausible ; I mean, the amazing celebrity and reputation, the French naturalist deservedly enjoys. I long hesitated whether I ought to prosecute my observations, and subject them to as rigorous an examination as might be possible ; or, whether it would be more proper to abandon them, lest they might

not be credited, from the formidable trial they had to undergo. I would actually have done so, had not my illustrious friend M. Bonnet, who is well skilled in such matters, diverted my intention. He strenuously advised me to study the spermatic vermiculi of various animals. I replied, I had already done so partially; but I had suspended my labours, upon finding my observations so very different from those of M. De Buffon, whose authority I respected. He had the goodness to answer,

“ You judge well, my valued correspon-
“ dent, that I am not much surpris’d to find
“ you in opposition to M. De Buffon, with
“ regard to spermatic vermiculi; nor do I for-
“ get what he in some measure told us, and
“ which I repeat after him, that his theory
“ precedes his observations. Now, you know
“ as well as I do, that a favoured theory is a
“ mirror which changes the appearance of ob-
“ jects.

“ Do not fear that the authority of M. De
“ Buffon will in the least invalidate your dis-
“ coveries upon spermatic vermiculi. You
“ have proved yourself an excellent observer;
“ you have acquired the right of being believ-
“ ed. You have cherished no theory: you are
“ satisfied with interrogating nature, and with
“ giving the public a faithful account of her
“ responses. You will always be listened to
“ by

“ by philosophers ; and they will judge your
“ observations so much the more sure, that
“ you prove you possess the art of observation,
“ and that you neglect no rule of this art, so
“ little known. Proceed then, my dear Mal-
“ phigi ; extend your researches upon sperma-
“ tic vermiculi as far as you can ; and insti-
“ tute all the comparisons in your power be-
“ tween them and animalcula.”

I was encouraged, by those obliging invitations, to draw my observations from the obscurity in which I had left them, and to continue to encrease them as much as my humble talents would admit. Without interruption, this was my employment during the greater part of three years. But the different facts I gradually discovered, of which I shall give an abbreviated narration, little agree with the relations of M. De Buffon : at the same time, it appeared to me, that I had, during this long research, discovered several reasons which might have induced this naturalist to think as he does.

One of the principal phænomena which the French author views as the chief basis of his hypothesis, is, what is seen in the actual formation of the spermatic vermiculi, in the mucilaginous parts of the semen and its filaments, which, under the observer's eye, change into animated beings, as he imagines he has seen
in

in the semen of man and of the dog. Mr Needham readily embraces this opinion. He thinks that seminal vermiculi do not exist in the semen, while in the body of the animal, but that they are formed some minutes after it comes from the body, when it begins to change and decompose by the influence of the air ^a.

In the experiments I have made upon semen, which are related in the first chapter, I have not mentioned this actual formation, because I had not seen the least indication of it. When I made observations upon semen, whether I saw the vermiculi at first, or not till the semen settled, I never perceived the gross or filamentous parts give existence to the vermiculi. It is true, in the first course of experiments, I did not think of examining this part of the subject profoundly; my attention was not fixed upon it, as it had been in my other experiments. I therefore directed my whole observations to what happened to the solid and filamentous part of the semen, at the moment of its dissolution. But I never could see the actual production of the vermiculi. I have even incontrovertible evidence of the contrary. In the meantime, I have discovered the paradox of the French naturalist. I wish to demonstrate this, accompanied by some facts which I must be permitted to detail.

From

^a New Microscopical Observations.

From human semen, as yet but partially coagulated, I separated two little clots, forming a line of filaments, which I meant to consider with attention. The vermiculi included in the filaments occasioned a motion; the filaments dissolved under the eye; and the two clots, in a few seconds, became two drops of semen. It was with singular surprise I saw the small number of vermiculi in these drops, compared with the numbers to be seen in others much smaller, not formed, as they were, by the dissolution of the coagulated semen, but by a portion of the semen found fluid in the seminal vessels.

I repeated this experiment upon a clot not so solid as the two former: there I found still fewer of the vermiculi. I then began to suspect, that perhaps the vermiculi did not inhabit the grosser parts of the semen, but those that were fluid. This suspicion was fortified, upon my seeing all the corpuscula perish when the fluid evaporated. But, that this idea might be verified or destroyed, I examined many clots from each spermatic fluid, which at first was very difficult to accomplish; for the solid and consistent parts of the human semen, even while in the seminal vessels, are commonly immersed in the fluid parts. I was at length able to overcome those trivial obstacles. With the extremity of a pair of small pincers, I drew
from

from the feminal vessels a portion of human semen, similar to milk a little coagulated: as it was moist, I drew it along a dry piece of glass, that it might deposit its humidity. I then put it in a watch glass, attending its dissolution, in order to examine it with the microscope. It was not destitute of vermiculi. I made a general comparison of the number, with the number of vermiculi in the vessels, which had been put into the glass of a watch on purpose for this comparison. But there was no proportion; the number in the solid parts was so much smaller than in the fluid parts.

These results did not satisfy me. I saw that the few vermiculi in the solid semen, might be owing to some little portion of the fluid remaining in the solid piece. At the same time, instead of finding few, I could have wished to find none at all, or at least a very small number. Having taken another clot of human semen from the vessels, I endeavoured to disengage it as far as possible from the feminal fluid that might remain about and within it. Here I should observe, *en passant*, that this operation of drawing along the solid part to take away the fluid, should be performed with great adroitness and celerity, otherwise the solid part gets time to dissolve during exposition to the air. And it then happens that drawing it long,
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or slowly, along the glass, instead of drying, it always becomes more humid. I succeeded, at last, in freeing a clot of human semen from all sensible moisture. I put it in a watch glass, and, when dissolved, examined it with the microscope. Truth obliges me to declare, that here I found no vermiculi; nor did I find any in other clots managed after the same manner, although they were numerous in the fluid parts of the semen in the seminal vessels of the animal which had afforded me the clots and the semen. I did not delay a repetition of this important observation. In my journal of experiments, I find this has been done fourteen times, in thirteen of which the dissolved clots exhibited no vermiculi; and only once I found a small number in the coagulated clot.

The seminal fluid of the rabbit is always partially coagulated; so that whenever taken from the vessels, it afforded me the means of repeating my experiments. But I have never discovered vermiculi, when I was able to take the spermatic lymph completely away. These united facts convince me, that the habitation of the vermiculi is in the fluid part of the semen. The same facts ascertain the degree of credibility we should bestow upon what M. De Buffon says of their formation. It is evident, that far from being generated by the solid or filamentous part of the semen, from which they

they detach themselves, they do not branch out as I have demonstrated: if they are even found there, they proceed from the fluid part, which is the place of their natural abode, and which is at this moment mixed with the solid. It is doubtless this which occasions the error of M. De Buffon. He saw the most gross and filamentous part agitated and moving: amidst its agitations and contortions he saw vermiculi proceed from it: he even observed the number encrease, proportionally as the bulk of the gross and filamentous part decreased: he likewise remarked, that the number was greatest when the filaments had entirely disappeared; and, allowing himself to be deceived by these appearances, it was easy to believe the decomposition of the filaments was the productive cause. But the fact is, the vermiculi pre-existed in the filaments; they were concealed and enveloped in the parts immersed in the seminal fluid; and, only when disengaged, do they become visible to the observer: nearly in the same manner, as if one had steeped a piece of ice in an infusion full of animalcula, and taking it out it was carried to the fire; as it melted, it would exhibit the animalcula that had insinuated themselves into the crevices.

M. De Buffon, by a very simple experiment, might have satisfied himself, that the vermiculi
existed

existed before the dissolution of the semen. He should first have examined the fluid part of the semen, which, upon opening the seminal vessels, we find mixed with the solid. There he would have found abundance of vermiculi, although at that time they could not be produced by the dissolution of the solid semen; since we see that this solid part begins to dissolve, only when it proceeds from the animal, and experiences the influence of the air.

What I have already said, and what I shall continue to say, proves the falsity of Mr Needham's opinion, who assures us, the vermiculi are produced some minutes after the semen comes from the animal's body; that is, when it begins to be altered and decomposed by the action of the air. Respecting the semen of man, it is necessary to consider whether the portion is solid or fluid. If the former, when deprived completely of the spermatic fluid, no vermicule is ever seen, although it remains during some time exposed to the air, and although it changes and is decomposed. If the latter, the vermiculi appear in it before the time required for this alteration. It has often happened, that the time I consumed in taking the fluid matter from the seminal vessels still warm, to present it to the microscope, did not exceed a second; yet I found the same number of vermiculi as afterwards, even when the fluid had
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been exposed to the air sufficient time to effect its decomposition.

The observations I have made upon the semen of other animals, further convinces me of the falsity of such an opinion. I prepared a ram, alive, and vigorous; so that a friend cut away the epididymes, which are commonly full of semen; and while he cut them, one might take a drop of semen, and immediately present it to the microscope, where I kept the eye ready to observe. It may easily be seen, that the time in performing this operation could not be shorter. We saw numbers of vermiculi, and all were very vivacious. I repeated this experiment upon the semen of the newt, and I saw the same. Further, in the spring, when the *vasa deferentia* are full of semen, it was unnecessary to open the animal, to obtain it for observation. Upon pressing the belly gently, it escaped by the anus, where the two vessels terminate.

This animal has afforded me a proof still more decisive. I let the males suffer hunger so long, that they became very emaciated; then the *vasa deferentia* contained a very small quantity of semen, and, from the transparency of the tunics of the vessels, which were rendered thin, it might easily be observed with a magnifier. Opening the abdomen, and applying the microscope to the vessels, without affecting or deranging

deranging their situation, we saw the vermiculi swimming in the fluid ; and they were visible to those who were least skilled in the art of observation, because, as I have remarked in the first chapter, they were much longer than others.

I may therefore conclude, that the spermatic vermiculi of man, and of animals, exist in the semen, previous to any alteration or decomposition by the influence of the air ; and that they are active in this fluid, even while it is included in the organs of generation.

CHAP. IV.

I CONTINUE to examine the other principal results, from the observations of M. De Buffon. In this number I place the appearance of those very long and slender filaments, the vermiculi drag along in their course ; the contraction and disappearance of the filaments ; the activity the vermiculi acquire with age ; the facility of motion in every direction ; the change of form, division, diminution of size ; and, lastly, their total destruction in a few days.

To begin with the first, I may observe, that Lewenhoeck and other naturalists have remarked, that each vermicule in the semen of man,

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or of several other animals, has a long appendage, which it drags along with it as it swims. But this appendage is not such as M. De Buffon would incline; he imagines it a thread or long corpusculum, having no analogy to a tail, or any other member, and entirely foreign to the spermatic vermicule. I have always found, that the appendage, as I have shewn in my first chapter, has every characteristic of an actual tail. It has the shape; and the diameter always becomes greater, as it approaches the body of the vermicule to which it is united. So that it clearly appears to form one whole with it, as is seen in the rat-tailed worm: so much the more, as, in swimming through the spermatic fluid, the vermicule makes use of the tail. In effect, it bends it first to one side, then to another, and in every different direction, as aquatic worms are wont to do while they swim. I have often seen this, and in the most distinct manner. So that I must discredit my eyes, if I thought or wrote differently.

It is true, that this microscopic observation upon the tail of human spermatic vermiculi, is the most nice and delicate of any I have made; it demands the greatest care, and the most strict attention. The tail is wonderfully slender, and at the same time transparent; whence it happens, that too strong a light confounds it with the seminal fluid, so that we entirely lose sight
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of it. The choice of the light fit for observation, is then of the greatest importance. The direct light of the sun is too powerful, as is that of a lamp; at least, unless it is by some method moderated. The light I have found most apt for this fine observation, is that of a window opposite a white wall, moderately illuminated; as, for example, when it is exposed to a part of the sky covered with white clouds.

In the second place, the finer and thinner the sliders are, on which the drop of semen is deposited, the more easily are the tails discovered. I prefer talc to chrystal.

Thirdly, the spermatic drop should be as thin as possible, otherwise the origin of the tail will not be seen, the rest is concealed in the liquid: and, even when the feminal fluid is troubled, it is necessary to refine it with another portion more clear.

Fourthly, when the vermiculi swim, as the tail is always a little lower than the body, we must depress the focus of the lens a little, to observe it.

Finally, a microscope of a single lens, such as that called Lewenhoeck's, should, for this observation, be preferred to the compound microscope.

Although M. De Buffon mentions, in his observations, only one or two of the precautions alluded to, and which he seems to have

used, yet I am not willing to think he has neglected the rest ; for the exception of only one, would prevent him from making the real observation. He says, that he always used the compound microscope, to examine the semen of man and animals. I doubt not, his microscope was as excellent as he says ; but it was a compound one, and had all the defects of compound microscopes, among which this is specially placed : the object observed is never seen so distinct, or its outlines so well defined, as with a perfect microscope formed of a single lens. This fact is acknowledged by all observers ; and the observations I have made upon human spermatic vermiculi, with Lewenhoeck's microscope, confirm it more. Using the first, I saw the vermiculi precisely as I have described them ; and with the other, I had a confused view of the body, which I frequently doubted to be round or oval : the outline was always in a sort of mist or cloud. The tail, which is infinitely more delicate, appeared less sensible ; and I only knew to distinguish it as a long slender body. It is not surprising, therefore, that M. De Buffon calls this part of the vermiculus, a foreign body, a sort of long, delicate, and subtile filament, since to him it appeared truly such, when viewed with the compound microscope.

But

But it may perhaps be thought, that the compound microscope used by me was not so good as M. De Buffon's. However, I can assert, that my microscopes are the most perfect now made in London. I shall also add, that I was desirous to view spermatic vermiculi with the microscope M. De Buffon has used, that is, Cuff's compound microscope, which was precisely that of Mr Needham, and which, M. De Buffon says, he used in his observations upon the seminal fluid. But it showed me no more; and I may say, that my observations and experiments, as well upon spermatic vermiculi, as upon the animalcula of infusions and other similar beings, could never have been exempted from contempt, I will even say error, if I had preferred the compound microscope to that of Lewenhoeck.

Let us stop a moment, to establish the certainty of the existence of tails in the human spermatic vermiculi. The fact is confirmed in so convincing a manner by the experiment upon talc, where the tails appear complete, and not confounded with the semen, that this experiment would put it beyond a doubt, was there no other proof. In the mean time, we may observe, that such extreme circumspection and so many precautions, are not necessary to distinguish the tail of the vermiculi of many

animals, when Lewenhoeck's microscope is used for examination.

Respecting the contraction and entire disappearance of the tails, which M. De Buffon says he observed, when the vermiculi remained long in the semen after it came from the animal's body ; I can oppose nothing, except that, in my numerous observations, I always saw the contrary. The vermiculi constantly preserve this member, not only while alive, but also after death, and even long after death ; so that it only begins to spoil, and be destroyed, when the vermiculi themselves arrive at this state. I shall say more : the maceration of the dead bodies of the vermiculi produced by boiling or infusion, is not sufficient to destroy their structure or their form. Vinegar, nay urine, fluids which entirely destroy the contexture of most animalcula, cannot, until after a considerable time, affect the body or the tail of our vermiculi. It would therefore be necessary to deny the evidence of all these facts, to agree with M. De Buffon concerning the contracting and disappearing of the tails of femina! vermiculi. That my observations may have greater weight, to establish the certainty and continuance of those tails, I shall cite the authority of the great Physiologist of Bern—" Nunc quod caudas attinet perpetuam particulam vermiculi spermatici, ea nimis certos et fide dignissimos testes

“ testes habent, quibus meum suffragium per
“ experimenta natum addidisse liceat ^a.”

The other phænomena of spermatic vermiculi observed by M. De Buffon, such as, an increase of action they acquired with time, their change of shape, their diminution, their division, and the duration of their lives from four to eight days, seem to me no less paradoxical, if we attend to the repeated observations mentioned in the first chapter, and if we recal those of the celebrated Lewenhoeck. The acknowledged merit of M. De Buffon, prevents me, at the same time, from considering this collection of facts as chimerical: but, not being able entirely to reject his singular microscopical observations, however defective his microscope might be, it could not occasion such a difference of phænomena. I therefore determined to resolve his doubts, by taking the trouble to make a new course of experiments upon the human semen, and also upon that of other animals: but, notwithstanding all my care, precaution, and all my possible vigilance, I could discover nothing new, at least essentially affecting the facts I have spoken of. But, with reflection upon M. De Buffon's observations, and the repetition of my own, I cannot reproach him with seeing what did not exist. I thought that all this might be an equivocal

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effect,

^a Haller. Physiolog. tom. 7.

effect, which seemed to me the more likely, as the phænomena he says he observed in spermatie vermiculi, might be occasioned by beings of a very different nature. My experiments upon infusions, suggested this idea. I had remarked, that there is no part of an animal which, when infused, does not give existence to a particular kind of animalcula. They are produced indifferently, by the muscles, brain, nerves, membranes, tendons, veins and arteries. The same is done by the blood, serum, milk, chyle, saliva, &c. mixed with water, or even by themselves. I had not yet made experiments upon the human semen for this purpose; but it was most probable, that the putrefaction of this liquid would give existence to particular beings:—and who knows, said I to myself, that they have not inadvertently been confounded with feminal vermiculi, and that M. De Buffon has not ascribed to the latter the properties and phænomena exhibited by the former? When I again perused Haller's Physiology, I found that he was of this opinion, and that he even suspected, M. De Buffon had never seen the spermatie vermiculi.—“ Porro Buffonius, ut
 “ cum illustri viri venia dicam, omnino non
 “ videtur vermiculos feminales vidisse. Diu-
 “ turnitas enim vitæ quam suis corpusculis tri-
 “ buit, ostendit non esse nostra animalcula, id
 “ est spermatie, quibus brevis et paucarum ho-
 “ rarum

"rarum vita est." And in another part of the same volume he adds—"Ea experimenta, id est Buffonii, manifeste ducunt ad animalia putredinosa." However, I thought of ascertaining this fact, by observing what successively happened to the semen when long preserved in a watch glass.

I made my first experiments upon the human feminal fluid ^a. The vermiculi died in three hours and a half, and were precipitated to the bottom of the glass. Upon the sixth day, the feminal fluid began to exhale a fœtid and disagreeable odour; but I could perceive no animated being; only, upon taking some drops from the bottom of the glass, I observed the dead bodies of the vermiculi apparently very entire. The seventh and eighth days, I saw no change; but the fœtor encreased. On the ninth, I discovered very minute animalcula, their size nearly equalling that of spermatic vermiculi; but they had no tail, and greatly resembled most minute spherules. Like the animalcula of infusions, they sometimes stopped at little fragments of corrupted semen: sometimes their course was very rapid, retrograde, rising and sinking in the fluid: in a word, they possessed every property of infusion animalcula. They were seen in every stratum of the semen; and those at the bottom, put in motion the dead bodies

^a February 11. The thermometer 45° above 0.

bodies of the feminal vermiculi, which were still entire, and remained so some days longer. In the feminal animalcula, we remarked the same terms, with regard to the encrease and diminution of the numbers, and their end, which we generally remark in the history of other animalcula; only, when the globular animalcula decreased, there came other two generations: the last of which could scarcely be discerned with the naked eye, and, in some observations, remained until the eighteenth day. While examining the semen in this glass, I observed a portion of the same kind which I had put in another glass, and placed it in a stove, that it might experience a greater degree of heat. The effect of this temperature was such as we might expect. The vermiculi lived longer, and the infusion animalcula appeared sooner. The vermiculi died in five hours, and the animalcula were seen in five days. They were of a globular figure, had no tail, and all were of the same size and species as those of the preceding observation. The other two colonies of smaller animalcula then appeared; the globular always remaining.

Having again prepared the same kind of semen ^a, I had an opportunity of seeing the effect of heat in accelerating the production of animalcula. I found some in the feminal fluid, in

^a May 22. The thermometer 65° above 0.

in less than twenty-three hours after it was taken from the dead body. The species were different from the globular; they were three times as large, and of a cylindrical shape. When swimming, the body undulated like a serpent; which did not occur, or was not observed, in the globular animalcula. The dead vermiculi appeared to be their food; since the animalcula, incessantly in motion, surrounded them, and pecked at them with the anterior part of the body. In three days I saw other animalcula, as minute as the spermatic vermiculi, along with the cylindrical animalcula; and I remarked a circumstance, which in semen I had not observed before. In the treatise upon the animalcula of infusions, I have spoken at large of a propagation by a natural division, which happens with many species. I have said, that, in several, the division begins in such a manner, that the animalcula is gradually cleft asunder, until it is divided into two equal parts, which become two animals smaller than the first. But it remained to enquire, whether this mode of propagation could happen with animalcula similar in size to seminal vermiculi. More than ten divided transversely before my eyes; and this division, or propagation, continuing several days, the seminal fluid, which had become excessively foetid, now teemed with life: but the number of these, as well as of the cylindrical

cylindrical animalcula, gradually diminished, in the same way as the numbers of infusion animalcula decrease; and upon the twenty-first day all had disappeared. There only remained in the semen, a universal obscure fermentation, in no particular direction: but the seminal molecules were tumultuously driven about in every direction. I was not long of perceiving, that this irregular agitation was occasioned by a multitude of the most minute animalcula, concealed amongst the semen, which, in their course, they put in motion: and of this I had complete evidence, by diluting the semen with water, as they were then accurately discerned, appearing about half the size of the spermatic vermiculi.

The phænomena, discovered in the semen of the horse, were analogous to those I found in the human semen. The vermiculi lived seven hours, and then were precipitated to the bottom of the semen, where they long remained entire. The body and tail were complete for a month. In fourteen hours the semen began to exhibit symptoms of putridity, and then were seen the animalcula of infusions: they increased; and, at the beginning of the fifth day, there were many species. One particularly demands our notice. Not only did it multiply by a longitudinal division, but the figure and size of the animalcule changed every moment.

Sometimes

Sometimes the animalcule contracted, and became round ; sometimes it dilated, and became elliptical ; as I have remarked in several species of infusion animalcula.

When this experiment was made upon the semen of the horse, I made one, similar, upon that of the rabbit. The vermiculi perished and fell to the bottom in four hours ; the animalcula of infusions appeared only after fifteen. Two species multiplied by division, and one exhibited the contractions and inflations I mentioned before.

I made the same experiments upon the seminal fluid of the ram, the dog, the bull ; upon that of frogs and newts. The results were constantly analogous. At the beginning, and during the progress of putrefaction, each produced animalcula ; which occasioned our remarking, in all that appeared, a number of various circumstances. Their number encreased, it diminished, and became very small. The animalcula were different in figure and size ; and some species multiplied by division : which evinced, that animal semen resembled infused vegetable seeds, from the many kinds of beings to which it gave existence.

These facts afforded new light to illustrate a truth, sufficient to satisfy me, how erroneously M. De Buffon had ascribed to the seminal vermiculi, properties belonging only to the animalcula

animalcula of infusions. Let us, in a few words, collect the circumstances. According to this author, after a certain time, the vermiculi were deprived of their tails. He should have said, that the animalcula of infusions came in the place of the vermiculi which were already dead, and precipitated to the bottom of the liquid. He was arrested by their first appearance, and took them for seminal vermiculi deprived of the tail, which, in truth, they often very much resemble. When disengaged from the tail, M. De Buffon adds, they acquired greater activity. This was a necessary consequence of the first mistake. When the infusion animalcula had taken place of the vermiculi, their encreased agility could not be unobserved, since the animalcula move with much greater quickness than the vermiculi. This erroneous supposition being admitted, M. De Buffon had to relate, as he has done, the remainder of the phænomenon. He had to speak of the imaginary changes of the vermiculi, of their division and their diminution, with the more confidence, as his opinions had to be confirmed by a repetition of his experiments, if not upon all, at least upon many species of infusion animalcula in the semen.

I think I have had too numerous, and too decisive proofs, to doubt that the phænomena seen in infusion animalcula of semen, are different

ferent from the phænomena exhibited by the feminal vermiculi. Will I not be permitted at once to oppose my experiments to those of the illustrious M. De Buffon? I see that all the feminal fluids he has studied, and where he has discovered the phænomena of which I have spoken, have also been studied by me. I perceive that I have even examined more, both of cold, and of warm-blooded animals. His observations upon the fluids, were only made at certain times, and in one season. I esteemed it necessary to extend my observations to all seasons. My microscopes were not inferior to his, as I have said; nay, I may say they were much better. How then is it possible, that, being in the same circumstances with the French naturalist, and being even in situations more advantageous, for observing the phænomena of spermatic vermiculi; how is it possible, I say, that I have never observed those phænomena, that I have never observed some, or remarked but a single one? What do I say? Not only have I never observed any of those phænomena; but I have seen what completely contradicts M. De Buffon's observations, and remarked it in all the fluids I have many, many times examined. Such, for example, is the imaginary activity the vermiculi acquire, in proportion to the time they remain in the semen, which would confute all I have
seen

seen and related in the first chapter. For, after being exposed to air, their motion becomes much more languid : before this, it is quicker : and its greatest rapidity is, when the semen is in the body of the animal, which, as I have observed, was remarked by Lewenhoeck. Finally, M. De Buffon says, he observed those phænomena three or four days after the semen proceeded from the animal, and even upon the eighth day, in that of the rabbit. But this must be impossible : because, it cannot happen that these vermiculi, like those of human semen, which live longest when exposed to air, could live during the time mentioned by M. De Buffon ; and, in this state, they could not live above seven or eight hours. When secured against the influence of air, in glass tubes hermetically sealed, they do not live three days, as we shall afterwards see. It is therefore decided, that the phænomena observed by the author in his experiments, could be exhibited only by the animalcula developing in the seminal fluids when on the verge of corruption, or when they do corrupt, as happens to other liquids which will corrupt, or are already corrupted.

I cannot suppress my surprize, that M. De Buffon never doubted that the animated beings he saw in semen, were really spermatic vermiculi, or only animalcula originating there ; I mean

mean to say, the animalcula of infusions. He well knew, that this last species of animals originates no less in animal, than in vegetable substances, when they begin to corrupt. For he says, that in two infusions, one made with the testicles of a ram in water, and the other with those of a dog, he, in some days, found living animals similar to those he had seen in the semen of animals; that is, globular or ovular vermiculi, without tails, moving with great activity, and often changing their shape. If the animalcula of the two infusions were perfectly similar to those he had observed in seminal fluids, how did he not suspect, that, instead of being spermatic vermiculi, they might have been infusion animalcula? He had an additional cause to think so; for he must have remarked, that those changes of figure, divisions, diminutions of size, were not to be seen in recent semen, but in that kept for some time, and about to corrupt. Of this, he must have had certain indications, and an indubitable proof, from the foetid and cadaverous odour the semen then exhales; which is also a convincing proof, that animalcula are produced in it, upon account of its putrescence; and consequently, that we cannot confound them with the spermatic vermiculi. Another remark might have occurred, and might have saved him from mistake, had he chosen to

make it, was, not to examine the surface of the feminal fluid only, but also the bottom. There, he would have found the vermiculi entire, although dead ; which would have demonstrated, that the animals he then saw in motion could not be feminal vermiculi.

But all I have hitherto said against the theory of M. De Buffon, receives greater weight from the comparison I have drawn between the spermatic vermiculi, and the animalcula found in putrid semen. In another work, I have shewn, that a considerable part of the animalcula of infusions, appear, before the microscope, an aggregate of minute vesicles, smaller or larger, and in greater or lesser number ; that the vesicles are invested by a common pellicle, which forms the exterior of the animal ; that the pellicle and its vesicles are lost and destroyed when the animalcula die ; and if, while alive, they are wet with urine or vinegar, the body is destroyed, and reduced to nothing ^a. All these singularities are amply exhibited and verified, in the putredinous animalcula of semen ; but, with all care and attention, I have never been able to see any thing like feminal vermiculi. The texture of the body and tail is not vascular : it is uniformly homogeneous ; equally solid and compact. For this reason, perhaps, the vermiculi, when dead, fall to the bottom of the feminal fluid ; and the in-

fusion

^a Saggio de osservazioni microscopiche.

fusion animalcula commonly swim. Likewise, the vermiculi continue entire, long after death : urine, vinegar, even ebullition, cannot dissolve or decompose their contexture. We must therefore conclude from all this, and from all that has been already said, that the animalcula of infusions, and consequently those of putrid semen, are of a nature and constitution essentially different from those of spermatic vermiculi. We may easily determine that this must be real, since a fluid, which affords a salutary dwelling to the one, is fatal to the other. The putredinous seminal animalcula develope and live in corrupted semen, but they die in that which is recent and entire. On the other hand, the vermiculi live with safety in recent semen, but they die when put in that which is corrupted. The animalcula become more lively and more active when water is put in the semen ; the vermiculi, at least several species, then become motionless and die. Of all these facts I have often convinced myself. Whence I conclude, infusion animalcula, and spermatic vermiculi, to be two species of animals, which we cannot confound without offending nature.

CHAP. V.

As I have taken the liberty respectfully to remark the errors of M. De Buffon concerning this subject, I must still request permission to show the deductions which may be made from his noted theory of organic molecules. But, to perform this with the greater success, it will be necessary to bring under our view, some of the chief points of that theory.

The illustrious French naturalist supposes, that every animal and vegetable substance includes a number of organic molecules, that is, active and incorruptible particles. He thinks they serve for the nutriment, encrease, and multiplication of all beings, whether they enter the body of animals by means of food, or that of plants with the sap they imbibe from the earth : that they intimately penetrate every part, there unite, and are identified, if we may say so, and afford nourishment to the plant or animal. If the plant or animal is young, it appropriates all the organic molecules, incorporating them with itself. The molecules, expanding, extend their fibres, and by this means effect increment. But if the being is already an adult, if it is no longer susceptible of expansion, then all the molecules,
being

being unemployed in nutrition, those which are superfluous are deposited in the organs of generation, and serve for the propagation of the species, which takes place when the organic molecules of the male are mixed with those of the female; so that the most analogous tend to approach each other, in virtue of certain relations, and form singular wholes, resembling in miniature the different parts of the two individuals in which they are modelled. From the wholes together, there results a general whole, which is the embryo.

If the organic molecules, afforded by the male, are more numerous or more active than those of the female, the embryo will be a male; if the molecules of the female are more numerous, it will be a female. The greater number afforded by the male or the female, will occasion the greater resemblance of the embryo to the individual from which it has received them.

Large animals are less fruitful than small: the reason is evident. The former extract fewer organic molecules in nutriment than the latter. A bull draws less nutriment from hay and straw, and consequently fewer organic molecules, than a bee does, in proportion, from the finest parts of flowers. Animals covered with scales are more fertile than those covered with hair; probably because the former per-

spire less than the latter, and because they accumulate a greater number of organic molecules.

If, instead of collecting in the organs of generation, the molecules are carried to other parts of the animal, there they form minute living animals, as the *tenia* and *ascarides*; worms sometimes inhabiting the intestines, the liver, and the sinuosities of the brain.

It is thus that M. De Buffon, in his theory, explains those phænomena, and some other, which, for the sake of brevity, I shall omit. As he wishes this theory, the offspring of his genius, to be adopted by nature, he recurs to the feminal fluids of animals, and the infusions of plants, because, in both, according to his opinion, are organic molecules, clearly exhibited under the form of globular, ovular, or other shaped corpuscula, endowed with motion, subjected to various changes of figure, may be decomposed into several small bodies, and, having so much the greater activity, are further decomposed, until their minuteness renders them invisible.

This last trait of M. De Buffon's theory, proves that it rests entirely upon the facts related by its author, that is, upon a false hypothesis. For with respect to infusions, we have already seen, that there is in them nothing to indicate organic molecules, since the moving corpuscula

corpuscula there, are actually animals, some of which are viviparous, and others oviparous; and because those multiplying by division, do not produce that progression of successive diminution in size, which M. De Buffon imagined he saw: but the smallest encrease like other animals.

Having found the living putredinous beings of semen, to be precisely of the same kind as those of infusions, by a direct and conclusive consequence it follows, that they could never be confounded with organic molecules, and we may say the same of seminal vermiculi, whose animality I have sufficiently proved by the facts already related in this treatise, and by those which are engrossed in the subsequent chapters.

The theory of M. De Buffon is thus completely destroyed. Such is too often the fate of those hypotheses, favoured by the inventive imagination of some philosophers, and which they afterwards search for in nature. This ingenious naturalist, dissatisfied with the theories of generation already framed, and hurried on by his taste for novelty, imagines in the body an animated matter, original, incorruptible, and always active, which he speciously denominates *organic molecules*. Making them act according to certain terms, and with a certain effect, he thinks he can explain the great work of generation, and the most inscrutable phæ-

nomena it presents. In this attempt, he uses all the powerful and persuasive eloquence which characterises him as the orator of the age. Prejudiced in favour of his theory, it was not difficult for him to find it in nature. His views were less directed to see what actually existed, than to what he wished to find. Like his celebrated countryman, the Reformer of Botany, who fancied that metals and stones vegetated, and thought he had evidence of this imaginary vegetation; that he saw seeds and plants, where there were none. If this learned academician, who has ever possessed my full esteem, would take the trouble to repeat his experiments, upon the semen of man and animals, with the best microscopes, and, forgetting the organic molecules, so dear to him, by imposing a rule upon himself, to receive, as truths, but the images transmitted by the senses, without adding the corrections of his imagination, as is the duty of a veracious naturalist; I may assure him, that he will find all I have so largely described in my works. I earnestly entreat him, not to reject this without a trial, which must certainly result to the advantage of truth.

I mean now to examine M. De Buffon's objections, to prove that the living beings seen in spermatic fluids are not real animals. Some of those objections I have already answered in the beginning of chapter 3. And it appears, from what

what I have hitherto said, that some of them are false ; such as, the formation of the animals under the observer's eye, the loss of the tails, and the diminution of their size. Two yet remain, of which I have not spoken ; their division into parts, and their frequent change of shape. These phænomena are real, although, as we have seen, they are never observed in seminal vermiculi, but only in the animalcula of putrid semen. M. De Buffon esteems such phænomena incompatible with the state of animals.

With regard to the objection concerning the division of the animalcula, I judge it needless to stop here for a discussion ; and I pass to the second, which relates to the metamorphoses of the putredinous animalcula. It is true, that the body of the animals familiar to us, is so constructed, that the shape never varies, and cannot be materially altered without destruction. But it is no less true, that there are others, and even many, experiencing the contrary ; as for example, among insects, several species of worms. To be convinced of this, it is only necessary to open the works of naturalists, and to glance the works of nature. Several worms of this kind have been elegantly described by Rhedi and Vallisnieri. In short, a single species may serve for many : this may be the *cucurbit worm*. Is it not certain, according

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ing to the observations of those two, and especially of the Tuscan philosopher, that they assume various shapes, and, in the words of this learned naturalist, “sometimes they contract
“and swell like purses, sometimes they extend
“and curve in a semicircle?” Does not Reaumur say the same of certain worms changing to flies, whose head, that part constantly the same in most animals, in this insect, so wonderfully changes its appearance, that it is sometimes extended, sometimes depressed, sometimes contracted, sometimes obtuse in one part, and acute in another? Do not we daily see the same changes in earth worms, snails, and particularly in leeches; extending the body till it becomes long and slender, then contracting it till it becomes short and corpulent; grows thick at one end, and small at the other? What shall we say of the wheel animal, that aquatic creature, which, from its wonderful and incredible metamorphoses, we may term the Proteus of the insect tribe? If wheel animals, leeches, shell and naked snails, so many species of vermes, are not degraded from the quality of an animal, notwithstanding their metamorphoses, the same should be the case with the animalcula of putrid semen.

I have still to appretiate two other objections. One is deduced from the singular motion of seminal vermiculi; the other from the
different

different effects of heat and cold upon them. Let us attend to the first. An animal, whatever it is, according to the reasoning of M. De Buffon, is subject to change its inclinations; sometimes its motion is quick, sometimes slow; then it stops and rests. But nothing of this is seen in spermatic vermiculi: they are in continual motion, they never stop, they never rest; and when once they stop, it is for ever. Hence, they cannot be animals.

This objection is like the former, both being founded upon the analogy of the large animals best known to us; but as they preserve their shape, they likewise have essentially the vicissitudes of rest, of motion, and of an accelerated or retarded motion, &c. But, to be certain that such vicissitudes are a characteristic quality of animals, it is improper to attend to large animals only; but it is necessary to examine other genera, and other species, and to dwell particularly on the smallest, especially those inhabiting fluids, which have more analogy to spermatic vermiculi. It is certain, that among them, M. De Buffon would have found several animals, which, far from having the alternatives of motion and rest, are naturally in motion, so that their life seems to consist of a perpetual motion. To be assured of this, it is sufficient, during the Spring, to observe the water of marshes, ponds, and ditches, where
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all insects are. There we shall see some in motion, but by contortions: such, for example, are the worms mentioned by M. Trembley, serving the polypi for food; the body is in a constant oscillation ^a. But, without the trouble of seeking them in the country, M. De Buffon may, at his leisure, observe them at home. The eels generally found in vinegar will evince it. If a small portion of this liquid is put in a thin glass vessel, and opposed to the light when the sun is bright: examining with a magnifier the higher parts, where the eels are more distinctly seen: their contortions appear incessant; they dart from one side to another; and this continues, without intermission, from morning till night. Thus, they persevere for several months; that is, till the end of their lives. It would seem, that this perpetual motion cannot be a sufficient reason to prove that spermatic vermiculi are not animals.

But, farther, such a motion is not natural to the vermiculi; it is forced and violent. When they have quitted their natural abode, to enter our atmosphere, they experience the lively impression of the air, which pains them, and obliges them to perpetual flight. Doubtless, the air is noxious to them, and occasions their continual motion, as is proved by the facts. To ascertain the point, I waited until the human semen

^a Mémoires sur les Polypes. Mem. second.

femen cooled ; I then took some drops, spreading them very thin upon the glass of a watch. It constantly happened, that the vermiculi of the spread drops, although of considerable thickness, perished sooner than those of the complete portion ; because, as I think, the former were more exposed than the latter to the air. At the same time, I put two equal portions of the same semen, one into a close, the other into an open vessel : the vermiculi in the latter always died sooner than those in the former. The privation I made them undergo, proved well how injurious this element was to them. They lived longer in vacuo, than in the open air ; so that when in vacuo they were still alive, all were dead in the open air. The difference of time at which they died was an hour and a half, two, even three hours, and sometimes longer, according to the seasons when the experiments were made. These facts demonstrate, that the air is noxious to the vermiculi, and the following prove that it is the cause of their being in continual agitation. With the blowpipe I formed capillary tubes, one end of which I immersed in recent semen : it ascended the cavity, filling the tube to a certain height. Breaking the tubes near the part to which it ascended, I presented this extremity to the blowpipe, and immediately sealed it hermetically. I did the same to the other end, by which
means

means the feminal fluid was deprived of all communication with the external air. I drew out the tubes so, that the thinness of the glass permitted me to see the vermiculi within. The peculiarities presented by the vermiculi in the tubes, were very different from those of the rest. All, or at least most part of them, had a singular mode of moving. Some had that sort of activity observed in those which experience the influence of the open air. Others had a continued irregular motion; they changed from quickness to inactivity, and reciprocally. Others stopped entirely, and, after resting some minutes, resumed their former velocity: besides, we did not see them inconsiderately running against the solid portions of the semen, as I remarked in the first chapter, but always avoiding them, turning aside, or retreating. It is true, these peculiarities always succeeded better, and with more uniformity, when the tubes were kept warm. I have before said, the longest period of life of the human spermatic vermiculi, was seven or eight hours, when exposed to the open air; but this period is greatly prolonged when they are included in tubes. In Summer I have succeeded in preserving them two days and more; and in Spring and Autumn they have lived almost three.

It is undoubtedly very surprising, that the vermiculi live longer in Spring and Autumn, than

than in Summer: the reverse should take place; because the heat of Summer should be more congenial to them, as it approaches nearer to the natural heat of man in life. At first it gave me considerable surprise, which increased upon reflecting, that in the open air the vermiculi live much longer when the weather is warm. This circumstance induced me to repeat my experiments; but I constantly found, that during Summer, they never lived as long in the tubes, as during Spring and Autumn. In Summer, I may even say, that they die sooner upon the warmest days. With a little reflection, it is not difficult to comprehend the cause of the difference. We have seen, that the semen of man, and of animals, when removed from its native situation, very soon becomes putrid; and that this happens sooner, as the heat is greater to which it is exposed. It is to this cause, therefore, that I ascribe the more immediate death of the vermiculi, in capillary tubes, during Summer. Having filled similar tubes with recent semen, and sealed them hermetically, I exposed some to the heat of the atmosphere at about 63° , and others to the heat of the human body, keeping them under the arm-pit in a large glass tube. My own heat, when in a state of health, is about 97° . The vermiculi exposed to the heat of the atmosphere, lived two days and a half; some even three: but those

those experiencing the heat of my body, supported it only from eleven to thirteen hours. This more immediate death cannot be ascribed to the greater degree of heat they suffered, since it is inferior to that they usually experience, or in which they naturally live. Nor can it be occasioned by the air; for the tubes being hermetically sealed, the air can have no influence. We cannot ascribe it to any other noxious principle arising from the nature of the tubes, since they were absolutely similar to those exposed to the atmospheric temperature, in which the vermiculi live much longer. It is therefore necessary to recur to some alteration, or some noxious quality contracted by the semen, which makes the heat accelerate their death; and I acknowledge, that I cannot find this alteration, or this bad quality, but in the principle of putrefaction of the semen, which is manifested by the foetor experienced upon breaking the tubes. It is doubtless this odour which must be fatal to the vermiculi, as I have demonstrated in chapter 6. The putrefying principle does not take place in semen exposed to the open air seven or eight hours in Summer, as I have convinced myself; but, upon the other hand, the heat of Summer, more nearly than that of any other season, approaches the heat which the vermiculi experience in us. Thus, we clearly see, why they
live

live longer in the open air during Summer, than at any other time; of consequence, we may comprehend why their life is abridged in proportion as the cold encreases.

But it is time to come to the objection of heat and cold, which our author thus proposes. Upon exposing the seminal fluid to the cold air, the vermiculi did not seem to suffer from it; and they continued to move with their usual quickness, as long as those that were not exposed, although the seminal fluid had acquired that degree of cold, in water upon the point of freezing, as one may be convinced by touching it. Upon the contrary, if the same vermiculi experience heat, their motion ceases, although the heat is moderate. In consequence of these facts, if the vermiculi are real animals, (it is M. De Buffon's reasoning I relate), they would present an appearance and constitution very different from the appearance and constitution of other animals; as too great a degree of cold relaxes and destroys their motion, and a mild and moderate heat preserves their activity.

It is to be regretted, that our author, instead of employing taction to judge of cold and heat, did not use a thermometer; for all philosophers know, that the touch is a very equivocal proof. He ought to have discovered precisely, at what degree of heat the motion

of the vermiculi ceases, and what is the degree of cold by which it is not relaxed. I have therefore esteemed it most essential to supply this defect, in order to obtain arguments more conclusive and decisive, and to know, if, with respect to heat and cold, the vermiculi are of a nature and constitution differing much, as M. De Buffon imagines, with the nature of other animals.

Although the observations of chapter 1. do not seem to bestow upon them that vigour of constitution sufficient to enable them to resist the cold, because it appears that their motion decreases with the heat of the atmosphere, so that, when the thermometer stands at 36° , they continue to move only an hour and a half; I resumed my experiments, resolving to extend them further, and to subject the semen to a degree of cold equalling freezing, carefully observing what should happen to the vermiculi. The semen I used, was that of the horse. Upon the 14. of January, I exposed it to cold: the vermiculi could not be more vivacious: but this diminished under the eye; and, in sixteen minutes, all were motionless, although the semen was not frozen.

The cold becoming more intense, upon the 18. I repeated the experiment. The vermiculi became motionless in eleven minutes; the thermometer

nometer standing 7° under freezing. The semen was still fluid.

I often repeated those experiments during this Winter. They constantly proved, that the duration of the motion of the vermiculi, was proportioned to the temperature of the weather.

In Summer, I prosecuted my experiments upon the semen of the horse. Then a thought started, of subjecting the vermiculi to the cold of freezing, by putting the glass in which they were, among snow. The same effect was produced by snow, as by the Winter's cold; that is, in fourteen minutes, it made them motionless; although, when exposed to the heat of the atmosphere, they continued to move seven hours and a half. But an accident that happened in this experiment, executed during Summer, afforded new intelligence, and divested me of a prejudice. Observing that the vermiculi had become motionless, I took the glass from the snow, and left it exposed to the air, when the heat was 81° . An hour after, by chance observing this semen with the microscope, I was astonished to find all the vermiculi reanimated, and in such a manner, as if they had just come from the seminal vessels. I then saw, that the cold had not killed them, but had reduced them to a state of complete inaction. I replaced them in the snow, and in three quarters of an hour took them away.

These are the phænomena I observed. In a few minutes, their vivacity relaxed, and the diminution encreased, until they lost the motion of progression, and retained only that of oscillation, which likewise ended in a few minutes more. Exactly the reverse was observed, when they passed from the cold of the snow to the heat of the atmosphere. The first motion that appeared, was that of oscillation: the body and the tail begun to vibrate languidly from right to left: then the motion was communicated to the whole vermicule; and, in a short time, the progressive motion begun. At first, it is scarcely perceptible: it soon encreases, and becomes very considerable. I may also add, that as cold does not destroy the motion of all the vermiculi at the same moment, but of some later than others; so does not heat affect them all with equal power.

I subjected the semen of man, and of the bull, to the same experiment, and had the same results as from the semen of the horse, excepting only that a degree of cold, less than that of freezing, destroyed all motion in the vermiculi of the bull.

Upon the approach of the following Winter, I resumed the same experiments, and I succeeded in reanimating the torpid vermiculi, by breathing upon the semen, by applying the finger to the tail upon which the drops were put, or
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by placing it near the fire. When removed from this heat, they fell into the same lethargy as when in Summer: they passed from the atmospherical temperature, to the cold of the snow. During this rigorous season, I exposed our vermiculi to a more severe trial. I subjected them to cold, more than 9° under freezing. As I expected, this immediately made them motionless. In five minutes, not a single vermicule moved. When they had been exposed five minutes longer, I transported them into warm air, leaving them there for some time. Although this intense cold continued ten minutes, the semen was not frozen; but it had fatally injured a complete third of its inhabitants, which exhibited no sign of life, when removed to, and kept long, in a warm situation; on the contrary, they had all the appearance of death. The other vermiculi recovered, indeed; but their motion was languid, in comparison to what it was before. This experiment was made 27 December, and I repeated it upon the evening of January 5, at a degree of cold 10° under freezing. I perceived, that in about a quarter of an hour, the semen begun to congeal about the edges of the glass. I then put it into a stove, but this had no effect upon the vermiculi. Not one recovered, and those enveloped by the ice perished, as well as those in the fluid part. The same happened to the

vermiculi of other two glasses, upon which I made the same experiments this evening; although I took care to regulate the different degrees of heat, lest too sudden a transition from cold to heat might be injurious.

Such were the experiments made by cold: any one may draw the conclusions. Very far from excluding the vermiculi from the rank of animals, it surprisingly confirms them in it: for, what can better prove animality, than languor and loss of motion, when affected by cold? What effect can more satisfactorily evince it, than to see this languor more immediate, as the cold is more intense; and to see the vermiculi revived, when brought to heat; and to witness their actual death, when the cold is of a greater degree? Such is the state of the greater part of small animals, when deprived of action, and rendered torpid by cold: with heat they recover life and motion, and yield under cold still more intense.

But how can these facts, multiplied, repeated, uniform, consequently certain and incontrovertible, subsist with the assertion of M. De Buffon, who thinks that cold does not impede the motions of the spermatic vermiculi? Instead of negating the assertion of this illustrious Frenchman, I think there is a method of conciliating our observations. We have already noticed the error which occasioned his confounding the feminal

minal vermiculi with animalcula, ascribing to the vermiculi those properties pertaining to the animalcula only.

It is very likely that the effect of cold, which, he says, he has observed upon vermiculi, is also a consequence of the same mistake; and this is the more probable, as it is seen in the putredinous seminal animalcula. Not only do the animalcula of infusions, at least several species of them, withstand cold of a great degree; but those found in putrid semen, are undoubtedly of that number. I am convinced of this from several experiments, which, to avoid the ennui of my reader, I shall not stop here to detail. But there is one circumstance I ought not to pass in silence. Although the animalcula can support a greater degree of cold than the seminal vermiculi, yet their motions become more languid, and they perish like the insects which yield under the greatest degree of cold.

When I found those modes of conciliation, between the observations of M. De Buffon and myself, with regard to the phænomena from cold, I attempted to find the same respecting those exhibited by the effect of heat. But this was impossible. My observations have been completely opposite to his. Those of M. De Buffon are comprehended in a few words: "The motion of the vermiculi ceases, when they are exposed to a small degree of heat."

I entreat the reader to examine mine, that he may be enabled to compare and form a judgement of them.

I placed upon the water of a vessel two watch glasses, one of which contained a given portion of recent semen, full of vermiculi; the other, an equal quantity of the same semen, old, and swarming with putredinous animalcula. To know the successive degrees of heat, I had put the ball of a thermometer into each glass. The water was gradually heated upon a slow fire. As the liquid in the thermometers ascended, I took some drops of semen from the glasses, for examination with the microscope. The putredinous animalcula lived at 99° ; at 104 , their motion begun to grow languid; and at 106 and 108 , all perished. The seminal vermiculi are of a more hardy constitution. At 106° , they were very active; some begun to perish at 120° , and at 131 there was not one alive; so that the difference which occasions the destruction of the one and of the other, is about 22° . The vermiculi were those of the human semen. I repeated the same experiments upon the semen of the horse, the bull, and the dog. Those of the horse and the dog perished at 126° ; those of the bull at 133 .

I varied the experiment. I filled some capillary tubes with semen, one part of which was full of seminal vermiculi, the other of the putredinous.

putredinous seminal animalcula. I sealed them hermetically, and put them at the bottom of a vessel full of water, which was gradually warmed; I also immersed the ball of a thermometer. When the water had attained 99° , I began to examine the tubes, one after another. In this new experiment, the seminal vermiculi of man, and of the animals I have mentioned, died at only 122 and 124° ; and the putredinous animalcula, at 106 and 108° .

These facts demonstrate, that, if we speak of animalcula originating in putrid semen, they are of a constitution better calculated to resist a degree of heat, which several other animals cannot support, and are killed by heat, only when it arrives at that degree, or about it, which is fatal to putredinous animalcula. If we speak of seminal vermiculi, we see, that instead of ceasing to move, and perishing at a small degree of heat, according to M. De Buffon, they support a degree which destroys several other animalcula. But this, far from being wonderful, is rather congenial with their nature, since they live in the bodies of warm-blooded animals; that is, in an atmosphere in general much warmer than the air, and the other fluids where the rest of animalcula are found.

CHAP. VI.

I FLATTER myself that the reader will not be displeased, if, in the rest of the observations, he does not find the same order and connexion I have before endeavoured to preserve. It is necessary to consider what follows as an appendix, which, I think, must indispensably be added, to prove, with greater certainty, the animation of the vermiculi, this being one of the chief objects I proposed; and, when once established, we shall not only see confuted the contradictory opinions that have been formed concerning the nature of seminal vermiculi, which have already been explained and discussed, but we may also anticipate every new hypothesis that may be suggested.

One day during Winter, I had a great quantity of semen then taken from a dead body^a; and, wishing to preserve the vermiculi some hours alive, I put the watch-glass where they were in the sunshine without a window. The heat of the sun was 70°, which kept them alive a considerable time; but, observing the vermiculi an hour after, I was extremely surprized

^a When the kind of semen used is not specified, that of man is always understood.

prized to find almost the whole motionless. I knew not whether this was the indication of real or apparent death; and, thinking to satisfy myself, by exposing them to a greater degree of heat, I transported them near the fire. Experience had taught me, how instrumental the influence of heat is in restoring the vermiculi to life. But this was vain; and although I kept them at this degree of heat a length of time, they exhibited no sign of life. It was otherwise with those I had left in the shade, and then carried to the fire; for I had another portion of semen in a watch-glass, in the same apartment. The vermiculi had become motionless like the first; but they soon resumed their original vivacity. What was surprizing in this phænomenon, seemed to me the effect of peculiarity; and I did not think of repeating the experiment during this Winter and the following Spring. But I had afterwards occasion to observe, that the sun, in a few hours, was constantly fatal to the vermiculi, although the intensity of the heat did not equal that degree which is fatal to them, of which I have spoken in the preceding chapter. This I ascertained, by means of the sun in Autumn: but the phænomenon, which I at first thought accidental, has to me appeared constant and invariable. The influence of the sun, at the same time, has no quality noxious to the putredinous

treminous animalcula of the same semen, provided the intensity of the solar heat does not raise the thermometer to 106 or 108° ; which also contributes to prove the difference between the vermiculi and animalcula.

The novelty of the results was sufficient to incite me to investigate the cause, experiment having shewn me, that a certain degree of solar heat quickly kills the spermatic vermiculi, although the same degree in an apartment does them no injury. I could not be persuaded that the simple heat of the sun occasioned this destruction. I imagined the cause entirely different. My first idea was, the agitation of the air. I thought that when I put the semen without the window, the vermiculi were more affected by the violent action of this element, and sooner yielded under it, than those within the apartment; at least, it was here less agitated. But this imagined cause was false: for I put two glasses, provided with the same semen, without the window, and equally exposed to the air; with this difference only, that one was in the sunshine, and the other in the shade. Those exposed to the sunshine, always died several hours sooner than those in the shade. Further, I put a division in the seminal fluid of the same glass, separating it into two parts; so that one was exposed to the sunshine, and the other was not. It always happened, that the

the vermiculi of this latter portion long survived those of the other.

Attentively contemplating the semen with the naked eye, I suspected another cause. I saw the semen not only greatly diminish, but become more dense, and change colour. I then thought that this grossness might be noxious to the vermiculi. To ascertain the fact, I employed an easy method, which was, to prevent the evaporation of the semen in the sunshine, because the density might be occasioned by the evaporation of the more volatile parts: and I attained my purpose, by hermetically sealing several capillary tubes full of semen, then exposing them in the sunshine, along with another portion of semen, in a watch glass. The heat of the sun raised the thermometer to 72° . The vermiculi in the watch glass did not live an hour; but those in the capillary tubes retained all their vigour at sunset, although the experiment was made in the morning; and an hour after mid-day, the heat of the sun equalled 104° . Upon the following days, I exposed to the sunshine other capillary tubes, prepared as above: the vermiculi were long alive. These repeated facts therefore prove, first, that the sudden death of vermiculi in the sunshine, is not properly the effect of the heat of the sun, as it would have killed those in the capillary tubes almost as soon as those in the open vessels; secondly,

condly, we cannot ascribe their immediate death but to some vicious quality or alteration acquired by the semen, when exposed to the air, and against which it is secured in a close vessel. But as it does not seem to arise from any other cause, than from the thickening of the semen, since putrefaction cannot begin in so short a time, we are induced to suppose this thickening to be the sole cause of their death, or at least an essential reason.

Those facts have been illustrated by the following. I placed in the sun two glass tubes, filled with semen to a given height, and stopped with a stopper well fitted; with this difference only, that the stopper of one tube touched the semen, and that of the other was an inch above it. The tubes were placed erect, each containing an equal quantity of semen. In an hour and a half, the influence of the sun had occasioned no evaporation in the tube with the stopper touching the semen: it was indeed impossible, as there was no vacuity between them. But the semen in the other tube had evaporated. I saw the inside of the glass covered with a thin pellicle, formed of a very transparent fluid, which could only be the more subtile parts of the semen volatilized by the heat. The quantity of the semen was diminished, which could not be otherwise, as it was a little thicker: neither of which circumstances was remarked

marked in the other tube. I examined the two femina with a magnifier. The vermiculi of that where no evaporation appeared, were full of vivacity; those of the other all were dead. Thus it is evident, that the solar heat does not kill the vermiculi, but that their death is occasioned by some noxious quality imparted by heat to the semen; which either consists in it becoming more gross, or in some other quality it thence derives, or which is produced upon this occasion. This circumstance also corresponds with the nature of the animals, which are injured and perish, if the ambient fluid in which they live begins to alter or spoil.

It yet remains to explain how two degrees of heat, different in effect, but equally intense, can have such opposite effects: for the immediate action of the sunshine changes the seminal fluid in such a manner, that it kills the whole vermiculi; while the same degree of heat, in a heated apartment, does them no injury. I have not made enough of observations to solve this problem.

We have seen, that the seminal vermiculi continue to move complete days in close vessels, and perish in some hours in the open air. I have shewn, that this long continuance of life in close tubes, arises from their being sheltered from the influence of the air. From analogy one would think, that this might occur at all seasons.

seasons. But arguments drawn from induction should not be used by philosophers, since they are often supported by deceitful facts: the present case may afford a new instance. During Winter, the vermiculi, in tubes hermetically sealed, become motionless in the same time as those in watch glasses exposed to the open air: that they should become motionless, the cold of freezing is not required. In an hour and a half, I found them motionless in the tubes, the same as in the open air, when the thermometer was at 45° . That the vermiculi may live in small tubes, a certain degree of heat is necessary, which my experiments indicate to be 52 or 54° .

When the vermiculi become motionless from cold in open vessels, it is not always a sign of death: sometimes it only indicates a simple lethargy. When included in tubes, I have, by means of heat, from perfect rest restored them to their natural motion. I have even produced this rest and motion successively, by transporting the tubes from heat to cold, and *vice versa*. We must remark, that the repetition of this operation enfeebles the vermiculi so much, that in a certain time they cannot recover motion, and perish for ever.

I wished to learn how long the vermiculi could remain lethargic without destruction, so that, passing them to a warm situation, they might
still

still resume their power and motion. I have found myself unable to determine the limits of this; but it appears in a great measure to depend upon the degree of cold to which they have been subjected. If the cold surpasses freezing, and we delay some hours to remove them to a warm situation, then they revive no more, or but a few are re-animated, and these are generally very weak and ill. If the cold is less intense, and the thermometer falls only to 39 or 41°, they may remain lethargic fourteen hours, and even longer. I do not intend here to demonstrate to the reader, that the accidents happening to vermiculi in capillary tubes, completely quadrate with those experienced by animals exposed to cold, and that cold is fatal to them as to many insects: this may be understood without explanation. If we should now unite these traits, with the rest dispersed throughout this work; with the death of the vermiculi caused by poisonous exhalations, by the odour of camphire, the oil of turpentine, the fumes of sulphur and tobacco, by the effluvia of most ardent liquors, and the electric spark, as I have proved; we shall have an assemblage of proofs, so convincing, so decisive of the real and absolute animality of the seminal vermiculi, that I know not what other evidence could be re-

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quired,

quired, to prove that atoms so minute are of such a nature as the vermiculi seem to be.

I wish to excuse the inconsistency between what I have hitherto said of seminal vermiculi, and the little I said of them in my first treatise upon infusions. There, I spoke carelessly of them; I had not then studied them; I had consulted what others had written upon them, and adopted the theory which seemed to be best supported by facts. I did not hesitate to adopt the opinion of M. De Buffon, and with him supposed the vermiculi not to be real animals; as this opinion appeared to be supported by observations more numerous, better detailed, more connected and convincing, than those of Lewenhoeck. Then, I thought thus, and should always have thought the same, had not the observations I have related convinced me of the contrary; and I flatter myself, that I shall not be reproached if my former opinion was different from that I now have.

This chapter shall be finished with reflections upon some questions, as curious as nice, respecting our vermiculi. To me they were communicated in a letter from M. Bonnet; and the reader cannot better judge of them, than when he has them before his eyes. After informing me of the singular opinion of Linnaeus, that the vermiculi are inert corpuscula floating in the semen, he adds, ‘ I return to
‘ the

‘ the seminal vermiculi, and I cannot doubt
‘ their existence. They are, of all the animal-
‘ cula of liquids, those which have most excit-
‘ ed my curiosity : the element in which they
‘ live, the place of their abode, their figure,
‘ motion, their secret properties, all, in a word,
‘ should interest us in so singular a kind of mi-
‘ nute animated beings. How are they found
‘ there, how are they propagated, how are
‘ they developed, how are they fed, and what
‘ is their motion? What becomes of them
‘ when the liquid they inhabit is returned by
‘ the vessels, and mixed with the blood? Why
‘ do they appear only at the age of puberty ;
‘ where did they exist before this period? Do
‘ they serve no purpose, but to people that
‘ fluid where they are so largely scattered?
‘ How far are we from being able to answer
‘ any of these questions! And how probable
‘ it is, that future ages will be as ignorant of
‘ the whole, as our own! if, as I have said,
‘ part 12. and 13. of the *Palingenesie*, our
‘ world has been chiefly made for understand-
‘ ings, which enquire into the history of sper-
‘ matic vermiculi, and that of the most myste-
‘ rious productions of the globe. You may
‘ see, in articles 131, 132, 134, 135, of my
‘ *corps organisés*, that, in my youth, I attempt-
‘ ed to consider our animalcula. Observe what
‘ is said upon this occasion in article 135, con-

cerning the animalcula of infusions : “ Re-
“ specting the appearance of animalcula in sub-
“ stances which have been boiled, or subjected
“ to a degree of heat, at which we cannot con-
“ ceive that any animal may live : the difficul-
“ ty ought not to surprise us too much, as it
“ is founded only upon our ignorance of the
“ heat which certain animals may support.
“ Besides, it is not certain, that those animal-
“ cula were in the infused substances. Per-
“ haps they might inhabit the air confined in
“ the vessel, and pass from it to the infused
“ matter. Perhaps there is a perpetual circu-
“ lation of those aerial animalcula in organised
“ bodies, and in bodies organised in the air.”

‘ I know no animalcula more fit than semi-
‘ nal vermiculi, to demonstrate how well it
‘ pleases the Supreme Wisdom to multiply sen-
‘ tient beings, and to leave no portion of na-
‘ ture void. Could we have suspected, that
‘ this precious liquid, the reproductive principle
‘ of large animals, was at the same time desti-
‘ ned for the aliment and pleasure of an innu-
‘ merable multitude of most minute animated
‘ beings ? It is thus, that this Adorable Wis-
‘ dom has presided over the formation of the
‘ universe, and has known to make the same
‘ production serve for such different purposes.
“ The Author of Nature,” have I said, *Con-
templation, partie 5. chapit. 17.* “ has left no-
“ thing

“ thing useless. The pollen consumed in the
 “ generation of plants is very little, compared
 “ with the whole quantity each flower affords.
 “ Wisdom has, therefore, created the indus-
 “ trious bee, which uses the superfluous part
 “ of this dust, with an art which the most
 “ skilful geometers know not sufficiently to ad-
 “ mire. The pollen of the stamina apparent-
 “ ly supplies the necessities of many other in-
 “ sects.” And those insects are in some re-
 “ spect to the pollen, what the seminal vermi-
 “ culi are to the seminal fluid.

‘ The origin of certain worms in the human
 ‘ body, and in the body of animals, is a pro-
 ‘ blem as yet unsolved by naturalists. Such,
 ‘ in particular, is the origin of the *tenia*. I
 ‘ spoke at length of this singular worm in my
 ‘ dissertation. The origin of spermatic vermi-
 ‘ culi is a problem still more profound. How-
 ‘ ever, I should much incline to presume, that,
 ‘ as those I have mentioned in my dissertation,
 ‘ they originate from without. The change of
 ‘ temperature, abode, and nutriment, may pro-
 ‘ duce, first in individuals, and then in species,
 ‘ very material alterations, to our eyes disguising
 ‘ the primitive appearance. A worm destined to
 ‘ live in the waters, and transported to our in-
 ‘ testines, might not perish, and yet be very
 ‘ much disguised, especially if introduced when
 ‘ young, or under the form of an egg, or of

‘femen : and, if the worm was to propagate,
‘the subsequent generations would be still
‘more disguised. Let us suppose, therefore,
‘that the femina of certain infusion animalcu-
‘la may be introduced into the seminal refer-
‘voirs, by the circulatory ducts : they might
‘be developed and live there. There is no
‘doubt, that this new abode, a temperature
‘and aliment so different, may greatly affect
‘the original form of the animalcula ; and at
‘length occasion changes, which may more
‘and more remove them from their first ap-
‘pearance. All mankind had the same origin.
‘What varieties, and striking varieties, are
‘there in the human species ! Let us com-
‘pare the inhabitants of the Frigid zone, with
‘those of the Temperate region ; and those of
‘this, with the inhabitants of the Torrid zone ;
‘and we may see the different species of men.
‘The femina of certain infusion animalcula
‘are probably so minute, that they may easily
‘arrive at the reservoirs of the seminal fluid.
‘Apparently, they are excluded only in those
‘seminal liquids that have acquired a certain
‘perfection ; which happens only at the age
‘of puberty. It would be a most curious ex-
‘periment, to try whether the animalcula of
‘infusions would live in some seminal fluids :
‘and, in the same manner, to try whether the
‘seminal vermiculi would live in infusions.

‘ It

‘ It would, above all, be necessary to regulate
‘ the temperature of the place, and of the fluid.
‘ Who can say, that this experiment, which is
‘ certainly very new, will not succeed. I com-
‘ municate to you all the ideas that suggest
‘ themselves to me. My maxim, in natural
‘ history, is to despair of nothing ; and to in-
‘ terrogate nature in every way, even the most
‘ uncommon. Why should we say a thing is
‘ impossible, because we have not seen it suc-
‘ ceed ? I found my maxim upon our pro-
‘ found ignorance of the secrets of nature, and
‘ upon the deviations which, in many cases,
‘ she seems to make from her ordinary course.
‘ Every where I see an universal latitude, the
‘ limits of which I am ignorant. They can be
‘ discovered by experiment alone. And how
‘ much may experiments of every kind be
‘ combined, multiplied, repeated, and per-
‘ fected ! ’

The difficulty of the questions proposed in this valuable extract of a letter, is too well defined by its illustrious author, not to be seen by one who has the smallest portion of philosophy. It will always afford me a good excuse, if I only attempt to answer the doubts by distant conjectures. The questions may be reduced to three.

1. What is the origin of the seminal vermiculi ?
2. How do they propagate ?
3. What purpose do they serve ?

I return to the first. Although M. Bonnet makes no assertion, we nevertheless perceive his inclination to think the vermiculi have an external origin. Such has been the opinion of many authors in esteem; and such is the opinion of those who think that the worms in the body of man and animals originate from without. Sir Charles Linnæus believes the abode of the *tenia* to be in the waters; there he has found them very small, and even in some fishes, particularly in tench, which seems to favour this opinion^a. But we should be certain of the identity of the species of *tenia* found in the waters, with that found in the human body; and of this we have not yet sufficient proof. We cannot deny, at the same time, that very certain observations demonstrate that some worms of the human body, at least of the bodies of particular animals, are actually produced by insects of the great world. Such are those inhabiting the rectum of the horse, the frontal sinus of sheep, goats, and the larynx of stags; by the discoveries of the celebrated naturalists, Vallisnieri and Reaumur.

With respect to seminal vermiculi, my observations will not allow me to ascribe them to an external origin; for, was it so, certainly I must once have perceived it. More than fourteen years have elapsed since I have been occupied with

^a See the Italian translation of *La Contemplation*, part. 10, chap. 26. Note, at the words *molte centinaia de piedi*.

with infusions, since I have studied the waters of marshes, ponds and ditches. I can however say, with absolute sincerity, that among the innumerable multitude of minute animals, there are none resembling the seminal vermiculi of man and other animals ^a. I do not deny, that by supposing they pass from the water to animated bodies, they may possibly undergo some change or alteration, operated perhaps by the causes the Genevese philosopher details, which he has rendered probable by demonstrating, that animals changing their climate and aliment, suffer an alteration: “*Ranæ in Ebusum insulam*
“*delatæ colores mutant; aves in regione Sep-*
“*tentrionali albescunt, in Meridionali nigres-*
“*cunt: sic vulpes, ursi, lepores mutato loco,*
“*colores et quodcumque mores mutant.*” I also acknowledge, that the form of the body of vermiculi may change in its proportions; that the

^a This had been already remarked by Lewenhoeck, as appears in his 301. letter: “*Licet varias et indole diversissimas aquas contemplatus sim, nec istius modi animalcula (id est spermatica) nec quidquam quod animalcula ista similitudine aliqua vel figura referret in ullis unquam aquis observaverim.*” Other observers agree as much as I could desire, with Lewenhoeck and myself. Among 146 genera which Muller has classed, he has only seen one resembling the spermatic vermiculi of the sheep. It is that he calls *Cercaria*. But this species, which is the only one that has been observed, and very seldom found, (in infusione animali raro), would be far from being able to give existence to all the different species of those vermiculi.

the parts may become larger or smaller, according as the new element agrees with their nature : but I cannot think they will lose their pristine figure, to assume one very different ; or that the first change will be such as to prevent them being recognised ; for then the internal structure must be changed also. But it would be as if one should say, that after their former organs were entirely or partially changed, they were new beings. This would rather be a creation, than a change.

The seminal vermiculi not only differ from animalcula in shape ; I have likewise shewn them to be of a nature and constitution essentially different. Such are the differences mentioned in the former chapter, and in that preceding it ; and I omit them here, to avoid repetition.

M. Bonnet suggests an ingenious experiment : to try whether the seminal vermiculi would live in infusions, and the animalcula of infusions in seminal fluids. I had already done this in part ; but upon passing the animalcula of putrid into recent semen, and the vermiculi of recent semen into that which was putrid, I saw the whole vermiculi and animalcula perish. At the same time, to satisfy the curiosity of my illustrious friend, instead of corrupted semen, I made use of vegetable infusions. I took the precaution, that those where I put the vermiculi were of the same heat as the semen when
in

in the body of the animal ; and that the semen wherein the animalcula were put, should have the temperature of the atmosphere : but I never could by this method prevent their death. There was, however, a little difference : the seminal vermiculi perished immediately, and the infusion animalcula died in a few minutes.

The very great difference of aliment to which the vermiculi must be accustomed, upon passing from without to the semen of animals, seems to me a sufficient reason why that cannot be their origin. For, the smallest animals of our world perish when obliged to change their food, as we see in caterpillars feeding upon determinate plants ; when the plants are changed, they die. So that, if we give silk-worms any other than mulberry leaves, they very soon suffer, if they do not immediately die. Besides, not only do insects perish when the plants are changed, but also when the places of the same plant they naturally occupy are changed. How many hundred species of insects feed upon the pear tree ? The ligneous parts serve some for food and habitation : some insinuate themselves between the bark and the wood, and never quit their retreats : others, to conceal themselves, fold several leaves together, and feed upon the most delicate parts : others prefer the branches, and, penetrating, form tumours there : some menace the flowers, and others the fruit. Let us
change

change this order : let us transport the insects of the wood into the bark, and, reciprocally, those of the leaves to the roots, and so on with the rest, changing their nutriment and abode. Doubtless, we shall see them quickly perish. I cannot perceive why the same thing should not happen to spermatic vermiculi, if they passed from water to the semen, since their aliment is completely changed. It is not enough to say, that we have an example in the worms in the horse, the sheep, or the stag, which live in a place, however, where their existence did not begin ; for I may reply, that they have not passed from a large to a little world, after living in the former, but are developed in the quadrupeds where they have been deposited by flies, where they remain until maturity, and where they feed upon the substance of the animal. If, before they acquired this maturity, their situation was fortuitously changed, it is most likely they would perish. We should see them perish, or rather, not expand, if the producing flies laid their eggs elsewhere. Whence it follows, that this example confirms the general law.

But if we cannot believe that the vermiculi come from without, what is their origin ? We shall reply, what Vallisnieri has said upon the origin of the worms of the human body : They are produced, fed, and they multiply in us
and

and in other animals : they pass from generation to generation with the nutriment the mother affords in the uterus, and the milk which is imbibed. This hypothesis seems to me more likely than the other. According to M. De Buffon's assertion, the semen of the female is full of vermiculi perfectly like those in the semen of the male. I doubt not the truth of this : it has been observed before him, and described by Signor Bono, a celebrated physician, and an excellent observer of spermatic vermiculi, incapable of altering any fact, as he is unprejudiced in favour of theory ^a. What has been observed in the female semen by those two naturalists, I have sometimes, but rarely, seen in the blood. In my long researches upon the phenomena of circulation, I happened to observe in the mesenteric blood of a frog, and three newts, I know not how many of the seminal vermiculi peculiar to those amphibia. There was no hazard of being deceived, because there was no room for mistake. One could not say that those vermiculi fortuitously mixed with the blood, from the rupture of some of the blood vessels of the testicles or the vasa deferentia, since the frog and two of the newts were females, and the blood vessels, as well as the generative organs of the male, were entire, as I assured myself by a careful examination.

^a Vallisnieri, tom. 2. edit. in folio,

mination. The vermiculi were actually imprisoned in the vessels, and were pretty vivacious. They were seen in the arteries, excepting a single time that I saw them in a vein. The artery of a frog tadpole shewed me some again; I even saw some in the blood of a sucking calf, and also some in the red globules of the blood of a sheep. I recognised them as real feminal vermiculi, for they had all their characteristics. Those observations prevented my surprise, when mixing a drop of semen with a drop of blood, so that the vermiculi were forced from their native place, they still lived as before. I have observed the same phænomenon with saliva; and it is natural to suppose that it will be found in other animal fluids.

From these facts I draw two conclusions. The first, that it is not absurd to suppose, that the mother may serve as a conveyance for the spermatic vermiculi, to pass them to the young. The second, that they live in the fluids of the young, particularly in the blood, and are *in a manner* retained there until puberty; then, the semen matured, affords them aliment, and a habitation fit for their propagation. I say, *in a manner*; because the rareness of the feminal vermiculi in the blood, sufficiently proves it not to be a fluid, which agrees too well with them, happening perhaps from the aliment found there

there not being very fit for them. Although the semen is derived from the blood, yet are they two very different fluids. To this it may be opposed, that the vermiculi found in the blood of the male are the seminal vermiculi, re-absorbed by the vessels, and mixed with the mass of the blood. This objection would be well founded in adult males; but it is insufficient where they are not so, as in tadpoles and sucking calves: in the former, the organs of generation are not developed, and those of the latter are not inhabited by vermiculi.

The father may be an instrumental cause of the propagation of the vermiculi. This may take place in almost every species of animals. This method I find at least more direct, or perhaps more natural, than the other: I speak of the act of fecundation, which may convey the germs of the vermiculi to the embryo, by the immediate vehicle of the semen. That the eggs of females may be fecundated, they must be bedewed by the spermatic fluid of the male, which must act upon the included embryo: it should act not only externally, but also internally, for we know that it regulates the parts of the foetus^a. It is therefore necessary that the foetus should be penetrated by it; and in this manner it will be easy to introduce the vermicular germs. These germs expand, and lay
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^a Bonnet, Preface de la Contemplation.

the foundation of a little colony of vermiculi ; which takes possession of the feminal fluid, and gives birth to a numerous people.

The second question respects the manner in which the vermiculi propagate. In the almost infinite number of experiments which I have made, I have always paid attention to this interesting point. After seeing a prodigious number of infusion animalcula multiply by a division of the body, I investigated whether the feminal vermiculi propagated in the same manner. But I have had no indication of this. It is true, when they proceed from the animal's body, or when the vermiculi begin to be ill, they are less fit for dividing than when in their natural state, when they are vigorous and full of life. I do not deny, that this is possible ; but, say it was so, it seems morally impossible that among so many millions of vermiculi which I have at different times observed, among so many species, there was not one in a state to divide, or which did divide, as is observed with the animalcula of infusions : neither have I observed, that the vermiculi propagate by shoots, like polypi. So that, abiding by the different modes of propagation hitherto known, it would seem a proper conclusion, that the vermiculi multiply by means of a foetus, or of eggs : but I must admit that I never saw either the one or the other.

I arrive at last at the question; What is the use of the vermiculi? Lewenhoeck's opinion is well known; he thinks they are the immediate authors of generation: so that those of man will be so many homunculi, and those of horses so many foals, and thus of the rest. We cannot deny, that this idea is very ingenious: it is unfortunate, that it is not real. I should depart from my plan, did I attempt to refute it: authors celebrated, and known to philosophers, have done this with success. But I cannot resist saying a word upon the beautiful discovery of Haller, which is completely decisive. He proves by facts, so convincing that we cannot refuse our assent, that the foetus belongs to the female; that, entire, it pre-exists fecundation. It is therefore clear, that the male vermiculi cannot be foetus. The circumstances establishing this discovery, are explained in his treatise upon the chicken.

If we refuse the vermiculi this noble purpose, we do not fail to ascribe others to them. Some physiologists have thought them the cause of venereal pleasures: others, that they preserve the fluidity of the semen: and others, that they effect certain unknown purposes. All have endeavoured to divine it. There exists in us and in other animals, this wonderful multitude of animated beings, and many larger vermes, of which Rhedi has given a history, in a work

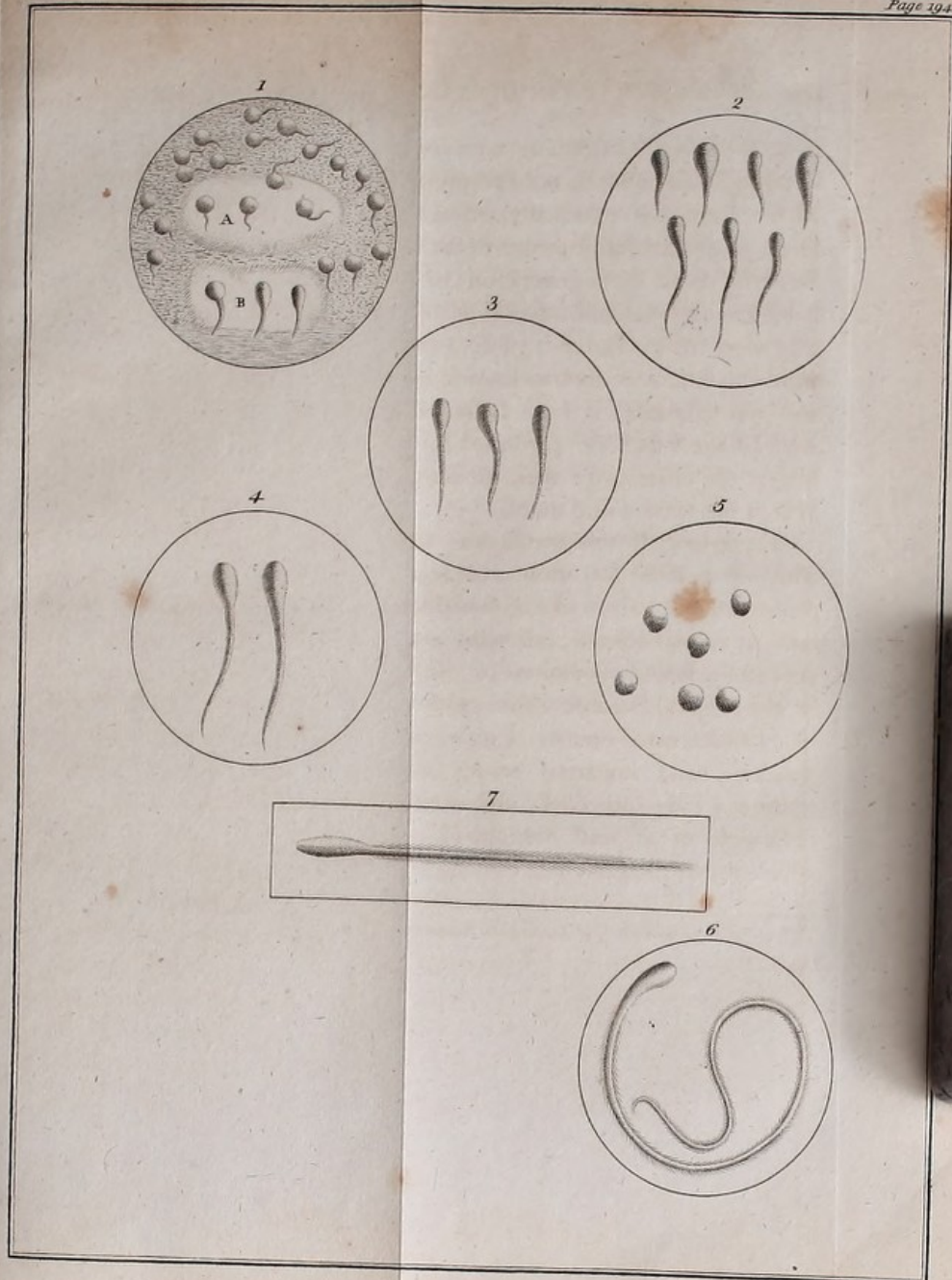
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upon

upon the subject, beginning with man, and descending to all animals, not excepting the smallest ^a. Each has externally animals which it feeds, as this naturalist proves in the same work. But why do all those generations of animals inhabit the external and internal parts of others; why were they created? This I think is beyond the sphere of human knowledge; and it will not displease, if here I should be silent. And I hope I shall be pardoned for only guessing at the other two: their intricacy has prevented me from doing more.

The intent of this work, was to examine, with all possible attention and accuracy, the properties and nature of the mysterious inhabitants of animal semina; allowing myself to discuss and refute the opinions of others learned in the subject, because their celebrity and division had created doubt. I leave the learned, judicious, and impartial reader to consider, whether I have succeeded, and whether I have dissipated, or at least diminished, the clouds which veiled them from the truth.

^a Degli animali viventi, negli animali viventi.



EXPERIMENTS AND OBSERVATIONS

UPON

ANIMALS AND VEGETABLES CONFINED IN
STAGNANT AIR.

INTRODUCTION.

IF the privation of air is a powerful method of preventing the production of animated beings, and of destroying life in those already in existence; the presence of air itself is considered equally noxious, when its circulation is impeded. A general rule has been established, That all animals and vegetables, forced to respire the air of close vessels, perish irrecoverably. It is thought equally certain, that seeds do not germinate, and that eggs are not excluded in the same situation. The great Boerhaave speaks thus of all. “*Ovula quorum-
cumque insectorum in vitris accurate clausis
non producunt, licet tepore fœta fœtus se-
mina plantarum vitæ macerata, optimæ com-
missa terræ atque requisito excitata calore,*
O 2 “ non

“ non tamen crescunt, neque dant vitæ ulla
“ signa actuosæ.”

Such is the opinion of all philosophers and naturalists, which for several years I confidently adopted, which I believed certain with respect to the animal and vegetable kingdoms. But my experiments upon infusions, inspired me with a just distrust of it. I had discovered, that the animalcula of infusions were produced and lived in vessels hermetically sealed; and I had seen the seeds I made use of, germinate there. Those facts did not agree with the opinion generally received, which induced me to investigate the limits and conditions of the maxim, *That air which is confined, and cannot be renewed, is noxious to the beings in life, whether animals or vegetables, subjected to its influence.* With this design, I resolved to repeat and to diversify my experiments upon the animalcula of infusions, and the seeds of plants growing in close vessels. I likewise wished to make experiments upon some animals, the great analogy of which with the animalcula of infusions, would induce us to suppose, that the noxious influence of stagnant air would not so easily affect them, as it would animals ranking higher in the scale of beings. I therefore made experiments upon the eggs of many terrestrial and aquatic insects; and nature has afforded those
illustrations,

illustrations, which former authors have sought in vain.

From effects I ascended to causes, and enquired why confined air could, in certain situations, be injurious to animated beings. Thus I passed from one research to another. The work insensibly encreased in my hands, and became much more considerable than I at first expected. Since I presume to publish it; to lessen the tædium of the reader, it is divided into three chapters or sections. The two last, in particular, explain the cause of the death of animals; and the first takes a comprehensive view of those which are subject to the impression of stagnant air only in certain circumstances.

CHAP. I.

I PROVIDED a certain number of vessels. In each I put an infusion of vegetable feeds, and then sealed them hermetically. I had got them expressly made at the glass-house: they were large, each would contain 14 or 15 pounds of water ^a. To examine the infusions, it was not necessary to break the vessels; but it sufficed to cause the included liquid pass to a dry part of the glass, and then observe it with a powerful magnifier. The glass was so transparent, that the animalcula were seen swimming in the fine pellicle adhering to the internal surface of the vase.

My experiments were made towards the end of Spring; and it was not long before animalcula begun to inhabit the vessels, and to inhabit the whole. The periods of their encrease, diminution, and destruction, were the same here, as in the open air.

Upon repeating the same experiment several times with different feeds, all afforded animalcula. I remarked one difference: the number of animalcula was never so prodigiously great, which

^a Twelve ounces in the pound.

which I have observed before. One mode by which those animals propagate, is the natural division of the body ; and this division likewise takes place in confined air. Applying the magnifier to the side of the vessel, I sometimes saw the animalcula dividing through the middle, so that the parts were connected by a short filament : others appeared like two minute elongated spheres, touching in several points : others exhibited, on the outline of the body, a contraction, or the rudiments of a division hardly begun.

The duration of life, and the multiplication I had observed in the animalcula of infusions, I likewise saw in the *anguillæ* of vinegar confined in close vessels. From the beginning of April, till the end of November, they were seen ; and they continually became more numerous. It is true, as the Winter advanced, the eels perished ; but the same happened to those in vinegar in the open air, the cause of which was the increased cold. We know that, during Winter, vinegar is without eels.

While I made those experiments, the water of some ditches was full of worms, insects, and the tadpoles of frogs. Upon these, I made the same experiments I had made upon the animalcula of infusions, and the eels of vinegar. I began with the larvæ of insects. Several I confined in vessels with ditch water,

that they might find aliment among the quantity of heterogeneous matter, of which the water was full. The larvæ did not suffer from this confinement : all changed to nymphs, from which, in time, proceeded winged flies.

Several tadpoles were also confined in the vessels along with a sufficient quantity of water, wherein I had put the marsh lentil to feed them. During twenty-four days their size considerably encreased ; and they died, it is probable, less from the stagnation of the air, than because the lentil was completely consumed. The tadpoles were young. I repeated the experiment upon grown tadpoles, whose limbs had already begun to appear. This period I chose, to learn whether they underwent their metamorphosis in confined air. Several did undergo it, and divested themselves of the tadpole form, to assume that of the frog. But others perished before attaining their new state.

Those animals inhabiting the waters, are not under the same necessity of respiring air continually renewed, as the animals which nature has destined to live amidst the air itself : I therefore thought of making some experiments upon them. But, as I had made experiments upon tadpoles, that is, upon a species of animal changing its state, I was desirous to repeat them upon some other animals of the same nature. Caterpillars first suggested themselves ;
and

and my first experiments were made upon silk-worms. Those I used with this intention, would a few days after have begun to spin their webs. Sealing them in a vessel hermetically, they were fixed upon the leaves of a mulberry branch, the ligneous extremity of which I had immersed in water, that the verdure of the leaves might be preserved for some time, and serve as food to the worms. More than a third perished; but the rest proceeded, as usual, till the eleventh day: they worked at the webs as usual, fixed them to the side of the glass, and there shut themselves up. Butterflies came from nine webs; from two there came none. Upon examining the sterile webs, I found the larva transformed to a chrysalis; but the butterfly did not come out, as it died while in this form. The webs of the eleven butterflies changing to caterpillars in this close vessel, were of good silk; and their only difference from others, was in not being so hard and elastic as webs generally are.

Other caterpillars, especially those of the elm and the oak, underwent the same changes as the silk-worms did in close vessels. I pursued the same method, of confining them in vessels where I had put some branches of the trees, immersing the lower extremity in water.

The metamorphoses of the larvæ of large flesh-flies, were more distinctly seen. I put a
piece

piece of flesh, nearly putrid, at the bottom of one of the vessels : it served them for food nine days, that is, all the time they were worms, and until they became nymphs. We know, that when their change to nymphs approaches, they abandon the mass of putrid flesh, and, seeking a dry situation, most commonly conceal themselves under an arid dusty earth.

Those I had confined, likewise abandoned the flesh, traversed the vessel, and were in constant motion more than half a day. Their anxiety to escape, was clearly evinced ; but being unable to effect it, they retired to the sides at the extremity of the neck, which was almost parallel with the table, and became perfectly tranquil. There they insensibly contracted ; their shape and colour disappeared ; they assumed a light chesnut colour, and exhibited every sign of being changed to nymphs. In this state they remained fourteen days ; then the shell of the nymphs bursting, the flies, completely resembling the parent fly, escaped. The winged insects lived several days in their prison, and then died, apparently for want of food.

I may say a few words concerning what happened to the seeds I used for infusions. They developed like the animalcula, all germinated well, and in a few days the budding of the branches and the leaves filled the capacity of my vessels. I should not neglect to observe, that

that all these vegetations appeared to be diseased, whether from their decay before producing fruit, or because their colour was yellowish. But, suspecting that this disease was not so much the effect of the privation of the air, as of the beneficent influence of the sun, and the moisture they must require to be supplied by the roots, which could not be obtained from the small quantity of water, I endeavoured to discover the real cause. I put the same quantity of seeds into vessels hermetically sealed; and, instead of water, substituted a given portion of earth well moistened. The plants soon sprung up; and being some hours of the day exposed in the beams of the sun, in a short time they reached the summit of the vessel, without yellowing, and they did become yellow only after a considerable period. I should add, that the seeds I made use of were, pease, maize, kidney-beans, red and bearded wheat, and rye. I planted two stalks of rye under the neck of the vessel, where there was a sufficient space for their elevation: the neck being very long, they exhibited unequivocal marks of vegetation, and the ear shot out of the calyx. This fructification would have made the greatest progress, but for the approach of Winter.

To complete what I have said of seeds confined in vessels, I shall remark, in passing, that although I have in this way observed more than
 twenty

twenty species at different times, I have not found one that did not germinate. We must not neglect a fact essential for their production. Whenever they spring in water, which takes place as well in close as in open vessels, the infused seeds must be partially above the water, otherwise they perish. This precaution was before me discovered by the celebrated naturalist M. Duhamel.

But if vegetable semina germinate without exception in confined air, what are we to think of animal semina and the eggs of insects, which, according to Boerhaave's opinion, and that of many philosophers, should remain sterile, even when the operation of circumstances the most favourable to their production concurs? With respect to this, I thought it was better to consult nature, than to trust to the science of others. I therefore made experiments upon many eggs. Those I made use of, were the eggs of several species of small beetles, flies, large flesh-flies, nocturnal and diurnal butterflies; and I took care to mark scrupulously what happened to each kind. I foresee the anxiety the reader must experience to learn the result of those experiments; and, in two words, I satisfy his curiosity by saying, the whole different species were produced the same in confined as in open air. Boerhaave, adopting the received maxim concerning the sterility of eggs in confined air, thus

thus expresses himself in the *Prælectiones Academicæ* ^a. “ Ova Bombycis in aere calido excluduntur si libere admittatur. Eadem in phiala clausa nunquam producant suum animal.” Now, the fact is, that those eggs hatch very well in close vessels, as I have been convinced by every experiment I made.

We must conclude, from all I have said, that the air of close vessels is not an impediment to the production of plants or animals ; but that plants, without any exception known to me, grow well there ; that animals encrease and propagate their species. Those which undergo metamorphosis, experience the whole successfully, in close, as well as in open vessels.

Why do we generally believe that stagnant air injures the production of animals and vegetables ? Analogy is, I believe, the cause of this remarkable error. It was observed, that the animals and vegetables upon which experiments were made, soon perished in close vessels ; that seeds and eggs confined there were sterile : and a general conclusion was drawn, that stagnant air was decidedly noxious to both the kinds of living beings.

In the beginning of this chapter, I have said, the vessels I employed were very large ; that each might contain fourteen or fifteen pounds of water. With those vessels I obtained the
results

^a Tom. 2.

results I have mentioned. But the consequences were very different when I used vessels successively smaller. In proportion as their capacity decreases, the eggs or seeds do not germinate, or they perish when scarcely developed. Animals of every species in a short time die there. All the naturalists who have seen in their experiments a different result from that afforded by mine, have undoubtedly made use of vessels too small, otherwise they would certainly have had the same consequences with myself. However, I do not deny that their error respecting animals, might arise from the nature of the animals upon which their experiments were made; for those animals must inevitably have perished, however capacious the vessel might be; for example, warm-blooded animals. To understand this better, and to evince the truth of what I say, I shall enter upon some details.

The animalcula of infusions originate, live, and propagate, in the vessels I first spoke of: this also happens in vessels one third of the size; only we then begin to perceive the disadvantages of the stagnant air. When the capacity is such as to contain three pounds and a half of water, the number of animalcula is less; they multiply less, and die sooner. Upon diminishing the vessels, the larger animalcula are not seen; and neither large nor small, if
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the internal capacity does not exceed seven or eight inches.

The nymph of the gnat seems to support this situation better than the animalcula of infusions. In five inches of air, several changed to the winged state. As the quantity lessens, they perish proportionally sooner.

The eels of white vinegar are particularly remarkable: they live and multiply prodigiously in a volume of air not exceeding three inches; and die in several days, only when confined in a tube where there is an inch vacuum. I speak of white vinegar, for the effects are very different in red. In my experiments, the eels of this did not live five days in a vessel where the vacuum was eleven inches. This did not happen because the vinegar underwent an alteration in the vessels, but rather because the eels of red vinegar are of a nature different from those of white; which I believe to be the more probable cause of the difference I remarked in the figure of each species.

Tadpoles perished in a very few days in nine inches of air; and in three hours, if the vacuum was only three inches.

When caterpillars and the larvæ of flies are confined in but eleven inches of air, they die before transforming to chrysalids. The larvæ, in particular, soon after being confined in a close vessel, deserted the putrid flesh put in a
long

long with them for food, and with agitation traversed the vessel, disregarding the flesh. They lost motion and life after various times, longer or shorter. If the vessel was larger, they lived longer: if smaller, they died sooner.

Larvæ, when changed to nymphs, suffered less from the small quantity of air. In a vessel where larvæ had died, I confined the same number of their nymphs. The flies of some came out: but it is necessary to observe, that the wings and body were distorted: they seemed to have been produced against the will of nature. This never happened to the butterflies of several species of chrysalids, although the vacuum of the vessel was very small.

What I have hitherto said, will apply to seeds and eggs. I omit telling the reader, the trouble I had to find the successive capacities of the vessels where the seeds and eggs ceased to germinate: but, adopting the general result, I shall say, that when the capacity of the vessels was but three or four inches, neither seeds nor eggs have developed.

We must therefore conclude, that the production of vegetables, and of some animals, takes place, as well in confined, as in open air, provided the quantity of air in the vessels is considerable; but, on the contrary, when it is not, that it becomes fatal to both. The pre-
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cise quantity which may be noxious, can only be determined by the nature, constitution, and quality, of the animals and vegetables confined.

My experiments being made at different seasons of the year, I discovered another fact, which is; That the death of animals is not only accelerated by diminishing the size of the vessels, as I have demonstrated, but also by the encrease of heat. This is particularly seen in those animals, which are easily procured at any time of the year, and live long without food: such are, newts, leeches, land and water serpents, vipers, and some species of fishes. I observed, as much as possible, that the individuals I took for my experiments should be of equal size, and equally vigorous; so that the comparisons might be more just. Here follow the facts which I found. Upon the fifth of April, along with other things, I prepared three jars; the first might contain six pounds of water, the second four, and the third two. In each, four newts were confined. My experiments led me to examine, whether the animals died sooner as the volume of air was diminished. This was the case.

In the smallest vessel, the four newts perished in forty-one hours; in the intermediate vessel, in two days; and in the largest, in seven days.

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Upon the same day of April, I made a similar experiment with leeches. I confined four in each vessel. They lived there a long time, in comparison to the newts. In the smallest vessel, they perished in three days; in the next, in nine; and in the largest, in thirty-two days.

I repeated this experiment 12. May, in the same vessels, and with the two species of animals. Both died much sooner. In twenty-seven hours, the newts in the smallest vessel died; in the next in three days, and in the largest in four days. The leeches in the smallest vessel died in two days, in the next in five, and in the largest in nine. The more sudden death of the newts and leeches in the month of May, I suspected to be occasioned by the increased heat of the season; seeing, in the month of April, during the greatest heat, the thermometer ascended to 57° , while in May it stood at 70° . My suspicions were realised, since, in the months of June and July, the death of the animals was accelerated. In July, the thermometer being at 82° , the four newts in the largest vessel died in twenty-three hours, and the leeches in thirty-five.

What has been said of leeches and newts, should apply to snakes, vipers, and fishes. The results from these, corresponded with the former. The death of the whole was not only
accelerated

accelerated in proportion to the small quantity of air they were forced to respire, but also in proportion to the encrease of heat: I observed the reverse only twice, which we must view as arising from some accidental circumstance.

I waited until Winter, to make the experiment inversely; that is, to learn whether the death of animals was retarded in proportion to the encrease of cold. This experiment succeeded with vipers and newts, which were the two species I then had at command.

The newts in the smallest vessel lived twenty-two days, in the middle sized thirty-four days, and in the largest two months. Vipers lived still longer. The vessels were placed in a situation where the thermometer stood at 48° .

In a greater degree of cold their life was protracted still longer. The newts and vipers did not perish in three months, when the smallest vessels were kept under snow. Both kinds died upon being exposed to the temperature of the atmosphere for some days during Spring.

Such are the principal results I have been able to collect, from the experiments related in this chapter. Those results are of great utility, because they elucidate the subject; but they leave us at liberty to fancy the cause, or rather the desire of seeking it. If the observer is a philosopher, he will endeavour to discover the

reason accelerating the death of animals in small vessels, and retarding it in large. Why is it accelerated more by heat than by cold? Whence arise those diversities in the time of an animal's death? Why may one quantity of air be noxious to one species of animals, and indifferent to another? The solution of these problems depends upon our knowledge of the cause of death in stagnant air. This ancient and most famous question has always divided the celebrated modern philosophers. Notwithstanding its importance, I shall attempt to discuss it. I shall examine what has been already written upon it, and shall adopt the opinion which to me seems the most consistent with facts; that is, with truth. Since we have seen that the eggs of animals, and the seeds of plants, remain sterile when put in a small quantity of air, I shall not fail to add a short sentence or two upon the cause of their sterility.

CHAP.

CHAP. II.

THOSE who have killed animals in close vessels, have, in their experiments, remarked two phænomena: first, that there is accumulated upon the sides of the vessel a quantity of vapour exhaled from the animal; secondly, that the air has lost a certain degree of its elasticity. These two phænomena have produced different opinions. By one, the death of the animals is ascribed to those exhalations which, being confined in the vessels, are respired by the animals, and thus become fatal. Another opinion maintains, that the exhalations cannot be mortal; but that the diminution of the elasticity of the air, occasioned by the exhalations, or a portion of the air being destroyed by respiration, becomes fatal to the animals.

The experiment of Pistorini of Bologna is specious. To appretiate the force of both opinions, he reasons thus. Supposing both opinions to be just, it should necessarily happen, that two animals confined in the same vessel die sooner than if they were alone, provided the vessel is the same, and the animals of the same size and species. We must therefore recur to the exhalations from the animal, or the dimi-

nished elasticity of the air, occasioned by the effluvia escaping from its body, or by the respiration itself. But it is always certain that, doubling the number of animals, the exhalation and respiration are doubled; and consequently the diminution of the elasticity of the air should be doubled. Pistorini did not find this result. Two animals died in the same time as one; although he used the same vessel, and animals of the same size and species ^a.

The singular consequences of the experiment, induced others to repeat it, among whom was the illustrious Professor Verati. His experiments were made upon birds, and upon frogs. A pigeon lived three hours and three quarters. Two pigeons in the same vessel lived only half the time. Three swallows died in little more than half an hour, two swallows in less than an hour, and one swallow lived almost two hours. He remarked nearly the same with sparrows and quails: three died sooner than two, and two sooner than one. But with frogs it was quite different. In eight days four died as soon as two, and one alone lived no longer than three. So that the experiments upon birds were very different from those of Pistorini; and those of Pistorini agreed with the experiments Signor Verati made upon frogs. And here we see, that, in this mode of death, nature is different

^a Act. Bononien. tom. 2. part. 1.

ferent in different animals, which is the cause of the discrepancies between Verati and Pistorini.

Signor Cigna, a celebrated professor, has also engaged in an examination of those differences: he thinks to have destroyed them by his exact experiments; the result of which is this. Where the frogs confined in the vessels are deprived of water, as it would appear those of Signor Verati have been, there, it is true, that the plurality of frogs does not accelerate the death of those confined; at least, this is often the case. When frogs are confined along with water, which is their natural aliment, it is almost certain that the acceleration of death is in proportion to the number of those amphibia^a.

The experiments related in the preceding chapter, induced me to engage in this enquiry also. If it was true, that several animals of a given species confined in the same vessel, died in the same time as when there was only one animal, and if the phænomenon was not accidental, but constant, it must (as I said before) be regulated in a manner proportioned to the smallest quantity of air confined along with the animals. But we may easily see, that the smallest quantity of air should always be where there are most animals. As frogs, according to Sig-

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^a Miscell. Taurin. tom. 2.

nor Verati, had disturbed the order established by this law, and as I could not repeat the experiments of Pistorini, because he has not specified what animals he employed, I made my experiments upon frogs also; some in vessels with water, and some in vessels without, thus to come near the method followed by those authors. In three vessels, each of which would contain five pounds of water, I hermetically sealed up frogs; that is, two in one, four in another, and eight in the third. In this last, the eight frogs perished in twenty-six hours; in the second, the four frogs perished in one day; and in the first, the two frogs perished in two days.

At the same time, I made a similar experiment with other three vessels, as large as the first, with the same distribution in the number of frogs; so that there was no difference between this experiment and the preceding; only, in it, there was no water in the vessels, and in this four ounces were in each. In two days, none of the frogs were alive in the vessel with eight: in that with four, they lived three days and a half; and five days where there were two frogs. During these experiments, the thermometer stood between 63 and 70°.

I repeated both experiments; the circumstances the same in every respect, excepting that the heat of the weather was greater, and then

then the thermometer ascended to 90° . In the first of these experiments, the eight frogs in the first vessel died in twenty hours; the four in the second in nineteen; and the two frogs in the third in about two days. As to the second experiment, made at the same time, and in which each vessel contained about four ounces of water, the result was as follows. The eight frogs in the first vessel died in thirty-two hours; the four in the second in two days; and the two in the third in three days and a half.

I repeated both experiments several times, which, to avoid the tedium of dry details, I shall not circumstantially describe, and will only speak of the results which I had from the vessels without water. Sometimes I observed those irregularities which I have already remarked. It sometimes happened, that a greater number of frogs perished in the same time, and sometimes later than a lesser number. But when the frogs were in water, they constantly perished sooner, as their number was greater: eight died first, then four, and lastly two. It happened only once, that all the eight were alive when one of those was dead in the vessel confining four.

From all these facts, added to those related by Signor Cigna, it must result, that frogs corroborate the general rule, *That animals, without exception, perish in confined air sooner, according*

ding as their number is encreased. At the same time, discrepancies will be seen in frogs confined in vessels without water: but I know not whether they should be regarded as such, because the privation of water is injurious to those animals; which is remarked by Signor Cigna. Frogs in open vessels die in a short time, if they want water. It is therefore absolutely necessary to proscribe this cause disturbing our experiments.

The reason of the discrepance in frogs being found, perhaps it would not be so difficult to find it in the animals of Pistorini, had he mentioned the species, and the manner in which he conducted his experiments. With respect to what Signor Verati relates, we only know, in general, that he made use of birds. But he has found that this kind of animals, as I have myself found, and as I shall soon observe, agrees well with the rule of which we speak. We have therefore reason to suspect, that Pistorini's experiment has met with some accident, without knowing in what the accident consisted, which rendered his results different from those of others. Perhaps it was occasioned by the birds themselves: perhaps, that one which he confined alone, was less vigorous than those he confined together; whence they all died in the same time. Perhaps, in the experiment with the two birds, all communication with
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the external air had not been prevented ; which might easily happen, if the top of the vessel had been covered only with leather, or any substance of a similar nature ; or if, upon inverting the mouth of the vessel, it had not been well fixed with mastic, or some such substance, to the plane of position. It is very possible that some invisible hole might remain, or some opening, by which the air might get admission to the vessel. To take away all suspicion of foreign air, it is necessary to seal the vessel hermetically, or to immerse the mouth deep in water ; as I shall more clearly explain.

In the preceding chapters we have seen, that the heat of the weather accelerates the death of the animals confined. May not we suspect that Pistorini had made the experiment upon a single bird in very warm weather, and that upon the two birds in very cold ; and the death of the two birds by this means be retarded, by the coldness of the air, which would occasion the death of both, in the same time as that of the bird which was alone, because the death of this bird might be occasioned by the greater heat ? Here I shall leave this irregularity. But, before resuming my principal object, I should throw a glance upon a doubt which occurred upon seeing animals die sooner in a close than in an open place, when the number of them is encreased. I know not whether the more im-
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mediate death is caused by the diminution of the volume of air alone, or if the number of animals will in another manner effect the more immediate death, and become a new cause why it should happen. To be satisfied which was the case, I selected three equal vessels, and a certain number of the largest frogs, as equal in size as possible. I confined two in one vessel, along with a pound of water; and put one alone in each of the other two vessels, along with a pound of water, adding a quantity of water equal to a frog in size. To discover what quantity equalled the bulk of a frog, I immersed it in a vessel, observing the quantity that overflowed. By this method, the quantities of air in the three vessels were equal, although in one vessel there were two frogs, and in each of the other only one. If the greater number of animals occasioned the acceleration of their death absolutely, because the volume of air was diminished; in this case, the bulk of air was equal in all the three vessels, and the four frogs should die in the same time nearly. If the greater number of animals influenced the acceleration of death, the two frogs in the same vessel should die first. I have said nearly, for although every thing is equal, it would be very remarkable if the animals died in the same time. The two frogs in the first vessel lived two days; that in the second vessel died in
three

three days and seventeen hours ; and that in the third, died in three days and a half. This result will demonstrate the influence of number in accelerating the death of animals ; and other experiments confirm it in a manner which cannot admit of contradiction. Five times I have repeated the same experiment, with the same consequences. The two frogs in the first vessel, always died sooner than those in the second and third ; and the difference of time was very perceptible ; it has uniformly been a day, sometimes a day and a half, and sometimes longer.

I made a variety in the experiment in another manner. Instead of two frogs, I put three in the first vessel, and only one in the rest ; but equalizing the volume of air, by adding a quantity of water equal to two frogs in bulk, which made the quantity of air equal in each. The three frogs in the first vessel died not only sooner than those in the other two, but the difference here was still more conspicuous than before ; it was two days and seven hours in the death of the frog in the third vessel. Increasing the number of frogs in the first vessel, their death was constantly accelerated with respect to the frog in the second and third ; although I took care to equalize the volume of air, by adding water equal in bulk to the frogs in the first vessel.

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This experiment upon frogs I extended ; and changed the subjects ; making experiments upon several small terrestrial quadrupedes and birds. But, notwithstanding the equal quantity of air in the vessels, I always found that as the animals were more numerous, they died sooner, and that the acceleration of their death was constantly in proportion to the encrease of the number.

Thus it is evident, that the death of animals in close vessels follows this invariable rule, that it happens sooner as the animals are more numerous. But, by what physical agent ; by what means are they in this manner destroyed ? Perhaps it is by their breath, or perhaps by the humidity of the air which they respire. Let us examine these two opinions with care ; beginning with that which is founded upon an alteration in the state of the air.

That the air loses part of its elasticity, evidently appears by barometers placed in the vessels where animals are confined. The descent varies. Stair observed, that his barometer fell an inch in a vessel where a rat had died. In the experiments of Verati, it sometimes fell eight lines, sometimes nine, twelve and more, according to the nature and number of the animals confined. The experiments of Mayow, Boyle, Hales and others, agree in establishing,

establiſhing, that a portion of the air is deſtroyed by animals confined in a cloſe veſſel.

We cannot from thence conclude, that the death of animals in cloſe veſſels is occaſioned by the diminſhed elasticity of the air ; at leaſt, there are no experiments poſitively proving it. We muſt firſt ſee whether this alteration of the air always takes place when animals die in cloſe veſſels. Secondly, whether this degree of alteration is ſufficient to kill the animals : for we know that every degree in the diminution of the elasticity of the air is not fatal to them.

In this Signor Cigna has laboured in a manner worthy of praiſe, and I ſhall make uſe of ſome of his ideas. I have made a courſe of experiments with the ſame intention, which ſhall be abbreviated after mentioning the methods employed. I uſed ſeveral air-pump glaſs receivers. I immerſed them in water, inverted on a plane. They were of that kind opening and cloſing above by means of a ſtop-cock. The receiver being left open when immerſed in water, there was a free paſſage left for the internal air to eſcape above, in proportion as it was compressed by the riſing water ; and thus the remanent portion of air in the receiver preſerved the natural degree of density of the external air, which was abſolutely neceſſary for the exactneſs of the experiment.

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This being done, I closed the vessel; and, to make it more secure, I passed several folds of leather around the stop-cock, which took away all communication with the external air: I was certain that the external air could not insinuate itself, for I used the same receivers as in my pneumatic experiments. Having put the animals in the receivers, I could mark the diminution of the elasticity of the air, by the ascent of the water within. If the animals were aquatic or amphibious, I let them remain in the water. If terrestrial, I put them in a vessel which was suspended by a hook at the top of the receiver.

My first experiments were upon frogs. I confined seven in one receiver, leaving a bulk of air equal to a pound of water. In half an hour, the water within began to rise above the level of that without; which evidently proved, the elasticity of the internal air was affected. The ascent continued until all the frogs were dead, or dying. The water had risen eleven lines.

I repeated the experiment in the same manner, confining only four frogs in the receiver. When they were dead, the water had ascended ten lines.

The elevation of the water was one line higher, in an experiment with two frogs.

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The water ascended nine lines, when I used only one frog.

I made these experiments upon newts, reserving in the receiver the same quantity of air as for the frogs. The death of eight newts raised the water scarcely an inch; of four newts, nine lines; of two, six lines; and of one, five lines. The elevation of the water, therefore, diminished with the decrease of the number of newts.

After the death of eleven leeches, the water ascended five lines and a half; and after the death of three, only one line.

Several naturalists have remarked how much small animals destroy the elasticity of the air. I had remarked this also in birds: it was chiefly upon them that the researches of Verati were made. But nobody, that I know of, has made experiments upon an animal which partakes both of the nature of a bird and of a quadruped, although it is not properly either the one or the other. I speak of the bat; this animal, so disgusting and forbidding in appearance, but which is at the same time as perfect as other animals, and connects quadrupedes with birds. The ambiguity in the nature of those animals, made me wish to make them respire the same air in close vessels; but I thought first of trying how long they could support a vacuum. They died sooner than cold-blooded ani-

mals. Five bats, successively subjected to the experiment, did not live three minutes. They were of that species, called by M. D'Aubenton, the horse-shoe bat, from the circular line upon the nose.

Although the death of bats in vacuo was sudden, it was under limitations. Four bats confined in a close vessel, lived scarcely an hour and a half: one lived almost three hours. The water of the vessel in which were the four, ascended an inch and seven lines; that in the vessel where there were two, an inch and three lines; and in that with one, eleven lines.

I extended my experiments to several reptiles, vipers, and some land snakes. Both species of animals having died in close vessels, the water was elevated to a certain degree, as their number was greater. The greatest elevation occasioned by three vipers, was an inch and three lines; and the least elevation by the death of one viper, was six lines. The death of one snake, raised the water four lines, and of five snakes, an inch and seven lines.

There is the same law with small lizards and fishes. The elevation of the water is proportioned to their numbers.

I have repeated all these experiments at all seasons, and have always seen the water within the receiver rise, with this single difference, that the elevation is more accelerated in warm,
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than in cold weather, as well as the death of the animals upon which the experiments are made. I have also constantly observed, that the elevation of the water is so much the less, as the animals are smaller. Vipers, snakes, and bats, raised the water more than frogs, newts, and lizards; and lizards raised it more than leeches. The death of a barbel weighing a pound, raised it above an inch; that of one weighing two ounces, raised it only two lines. It seemed to me, that in consequence of this proportion, the death of the smallest animals should raise it very little, or hardly at all; which would happen, if their death was not occasioned by the diminished elasticity of the air; because, the water within the receivers remaining at its original level, would evince, that the elasticity of the air underwent no alteration.

To discuss this fact, was of the greatest importance. I began with the larvæ of large flies. Thirty were put into a very small receiver. They were extremely minute, being newly produced. I left them upon the flesh where they had been deposited by the mother. They lived only seven hours in the receiver; and the water ascended a line and a half. I repeated the same experiment upon other fifteen worms. After their death, I could scarcely perceive that the water was elevated above the

level; and it undoubtedly was not, when I repeated the experiment upon eight worms, although the whole died.

The larvæ of common flies exhibited nearly the same phenomenon. The water rose a third of a line, when the number was great; when it was small, the rise was not sensible.

Seven earth worms dying, did not raise the water.

The larvæ and nymphs of gnats, had only an inch of air, and died in less than a day. They were to the number of some hundreds; yet, after their death, the water in the receiver stood at its original level.

The death of five rat-tailed worms confined in a receiver, did not sensibly alter the level of the water; but the death of a greater number occasioned a perceptible elevation.

Some stagnant waters are full of a sort of minute animals, called by naturalists, waterlice or fleas. Those animals are in constant motion, darting through the water where they are. Several thousands lived two days and some hours in a receiver, and died without a sensible elevation of the water.

I could perceive no elevation in water full of animalcula, confined in a receiver, although the whole died in two weeks.

The experiments I made upon several insects undergoing no metamorphosis, as, shell
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and naked snails, millepedes, &c. and even upon others which do change, as, caterpillars, chrysalids, and nymphs, demonstrated to me, that the death of a great number raises the water a little in the receiver; when the number is small, there is no elevation.

We have now enough of facts to decide the question in which we have engaged, especially when they are compared with those of Signori Cigna and Verati; and I draw two principal conclusions. First, That there are several animals, which, dying in close vessels, do not diminish the elasticity of the air: The second, That those which diminish it, do so very little. The first result is unfavourable to the opinion of those, who attribute the death of animals to the diminished elasticity of the air. We know, that we cannot ascribe it to this: for if, in many cases, animals die, without the air losing its elasticity, we must conclude that their death has another cause. The little diminution which the elasticity of the air suffers, makes me doubt much, whether, in the other cases, it occasions the animals' death. By Signor Verati's experiments, it appears, that the barometer fell little more than an inch at the greatest alteration. According to my experiments, the water in the receiver sometimes rose a few lines, sometimes near an inch, and at the greatest degree an inch and seven lines, that is, one

fourteenth of an inch, and seven fourteenths of a line of mercury. But we know, that, in the changes of weather, there is a greater difference in the weight of the air. The mercury in the barometer sometimes falls more than an inch: it falls quickly, especially in storms, without affecting animals: otherwise, both the cold and warm-blooded animals would not live in safety on mountains, where the mercury falls lower than in the barometers placed in close vessels. Animals can not only live in air which has lost its elasticity, to such a degree that the barometer falls some inches, as upon the summits of lofty mountains; they can live in air which cannot be renewed, where the barometer falls to less than half its natural height. Such is the ingenious experiment of Signor Cigna. This acute observer confined two sparrows in the receiver of an air-pump. One was left at liberty; the other was put into a glass vessel, around the neck of which there was firmly tied a very large Florence flask. Then he began to exhaust the air, until the mercury within ascended in the barometer twenty-seven inches and a half, and fell without, by the index, to nineteen inches. After this, he let as much air into the receiver, as depressed it two inches within. In a little afterwards, he drew the same quantity from the receiver, which he soon returned; and continued

ed this alternate exhaustion and return for half an hour; and the two sparrows were always kept in air rarefied so much as to support eight inches and a half of mercury, or at most ten inches and a half. Only the sparrow at liberty had the benefit of respiring renewed air; while that which was confined always respired the same. This last expired, soon after being taken from its vessel; whereas, the other came from the receiver in perfect health. Boyle tells us, that animals perish in condensed air rendered denser than the atmosphere. I often repeated this experiment, condensing the air sometimes twice, and sometimes thrice, and even more, than the natural air. I have observed, with Boyle, that the air become most elastic, kills the animals slower, but that they absolutely perish.

It is then demonstrated, by experiment, that the diminished elasticity of the air, is not, and cannot be, the efficient cause of the death of animals confined in vessels hermetically sealed. It remains to see, whether their respiration contributes to their death; which is the opinion to be discussed, and will be the subject of the following chapter.

CHAP. III.

THERE are three things to be enquired into, in an examination of the first question, Whether the death of animals in confined air, is occasioned by their respiration? 1. If we actually find exhalations in close vessels where animals have died. 2. If the exhalations are the occasion of their death. 3. Supposing they are, how do they effect it.

I return to the first article. Signor Cigna, who has elucidated this theory, has proved the reality of the influence of the exhalations of respiration, by the foetid odour we are sensible of upon opening the vessels where animals have died, and by a sort of pellicle of vapour covering the internal surface of the vessels. I shall here relate what I have observed in my experiments. I have almost always seen this pellicle over the internal surface of the vessels where warm-blooded animals had died; such as, birds, rats, bats, &c. But I have never, or almost never, remarked it with cold-blooded animals. I have indeed felt something, upon opening the vessels; and the odour was certainly foetid and cadaverous. I have felt it in all my experiments, which have been very numerous,

ous, and even in those made with the smallest animals ; so that the existence of this vapour is not to be doubted, although it does not always appear within the vessel, whether because it is not in sufficient quantity, or whether because it is of a dry nature. Both cases may prevent it being seen under the form of an aqueous veil.

It appears, at the same time, incontestible, that those exhalations are the real cause of the death of the animals. Signor Cigna endeavours to prove it by the resemblance we remark, between the phænomena seen in liquids which evaporate in confined air, and those exhibited by animals respiring in close vessels. The evaporation in close vessels, as he has observed, is proportioned in duration to those two conditions ; namely, the size of the vessel, and the rareness of the air where it operates. Animals are also subject to those two conditions. They live so much longer as the quantity of confined air is greater, and die sooner when the air becomes more rare.

Collecting some of the results which I have hitherto established, and connecting them with those which I shall establish hereafter, it seems to me easy to prove this fact. I have observed that the death of animals is accelerated by two circumstances ; by the increased heat of the atmosphere, and the number of animals confined.

ed. In those cases, the exhalations are more copious : reason persuades us of this, and experiment confirms it, by the more foetid odour exhaled upon opening the vessels. As we cannot ascribe the death of animals to the diminished elasticity of the air, and as we see no other causes, why may not we recur to those exhalations which have become more dense and active? And further, as we shall soon see, animals die sooner in close vessels, where there are others already dead, because there are in these more exhalations. We comprehend by this means, how animals confined alone, or in small numbers, live much longer than when there are many ; and also, why they live much longer in cold weather ; because in the two hypotheses, this lessens the afflux of the vapours. For this reason, the life of animals will be abridged, in proportion to the smallness of the vessels. The vapours become denser, from the little space they have to spread.

After all those facts, we cannot say, that the cause of the death of animals in close vessels must be ascribed to the diminished elasticity of the air, instead of the exhalations of respiration ; and if the diminished elasticity of the air does not contribute to it, some great change which the air undergoes, produces this effect : particularly, as the confined air, by the inspirations and expirations, will cease to be the same,
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which must always happen when a free communication with the external air is interrupted; for then it should lose the *pabulum*, which is a substance or quality known only by name, but upon which we make the life of animals depend; so that the air will be decomposed or corrupted, and assuredly will become unfit for respiration. But the slender support which the opinion, that animals die upon account of the diminished elasticity of the air, can receive, is demonstrated by the animals themselves dying in situations where the communication of the external with the internal air is uninterrupted. When seven frogs had expired in a receiver, the higher part of which was open, and thus the external and internal air preserving an equilibrium: an hour after this, I confined other two frogs, the upper part of the receiver remaining open; but these two frogs died in an hour and a half. I shall further observe, the frogs in the receiver perished sooner, according as the number of dead frogs was greater; although the diameter of the opening above was at least two lines.

Several birds, reptiles, and small quadrupeds, perished when put into a receiver where dead animals were, although the receiver was open above. The constant communication of the external with the internal air, destroys the opinion which ascribes the death of animals to
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the disorder or corruption of the air. But it remains certain, that death is occasioned by the exhalations of respiration ; for I have observed that animals die later when the receiver is open above, as some part of the exhalations escape.

I may remark, in passing, that the death of animals, in vessels where the upper part is open, is an argument which may decisively prove, that they do not die from the diminished elasticity of the air.

Enlarging the opening of the receiver, so that the exhalations might more abundantly escape, I made an experiment, to ascertain completely, whether they occasioned the death of animals. I thought of passing the exhalations from the receiver into a vessel applied to the opening, which was easily accomplished. I confined two swallows in the vessel, after perfectly securing the opening with a wooden stopper. Swallows were preferred, as eight had died in the receiver. The influence of the exhalations was such, that eight swallows died in a quarter of an hour, although two confined in a similar vessel lived two hours.

I varied the experiment, by collecting in the vessel various quantities of the vapours. The life of the animals has always been proportioned in duration, to the quantity of vapour. Long after the vapours have been confined

finned in vessels, they retain the power of destroying animals. It makes little difference in their influence, whatever animals have afforded them: they are equally fatal from all. The exhalations from birds kill quadrupedes, and those from quadrupedes are fatal to birds. It is the same with those of fishes, with respect to reptiles, insects, and the rest. To finish the proof I had engaged in, I made the following experiment. Several animals having expired in a receiver, during very warm weather, I opened the hole at the upper part of the receiver, and presented a bird to the very foetid vapour which escaped; so that in its inspiration it was forced to inhale the mephitic air. Every bird treated in this manner died.

Although I believe it impossible that any animal can live in confined air, when the vessels are very small, yet it is certain that some may live longer there than others. In general, cold-blooded animals can support this situation better than warm. In the same air where a newt or a frog will live a day, a sparrow, a bat, or a rat, will not live an hour. Among cold-blooded animals, there is even a sort of gradation. A newt lives longer than a frog, and a frog shorter than a leech. The same is the case with a number of insects. In the opinion of some, it is not difficult to ascribe a reason for the variety in such phænomena. Cold-blooded

blooded animals are not only more tenacious of life, but they have also less necessity to respire, than warm-blooded animals have.

A frog, a viper, a toad, will live longer in vacuo than a bird: those animals should also absorb a smaller quantity of exhalations, from not being obliged to respire so often as the warm-blooded animals. It will not then be surprising, that they live the longer of the two. It is doubtless a reason analogous, which occasions the difference of time that we see a cold and a warm-blooded animal can live in close vessels. Experiment proves that frogs die sooner than newts, as I have myself found, by keeping both immersed in water. We may reason in the same way respecting insects, without recurring to their stronger or weaker constitution, which may likewise produce similar discrepancies.

It remains for me to explain, how the exhalations of respiration are noxious to animals: and here, in particular, Signor Cigna has displayed his wonted abilities. The exhalations of respiration kill animals, according to this observer, by irritating the bronchiæ and the lungs, forcing them to contract and become rigid, thus preventing the entry of new air. And, according to this author's opinion, animals in infected air, should die from suffocation. He endeavours to shew, that this is the actual
cause

cause of their death, from the different symptoms observed in the respiration of confined animals. When the exhalations begin to collect in the confined air, the respiration becomes more frequent and fainter, because the air is scarcely inspired, before the animal is forced immediately to expire it, from the quantity of exhalations it contains. These exhalations continuing to encrease, the respiration also continues frequent, but becomes more laboured. The animal is soon exhausted by this laboured and frequent respiration, when confined in air where other animals have died. All this proves, in the opinion of Signor Cigna, that the exhalations injure the animal, because they irritate and make rigid the organs of respiration, and obstruct the entry of the air.

In my numerous experiments, I have seen the same symptoms of altered respiration. They manifest themselves in the warm-blooded animals, and particularly in birds. They are not so evident in cold-blooded animals; but, at the same time, they are seen, if we pass the animals from the open to confined air, which has previously been vitiated by exhalations. Signor Cigna had before experienced this in several animals. He confined a frog in a receiver, where there were five or six already dead: the frog instantly became agitated, and leapt against the sides of the receiver. The
respiration

respiration was at the first frequent and labour-ed ; apparently because it was more painful ; and it, in a short time, ended with death. I am, therefore, with the Turin Professor, fully convinced, that the respiration is affected by this air ; but I cannot agree, that there happens in animals that contraction of the organs of respiration which kills them by suffocation. When several frogs are confined in a capacious vessel, they live a long time. If the weather is not warm, they retain their vivacity for more than a day ; but after this, they become sluggish, and swell excessively. If they are males, the two vesicles upon the sides of the head encrease ; and their inflation is such, that they prevent the frogs from sinking in the water ; but keep them upon the surface. After remaining some time in this state, the frogs expire. When taken from the vessel and opened, we discover that the swelling proceeds from the inflation of the lungs, which cannot possibly be more distended. The same inflation is found in the lungs of toads and newts dying in the same manner : they also are necessitated to swim. The lungs are so far from being contracted and become rigid, that they are much dilated, and considerably charged with air. For this reason then, and for others of which I shall afterwards speak, I cannot admit that animals die from the impeded respiration. In
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very small vessels, where several animals were already dead, I confined some vipers and frogs; and at the same time I put an equal number of the like animals in vacuo. It is incredible, how much sooner the former died than the latter. Some of them did not survive a minute; but those in vacuo were alive at the end of several hours. Therefore, it was not suffocation, or an obstacle to the air entering into the lungs, that occasioned the death of the former animals, otherwise they would have lived much longer, or at least as long as those in vacuo.

At the same time, I forced some frogs to remain under water, preventing them from rising to respire at the surface. In others, I have tied the origin of the lungs in such a manner, that the air could not enter. Others, I have deprived of the lungs, and confined them with other frogs in a small quantity of air become very foetid by the exhalations of respiration. The last expired in a few minutes, sometimes in one, even in less; while those deprived of respiration, by immersion in water, having the lungs tied, or cut away, have lived in vacuo for several hours. I have found the same with toads and water-serpents. Since the death of all those species, occasioned by deprivation of respiration, has been, without comparison, later than the death of the same animals killed by the exhalations of their respiration, we must

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conclude,

conclude, that those exhalations do not occasion death by the suspension of respiration ; but that they are, with respect to those animals, one of the poisons most fit to destroy life ; and that it acts as suddenly as any other poison, and even kills almost instantaneously when collected in a great quantity. This poison penetrating the body by means of respiration, when animals inspire the air, will cause that laboured breathing they experience ; for it is more than probable, that it will make, upon the organs of respiration, a violent and painful impression. At the same time, the vessels of respiration are not the only vehicle to convey the poisonous vapours to the animal. Earthworms, leeches, and some other insects, which are not only without real lungs, but also without stigmata or trachea, die in like manner with the rest in confined air. We must say then, that the exhalations act upon them, either by insinuating themselves through the pores of the skin, by the channel of the aliments, or by both. The deleterious quality of the exhalations is so formidable to every species of animal, that it extends to those which never feel the lively impressions of the air, from their constant abode at the bottom of the water. This induced me to observe some of the snails and snakes of stagnant waters, put in an open vessel with water. They crawled over
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the bottom, without betraying any sign of uneasiness; but when confined in a very small vessel, they were incessantly agitated; they ascended the sides of the vessel, contrary to their usual custom, leaving the water, and soon expired. The influence of the exhalations thus acts beneath the waters, which we cannot doubt, from the foetid odour communicated to the water within the receivers, as well as to the external water, whenever the odour is to a certain degree.

But since those pestilential exhalations do not kill animals by depriving them of respiration, how do they occasion the death of animated beings? It is not by coagulating the fluids, by dissolving them, or by destroying them. Immediately after the death of animals killed in this manner the blood preserves its original fluidity; the serum continues to flow, and the minute globules retain their size and figure. Besides, did the exhalations coagulate the blood, or contribute to make it more fluid, this could not be the cause of the sudden death of animals killed by them: for they could effect it only by interrupting the whole circulation, or depriving the blood itself of fluidity^a.

I have suspected that this poison kills by destroying the irritability of the muscles. The irritability may be lost in two ways; the mus-

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^a De phenomeni della circolazione, tom. 1. c. 6.

cular fibre may either become too relaxed or too rigid. The muscles do not seem relaxed, but rather too rigid, when the animals are taken from the confined place where they have died. Such were the phænomena I observed in frogs: their hind legs and thighs were extended in a straight line, as if they had been dried. If their position was changed, or if they were bent, a resistance was found, and when at liberty they resumed their original position: the muscular substance seemed harder under the finger, and under the knife: but I soon discovered those changes in the muscular fibre did not precede or attend upon the death of the frogs, but that they followed it. If I took from the vessel frogs dying, or scarcely dead, I did not perceive their limbs extended, and their muscles preserved sufficient flexibility: on the contrary, the extension and rigidity only took place when they had continued some time in the vessel. I have observed the muscular hardness and the extension of the limbs in dead frogs which had remained some time immersed in water. Finally, this change in the muscular system, by no means happens to all animals.

The following facts have made me entirely renounce the idea which caused me suspect the loss of irritability. The muscles of a frog's thigh display great irritability when cut or pricked:

pricked: they vibrate and suddenly contract, not only where the point of the needle or the edge of the instrument touches, but far beyond it; and the vibration continues during some time, although the cause which occasions it is suspended. Having taken from a close vessel a frog upon the point of death, I will not say that the muscles retained their irritability as in a state of health, or that the approach of death did not weaken it; but I will say, that the vibrations and contractions of the thighs, when pricked and cut, re-appeared, and even continued after the entire death of the animal.

I consequently abandoned the idea of the loss of muscular irritability: and, after matured reflection, it seems to me, that the nervous system is the part upon which the exhalations act. And here are the reasons which have suggested that opinion. I submit them to the judgement of philosophic readers. Convulsions commonly precede, and attend the death of our animals. They are clearly seen in frogs: the whole body is sometimes convulsed, but particularly, and more violently, the limbs. In those convulsions they die. As in Winter they longer resist death, the convulsions also continue longer. If taken from the vessel before death, it appears that the seat of feeling has suffered. They are sluggish, although vio-

lently agitated : they do not change their place, when stimulated ; the convulsions re-appear ; their lethargy augments, even when placed in the open air ; and they generally die. Those convulsions are doubtless the effect of affected nerves. The reason seems natural ; but here is a convincing proof. I wounded the muscles of a frog, which had not been exposed to confined air : the wounding excited in them the motions which stimulating ordinarily awakens ; but they were never convulsive motions. On the contrary, if I touched the origin of the crural nerves with the same instrument, which was extremely fine, the limbs suddenly became convulsed ; and then, in the same manner as when in confined air, when I pricked the spinal marrow, or the brain, the convulsions became universal over the body. I have never been able to see the convulsions partial or general in frogs, but by wounding the nerves. It is on this account, that the convulsive spasms in animals confined in close vessels, make me suspect that the poisonous vapour acts upon the nervous system.

I had still one doubt to solve. I could not reconcile the almost immediate death of frogs exposed to the strong exhalations of respiration, with the preservation of their life during a long time, although deprived of the brain. I have shewn, in my work upon Circulation, that

that frogs will live several days, although the brain is taken away. But the doubt disappeared, when I saw their death become sudden, when, instead of wounding the brain, I wounded the spinal marrow. If a pin was introduced where the spinal marrow unites with the brain, the frog in a moment died convulsed. Animals as tenacious of life as frogs, die with the same suddenness, when the spinal marrow is injured. It is not therefore very extraordinary, that death so immediate is occasioned by the pestilential exhalations insinuating themselves in quantities into animated bodies, and, not affecting the other parts, attack the whole nervous system, and momentarily deprive it of the sensitive faculty.

But, what can we say of the death of those animals in close vessels, if very small, in which there is found no nerve, as in the eels of vinegar, and the multitude of infusion animalcula? I agree that those most minute beings are killed by the exhalations. The analogy of so many other animals which perish from the same cause, renders this conjecture very plausible. The exhalations can only destroy them by contact. We must consequently say, that they act upon the organs of those animals, and there produce an effect similar to that they operate upon the nervous substance of other animals; although their organic structure may be with-

out nerves, so much at least that we cannot judge of it by the microscope. But, for this reason, they cannot evade the fatal influence of the exhalations ; nor do I see how they can escape it, when they cannot resist the effects of the electric vapour, and particularly of certain odours.

Besides, I beseech the reader to view this opinion as a conjecture. I have not made that collection of facts necessary to give it authenticity ; but I have not had leisure to enter into all the details, and to make the most profound researches. I wish that others would undertake it ; and I shall always have the same regard for those who attain their purpose, whether they confirm or confute my conjecture ; for I have no other view but the search of truth.

In the first chapter, I have spoken of eggs and seeds which did not germinate, when confined with a small quantity of air. It is possible, that this sterility may be occasioned by the same causes that destroy animals confined in small vessels. A comparison will help to convince us. Butterflies, as we have seen, chap. 1. do not come from chrysalids confined in small vessels. I find that M. De Reaumur has had the same results, although the object of his experiments was very different from mine. In glass tubes four or five inches long, he hermetically

tically sealed chrysalids. Part of these were from the cabbage caterpillar, and part of that kind transforming to phalænæ. They always remained in their original state, although they had been confined for several months. They never developed; for, as he observed, they perspired; and perspiration is necessary for chrysalids to change to butterflies. These two facts he proved in a decisive manner. When the chrysalids are in a very confined place, as in a tube of several inches, the moisture which transpires cannot dissipate: on the contrary, it falls back upon the chrysalids. Thus, in some days, we see them moist; and this humidity, insinuating itself into their bodies, renders them diseased. Therefore, the death of the chrysalids happens nearly from the same cause, as that of animals in stagnant air. What I say of chrysalids, may be applied to the eggs of insects, and the seeds of plants. We know, that eggs hatch only at a certain degree of heat, which occasions perspiration: if confined in a small vessel, they re-absorb the exhalations which they have before transpired, and then they corrupt. A proof of this, is the humidity which after a certain time covers the eggs, and sometimes, in considerable abundance, the sides of the vessel.

The same happens to vessels confining vegetable seeds. As I have been often wont to put
seeds

feeds in a little water, that they might germinate; upon taking them out, I saw the part which had been exposed to the air covered by a humid pellicle.

From opposite reasons, we see why eggs and feeds are excluded in large close vessels. They are always in safety; for the vacuity being great, the exhalations may disperse. In the same way, butterflies come from chrysalids, when the capacity of the vessel is considerable.

OBSERVA-

OBSERVATIONS AND EXPERIMENTS

UPON

SOME SINGULAR ANIMALS WHICH MAY BE
KILLED AND REVIVED.

CHAP. I.

IN the history of the animalcula of infusions, which I have treated so much at large, it has been said, that when once those animals perished from a defect of water or humidity, they could never again be brought to life, although water was supplied, and their immersion continued long. Of this I have had the most convincing and repeated proofs, in the experiments which I proceed to narrate. But there are other animalcula, which, notwithstanding they are the inhabitants of infusions, are so much distinguished and privileged by nature, as to enjoy the advantage of resurrection after death. Such, among others, are the Wheel-animal, the Sloth, the Anguillæ of Tiles, and those of Blighted Corn.

A microscopic animalcule inhabiting the sand of tiles and sewers, is by naturalists termed the Wheel-animal. The abdomen is large, and situated towards the middle of the body : in the opinion of some, there is an heart. The posterior part of the animal is provided with a minute trident ; and the anterior part divides into two trunks, bearing, at the vertices, the semblance of two most singular wheels. From these it has obtained the name of the Wheel-animal. One, magnified, is represented Pl. 3. fig. 1. If the sand I have mentioned is put into water, the animalcule exhibits all its organs to the observer, provided the sand remains a certain time infused. If the water fails, the action of the wheels, and that of the heart, ceases ; the animal gradually loses motion and life ; it contracts, becomes very minute, and assumes the resemblance of a dry and emaciated skin. To revive it, it is sufficient to immerse it in water : then the body soon extends, the wheels and the trident appear, the heart is re-animated ; motion is regenerated in the whole animal ; it begins to swim in the water, and exercises all the functions of life. It is of no importance that the animal has remained long dried in the sand. Lewenhoeck, the first who had the good fortune to discover it, and from whose works I draw the chief part of what I relate, has seen wheel-animals re-animated, after being kept in
dry

dry sand almost two continued years. With this naturalist we must observe, that when the animal revives, the trunks and the wheels are not always completely displayed, but are sometimes exhibited as in fig. 2. A.

Such are the three figures of the wheel-animal which Baker has, after Lewenhoeck, given, in his treatise intitled, *The Microscope made easy*. He contents himself with repeating only what the Dutch naturalist had written before.

Although several naturalists have treated of the wheel-animal, they seem to me to have done it but superficially, and all to have proceeded upon the relations of Lewenhoeck. I thought it would not misapply my pains to investigate this interesting subject, and to illustrate it by additional facts: and I was particularly induced to this, by the relation between that animalcule and the chief objects of my work. I have therefore composed a short and methodical history of the animal, from the materials with which I have been furnished by experiment and observation. But, what renders it so very remarkable is, the singular faculty of resurrection, possessed by the wheel-animal. When upon the point of publishing the fruit of my labours, another work of Baker, written in English, fell into my hands^a: there, much is said of the wheel-animal. I perused it rapidly, and

^a Employment for the Microscope. London, 1764.

and seeing at once, that the author proposed to treat the matter *ex professo*, I designed to suppress in this work all that concerns the wheel-animal; because it would have been useless to treat of a subject already discussed by a learned observer. I should certainly have done so, had I not perceived that Baker's observations were materially different from mine; because his wheel-animals had been of a different species. I therefore resolved to publish my treatise, which was improved by that of Baker, its imperfections lessened, and new important matters added.

I proceed to recite my experiments and observations, beginning with a fact which may be patent to every one. I examined the sand from a sewer, about three hours after it had been put into water. I had no trouble in finding what I sought for. In the first drop presented to the microscope, appearing like muddy water and sand, I saw three living beings, which I immediately recognised as three of Lewenhoeck's wheel-animals. Upon the anterior part of the body was a horn: the size of the body increased towards the middle, and the posterior part was terminated by three points: but the anterior part had neither trunks nor wheels, and the animals were nearly as in fig. 2. A.

The body is transversely annulated, and longitudinally radiated with some parallel prominent

nent rays ; fig. 3. The indistinctness of the rings and lines renders them difficult to find. To be able to see them, one must be accustomed to observation, and have an excellent eye. In the middle, there is obscurely seen a little longitudinal fascia, covered with specks ; and above it, more visible, a circle formed, as if by two C's touching at the extremities. At the upper part of the circle is seen the origin of a little canal, A B, fig. 3.

The animal being very flexible, it assumes some extraordinary shapes in its progression. Sometimes it extends, and becomes very slender ; at other times it contracts, and becomes very corpulent. Sometimes the anterior part is contracted and concealed in the body, or the same happens to the posterior part. One part of the body will be inflated, while the rest is flaccid. In a word, its motions are as singular and easy to see, as they are difficult to describe with precision : and all those remarkable figures are exhibited, although the animal remains stationary.

The mode in which the wheeler transports itself from one place to another, is this. It fixes the tail to the plane which it means to traverse : the whole anterior part of the body is extended. When in this state, the animal detaches the tail ; and, contracting the posterior part of the anterior, it advances. The extremity of
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the tail is again fixed to the plane ; the body is extended as before ; the extremity is detached ; and, by the contraction of the anterior part, a new step is made. This operation is repeated, and the animalcule passes along with such agility, as soon to traverse the field of the microscope.

This method of progression, by means of the contractions and extensions of the body, is common to several insects ; but, in particular, to apodal vermes, which is generally known. A circumstance peculiar to the wheel-animal is, that it fixes itself by the point of the tail ; which is so essential to its regular progress, especially over a polished surface, that without this precaution, it would constantly go wrong. When the animal has found some point of support, and is fixed by the tail, it frequently stops for some time, and extends the anterior part, as if from a centre examining around, what direction it should take : then, suddenly detaching itself, it advances in a given line.

In Lewenhoeck's opinion, the wheel-animal fixed itself by all the three points at the extremity of the tail. At first, I likewise thought them necessary ; but, upon examining them with more attention, I perceived that the middle point only was used for that purpose. To see it with precision, the drop where the wheel-animal is, must be thin and transparent, and
free

free from sand: then it is easily perceived, that, so far from the lateral points fixing to the plane of position, they do not even touch it, but are distant a considerable degree; and that the point in the middle is the only fixing one. When viewed with a powerful magnifier, this point seems to be composed of a number of infinitely fine similar points, which are almost imperceptible; Fig. 3. D. It is by means of these points that the wheel-animal advances.

The three wheel-animals, which I then observed for the first time, were not swimming; they crawled along at the bottom of the drop. I soon perceived, that this was their custom when the wheels were not in action. Any one may satisfy himself of it, by putting a quantity of sand, mixed with wheelers, into a watch-glass half full of water. He will immediately see, that those at the surface of the sand constantly crawl upon it, and do not commit themselves to the water. The same is the case with those buried in the sand, when brought to the surface.

The three wheel-animals moved with agility in the drop which I first observed; the anterior part searching among the sand as if for food; but they never left the fluid: they approached the edges of the drop, and instantly returned. As the drop began to evaporate, their motion became languid; and the languor

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encreased so much, as to deprive them of power to change their place. Although they remained upon the same spot, they turned around and stretched themselves. Such motions were particularly conspicuous in the head and tail, which proceeded from, and re-entered the body; and they were hid when the whole drop had evaporated. The appearance of the wheel-animals then changed, not only because life was gone, and they were completely motionless, but also because their size was greatly diminished. They became three minute corpuscula, so distorted, that it was impossible to recognise them for what they had originally been.

They were an hour in this state of apparent death. I then put upon them a drop of the same water that had evaporated. The reader may well conceive my attention in observing this resurrection, the success of which happened as I had foreseen. In a few minutes, the animals began to swell, a point appearing at one end, Fig. 4. D. This pointed part then began to move, by extending and contracting; the part opposite became pointed also, and began to move as the other. I perceived that those points were the head and tail of the animalcule, proceeding from the place where they had been concealed upon the evaporation of the drop. The transverse annuli, the longitudinal rays, the internal and external organs,
all

all re-appeared ; and the three wheel-animals resumed their original figure and size, which they in a very short time effected. They traversed the sand vigorously, and shewed themselves to be as lively as ever.

Discovering some wheel-animals in the sand of sewers, I repeated those experiments, and found that they always revived, during whatever time they had been kept dry. There is now before me a remarkable instance. I have still in my possession some sand, upon which I made experiments near four years ago ; which has been preserved dry in a small glass vessel. When moistened, the wheel-animals in it instantly revive. This fact agrees with what I have related from Lewenhoeck. Baker has observed a circumstance little less worthy of notice. With water he wet the inside of a glass wherein wheel-animals had been kept dry for some months, and he saw them recover their original vivacity. Here it matters little to know whether they revive oftener than once. Eleven times I have dried the same sand, and as often wet it ; and I have constantly seen the death of the wheel-animals attend the drying of the water, but their life recommence when the sand was moistened.

However, those facts should be understood under some limitations. Although the animals

do revive several times, and even after they remain a long time dry, it is certain that the number revived always decreases in proportion to the times the sand continues longer dry, and the times it has been wet, to revive the wheel-animals. It is true, I have seen their eleventh resurrection. The first times, they were very numerous; but the number afterwards continually decreased, and at last became very small. I should add, that, still wetting and drying the sand, none revived the sixteenth time. It is the same with respect to the sand remaining dry. I took a portion numerously inhabited by the animals, and I have preserved it dry in a box for three years, only moistening it every five or six months for my observations; but the resurrections always become fewer; and now, at the end of the third year, I do not exaggerate when I say, that one hundredth part does not revive. I have not extended the experiment further: but it is undoubted, that if the wheel-animals remain long dry, and if they continue to become rare in proportion, a period will finally arrive, when they will revive no more.

The time necessary to accomplish their resurrection, is unlimited. I have found, that in four minutes after moistening the sand, some begin to live; life then extends to more; and

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in an hour, all are reanimated ^a. I am ignorant what can occasion this difference in the time necessary for their resurrection. It may happen, either because some parts of the sand where some of the animals are, may be better moistened than others; or, although all should be moistened in the same time, it may happen, that all the wheel-animals are not of the same texture. In that case, the more dense or consistent will more slowly receive the impression of the water, and be longer in reviving; or, some may be diseased, and less fit for immediate resurrection.

I have not perceived any very sensible difference of time between the resurrection of those which have been dry for some hours, and the resurrection of those which have been dry for several days, several months, or even for complete years.

As I knew the influence of heat in restoring the life of animals and vegetables, I frequently moistened the sand with warm water. The wheel-animals then revived sooner than when the sand was moistened with water at the heat of the atmosphere.

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But

^a Baker's wheel-animals begun to exhibit signs of life only in half an hour. It would appear, that he speaks of those that were longest of reviving. Perhaps his experiments were made upon a species different from mine.

But there is one condition essential to effect the resurrection of the animals. It is absolutely necessary, that there should be a certain quantity of sand. Let us enquire further into this. One day, I had two wheel-animals traversing a drop of water about to evaporate; it contained very little sand. Three quarters of an hour after the evaporation of the drop, they were dry and motionless. I moistened them with water, that they might revive; but this was in vain, although they continued wet during several hours. Their members swelled, and they became three times their original size: but they still remained motionless. This circumstance seemed to me the more extraordinary, as it was among the first times I had wet the sand, and those two were the only animals that did not revive. To be certain whether the fact was merely accidental, I took a portion of the sand, and spread it out upon a piece of glass: I waited until the numerous reanimated wheel-animals should become dry, in order to wet the sand anew, and learn whether they would revive or not. The sand was carelessly scattered upon the glass, so as in some parts to be a thin covering, and in others to be in a very small quantity. The animals in these parts did not revive; but all that were in those parts where there was abundance of sand, revived. A difference so remarkable,
made

made me suspect that this species of beings inhabiting sand, require a certain portion of it to enable them to pass from death to life.

However, to acknowledge the truth, I did not at first adopt this conjecture; but I could not divest my mind of it, as it seemed to be confirmed by facts: besides, upon recollecting the experiments I had before made upon the resurrection of wheel-animals, I was certain, that those reviving always had been among sand. To certify or destroy the doubt, it was sufficient to repeat the last experiment; for, if those which revived had been mixed with sand at the moment of their resurrection, or if, upon the contrary, those then without sand did not revive, there would be a complete demonstration of what had been the cause; namely, that the presence of sand was essential to their resurrection. Upon repetition of the experiment, the consequence constantly was, that the animalcula never recovered life, unless in places where there was a quantity of sand.

One of my friends, an eminent philosopher, and an excellent microscopical observer, constantly has the same result from his experiments ^a.

The Abbé Roffredi, a good observer, when incidentally speaking of the wheel-animal, in

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^a Il. R. Padre D. Carlo Giuseppe Campi di Milan,

the Abbé Rosier's *Journal de Physique*, mentions the same phænomenon ^b.

To these we may add the following facts. If we spread out the sand with wheel-animals, in such a manner, that there is a considerable quantity in some places, and much less in others, and very little in the rest; and then moisten the whole: in the first case, where it is plentiful, the reviving animals will be numerous; in the second, where it is more rare, fewer will revive; and in the last, very few, sometimes none. If the most abundant quantity of the sand is dried and scattered thinly over any place, then there will appear only some animals revived, where they were before very numerous. Besides, the resurrection of the wheel-animals in this small quantity of sand is effected later than where the sand abounds: in this case, four minutes is enough: where there is little sand, their resurrection will require nine, or eleven minutes, and sometimes more.

But, have the animals which do not revive from a defect of sand, and resemble globules floating upon the water: have they lost the faculty of resurrection, or do they resume it upon being supplied with their native sand? To ascertain this circumstance, I have often taken the thin surfaces of sand, wherein the wheel-animals

^b *Journal de Physique*, 1776.

animals did not revive, and put them at the bottom of a watch-glass, with water upon them; but of twenty dead animals I saw scarcely one revive. It therefore appeared to me, that privation of sand deprived the wheel-animals of their innate faculty of resurrection.

How can the simple defect of sand produce so important an effect? What connexion, what physical relation is there between the presence of sand, and the resurrection of the wheel-animals? May not the cause which effects this phenomenon be entirely different; and cannot we say, that the sand may serve the place of some very simple external condition? I see that when the animals perish from want of sand, their bodies are exposed to the influence of the air upon the evaporation of the water; but they do not experience this, or at least in a far less degree, when they die covered with sand. We may say, this supposes that the immediate action of the air, by irritating and injuring the corpuscula, from its lacerating influence, while they are still humid, and while they are at the same time most tender and delicate, renders them incapable of reviving, from the alteration they undergo. The conjecture hazarded is founded on a fact, evincing there are animals, whose structure is so delicate and fragile, that they are unable to support the immediate impression of the air,
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and always live under cover. Such are the *miners*, a species of insects so named from their inhabiting the interior of the leaves of trees, where they live almost always concealed and protected from the influence of the air. My conjecture would perhaps require an experiment, which I had not time to make. We shall immediately see, that wheel-animals revive in vacuo. One might put some of those, swimming in water without sand, under an exhausted receiver, and then wet them, observing whether they revived; which they should, according to my supposition; because, in that situation, they could suffer nothing from the motion of the air when the water evaporated.

I arrive at another enquiry, more important than the preceding. I have hitherto supposed, that the wheel-animals *perished* when the liquid dried. It is true, they exhibit every appearance of death; the body is dry and disfigured; they lose the use and motion of their members. But I mean to examine this circumstance more deeply, since it presents the most paradoxical truth hitherto found in the history of any animal, and we cannot hesitate too much in distrusting truths of a similar nature. Let us therefore enquire, if it is not possible that the wheel-animals, dead to appearance, may preserve a spark of life. To elucidate our enquiry, let us recur to the analogies between
large

large animals. Cold, so injurious to insects, renders those it does not destroy, lethargic during winter : they are in such a degree of torpor as to seem dead to the sight and the touch ; their limbs stiff and contracted, their wings depressed, their bodies emaciated. They have no external motion ; no mark of feeling when stimulated, or when cut in pieces. This we see in the hundreds of insects which we casually find upon the coldest days of winter, in the earth, in the clefts of trees, in the holes of walls. In this manner does cold operate upon beings possessing the highest rank in the animal scale. In the midst of winter we have found marmots, which are a species of rat, so lethargic, that the flame of a candle burning their limbs could not awaken them, or recal the sensations of life ^a. Terrestrial and amphibious animals, when kept long in water, exhibit the same appearances. Rhedi having immersed flies in water an hour and a half, found them with all the semblance of death. Reaumur made the same experiment upon bees. It is well known they are vindictive when injured. This naturalist left a whole swarm in water, I know not how long, and found them so completely deprived of sensation, that he handled them at pleasure ; he took them from the water, put them upon a table,

^a Buffon, *histoire naturelle*,

table, and examined whether or not there were several queens : they afterwards recovered. I may say the same of the apparent death of frogs and newts. After some hours immersion in water, their bodies became stiff and fragile, just as happens in death.

May not the apparent death of insects and other animals be similar to that of wheel-animals among sand ? But those animals preserve a real principle of sensation and life, which the concurrence of certain circumstances is required to unfold, and put in a state to animate the whole system. If the temperature of the air is a little encreased, motion and life reappear in animals rendered torpid by cold : if the bees which have been immersed in water are exposed in the sunshine, they soon begin to move, walk, expand their wings, and fly. In the same way do frogs and newts recover their natural vivacity, when they have remained a short time in a dry situation. Why then cannot we say the same, that there is in the wheel-animals some latent spark of life, which the aid of water is required to discover ?

Considering these facts, and allowing them their just value, I see that we cannot deny that there is a resemblance between the state of dry wheel-animals, and the state of the animals I have named, with respect to their appearance, their perfect immobility, and the complete in-
action

action of their members. But I remark a most sensible difference, which must create a great distinction between them. When animals are torpid from cold ; whatever is the power of the agent depriving them of sensation and motion, it only does this, by destroying the harmony between the fluids and the solids ; but it does not derange them so far, as to deprive them of what constitutes their fluidity or solidity. This harmony exists in the most internal parts of the body. I have often opened newts, frogs, toads and lizards, when torpid from cold, and apparently dead ; and I have found that the blood did not circulate in the limbs, while it continued to circulate in the large vessels, although the circulation was languid. If a greater degree of cold has penetrated the solids, if it has coagulated the blood, then, it is certain, the animals perish. This has already been observed in many animals, by several naturalists ; and I have myself found the same in the toads, frogs and newts, of which I speak.

I have found a remnant of motion in the blood and in the heart of half-drowned animals ; and I doubt not that this motion continues in bees and flies. When all those animals remain long in water, the motion, whatever it is, is destroyed, and all hope of recovery is lost. It is therefore very certain, that, in the animals which revive, that quality which
constitutes

constitutes the existence of their solids and fluids is not lost, and the harmony which reigns between them is not totally destroyed. But, how many essential differences are there in wheel-animals? While they traverse the fluid, they resemble gelatinous substances; they are lacerated and destroyed when touched with the point of a needle. When dry, the solids are contracted and distorted; the whole body of the animal is reduced to a hard shapeless atom of matter: when pierced by a needle, it breaks in pieces like a grain of salt. But how does this atom of matter, where the solids preserve no vestige of their former humidity and pliancy, and where the fluids exist no more: how can we believe that this dry and disfigured atom retains a principle of life? Can we think that life exists in a frog, a toad, or a newt, when dry and rigid, as the wheel-animals are among the sand? Can we conclude, and conclude with reason, that in dry and shrivelled wheel-animals, we must admit the same as in the reviving animals I mentioned before? Can we conclude, I say, that their life is entirely gone, not only because the reciprocal action of the fluids and the solids is destroyed, but also because the fluids are entirely evaporated, and because the dryness and rigidity has changed the natural state of the solids? If, when we put a stiff and contracted

frog,

frog, toad, or newt, in water, we saw it gradually become animated; then, as we would say, it was a real and absolute resurrection: so may we call what happens to the reviving wheel-animals, a real and absolute resurrection also.

But it is time to resume the history of those wonderful animalcula. We have already described their figure and their properties; but we have not yet examined their organs separately, which is necessary to make us well acquainted with their structure and motions. I mean to speak of the heart, the two trunks, and the wheels revolving at the vertices. I could not properly treat of them before, because it would have prevented me from pursuing the plan I intended to adopt, which led me to relate some facts, following the order of the time when I learned them. In all my observations, the wheel-animal has shewn me those three organs only. Their appearance depends entirely upon the will of the animal. While animated, it is not unusual that it does not shew them at first, or displays them very slowly. This is what I have observed in my wheel-animals, and what some of my friends have observed along with me. I did not observe all the three organs until after twenty-one days examination. Let the reader represent to himself a snail proceeding from its shell: it extends
itself,

itself, and puts out its head and horns; then retiring within its habitation, contracts itself, and conceals the head and horns in its body: in this way, he will sensibly figure the motions of the trunk and the wheels of our animalcula. The wheel-animals I then examined, and those which I afterwards saw, did not always at the same moment display both the trunks and the wheels, but, like snails, concealed sometimes the one, and sometimes the other; which happened whenever they contracted themselves; and when they remained long extended, the trunks and wheels were kept out a long time also. The wheels cannot receive this appellation but in a very improper sense, and by means of a sort of latitude or accommodation. In the tract upon the Animalcula of Infusions, I have treated at length of the minute, long, and slender fibrils proceeding from the orifice of the mouth of many of those beings: I have said, the fibrils were in constant vibration; that they produced a certain vortex in the infusions, which drew to the mouth of the animalcule the corpuscula serving it for food. The wheels observed in the wheel-animals, are only two circular lines of those fibrillæ constantly in motion: they produce the same effect with the vibrating points or fibres of infusion animalcula: they form in the water two great vortices, which convey to the animal the substances whereon

whereon it feeds. A wheel-animal is exhibited with the trunks extended and the fibrils, whose motion resembles that of two wheels, and forms two vortices; Fig. 5. pl. 3.

When I say that the wheels of my animalcula are such only in appearance, I do not mean to insinuate that this will extend as a general rule to all. This optical illusion, indeed, has been corrected by some naturalists, in particular by Messrs Trembley and Bonnet; but it is also certain, that the opinion of celebrated observers is different. Lewenhoeck, that acute observer of the most minute objects, actually calls them wheels, which revolve like those constructed by mechanics. Baker, who is not inferior to him in accuracy of observation, and who has studied the wheels attentively, to discover whether they are truly such, or only vibrating fibrillæ, is very much inclined to believe them wheels. The observations of these two illustrious observers may very well agree with mine; for their wheel-animals have been different. A singular aperture for a mouth, situated between the wheels, a sort of ring beneath it, a number of serpentine vessels in the head, the peristaltic motion of the intestines, the irregular agitation of a transparent fluid in every part of the body, a particular undulation of that fluid in the intestines and the skin, were observed by the English philosopher in his wheel-

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animals;

animals ; and although he has accurately described them, I have been able to find none similar in mine. There is no doubt, I might have seen all those organs, both because I used Cuff's microscope, as Baker had done, and also some microscopes which were much superior. With this different organization, it is not wonderful that his wheel-animals exhibited another organ which I have not found in mine ; that is, a pair of wheels proceeding from the two trunks, the revolution of which produces the same effect as the vibration of the fibrillæ : a rapid current is formed in the fluid, which carries the food to the mouth of the animal. With Baker we must remark, that this apparent rotation is not always made with the same velocity, and in the same direction. Sometimes it is quicker and sometimes slower, which alternatives are executed momentarily or by degrees. In the same manner, we see the animal at one time turn to the right, and immediately afterwards to the left. It frequently happens, that after it has moved long at one side, it stops and turns diametrically opposite.

Let us leave this digression, and return to the animals. When their fibrillæ appear, they no longer crawl at the bottom of the water, but they swim through the whole fluid with the greatest velocity. Examining them while they swim, I have often endeavoured to discover, whether

whether they swim by the undulations of the body, or by the vibrations of the fibrillæ, which not only regulate the vortices, but, by their action against the water, may raise the animal. This I have not been able satisfactorily to elucidate; but I should think they swim by the vibrations of the fibrillæ, since they sink when these are drawn within the body.

I have already spoken of a little circle situated towards the head of the wheel-animals, which is in appearance like the junction of two C's by the extremities. This part is in constant motion, by alternate contraction and dilatation, while the animal forms the vortex, and while the fibrils are extended. The part has been observed by Lewenhoeck and Baker: they have thought it the animal's heart. The situation, the shape, contracting and dilating motion, concur, according to the English naturalist, in establishing this opinion. But, if this is a heart, it is a voluntary muscle, which beats at the pleasure of the animal; that is, when it protrudes the fibrillæ and forms the vortex; and this voluntary act has, before me, been observed by others. But, are there animals whose hearts beat by intervals, so that the beating may cease when the animals chuse? Besides, the wheel-animals sometimes remain alive in the water for several weeks, without making the vortex, consequently without moving the

heart. Is it possible that any animal can live so long without the beating of the heart, the animating spring of the whole machine? These are two paradoxes, which may be no less true than there are others more wonderful: such as, the resurrection of the wheel-animal itself. Although it may be thought that this part is actually the heart, either from performing similar functions, from its situation in the region of the breast, from its contraction and dilatation, like a heart; I cannot think these are convincing reasons; for it may be an organ destined for a purpose completely different. I ought to say, as I think, that it is more natural to believe this organ serves for the aliments; so that it contracts and dilates to receive the nutriment, and transmit it to the stomach. Such an hypothesis will easily explain why it is in motion only while the vortex is formed: it is because the aliment is then drawn to the mouth and transmitted to the body. If the part remains long motionless, it is because no food is taken, and this commonly happens while the animals, in an unsuitable situation, languish and die; which is sometimes the case with those revived in sand kept in close vessels. I have seen, that although the sand swarmed with wheel-animals during the first days, the number decreased; and the decrease was to such a degree, that in twelve or fifteen days, the whole
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were dead. They appeared motionless and disfigured at the bottom of the vessel, and a great many of them were even reduced to nothing. During this period of disease, the vortex was seldom made; but it is most frequently formed by them when we find them in sewers, and in pits full of rain water.

The idea, that this moving particle is an organ formed for the reception of the aliments, in order to transmit them to the stomach, is not imaginary. It is founded, 1. Upon observing in my wheel-animals a kind of little canal united to this part, Fig. 3. B, Fig. 4. E. The canal rising towards the head, has great resemblance to an œsophagus. 2. This part is surely destined for that purpose in other aquatic animals, bearing a great relation to the wheel-animals, and afford cogent evidence of the fact. Such is an animal often found in the tremella. It is shorter, and a little thicker, than the wheel-animal. The posterior part is provided with two small diverging filaments, with which it fixes itself to any substance. At the anterior part, there are long slender fibrils, which being put in motion by the animal, occasion a vortex in the water, Fig. 6. There is no vortex when the fibrils are at rest: while they move, and during the continuance of the vortex, there is seen, almost in the centre of the animal, a particle, A, similar in figure to

that part in the wheel-animal of which we speak : it alternately contracts and dilates ; but the motion ceases with the cessation of the vortex. This difference only is to be remarked : the particle in the wheel-animal is formed by two semicircular cavities ; whereas that in the tremella animal, resembles a bladder or folliculus. The particle and canal towards the region of the head, are connected as in the wheel-animal. There is a short duct, B, terminating at the mouth of the animal, and at the opposite extremity ; it enters another folliculus, C, which moves with alternate contraction and dilatation, undulating like a wave almost at rest. This folliculus is precisely the receptacle of the aliments. It is always full of a yellowish green matter, which proceeds from the posterior part of the body, by means of the undulating or peristaltic motion. But we not only see the food discharged from, but we also see it enter the body : that is, we observe fragments of the tremella drawn by the vortex to the animal's mouth, infusion animalcula of various sizes, and fragments of other substances. Some of the most minute enter the origin of the œsophagus, traverse the moving particle, and arrive at the passage we see in the receptacle of the aliments.

The same thing is observed in another animal of the tremella, which I have mentioned
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in the tract upon the *Animalcula of Infusions*. The moving particle of this animalcule, for it has one as well as the wheel-animal, contracts and dilates, while the aliment collected by the vortex passes from the œsophagus to the stomach.

In those animals, therefore, we see this particle, which, in place, figure and motion, resembles an heart, although it is not one, but is an organ destined for the use of the aliments. For these reasons, I have ascribed the same use to that of the wheel-animal.

Should I be right the wheel-animal has no heart, we see no other part, no other organ, that can merit this name. If we may judge of this by the senses, I say it has none, more than the two tremella animals I have mentioned, a number of infusion animalcula, the prodigious abundance and variety of polypi visible by the microscope and to the naked eye. I stop at them, to say no more of many other animals. I have never seen the appearance of circulation in wheel-animals, nor in the animalcula of infusions, those of the tremella, nor in polypi. Although Baker has observed in wheel-animals, the irregular agitation of a fluid, he ingenuously avows, that he never has perceived any trace of a real circulation: yet all feed, encrease and multiply, as those animals which have a heart and circulation. Neither are these es-

essential to the life of many animals. For this, it is enough that there is a just equilibrium, a corresponding harmony, between the fluids and the solids. The ideas we have of a heart and of circulation, are particular notions, taken from a definite number of animals, which demonstrate the bounds of our knowledge and intelligence, and which would ill apply, if we meant to fit them to the immensity of models framed by nature.

The wheel-animals inhabiting the roofs of houses, of ovens, or other buildings exposed to the inclemencies of the weather, should be of a constitution calculated to support the most severe influence of cold and heat. I put them to the test. From a sewer exposed to the south, I took wheel-animals sand, which had for twenty-nine days been subjected to the heat of the sun in the middle of Summer. The thermometer in the sunshine stood, during this time, at 129, 131, 133°. The heat did not injure the animals; for, when the sand was wet, I had a great number very vivacious.

I exposed a little of this sand in very thin glass tubes, without a south window, where the reflection of a neighbouring wall excited an extreme heat, and I left them there the whole Summer. During some of the hottest days, the thermometer rose to 142°; but the heat did not injure the wheel-animals: upon wet-
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ting the sand, they appeared with the same liveliness and vigour, and in the same abundance, as in other sand from the same place exposed in a north window, and seldom or never experiencing the solar rays. I have concluded, that the powerful heat of Summer did not deprive the wheel-animals of the faculty of resurrection; but, is it the same when they are revived? Is this degree of heat then equally supportable?

I have also exposed those tubes with sand and water, where there was a number of wheel-animals, in the warm situation above mentioned. The effect was very different: in half an hour, the heat of the sun at 135° killed the animals. Thus, it is not the same with wheel-animals, when dry and deprived of life, as when in life and motion.

I afterwards saw, that resuscitated wheel-animals died when exposed to a more gentle degree of heat, when exposed in the sunshine at 113° .

The heat of the fire has the same effect as that of the solar rays. But, while the revived animals perish at 111 and 113° ; if exposed to 144° when dry, they do not lose the faculty of resurrection. With common fire, I could extend my experiments farther than with the heat of the sun. I raised the heat to above 144° , to see whether dried wheel-animals ceased to revive; for it was to be thought there were limits

limits here ; and I found those limits at 153° . Sand exposed to this degree of heat, presented few wheel-animals, and at 158° none. But there is a circumstance here necessary to be elucidated. I made those experiments, as I may say, dry, keeping the sand for two or three minutes exposed to the degree of heat I fixed upon. The consequences were very different, when I used wet sand, and immersed it two or three minutes in water warmed to that degree ; then, the wheel-animals did not revive beyond 131° .

It is not difficult to explain, why the destruction of re-animated wheel-animals is more easy than when in their state of desiccation. The former are a sort of jelly ; of consequence, very fragile. Their minute filaments are easily broken and destroyed by the penetrating power of the fire, which does not take place with such facility when they are dry ; then they are hard, and partly concentrated in themselves. Besides, in this state, their globular figure presents less surface to the action of the fire ; also the heat acts alone upon dry wheel-animals ; and, when they are alive, it acts along with the water, which powerfully concurs in lacerating them, and destroying their organization : and the heat has rendered that lacerating power more active and penetrating. It is for this reason
that

that dry wheel-animals can less resist the heat of warm water, than of the fire itself.

Having seen the effect of heat upon wheel-animals, it was necessary to see the effect of cold upon them. For this purpose, I took the sand of sewers, of bent and flat tiles, where the animals are found during the most intense cold of winter, when the roofs are covered with snow and ice. The sand moistened with water, by the cold became so firm and connected, that it was as hard as a stone; but this cold had not injured the wheel-animals. After melting the mixture of ice and sand, I saw a great number soon revive, only their resurrection seemed to me less immediate.

The greatest cold in Winter was 16° : I thought therefore of subjecting those wheelers I found upon the roof, to a degree more intense; and, taking some portions of frozen sand from the bottom of a sewer, I put them in a little glass vessel, which, for three hours, I placed in a degree of cold 11° below 0, by means of a mixture I have often mentioned: But the animals revived when the ice melted, which prove, that degree of cold had done them no injury.

After being certain of this, I sought for the results of the experiment inverted; that is, what will happen when we pass wheelers from the degree of heat at which they are animated,

mated, to different degrees of cold, always more intense. One morning I transported some swimming in a watch-glass to a north window, where the thermometer stood at 27° , observing what happened when the water in the glass became so cold that the hand could scarcely be kept in it. The wheel-animals stopped the vortex, and fell to the bottom, crawling languidly along the sand. The water was soon frozen; then they moved with difficulty; and this motion in a short time ended. When the water was more frozen, they contracted within themselves, forming into globules, which I clearly saw from the transparency of the ice. Thus, the wheel-animals passed the whole day and the following night, which was very cold. The next day I removed them to a warm apartment, to see whether those in the ice, under the figure of globules, would recover life and motion when the ice melted. This took place even when I made the globules remain longer in ice: even when I increased the natural, by means of factitious cold, to 11° under 0.

Reasoning from the experiments of cold, as upon those of heat, it would appear, that the revived wheel-animals should not support the same degree of cold as those which are dead. If, upon the other hand, these last facts do shew, that when the cold begins to act
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with power upon revived wheel-animals, they then pass from life to death, as appears by the cessation of their motion, their contraction and disfiguration, so that they become precisely as when the evaporation of the water leaves them dry in the sand.

Besides, supposing that an extreme cold exercises its influence upon living wheel-animals, I know not if it could have the power of depriving them of life. It is certain it could not destroy animalcula, as delicate as some of those of infusions, and the eels of vinegar. But, what is more surprising, the animalcula of infusions, and the eels of vinegar, perish at a less degree of heat than the wheel-animals. They cannot support more than 111° above 0. At this degree of heat, several species of beetles, of chrysalids and caterpillars, perish, as I before observed, although they support the cold of 34° under freezing. By this we see, that several animals support cold better than heat.

Those facts teach us, that there are two principal causes destructive of the resuscitating quality in wheel-animals. These are, the want of sand and of heat. But, are there also others producing this effect? I could not discover this but by conjectures, and by using the different methods which are noxious to the production and life of other animals, especially those that have the greatest analogy with wheel-animals; such as the animalcula of infusions. It has
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been proved, that these are produced in vacuo. This method of injury does not seem sufficient to prevent the resurrection of wheel-animals, though we cannot deny that their resurrection is facilitated by the influence of the air. The mean of the principal results of repeated experiment is, 1. Wheel-animals revive sooner, and in greater numbers, in the open air, than in vacuo. 2. Those which do not revive in vacuo, revive when put in the open air.

However much the air may promote the resurrection of wheel-animals, it is absolutely necessary for the preservation of their lives. When they revive in vacuo, or are put in an exhausted receiver, they die in a few days.

Although wheel-animals revive in vacuo, it is not so successfully as in the open air: it is natural to think they would revive in confined air, although this air prevents the production of other animals, and kills them when confined in very small vessels. But in the vessels where I had the wet sand of those animalcula, although hermetically sealed in small tubes, they have always very soon revived, and in abundance: they have even lived long in very small vessels, where there was very little air.

Wheel-animals suffer from different fluids, what they do not suffer from privation of air, or from confined air. I shall mention the liquids that do, and do not, injure them. Those
which

which are not noxious to them, are either those in which they revive, or in which, after resurrection, they preserve life. Such is, pit, river, ice, snow, and rain water; distilled water, that of ditches, marshes, pools; the foetid water of mud and dunghills. Respecting the fluids noxious to them, they are either those impregnated with pepper, common salt, sal-gem, vitriol; those in which are expressed the juice of the onion, garlic, urine, ink, wine, verjuice, oil of olives, of nuts, brandy, vinegar, &c. Having put in each of those fluids, the sand of wheel-animals, I never saw one revive; likewise, when revived wheel-animals were put into them, they all perished. Some strong and penetrating odours have been equally fatal to them. Such is the odour of camphire. When revived, if they are long subjected to it, all die, and do not revive when dry. The oil of turpentine produces only the first effect. But if this odour becomes more active, as by melting or burning the turpentine, the fumes prevent the reviviscence of the animals. The fumes of burning sulphur and camphire produce both effects: the fumes of leaf tobacco only destroy revived wheel-animals.

Reflecting upon the experiments made by means of heat, liquids and odours, I have sometimes doubted whether these three agents
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had deprived the wheel-animals for ever of the property of resurrection, or if there was any place to hope it might be recovered. This hope did not seem chimerical in a being like the wheel-animal: it would not be wonderful to see it recover the faculty it naturally possesses. I have preserved sand which has been exposed to heat; I have from time to time wet it with pure water, and have often observed it. The same has been done with sand exposed to liquids and odours, keeping it in the air and wetting it with fresh water, that the noxious qualities which injured the wheel-animals might be destroyed. But these methods have never re-animated the numerous dead bodies.

The wheel-animals which suggested the opinions I have laid down, were for the greater part those found in sewers, the ridges of flat and crooked tiles. In this matter, which for brevity I have called sand, though, to speak more properly, it is a mixture of earth, sand, and the fragments of tiles, (I shall continue to call it sand), is the dwelling of wheel-animals: but we must observe, that in some kinds they are much more numerous than in others. If the sand is red, it is almost a certain indication, according to Baker, of the presence of those animals: but they are always inanimate when the sand is dry. By one who is accustomed

customed to observe wheel-animals in the state in which they are found, when by the evaporation of the water they are dry, they are easily recognised when the sand is presented upon a slider to the microscope: then they are seen in the shape of minute dry globules, of a reddish yellow colour; which, when moistened, extend themselves to form the animals.

The animals are also found in certain waters of the earth. Both Baker and I have often seen them in ditches; and I have taken many from pools, marshes, and even holes of water.

The wheel-animals of the earth are, in my opinion, the origin of those of roofs; and it seems to me that it cannot be otherwise, at least, that we cannot say the wheel-animals of one roof come from another, which supposes a particular case: but to take the case generally, and consider it as in its origin, I should derive them elsewhere, and consequently recur to the waters.

The manner in which they pass from the earth to roofs, may be easily conceived: when in their dry state the wind may effect this, or when the air has raised the whole or part of the water where they are.

While studying those curious beings, I have always reflected upon a most important problem; which was, to enquire into the mode

of their propagation. For this, I have isolated them in watch-glasses like infusion animalcula : I have put one in each, but I never could see them propagate, either by shoots or divisions, although both ways are very common among aquatic animals. Neither was it by a fœtus ; but I had reason to think, that it was by means of eggs. Whenever the isolated wheel-animals had been some days revived, I have seen, in the body of the largest, an ovular substance ; Pl. 3. fig. 5. N. When I happened to find the wheel-animals dead, they had always this ovular body ; but in general it had passed from their own bodies into the glass, without my knowing how ; at the same time, with an important singularity. When entire, the isolated animal swam alone in the fluid ; but when the body was broken, there swam another wheel-animal, much smaller, along with it. This made me suspect that the new inhabitant had come from the ovular substance ; especially as, when the animalcula were excluded, the other eggs were broken. It might likewise be suspected that it had been carried thither by the air : but to be sure of this, it was necessary to see the animal's departure from the ovular substance ; which, notwithstanding all my care and attention, I could never accomplish.

My observations agree with those of Baker, who has not been more successful than myself.

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He thinks the wheel-animals are oviparous; because he has often found, in the water along with them, gelatinous eggs of a proportional size. Besides, he has in two species of wheel-animals, a little larger than the most common kind, discovered an oval body, the figure of which very much resembles the substance I have described; but he has never seen one excluded: nor has he seen a wheel-animal come from the gelatinous eggs, although he has kept them three years.

The learned Padre Roffredi has enjoyed the happiness denied to Baker and to me. In the *Journal* of the Abbé Rozier, he has given a finishing stroke to our observations. Speaking of the wheel-animal, he incidentally says, but in express terms, that he has seen it proceed from the egg. “Lewenhoeek is mistaken in
“ thinking the wheel-animal viviparous; and
“ what he takes for shapeless excrements in
“ the intestines, is really an egg which I have
“ seen it produce, and observed it many times
“ until I saw it hatched.” If Signor Roffredi’s observation is correct, I cannot doubt the fact. By this means, a circumstance in the history of the wheel-animal is elucidated, which should deeply interest the naturalist.

The solution of this problem, added to my observations, discovers another important truth; namely, that the wheel-animals undergo no

metamorphosis. I have collected many minute ones produced in a watch-glass, and kept them with great care: they always continued to encrease: but when I killed and revived them, their increment was slower than when they were kept continually wet. When full grown, each in general laid an egg, which produced another wheel-animal. Thus, I know, that from the time they are produced, until maturity, they undergo no metamorphosis. We cannot say it is a metamorphosis when they are produced; for they have then attained their greatest perfection. The insects which metamorphose, never propagate their species until they become winged animals, and have acquired the last degree of perfection to which metamorphosis will bring them.

Finally, my observations have demonstrated to me, that wheel-animals are hermaphrodites in the most rigorous sense. I have had the fifth generation, from many eggs isolated in watch-glasses, making use of isolation to take away all suspicion of copulation.

CHAP. II.

THE sand of tiles, the mud of ditches, and of marshes, which pass in the vulgar eye for the vilest of matter, are to the philosophic observer a source of wonder, from the singular beings they contain. To ditches and marshes we owe the armed, club, funnel, bulb, and knotted polypus. It is there we find the fresh water worm, the boat worm, and the springing millepede. Those animals have confounded the human mind, and have created a new philosophy. When the sand of tiles does not serve for an abode to wheel-animals, it will not for that reason be less famous or remarkable. An animal, which revives after death, and which, within certain limits, revives as often as we please, is a phenomenon, as incredible as it seems improbable and paradoxical. It confounds the most received ideas of animality: it produces new ideas, and becomes an object no less interesting to the researches of the naturalist, than to the speculations of the metaphysician. But the celebrity of this sand will encrease, when we learn, that it contains other animals, which, like the wheel-animal,

possess the property of resurrection; so that we may almost say, all the animals living in sand are destined to be immortal. I have discovered in sand two new species of animals, which I proceed to describe. I lament that their rareness has prevented me from extending my observations as far as I could have wished, or rather as far as the importance of the subject would have required.

Upon wetting the wheel-animals sand, I several times observed a yellowish animal, three or four times as large as a wheel-animal, with six legs; but I paid no attention to it, as I thought it was some little terrestrial insect that had fallen by chance into the watch-glass where the sand was kept. The reason inducing me to think it such, was, because I had always seen it move obliquely, and very slowly, at the bottom of the water: as if unable to walk, it was frequently supine, and always made efforts to recover its natural position; but those efforts were in general fruitless, as happens to many aerial and terrestrial insects falling casually into water. At the same time, with a more continued and careful observation, I recognised it as an animal really aquatic. I saw that it walked in so laborious and awkward a manner, from the smoothness of the glass-slider upon which it had been put for examination; and, when placed upon sand, it had a regular
progressive

progressive motion, slow indeed, and, compared with the wheel animals' motion, resembling the crawling of a tortoise. Thus, to design it by some descriptive name, I called it the *Sloth*.

The whole body is granulated; the anterior part is obtuse; and the posterior part terminated by four hooked filaments, which serve to fix it to any particular place. The limbs have small shining claws, which, as far as one can judge, are of a corneous substance, the points turned towards the body, as we see in the recurved claws of several insects. The corpulence of the sloth rendering it opaque, prevents us from seeing the internal organization. It only allows us to see a small elliptical spot in the middle of the body, which I suspect to be the reservoir of the aliments. The anterior part is also marked behind with a lucid spot, smaller, narrower, and longer than the other, which I have sometimes fancied the œsophagus. The figure of the body presents nothing agreeable; it very much resembles the testicle of a cock. The sloth is represented supine, Fig. 7. pl. 3.; and the profile is seen, Fig. 8.

This animalcule forms no vortex in the water; which is not surprising, as it has neither the wheels nor the vibrating fibrillæ of the animals which perform this operation. It appears that the wheel-animal cannot advance a step, without fixing the trident to some adja-

cent substance. It is not the same with the sloth; for it does not make frequent use of its hooked filaments. It never swims. It is specifically heavier than the water. It turns round upon the sand, or walks among it.

The phenomena of its death from the want of water, and of its resurrection when water is supplied, are precisely the same with those of the wheel-animal. The motion is gradually lost; the limbs are contracted and drawn into the body; the whole diminishes, is completely dried, and assumes a globular figure; Pl. 4. fig. 1. The reverse of this takes place, when, upon supplying new water, the sloth recovers life. As the wheel-animal can revive only a certain number of times; so is it with the sloth. It seems, that although sand is necessary for its resurrection, yet it is not so essential as for that of the wheel-animal. The degrees of heat injurious to revived wheel-animals, are also injurious to sloths; and it is the same with odours and liquors. Cold, however intense, does them no harm. Such are its similarities to wheel-animals.

Sloths are much more rare than wheel-animals. It is not common, for five-and-twenty wheel-animals, to find above three or four sloths. All sloths are of the same figure, although not of the same size. I have isolated several in watch-glasses, sometimes with sand, and sometimes with

with pure water, meaning to see how they propagated ; but, instead of multiplying, they all perished ; in the first case sooner, and in the second later, never attaining the sixth day in life.

The third species of resurgent animals found in sand, is that of minute eels, very like the *anguillæ* of vinegar. This species is much more rare, and it is not found upon every roof. The head and adjoining part of the body is very transparent, of a shining silver colour ; the tail is the same ; but the intermediate part of the body is a little dark, and is granulated upon the surface. The greater part of the tail is bent, and terminated by a very sharp point. The head, on the contrary, is obtuse ; and, a little below its extremity, there is a mouth, which terminates a little canal serving apparently for an *œsophagus*, and running through the length of the body ; Pl. 4. fig. 2.

If the sand they inhabit is very dry, they are seen motionless, dried up, and generally rolled in a spiral ; but, when moistened, they soon exhibit signs of life. The tail first begins a gentle motion, bending and turning in different directions : then the head moves, and afterwards the rest of the body ; so that the whole animal quickly becomes animated. The consequence is, that the same degree of humidity is not required to animate this species as

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by the wheel-animal and the sloth, which do not revive, unless completely immersed in water. The eels do not change their situation much ; they only extend, contract, turn, and bend themselves. If the sand is thoroughly wet, their agility and rapidity of swimming is as great as that of the eels in vinegar. They live a long time in watch-glasses, providing they have water ; and, if there is sand at the bottom, they hardly ever quit it, always moving about the grains, and touching them with the head, which would induce us to suppose they do so in search of food ; because some more minute and delicate substances are taken in by the mouth, and passed to the œsophagus. I never observed them propagate, although kept long in the glasses.

When the water evaporates they die ; but they resist death longer than wheel-animals and sloths. Sometimes a small degree of motion is preserved several minutes after evaporation. When dead, the figure of the body is changed ; the length is contracted, the breadth is diminished. Upon being moistened, they instantly resume their original size, and their animation re-appears.

There are conditions essential for their resurrection. When the eels are in sand, a quarter of an hour is necessary to recall them to life ; but when in pure water, there is a great difference,

difference, according to circumstances. If it is only the first or second time they revive, there is not much difference in the time required for their resurrection: but, in proportion as the number of resurrections encreases, the time necessary for resurrection always becomes greater: an hour at least is required, and sometimes this hour is not enough, for the fourth resurrection: for the fifth, there is still longer required; and so on for the rest. But the frequency of resurrection in pure water, as in sand, has its limits, like that of wheel-animals and sloths. The eels die for ever at the seventh, or eighth, or, at most, at the ninth resurrection; and, although placed in a humid situation, revive no more. I may add, that at each resurrection part of their agility is lost; so that the last is but a simple change, from immobility to languid contorsions of the members.

There, then, are three species of animals inhabiting the sand of roofs, of which nature has permitted the resurrection after death. Those three are the only inhabitants of this sand; at least, I do not think I have ever seen other animated beings there, having a permanent abode. They are not the only animals in the world that enjoy the privilege of resurrection; there are also others which possess it. Among those are the celebrated eels of blighted corn. All
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the world knows that Mr Needham is the author of this famous discovery. Examining the internal surface of blighted corn, he saw, with agreeable surprise, that it was composed of minute eels, which, upon being wet, acquired motion, and gave certain signs of life. This sudden resurrection, as he has thought, takes place when the ears are gathered still fresh and humid; if they have been gathered for some time, and have lost their humidity, it is necessary to macerate them, and this will not always be sufficient for the resurrection; it is even necessary that the eels which have come from the ear shall remain for a given time in the water. We also see, that when allowed to dry, they become motionless; and wetting them again, they recover motion and life. But, what chiefly surprised the author of this discovery was, after having preserved the blighted corn for two years and more, he observed, anew, the same phænomena upon wetting it.

The discovery was too wonderful for others not to endeavour to ascertain it. It has been found real by several good observers, such as the translator of Mr Needham's work, who speaks of this discovery ^a; by the Count Ginani ^b; but particularly by the celebrated Baker, in his excellent dissertation upon *the eels*
of

^a Nouvelles observations microscopiques.

^b Delle malattie del grano in erba.

of *blighted corn* ^a. Among other things, he has seen the resurrection of eels, taken from grain that had been dry during four years: this observation he made before Mr Folkes, then President of the Royal Society, and other friends. But he saw a resurrection much more wonderful, of the same eels, which was effected after a far longer time. In 1771, he had some blighted corn, which he had got from Mr Needham in 1744. He made some experiments upon this corn, and the resurrection of the eels succeeded perfectly, at the end of 27 years ^b.

In short, there is not at this day any professor, any amateur of natural history in all Italy, who does not take pleasure in amusing himself, and the learned curiosity of his friends, with those admirable resurrections. For this reason, I judge it needless to stop to prove, by new facts, their reality, and to speak of the origin and generation of those eels; for we know that this, which is the most essential part of their history, has been amply elucidated by the learning and labours of Italians. I shall but relate the results of some of my little observations, which may not only serve as proofs of their history, but are analogous to those we have related of other resurgent animalcula. The external

^a Employment for the microscope.

^b Journal de L'Abbé Rozier.

ternal colour of a grain of blighted corn which has been kept some time, is like foot; if broken, the internal substance consists of a dry whitish matter: when examined with the microscope, its appearance changes into a mass of long corpuscula, shaped like eels; but those eels are not only very much dried, but seem lifeless, and are so crowded and confounded together, that it is difficult to separate without breaking them.

If this grain has been infused some hours in water, and the extremity adroitly cut off without injuring the inside, and if it is pressed with pincers, through the hole there is seen pass a parcel of minute eels, just like a bit of paste drawn into a thread: when let fall into water, they are scattered about, and falling to the bottom, appear extended as so many straight lines, or a little bent, and remain in this position until they recover life.

I have attempted to know how long was required for their resurrection, computing from the moment when the grain was wet; but so many varieties have occurred, that I have never seen the same thing happen twice. In some grains, the eels were reanimated in three hours, and even in less, and others in four or five hours; there were some which required twenty hours and even more, and some, complete days. The eels of the same grain were not all reanimated

mated at once : it sometimes happened that two days intervened between the animation of the first and the last. The whole eels of blighted corn do not revive ; some are disfigured and lacerated : part of the whole are always of this description ; but some, apparently entire and unhurt, preserve perfect immobility.

The heat and cold of the weather are not indifferent to their resurrection : heat accelerates it, and cold retards it : but there are also irregularities here.

There are symptoms which announce the animation of the eels ; these I shall abbreviate. The first indication of life, is when the eels stretch themselves entirely or partially ; they are no longer straight lines, as when they are dead, but the head and tail begin to curve, though the rest of the body continues a motionless thread in a straight line. Sometimes the two extremities do not bend ; the middle of the body only becomes arched. Sometimes the one gently oscillates, while the other does not move ; sometimes they approach each other until they touch at the extremities, and form a circle ; sometimes one extremity rests upon the other, or glides over it ; sometimes the whole body is rolled in a spiral, in wider or narrower volutions. Those bendings, arcs, oscillations, circles, glidings, volutes ; those contortions, formed and destroyed, are repeated

ed at first with great languor ; then in a more marked manner, and with more vivacity. This astonishing variety of motion, with others which it is needless to describe, continues in the water during the whole time the eels live. Whence it appears they have nothing that may be with propriety called a progressive motion, which makes a difference between them and the other resurgent species of animals. During life they never rise in the water, nor do they crawl upon the sides of the vessel, but always remain at the bottom, where they are seen under the appearance of a nucleus, of a darker or lighter colour as they are more or less numerous.

If the water gradually fails, whether by evaporation or by taking it away, they gradually become lifeless ; and when there is no more water, they no longer move. The three other kinds of resurgent animals have the prudence to fly the places where the water dries ; but the eels continue in the same place without moving.

In some hours, they become very dry, and tenaciously adhere to the bodies applied to them, so that it is difficult to separate them without breaking ; but, when wet, they separate easily, especially with the point of a needle. They soon soften, and twist themselves, when it is seen they are of a gelatinous substance ; and they cannot be touched with an iron instrument,

strument, without being broken or injured. This at least happens when they are in life. When they have been dead several days, they are still very fragile, yet have more cohesion than one would think. They resist the point of a needle, and do not even suffer from a drop of water let fall from a height upon them. When they have been dry for a quarter of an hour, they are reanimated by the contact of water, and become as vivacious as before. Urine, salt water, and vinegar, produce the same effect, although those liquids are fatal to them in other circumstances, as we shall see. When they have been dry some days, they require a full hour to recover life. If one has patience to wet, and let them dry, their death and resurrection will be seen in this important limit ; which is, that the oftener the humectations are repeated, the less the number of resuscitants will be, and the longer the time required for their resurrection. I had a number of lively eels in a watch-glass the first time they revived : the thousandth part did not revive the eleventh time ; and the seventeenth there was not one. I have often repeated this observation, but always with the same success ; excepting that the reviving eels either went beyond the seventeenth resurrection, or died before attaining it.

Wheel-animals, floths, the minute eels of roofs, and of blighted corn, possess the property of resurrection, circumscribed within certain limits, beyond which it is lost. The body, to revive, must be entire. Eels cut into two or more parts, although often wet, and remaining long in water, never exhibit any sign of motion. When divided in two, they lose all sensation, after a slight universal vibration or convulsion of the body.

Like wheel-animals, I have subjected the eels to different experiments, and first to electricity, using Bevis' battery: those alive died instantaneously, and those dead at the time lost the property of resurrection. This did not surprise me; almost all were broken or disfigured by the traversing electric shock. In the blighted corn they have been subjected to the same experiment, but there was a difference in the results. When the grains had been previously macerated, few revived; if the grains were dry, many recovered life.

The eels are revived by salt water, urine, vinegar, when they have been a short time dry; neither are they so immediately killed by these liquids as other animalcula, for they will move in them several hours after immersion.

A vacuum does not affect their resurrection, whether the first time after proceeding from the grain, or after some resurrections; only the
resurrections

refurrections are not so soon accomplished as in the open air.

The heat of the sun or of the fire, at 140° , kills them in some hours: when raised to 144° , they are almost immediately deprived of motion and life. Heat more powerfully affects wet grains than dry. Sometimes the observer will have a number of eels from grains that have been exposed to 138° of heat; and sometimes the greater part are killed at that degree.

When freezing water becomes solid, the motion of the eels ceases. Cold 8° below 0 does not take away the property of resurrection; and when the ice melts, life re-appears.

Those who have never seen the eels of blighted corn, will find them designed, fig. 3, 4, 5, pl. 4, as they appear, when examined not by a very powerful magnifier, while swimming in the fluid. Seven blighted grains are represented of their natural size and figure, fig. 6, and three are magnified, fig. 7.

Plants are a kind of beings so analogous to animals, that we may excuse him who has defined them *ramosè animals*. In the works of Vallisnieri, Buffon, Bonnet, and, lastly, of the Abbé Corti, may be seen the numerous and various traits of analogy between the two kinds of organised beings. The subject which I treat, presents a new analogy; for, as different animals revive after death, so do many plants

spring again after they have perished. I should depart from my plan, did I mean to say as much of them as I have said of animals ; and I shall content myself with mentioning two, the Nostoc and the Tremella. The Nostoc, thus named by Paracelsus, is a terrestrial plant, which, from its sudden appearance in places where there was no mark of it before, was viewed by the ancients rather as a prodigy of heaven or of earth, than as a plant. So they have denominated it, heaven's flower, and earth's flower. It is seen in all seasons, but particularly in Summer, after heavy rains. Although it springs in every soil, it prefers meadows, arid lands, and sandy valleys. It is of a brownish green colour, and of an irregular figure, resembling a leaf carelessly folded. When separated with the fingers, some resistance is felt, such as when one tears a young leaf. If a sudden drought happens, the nostoc contracts and dries, remaining only a fine thin skin. If a sudden and heavy rain falls, it becomes green again, and resumes its original size. The nostoc is then, as Reaumur observes, who has furnished me with this intelligence, a plant of a singular kind, since it recovers life after being in a state which to others would be permanent death.

The Tremella is an aquatic plant, which enjoys the same privilege. Botanists have placed it in the class of Confervia. If in a vessel where
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the water fails, it dries and loses its verdure; but if water be supplied, it soon recovers its original state. Nature does the same with art. I have seen, from the beginning of July till the end of October, a ditch for watering land, covered fifty times with the beautiful verdure of the tremella, and seen it as often disappear, when there was no water: I saw only at the edges and at the bottom, colourless hairs, which, the microscope shewed me, were the tremella dry and dead.

But, what can be the reason why these species of animals and plants are thus privileged, in comparison with a very considerable number of others, which, perishing once, perish for ever? Shall we perhaps repeat what we have said upon the simplicity of the structure of those beings? But this does not seem to me well founded. There are many animals which do not revive, whose structure is as simple, and even more so, than that of the reviving animals. Several species of the animalcula of infusions, which seem composed of a simple aggregate of vesicles, enveloped by a fine membrane, are certainly more simple than wheel-animals, which are provided with vessels, wheels, intestines, and ovaries; yet they do not recover life when once it is lost. The simplicity of their structure would thence appear an obstacle to their resurrection; for, when the infusion has evapo-

rated, the simple membrane of several species bursts : then they are dissipated, and each is reduced to a mass, without order or connexion.

The armed polypi are no less simple than the animalcula of infusions, being composed but of a granulated gelatinous skin. If, therefore, simplicity of organization influenced the resurrection of animals, the armed polypi would certainly be of the number. They seem so much the better adapted for it, as they continue in life, notwithstanding every method used to deprive them of animation. It is demonstrated, that they receive no injury after being turned several times outside in, like a glove, or when cut asunder. If the head is cut off, there arises a sort of hydra with many heads, each of which receives food by a different mouth. If these new heads are cut off, there spring new hydras ; and each head creates a polypus fit for the formation of new hydras. In short, every particle of a polypus, the smallest fragment, develops and becomes a new polypus. If an animal so lacerated or mangled does not die, will it not appear very credible, that, only letting it remain dry, it will still retain the faculty of resurrection ? But facts prove the reverse. When the water evaporates, the armed polypus always dies, which happens equally, whether it is immediately exposed to the air, or concealed in aquatic herbs.

When

When the water is almost exhausted, the arms are drawn within the polypus: it contracts within itself, and then loses motion and life; which it never recovers, although afterwards copiously wet with new water. I speak of the armed polypus: it is the only kind I have been able to find, and it is the smallest of Mr Trembley's armed polypi.

After polypi, and the animalcula of infusions, according to my description, the organization of the stoth appears the most simple. We might say the same of the eels of tiles and blighted corn, which are two species of eels, easy to class from organization with so great a number of the inhabitants of fluids. Under the tremella, in the water, are often found minute eels, very like those of tiles in size, shape, and simplicity of organization. I have frequently had the curiosity to let them dry by the evaporation of the water: they all endeavoured to conceal themselves where the filaments of the tremella were thickest; and, when the evaporation was completed, they perished, remaining partly twisted among those filaments, and partly heaped upon each other. If immediately wet, they revive; but if a few minutes elapse, they do not.

The eels of vinegar give the strongest evidence of their vigour. Although they continue motionless when the vinegar fails, and are ap-

parently dead, they recover life and action if wet after a full quarter of an hour. Sometimes I have succeeded in reviving them after half an hour. I do not call this a resurrection; for, if it was such, I do not see why it should not take place anew, when wet with vinegar, after even a longer time. I shall rather say, they do not lose life so soon as the eels of the tremella, and several other aquatic insects, when left dry: when life is suppressed, it is preserved, and appears upon being wet with water.

I cannot find a greater simplicity in the tremella and the nostoc, than in many plants that do not revive. Let us throw a hasty glance upon the Truffle. That vegetable is more simple: no roots, tendrils, or fibres, internal or external, throughout a substance equally compact and uniform, only interrupted by veins similar to those winding upon some species of marble. In respect to other plants, not only terrestrial but aquatic, it has no analogous organization: yet truffles, after once drying in the air, do not revive when put in water.

These united facts shew how fallacious are the opinions of those who attribute the resurrection of animals and vegetables to the simplicity of their organization. To what principle then can we recur, since here we must proceed upon conjecture, rather than evidence, and the
view

view of truth? I shall suggest an hypothesis, without engaging to support it. The experiments of Haller demonstrate, that the vital principle of animals which have a heart, resides in the irritability of this muscle. His experiments are too well known, to need repetition. In animals which have no heart, it is almost probable, that the principle of their life resides in the irritability of their muscles; which being the case, if the state of animals is such, that the irritable nature of the heart and muscles is destroyed, so as to leave no hope of it being repaired, it is clear that the animal not only dies, but must always remain dead: but if the irritability is such, that, either by nature or art, it may be re-excited, it is certain that the animal should pass from death to life. It will not matter that it remains dead a long time, even for an age. The reader comprehends my idea. When wheel-animals, sloths, and the eels of tiles, are deprived of water, their irritability is lost, as is evinced by facts, and they die: when other animals have once lost this irritability, they never recover it; but wheel-animals, sloths, eels of tiles, &c. resume at once their original life.

This theory explains why, in certain cases, our animalcula lose the property of resurrection when exposed to powerful heat, to penetrating odours; or when certain liquids, or electricity,

lectricity, act upon them. Those agents injure the muscular structure, which appears by the destruction of the body, and from the irritating force residing there.

This perhaps is the reason why frequent humectations injure resuscitant animals. I have particularly seen, in the eels of blighted corn, that the frequent humectations injured and altered the members.

We must thence conclude, that as irritability resides in the glutinous part of the muscle, this part has, in resuscitant animals, qualities very different from the irritable parts of other animals, although we are ignorant of this difference, because we are ignorant of what the difference in the gluten consists.

I am a friend to sincerity. From my experiments may be deduced a conclusion against this hypothesis. Irritability is recognised by its appearance; that is, when the muscular fibre is touched by any stimulant, it contracts and becomes rigid. I have often stimulated the muscles of the eels of blighted corn and of tiles, with an extremely fine iron point, attentively observing what happened. It always appeared, that the muscular fibre contracted a little when touched; but I must acknowledge, that I saw the same thing in the eels of vinegar, and in other analogous animals which have not the privilege of resurrection. There are even

even minute aquatic and terrestrial worms, completely as irritable as the eels; since, if touched, however gently, they contract and swell, till they become twice as thick and short.

The objection is therefore confined to this; There are some animalcula which do not revive, although they are as irritable, and even more so, than those that do. This does not affect my hypothesis; for it does not place the principle of resurrection in the greatest and most perfect irritability, but in an irritability which, after ceasing, may be renewed by means of certain circumstances, although it would otherwise appear to be less active than in other animals. If this hypothesis does not entirely seem applicable to plants, at least in what concerns irritability, since we know, that, excepting a very small number, they have not this property, it is still possible to apply it to their organization. In general, dried plants do not recover life; probably because, in drying, they are so much injured, as to become incapable of imbibing the juices furnished by the earth, and of converting them to their own substance. Such is the cause of their perishing, and of their total destruction. If the drying of plants does not occasion this disorder, and if the action of their organs revives, when softened in water, and they resume their original form, it is undoubted they will here recover their former

mer verdure and natural freshness. This may be the physical cause, why the tremella, the nostoc, and other vegetables revive.

I shall end this dissertation with some reflections upon those beings which we may, at pleasure, pass from life to death. When this idea is presented to the mind, we are astonished, because they are isolated beings: they form a separate class, and are adverse to the received ideas concerning animated beings. But, when it is proved by a train of innumerable facts, that all is gradated in nature, that those beings are connected to other beings, and consequently, the isolations exist but in the general system, our wonder should cease, or at least diminish, since it only arises from our ignorance of the relations which afford us the connexion of the class of beings possessing the prerogative of resurrection, with the other classes of animals. This is not the only isolated fact that has existed, and, at first, been viewed as an exception to general laws. To be acquainted with many, it is necessary to read the works of Reaumur, Trembley, and Bonnet. Those exceptions appear singular at first, because they are seen only in a single instance. A plant, an animal of a new genus, or which possessed peculiar properties, was the origin of those exceptions; but farther observation, and solid experiment, have shewn them adapted to several

several cases, either in the same circumstances, or under some modifications, and proportioned as people have applied themselves to diversify the number of subjects which have verified it. Some are so extensive, that it is impossible to call them exceptions. This wonder, which affects the mind from something new or extraordinary in the facts which occasion it, gradually diminishes, and vanishes entirely. One or two examples will corroborate and elucidate my reflections upon resuscitant animals.

One of the most efficacious methods of destroying animals, is, to cut them into pieces. The experiment is common, and known. To say, that this was a method of multiplying some, would be to affirm what had a fabulous appearance. Yet it is the case with the armed polypus. And shall we say the discovery is bounded by this animal? The scalpel need only be applied to other animals, to prove the discovery extensive: the reproductions of the earth-worm, the boat-worm, the fresh water worm, some leeches, sea-stars, sea-nettles, &c. While art effects prodigies upon those species, nature prepares similar in silence; I speak of the natural division of the dart millepede, of several species of club, funnel, and bell polypi, and a great number of infusion animalcula. We also find, that nature forms not only two animals from one only, but even four, and a multitude

multitude so prodigious, that there are as many similar animals as there may be atoms in the generating animal. But the wonderful progress of the discovery of the polypus, does not end there. It is a chain passing from vegetables to animals, and leads to man. The tremella is the link connecting animals and vegetables; it is a real zoophyte. It has been discovered, that the filaments of which it is composed, spontaneously divide, and that a complete plant springs from each division. The polypus is joined to the tremella, and is united to a number of species dividing like itself, which in the same manner are linked with other species. But reproductions are not effected in the same way in all those beings. One may reduce a polypus, a fresh water worm, a sea star or nettle, to the smallest fragments, and be certain that each fragment will reproduce itself. The earth and boat worm cannot be cut into pieces so small, and reproduce itself. If a snail is decollated, a new head will germinate; but the head cut off, will not acquire a body. Water-newts, and frogs in the figure of tadpoles, recover their tails and limbs when deprived of them; but, if mutilated in other parts of the body, they perish^a. No warm-blooded animal is known, which reproduces itself when cut in pieces: but animals

recover

^a Treatise upon animal reproductions.

recover large pieces cut away. Duhamel has seen a ring of flesh cut to the bone, reproduced in the thigh of a chicken.

Similar reparations are every day observed in wounds, in the cicatrices of wounded animals; and we have certain evidence of the reparation of the tibia in a man ^b. It is thus that the discovery of the polypus, which, at first, seemed to revolt so much against rules thought general, extends to so many links in the animal chain. But, has this success so rapid, this advance so great, exhausted the subject of reproductions? No, certainly; it will appear but little known, if we consider how bounded the number of animals is upon which our experiments have been made, compared with those upon which we may make them. The element of water is most favourable to reproductions. How many insects, worms, reptiles, zoophytes, are there inhabiting the briny waters of the sea, or the fresh waters of rivers, pools, marshes and ditches, upon which there was never made an experiment, and which, from the great resemblance they have, whether in the modes of life and propagation, or in shape and external appearance with the reproducing animals, are doubtless calculated to reproduce themselves?

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^b This fact is related by the celebrated Bernard Mes-
sati, surgeon at Milan.

The idea of hermaphrodisim, until the beginning of this age, has been viewed as a thing more chimerical than true. Nature seemed to depose against it ; but in how many hundreds of animals have not the cares of modern naturalists found it ? This admirable property has passed by degrees from one species to another. The polypus is a perfect hermaphrodite, without sex ; it multiplies not only by divisions, but by shoots. The puceron of plants is less an hermaphrodite ; it has a sexual distinction : and although, during Summer, it multiplies without copulation, at the end of Autumn, it is observed to copulate. Earth worms, shell snails, naked snails, and many species of shell fish are more or less hermaphrodites. They are, at the same time, male and female, but insufficient alone. They give and receive, they fertilize and are fecundated. The discovery of resuscitant animals is far from being as extensive as that of reproduction, and of hermaphrodites ; but this arises less from the paucity of those beings, than from the fewness of philosophers who have entered upon that branch of natural philosophy. Lewenhoeck was the first who drew the curtain from before those wonderful objects, by his wheel-animal. He seems surpris'd at a fact, unique, and unexampled in nature ; and indeed no one thought that there was in all the animated world

world another animal possessing this prodigy. But, since the profound researches of more modern naturalists have made others appear, I doubt not that the number of those wonderful beings will encrease as the study is cultivated.

The shades seen in hermaphrodites are not observed in reproducing animals, and cannot be expected in the resuscitant. There may be a greater or less degree of reproduction: an animal may reproduce more or fewer organs: an animal may perhaps be more or less an hermaphrodite, if hermaphroditism is taken in a comprehensive sense: thus, both possibilities have been realised, as we see in the facts I have mentioned. But we cannot reason thus upon the animals reviving after death: we cannot say that an animal dies more or less, or that its resurrection is greater or less: death and resurrection are two indivisible acts; so much, that neither has any gradation in the animals hitherto known, and it cannot be expected in those we shall afterwards discover. It is not because those animals have no particular gradations, and are not connected with others of the same kind; but because those gradations are difficult to be observed. We have seen the state to which cold reduces some animals; we cannot call it death. There is then a thread of life, a lesser life, to connect resuscitant animals with the rest. The greater part of plants

lose their verdure during winter : they have little sap, and it is motionless : they cease to take nutriment, to grow, to multiply ; all which produce complete inaction. How many animals beside insects do we see with a less degree of life, even without excepting warm-blooded animals, which have a similar degree, and, among them, birds ! I am far from thinking with Guaguin, in his description of Muscovy, “ *Populos quosdam in Lucomonia regione Russiæ habitantes, quotannis vigesima septima Novembris die, ut solent hirundines et ranæ, sic et ipsos frigoris hyemalis magnitudine mori, postea redeunte vere vigesima quarta Aprilis die denuo reviviscere.* ”

We cannot at the same time deny, that man may sometimes be in a similar situation with animals overcome by cold : when immersed in water, we may say he hardly appears to be in life. I will not affirm with some physiologists, because there is neither pulsation nor respiration found in the body, the pulsation of the heart, and the circulation of the fluids, are suspended ; but I would rather think with Haller, that those motions are only too faint and obscure, to appear or be externally perceived. Examples of this we have in some animals half-drowned, where a degree of motion is perceived in the heart and in the blood. Besides, the life of man and animals, when half-drowned

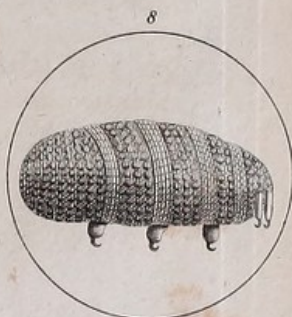
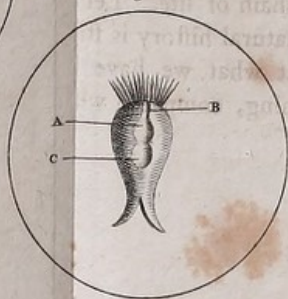
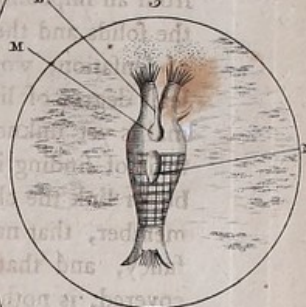
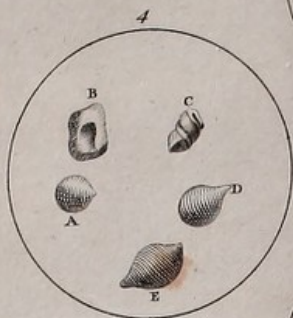
drowned or strangled, cannot be more faint; and we may regard it as another point in the passage from the resuscitant, to those animals which do not revive.

There are other two states which are very similar to the death of our animals. The one, the state of the embryo in a fecundated egg which has not yet experienced the heat necessary for its expansion; life is incomplete; there are but the rudiments or principles of life. The other state is that of a chrysalis. Among insects, when the caterpillar has lost its natural form, it assumes that of a shapeless mass, without the vestige of feet or of wings: it ceases to feed; indeed to eat would be impossible, for the organs are wanting: it has no longer loco-motion; and we would really believe it dead, but for some inflection and contortion of which the posterior part is susceptible. The appearance of death is still more sensible in the nymphs of many worms; they exhibit no symptom of motion or sensation.

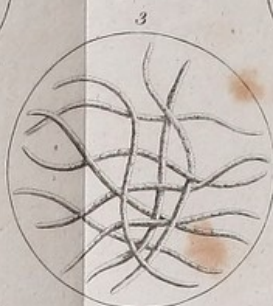
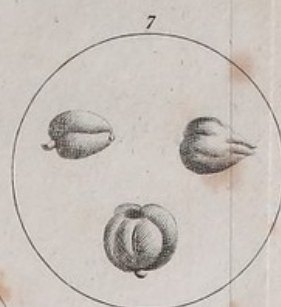
Thus there are in nature situations a little similar to the state of resuscitant animals when they are dry. We see in the resuscitant animals, that this state may be protracted or abridged at pleasure. That torpid animals may never awaken from their lethargy; that the embryo may never expand in the egg; that flies and butterflies may never proceed from

the nymphs and the chrysalids, it suffices to keep them continually exposed to cold. The reverse will happen when they are exposed to heat. Probably there are gradations of connexion, more immediate and more direct, between the animals that revive, and those that do not. Life, however feeble and obscure, is always life; between it and death there is a distance as great as between entity and non-entity. An animal, whose life is suspended from an impediment to the mutual action of the solids and the fluids, and which is devoid of sensation, would be the link connecting the least degree of life with death. Such an animal is yet unknown: but we should not despair of finding it, so many being found which better link the chain of life. Let us only remember, that natural history is still in its infancy, and that what we have hitherto discovered, is nothing, compared with what is to discover.

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EXPERIMENTS AND OBSERVATIONS

1804

THE ORIGIN OF THE PLANTULE OF MOUND.

The mould which I have examined, and intend simply and briefly to describe, is that which springs upon apples, pears, melons, and gourds, beginning to spoil. It may be considered as of two kinds, the one very simple, easy to observe and describe, the other complex and involved, which can only be explained by a generic description. Let us begin with the former.

One species of this mould is without branch: each individual bears at the summit a globe, as pl. 2. fig. 3. Another is ramose, but with this difference, some plants have a globe at the vertex of each branch, while others have it not, fig. 4. Those globes I shall always term the minute heads of mould. There is one observation necessary to be made concerning them. Without the microscope, they appear spherical, and

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and, even with it, they have the same appearance when viewed from above ; but when examined below, that is, where the stalk is inserted in the head, we observe that all, or the greater number, are shaped like mushrooms ; or, to speak more philosophically, they are real mushrooms. Two filaments are represented with the globules, fig. 7. ; the globules highly magnified, fig. 8. On pears there sometimes grows a kind of mould, which is a real tree in miniature, universally adorned with spherical heads or mushrooms. A stalk of this mould is represented, fig. 6. We must remark, that ramose mould is very often attached to vegetable substances, without any root : but the mould without branches, almost always has roots, originating from a round corpuscle, whence the filaments forming the stalks of the mould are detached. It is a singular circumstance, that as every root of mould has a greater number of stalks, the filaments from the root are proportionally more numerous. Upon tearing the mould from the substance where it springs, a degree of resistance is felt, which arises from the roots being well fixed : when torn up, they appear very crooked, while the stalks, if they have not suffered from the impression of the air, are very straight. Many of the stalks are of an equal thickness, at least they do not be-
come

come much smaller towards the top. Fig. 3. will perhaps render this description more plain.

Mould is at first of a most beautiful white; it then acquires a yellowish tint, and at last blackens; but the heads grow much blacker than the stalks and branches. The origin and encrease of mould is almost proportioned to the heat of the atmosphere: it is never more apt to appear and vegetate suddenly, than during the heat of Summer: a substance which at night exhibits but a single stalk, will frequently be covered in the morning, and then the mould has attained its full size and maturity. The height does not encrease so much as the thickness: the heads already black, are always of a larger size than those still young and white.

Mould is never so beautiful and vigorous as when it vegetates under some vessel or receiver, providing the communication of the external with the internal air remains. The reason is evident. As the stalks are very fine near the summit, and bear at the vertex a round corpusculum, oscillating by its own weight; each filament oscillates, as the ear of corn occasions the oscillation of the stalk. Thus we may easily perceive, that every breath of air, however gentle, will bend, break, and destroy those most delicate filaments; which does not take place, when the substances to bear mould are put under a receiver. Besides, the humidity of the

substance is better preserved, which is a circumstance most essential for the production and increment of mould. In the course of these observations, I have always made use of receivers. The injurious influence of agitated air is seen, fig. 1, which represents two quantities of mould with heads, as seen with the naked eye, when taken from under the receiver, and exposed for a short time to the action of the open air: their natural direction is lost, and they turn to opposite sides.

Several substances put in a situation to acquire mould, dissolve into water, by which the adjacent plane is wet in proportion as they spoil; and it is precisely upon this place the mould hitherto mentioned springs. From the same substance, there likewise exhales a lesser degree of moisture, which adheres to the inside of the receiver, forming a pellucid aqueous veil; and the encrease is so considerable as to form large drops, which run down the sides of the receiver in streams, meandering among the moistened places: the same quantity of mould grows upon the streams, as may be seen, if the receivers be transparent.

The other kind of mould we have described, is very complex, and always springs upon the vegetable substance itself, particularly upon gourds and moistened bread. When the putrefaction of these vegetables commences, a
white

white thick covering of mould appears upon the surface, and in a few hours is a line high. When ripe, it is three lines high, or more. This is a species very different from that of which I spoke before, which, at complete maturity, is scarcely half a line high. We have already said, that each plant of this mould may be observed separately, and without confusion, and that the observation may be continued, distinguishing each until maturity : but it is otherwise with the mould of which I now speak ; such an observation would there be impossible. The immense number of plants when the mould begins to spring ; the interweaving of the stems, and bending of the branches ; the entangling and interlacing of the whole in an hundred different ways, completely prevent it : the sight is confounded among a multitude of slender filaments, which become in a greater or less degree a whole, deranged and confused as the vegetation of the mould advances : the substance can only be torn asunder to see its progress. When the stalks of this mould are pulled from the substance upon which they spring, it would appear they have no roots. From each of the numerous stalks, many twigs and branches proceed ; but we should observe, that those subaltern productions are frequently of as great a diameter as the parent stalks from which they originate.

While

While the stalks vegetate and extend, there appear several groups of smaller stalks springing laterally, with minute heads at the vertex. These heads resemble mushrooms, partly globular, and, as the stalks rise, the heads encrease, until they blacken and arrive at maturity. New stalks with heads, are, besides these, seen to rise, and the multiplication continues while the mould vegetates. This little forest is full of stalks; and its skirts are terminated by delicate points and black globules.

In this manner does mould spring, ripen, and die. We need not ask whether it is a real vegetable; it appears to be such too evidently, by the observations I relate. But these vegetables or microscopic plants, do not possess two properties common to other plants. Ligneous and herbaceous plants, when exposed to natural light, always tend to take a direction perpendicular to the horizon. They endeavour to attain it, and even to recover it, when taken away. The experiments of M. Bonnet are excellent, as may be seen in his work *Sur l'usage des feuilles*. In mould, we do not see this tendency to perpendicularity; for, although many stalks are perpendicular to the horizon, this is not essential to their nature, and there are at the same time many other in a different direction. If a plant grows in the rent of a wall, although its first appearance is in a horizontal direction,

direction, it in a very short time rises perpendicularly towards the heavens. It is not so with mould. I have often cut a piece of melon, gourd or bread, into a cube: mould vegetated upon the four lateral surfaces, and the stalks had constantly every other than a perpendicular direction.

The other property which was discovered by the celebrated Genevese naturalist, is, the tendency of plants to turn towards the light. Beside the facts he relates, which are sufficient to ascertain the property, I have frequently observed it in legumes growing in infusions I kept shut up in a press. The plants always bended towards a chink, through which a very small ray of light penetrated: and, if I shut up this chink, and opened another in a different part of the press, the plants abandoned their original direction, to take this new one. I have endeavoured to learn whether this happened to mould, but could never discover that light had the least influence upon it. If ripe mould is shaken, a sort of black dust falls from it, which the celebrated botanist Micheli has thought to be the seed of the plant; but Dr Monti senior, a very eminent botanist likewise, has doubted the truth of this observation, and he rather inclines to think, that mould springs by a spontaneous generation.

Before

Before discussing this question, the object of which is so interesting, I think it proper for us to examine the place where the dust is found : to accomplish which, it is necessary to make a brief analysis of the heads of mould : this we can do only by examining and seeing it ripen. Before the heads are ripe, they are of a whitish or yellowish colour, the surface very smooth : upon being broken with a fine iron instrument, they seem to be membranaceous, and full of a granulated substance : if, instead of being broken, they burst, sometimes a number of minute round seeds come out. These are found both in the spherical and fungiform heads.

When the heads blacken, the appearance changes ; the surface seems unequal ; it is lacerated in several places, and resembles a parcel of black rags. When opened, a number of seeds is seen ; but young mould has white seeds, and old or ripe mould has black. When the heads are moistened, the seeds are seen more distinctly, and in greater abundance. Upon contact with the fluid, or a little afterwards, the heads burst, and scatter around a cloud of seeds ; so that I may affirm, without danger of exaggeration, there is a million in each head. The unripe heads do not open in this manner upon being wet ; they remain entire. It must be remarked, the ripe heads are not totally decomposed. Where they are round
like

like mushrooms, there is a little head in the centre, which continues to adhere to the stalk : this is cinder-coloured, with a degree of transparency, and does not appear black like the exterior : it is difficult to detach it from the stalk ; but, with a small degree of pressure, a little jet of seeds is raised from it, resembling those I have described : the central head then becomes a dry and empty skin.

If the heads, when black and ripe, are opened by means of water, such a quantity of seeds issues forth, as to adhere in great numbers to the plants, in particular to the stalks ; so that one would suppose the exterior composed of seeds alone, were they not seen in a different state before. One of those deceitful appearances is seen, Fig. 9. : it is intended to represent two plants of mould, one of which is entirely covered with seeds ; the head is tumid, and likewise in a great measure covered with seeds. Fig. 4. represents two stalks of mould with heads : the whole seeds of the one are exposed, and another is perfectly covered by the integument ; the third is entirely covered.

A quantity of those minute seeds constitutes the powder blackening the hands, when the mould is manipulated ; and they are considered as real seeds by the celebrated Florentine botanist. To ascertain the truth, he had recourse

to

to a method apparently decisive, which was, to sow the dust. He strewed it upon some vegetables, and saw them covered with plants of mould. But the Bolognese professor has repeated Micheli's experiment, without finding it so conclusive; the vegetable substances being equally covered, although no dust was put upon them. Thus, the question remained undecided; for I do not know that any other person has attempted to resolve it.

Perhaps I shall be taxed with presumption, when I say I have been able, by means of experiments analogous to those of Micheli, to ascertain the fact; but experiments much more numerous, more diversified, and more connected with each other. The consequences I shall venture to publish. I took two pieces of moistened bread, as similar to each other as possible, and from the same loaf, so as to be perfectly equal. I endeavoured to attain the same equality in all the rest of my experiments. I strewed one of the pieces with dust taken from a quantity of ripe mould heads, so that the surface was slightly blackened by it: the other piece was untouched, in order to compare the production upon each. This was done in Summer. Upon the following day, the *sown substance*, but, for brevity, I shall observe, that by this expression I mean, the substance of whatever nature covered with dust, and
by

by the word *unsown*, the other vegetable substance not covered with dust: upon the sown substance, I say, next day appeared a shade of mould; whereas upon the unsown, there appeared none. Before the third day, both substances were covered with mould; but the mould of the sown substance was almost double the height and thickness of that upon the other. Both species of mould were of the same kind, and perfectly similar to that which had produced the dust. Upon the fourth day, the mould of the unsown substance, although preserving the original thickness, was equal to that of the other in height: it was still higher upon the following day, but afterwards continued to become thinner. I repeated those experiments eleven times upon moistened bread, and the effect was, that the mould twice became equally high and thick upon both substances, and nine times it was higher upon the unsown substance, than upon the other: it constantly sprung first upon the sown substance.

Having collected a great quantity of ripe dust, I thought of varying the quantities, by scattering different portions upon moistened bread: the consequences were new. When the quantity of the dust was very small, there was almost no difference in the height and thickness of the mould; but the thickness encreased, if a greater quantity was sown. It

was

was never so thick as when liberally scattered over the bread ; but, in proportion as the thickness encreased, the height diminished. Those experiments were repeated again and again, upon apples, pears, and gourds ; and the results were all, in a certain degree, more or less similar to what I relate.

The consequences that may be deduced from those experiments, are these. 1. Sowing the dust accelerates the production of the mould : 2. The thickness is encreased : 3. The height is less. Considering these facts with respect to my object, it seems to me, that the second proves the dust to be the real feed of the mould ; so that scattering the dust occasions the more abundant production of the mould. If the thickness encreases with the encrease of the quantity scattered, it is natural to think, that the superabundance of mould upon sown substances is an effect of the dust, or rather of the minute feeds sown, and that all, or most part of the mould, originates from them. This being the case, it is not surprising that the mould upon sown substances is not so high as upon the unsown ; for, the plants being more numerous, each cannot imbibe the same degree of nutriment from the sown substance, as may be derived from that which is not sown, where the plants are less numerous ; which is also the case with other plants, as they are smaller and shorter

shorter in proportion as they are crowded. The first consequence deduced from the facts demonstrates, that the production of fown mould is more early than that of unfown. I have thought that this might happen, because the fown substances sooner spoil; since it appeared that the dust germinated earlier, if the corruption of the substances advanced, as I before remarked.

I diversified those experiments. Sometimes I covered an half, sometimes two thirds, or one, of a slice of bread, an apple, a pear, or a gourd, without touching the other half: the half, two thirds, or one third, were just in the situation of the fown substances. I likewise made another experiment. After covering half a slice of bread, an apple, or a gourd, with dust, I applied the surface fown to another surface similar, but unfown, leaving both in this state for several days. Upon the whole fown surface was seen a veil of mould, the vegetation of which had ceased, because it was spoilt by the substance applied; but there was no vestige of mould upon the unfown substance.

These last facts concur in corroborating the idea, that the dust is the real feed of mould, because the mould produced upon the places fown, was exactly of the same species with that which produced the dust. Notwithstanding

ing all those plausible and repeated experiments, I was not satisfied. Is it not possible, said I to myself, that this dust only renders the soil more fertile, so that it will produce a greater quantity of mould, as the earth, fertilized by foreign matter, will produce a greater number of plants? This was certainly not impossible; and, wishing to proceed with philosophic strictness, I judged myself obliged to realize or destroy the possibility: for which purpose, I thought that I should cover the substances where mould springs, with dust taken from different vegetables, different earths, and other matter volatile from the extreme minuteness. It appeared to me, that if this dust could contribute to render the substance more fit to produce mould, it did not alone possess this advantage. The roots, stalks, and green heads of mould, were not spared. I dried and reduced them to a very fine powder, but without effect. The greater part, instead of giving that abundance of mould, deprived the sown substances of the faculty of producing it; and those which did not prevent the production, diminished the quantity of what the unsown substances used to produce. All these united facts seem to prove, that the minute grains proceeding from the heads of ripe mould, are real vegetable seeds.

During

During those experiments, I was curious to learn, whether the seeds would germinate when sown upon substances that did not mould. I sowed a certain quantity upon hard bodies, as glass, metals, stones, &c.; also upon blot-sheet and writing-paper; upon cotton, sponge, &c. All those substances were kept moist, to see whether they would mould; but there appeared no mark of it, excepting some filaments I saw upon sponge. To expand, certain circumstances are required by the seeds of mould, and those are only found in certain substances.

The minute seeds, or dust of mould, possess the peculiarity of resisting a degree of heat which no other seeds can support, without losing the faculty of germinating. After boiling the seed in water, I poured out the water, then become black, upon substances that mould, and where the mould, according to custom, grows thicker than upon the substances unmoistened. I did the same with some dust exposed to a much greater heat, such as that of a hot chafing-dish, and have found, that as this heat does not deprive the seeds of the property of reproduction, neither does it alter their size and figure; as I have convinced myself by examination with the microscope, before and after exposing them to the heat.

But, does the mould, which springs without being sown, and by the care of nature a-

lone, upon an infinity of substances dispersed here and there, also derive its origin from the dust we may suppose disseminated through the air, and upon terrestrial substances? If natural and artificial mould are of the same species, and if the artificial is produced by the dust of the natural mould, I cannot see why the last should not derive its origin from the same principle, especially since it is demonstrated, that no other part of the mould, as the roots and stalks, aid the reproduction. The hypothesis, supposing that this dust is invisibly scattered through all, and gives existence to a great quantity of natural mould, is one of the most reasonable hypotheses in philosophy. If each head of ripe mould can furnish a million of seeds, as we have seen, and if each spot of mould contains a prodigious number of heads, it is clear, that in some years, the dust should be extremely multiplied; particularly, from its levity and fineness, it may be universally spread.

We have certain evidence that seeds may be kept a long time, without losing the faculty of germination. My illustrious friend M. Bonnet told me a singular fact. In 1748, corn was carried from Sicily to Geneva, and lodged in the magazines of the republic. Some individuals sowed part of it in a walled garden, 1771. Notwithstanding the length of time,

it

it vegetated very well, and nearly as thick as in common with this grain. The wonderful minuteness of the seed of mould, seems to adapt it well for long preservation; but I have given a cogent proof of this. Heat is undoubtedly one of the most powerful agents in depriving seed of its germinating faculty. In my tract upon infusion animalcula, it has been seen, that the number of seeds that can support the heat of boiling water, is very small: and, if the very singular case of M. Duhamel is recollected, where seeds germinated after experiencing 235° of heat in a stove, it is demonstrated, that those of mould are not destroyed by a degree infinitely greater. This being the case, it is not absurd to think, that seeds which resist the injuries of weather, may preserve their fecundity for ages. It is thus very easy to comprehend how immense the quantity of this vegetable should be, since its seed multiplies so much, and is preserved so long, and that it should be so largely diffeminated over all terrestrial substances, as always to be ready to germinate, when the circumstances, essential for this, occur.

Thus, the first of Signor Moscati's doubts concerning the universal generation of mould, is resolved, which induced him to think it was produced by a spontaneous generation. The other doubt is equally removed, from which he

deduced, that he had seen moulding substances, mould the same after being boiled : but, if the seed does not lose its virtue of germinating, after being exposed in a burning chafing-dish, it is not surprising that it is preserved at a degree of heat so far inferior as that of boiling water.

Although the substances upon which I made my experiments, were constantly kept under receivers, which was done with the view of having more luxuriant and beautiful mould, the communication of the external with the internal air was not interrupted. I wished to discover what would happen if the communication was cut off; and, first, what would be the consequence of lessening it. Moulding substances were put into very large glass vessels : the necks of the vessels were then drawn to a point, by means of the blow-pipe ; and the point could be made of any degree of fineness. I had vessels, into which a stream of air no larger than a hair could be admitted ; a little more in some, and still a little more in others.

All the confined substances moulded in a certain time. In the vessels with a very small aperture, I remarked two circumstances ; that the vegetation was slower, and the mould did not grow so high, as when the aperture was larger.

The

The vegetables within, always perspired so much, that the vapour collected at the apertures, and obstructed them, particularly when they were very small. This inconvenience may be corrected by sucking out the moisture; but, if this is not observed, mould will not grow, or hardly at all, in very small vessels.

My curiosity being satisfied with regard to this, I passed to another thing. I excluded the external air entirely, which was easily done by an hermetical seal. All the vessels were not of an equal size; some would contain six pounds of water, some only one, and others but a few ounces. This variety of size affected the mould. In the largest, although of the same capacity with those in which the mould was exposed to the air, it did not germinate in the same abundance, and was later of appearing: the mould in those of a middle size was still later, and more rare: its state was worst in the smallest vessels; none appeared in some, and in others a slight shade was scarcely seen.

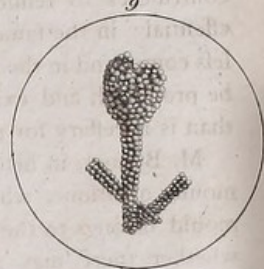
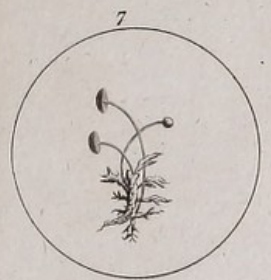
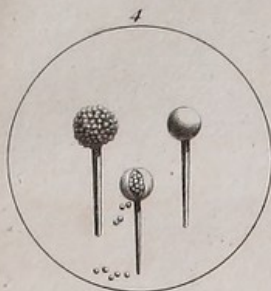
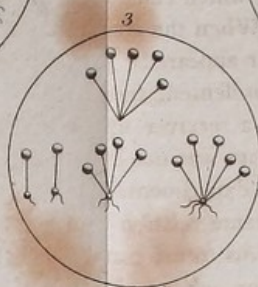
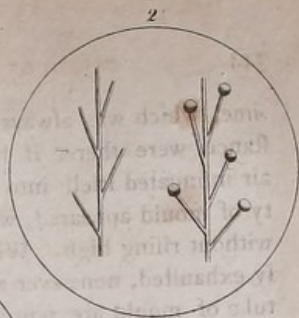
After vessels of three different sizes, containing vegetable substances, were hermetically sealed, I put them during several hours in boiling water. In the smallest vessels there was no mould, in the middle-sized a little, and plenty in the largest.

I put moulding substances in vacuo: the result of several experiments was, that during the

time, which was always several days, the substances were there, if by chance a portion of air insinuated itself into the receiver, a quantity of mould appeared, which came to maturity without rising high. When the air was entirely exhausted, none ever appeared. Some plantulæ of mould are represented, fig. 5. pl. 5.; they were produced in a receiver upon an air-pump, where the vacuum was not complete.

These three different experiments prove, that the plantulæ have the same relation with air as other plants, only it seems to me that air is not so necessary to them; for, when a thread of air entered the receivers, some mould vegetated, though the leguminous seeds within could not vegetate in that situation. Neither do seeds germinate in vessels hermetically sealed, although, as I have said, vegetables will mould there. The simplicity of mould undoubtedly contributes to render the presence of air less essential: in the same way as animals, that are less compound in the scale of organization, may be produced, and exist in a less quantity of air than is necessary for us.

M. Bonnet, in his judicious reflections upon mould, questions, whether we are sure that all mould belongs to the vegetable kingdom, and whether there may not be some species approaching the mineral, or may be the connexion between the two kingdoms. This is not impossible,



impossible, when we consider how prodigiously
diversified this class of ... is and how little
its practice are ... known, it is evident
that it is ... the lowest place of the ...

table ...
But the model described, I have not
attempted to show a picture upon many different
papers, and I must say, that I have found in
all cases, that the ... no longer than
real ... I must ... that the
various ... I have observed the ...
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other things, I ... the question con-
cerning the real ... of the most common
model. ... has not before been com-
pletely ... and it has led some persons
into the most and dangerous error of ...

... ..

impossible, when we consider how prodigiously diversified this class of beings is, and how little its species are hitherto known, if we esteem that fossils occupy the lowest place of the vegetable kingdom.

Beside the mould described, I have not neglected to throw a glance upon many different species ; and I must say, that I have found in all, very decided characteristics to judge them real vegetables. I must likewise say, that the various kinds of mould I have observed are very few, compared with the immense number yet remaining to be examined ; for there is almost no substance, animal or vegetable, which is not in certain circumstances liable to mould. Those who attach themselves to this branch of Botany, will have sufficient useful practice ; and perhaps they may succeed in discovering the link connecting vegetables with minerals, which will do Philosophy a most important service. For my own part, I shall be content if, among other things, I have resolved the question concerning the real origin of the most common mould. The subject has not before been completely discussed ; and it has led some persons into the ancient and dangerous error of Spontaneous Generation.

impossible, when we consider how prodigiously diversified this class of things is, and how little in species we hitherto know, it is almost certain that this class occupies the lowest place of the vegetable hierarchy.

While the mind dwells on this, I have not hesitated to draw a glance upon many different points, and I must say, that I have found in all very decided characteristics to judge them real vegetables. I have observed, that the various kinds of plants I have observed are very few, compared with the immense number of animals, and the still greater number of insects, animals or vegetables, which is now in certain circumstances liable to mould. Those who attach themselves to this branch of Botany, will have frequent need of practice; and perhaps they may be forced in discovering the link connecting vegetables with minerals, which will do I think a most important service. For my own part, I shall be content to, among other things, have pointed out the position concerning the real origin of the most common mould. The subject has not before been completely discussed; and it has led some persons into the notion and dangerous error of Spontaneous Generation.

EXPERIMENTS
UPON THE
REPRODUCTION OF THE HEAD
OF THE
TERRESTRIAL SNAIL.

BY
CHARLES BONNET.

EXPERIMENTS

UPON

THE REPRODUCTION OF THE HEAD OF THE
LAND SNAIL.

MEMOIR I.

I PUBLISH my experiments upon the reproduction of the head of the snail, only to afford an additional confirmation to the Abbé Spallanzani's beautiful discovery. It is well known how much this discovery has been disputed beyond Italy, and particularly in France. There are naturalists of my acquaintance, who, after decapitating hundreds of snails without success, have thought themselves at liberty to conclude, that the Italian observer had allowed fallacious appearances to impose upon him. One of those naturalists, writing to myself, did not hesitate to reproach me for having inserted an account of the imaginary discovery in the *Palingenesie*, and for reasoning upon it, as upon a fact the best ascertained. It will be thought with justice,

tice, that those reproaches did not weaken the confidence with which the ability and the sound reasoning of the celebrated naturalist of Reggio had inspired me. Besides, he had communicated to me, in a course of correspondence, the interesting history of his experiments at length: and it was easy for me to judge, only by a trial of the facts that the judicious observer had seen and reviewed, the new wonders he laid before me, and which, in a short time after, he presented to the public in an Italian tract, which appeared 1768, and was, the same year, translated into French. But, since the author did not detail, in that treatise, the precautions he had taken to secure his discovery against all dispute, I requested him to publish an account of his method, which he did in a letter from Modena, 11 September 1769, and it was printed in the *Avant Coureur*, 30 October. This letter, so well calculated to destroy every doubt, has only done so in part; some remain, and people continue to oppose to the experiments of Padua, others which they judge contradictory to, or apparently confuting them. This conflict of experiments, which has continued nine years, induced me myself to repeat the experiment of the learned professor of Padua. I proceed to give an account of it; the impartial public will judge, from the details, of the confidence it deserves,

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The species of snail upon which I operated, is of a middle size, and frequent in the fields, or in gardens, after a rainy day: then, numbers abandon their dark retreats, and in a short time one may collect hundreds. The shell of some is yellow, or yellowish: upon that of others are circular black or brown fasciæ.

It is not an easy matter to decapitate a snail. The moment it feels the instrument, it suddenly contracts into the shell. Thus, it will easily be seen, that it may happen we think it is decapitated, when only a portion of the integuments is taken away. To avoid being deceived, I have used more precautions than one. The snail is allowed to extend as much as possible; and an additional extension is procured, by immersing the animal in water. The instrument is presented to the origin of the head, several times before striking the blow; and I only esteem the operation completely performed, when the head is obtained entire, with the four horns well displayed, and the mouth, which may always be recognised by the lips marking the opening. The head, as it appears some instants after separation from the trunk, is represented a little magnified, Fig. 1. pl. 6. The two large horns, *g g*, are a little contracted. The small horns, *p p*, are entirely contracted within themselves. The mouth,

mouth, *b*, is closely shut. The lips are very evident.

A sharp-edged knife seems to me more fit for this operation than a scalpel. Scissars are still less convenient than a scalpel. I always have made the cut perpendicular to the axis of the trunk.

Immediately after the operation, the snail retires far within the shell, and in general does not appear again. At this time, a large portion of that viscous humour, of which it has so much, is diffused. If, at the same time, the severed head is viewed, some motion is perceived in the horns, chiefly in the large; but the motion very soon ceases, and I have in vain tried to renew it, by stimulating the head near the origin with the point of a scalpel. The four horns contract themselves to a certain degree, immediately after the operation; the small contracting more than the large.

By a very simple method, one may ascertain whether the operation is complete. This is, to immerse the decapitated snail in water. It is not long of leaving the shell: it extends as much as before the decollation; and then it is easily seen whether the trunk is entirely deprived of the head ^a. The anterior part of such

^a However, it may happen that it does not extend so much as one may wish, or as is necessary to judge of the progress

such a trunk, drawn from the life, is represented, fig. 2. ; and the profile, fig. 3. It may be remarked, that the flesh is strongly contracted, to close the enormous wound.

The viscous matter copiously diffused by the snail after decapitation, produces at the opening of the shell an operculum, which completely stops up the entry : it is very thin, and of a whitish colour. Frequently, two of those opercula are formed, one situated above the other. Sometimes there are three : the exterior is near the edge of the shell, the interior more or less within it.

The decapitated snail can reproduce several of the opercula ; but the viscous matter is by degrees exhausted, and the shell remains open, or nearly so. As the animal has no method of feeding while deprived of its head, it cannot continually repair the loss of this kind of varnish. It insensibly becomes emaciated, which is seen by the diminution of size, and a sort of transparency to be remarked in its interior. I have sometimes been astonished at the number of opercula that have successively

A a been
progress of the reproduction ; but it is only necessary to take the shell between the fingers, after the animal has been taken from the water, and it will soon extend as much as possible. Great care must be observed to avoid touching the snail ; because, upon the most gentle contact, it retires into the shell.

been reproduced by decapitated snails. All snails do not produce opercula ; but the number of those whose shells continue open is very small.

I have included my decapitated snails in boxes. Some remained at the bottom ; others reached the sides, against which they applied the opening of the shell ; others ascended higher, and attained the covering, where they fixed themselves in the same manner. These were apparently the most vigorous, or had suffered least from the operation.

When I wished to learn from week to week what was the real state of my decapitated snails, I had only to take away carefully the operculum or opercula stopping up the opening of the shells, and then immerse them in very limpid water. They are thus forced to proceed from the shells sooner or later. It has happened oftener than once, that they did not appear till several hours after immersion. This method, to which I have always had recourse to know the state of my snails, seems to me the best. They extend to the utmost limits in the water ; and the anterior part is then so completely exposed, that nothing can escape the eye of the observer. They endeavour to leave the water ; and gradually attain their purpose, if the depth is not too great. They crawl slowly over the bottom, and along the sides of

of the vessel, and advance until they reach a dry part. Then they fix themselves; and, to force them to appear again, it is necessary to re-immerge them in the water. Although absolutely deprived of a head, they advance forward as if they had one, only their progress is a little slower.

At first, I decapitated only a dozen of snails. This was 8 May 1777. I repeat, and I cannot too often repeat it, for I wish to anticipate the most trivial doubts, that I have esteemed the decapitation complete, only when I had upon my tablet the head entire, or attended by all its appendages. All the heads, in this manner severed from the trunk, have been ranged together upon one side of my tablet, and remain there at the moment I write this ^a.

I now proceed to give a sketch of the wonderful reproductions which were effected by the snails before my eyes. I will not enter into a minute detail: it is unnecessary: my purpose only is, to prove the reality of the reproductions, against the detractors of the famous discovery of my celebrated friend the Abbé Spallanzani.

The reproduction of the head of the snail does not observe so uniform a rule, as that of the head of those aquatic worms which I multiplied by sections 1741, and of which I published

^a July 14.

lished an account a few years after ^b. The reproductions of the snail present a number of varieties, which it would be tedious to describe. Some instances Signor Spallanzani has given in his *Programma*, and after him I have mentioned them in *La Palingenesie*, part 9. I refer to those two works. Here I should confine myself to my own observations.

A profile of the anterior part of a snail, decapitated 8 May, and delineated 21 June, is represented a little magnified, Fig. 4. The two large horns, *g g*, begin to extend: the left is further advanced than the right, the origin of which is just visible. A brown and rather blackish line, *t*, proceeds from the large right horn: this is the optic nerve and its muscle, the various motions and structure of which, Swammerdam has exposed to our admiration. A degree of transparency is seen in the flesh; and as it encreases greatly in the snails that have fasted a month or two, the optic nerve and muscle are also much more evident. A white line, *l*, runs along the back: I am yet ignorant whether it is a vessel. The anterior part of the same snail, viewed from before, is represented from the life, Fig. 5. Only the higher extremity of the large horns is seen, and at the extremity there appears a minute black point: this is the eye of the snail, in which,

^b *Traité d'Insectologie*, Paris 1745.

which, Swammerdam assures us, he has found the three humours of our eye, and two tunics, the uvea and arachnoid. Here the eye is already visible, although the horn only begins to grow. I have perceived it in horns that had made still less progress, as I shall immediately observe. The small horns do not yet appear. We know they have no eye at the extremity; neither are the new lips of the mouth, *b*, visible. This snail I shall design by the letter A.

The anterior part of another snail, drawn 23 June, is represented a little magnified, fig. 6. The reproduction is a degree advanced. One of the small horns, *p*, is very evident, and appears to be completely regenerated. The corresponding horn, has not begun to extend. Above the small horn, we perceive the origin of the large horns, *g g*, which has made but too little progress. Here is a striking example of the varieties to be seen in the regeneration of the head of the snail. One of the small horns has made the greatest advance, while the corresponding horn is not yet seen, and the large horns only begin to be observed. A profile of the same snail is seen, Fig. 7. The transparency allows the optic nerve, *t*, to be seen proceeding from the origin of one of the large horns: the eye of this horn is distinctly observed: the lips of the new mouth, *b*, are likewise visible. This snail I shall design by the

letter B. The anterior part, drawn 2 July, is represented a little magnified, Fig. 8. The plan is seen, Fig. 9. The mouth, *b*, cannot be mistaken. The large horns with their eyes, *g g*.

Another snail, which, upon 23 June, seemed to be completely repaired, shall be designed by C. The four horns were perfect, and had acquired the natural size of the horns of this species. The mouth appeared to be repaired: the opening complete, and the new lips very distinct, were of the figure and proportions they ought to be. In a word, this snail so much resembled other snails of the same species which had not been mutilated, that I could distinguish it only by the transparence and the diminution of size. It is represented, drawn from the life, Fig. 10. The section of the anterior part of the same snail is represented, Fig. 11. The new mouth and its lips are distinctly seen. Above it, at a little distance, there is seen, from the transparence of the flesh, an oblong spot, *t*: this is the teeth of the snail, which the lips can approach or recede from. These two figures were not drawn till towards the middle of Summer. After the 23 of June, I began to supply the snail with young vine and lettuce leaves; but it did not touch them. After traversing the leaves and the sides of the vessels for some time, it commonly fixed itself to the covering, and remained there
for

for whole weeks. Notwithstanding it fasted more than two months during Summer, it always seemed to be in good health, and is still well when I write this, 21 July.

I have said, the eyes appear, although the large horns only begin to repair. This I saw in one of my snails decapitated 8 May, the head of which, separated from the trunk, is represented, Fig. 1. The regeneration had made very little progress, 6 July: this day I caused it to be designed. Fig. 12, the plan of the anterior part. The origin of the large horns is seen; they do not yet begin to extend, and their place is indicated only by the eye, which is already perceptible: it appears like a black point, as small as it is possible to make with the finest pen. The snail has been designed at the moment when extended to the utmost; and I have taken the same precaution with respect to all the snails designed. In that of which I now speak, neither the small horns nor the mouth yet appear.

When the snail retracts the large horns within, the black point or eye is easily perceived through the flesh. Oftener than once I have discerned it with the naked eye, and even in those snails whose reproduction is very little advanced.

I should not neglect to observe, that, of the twelve snails decapitated 8 May, only one

A a 4 died.

died. All the rest seem to be well while I write this, 27 July; but the progress of reproduction is very various. In some, it seems to be but begun: in others, only the large horns are repaired, the origin of the small is not yet perceived, and the mouth is not well defined. The large horns of some of these are only half, or two thirds of a line long; while there are others, whose large horns are a line in length. Such are those of the snail, which I have hitherto designed by the letter A, the anterior part of which, as it appeared 21 June, is represented fig. 4.; and the anterior part of the same snail, as it appeared 26 July, is seen fig. 13. Something singular is presented by the large horns. They are thicker in proportion to their length, than those of un-mutilated snails. At the extremity, we remark a kind of deformity, which seems produced by a certain plication of the flesh, giving the horns the appearance of being monstrous. The eye, however, is very distinct. The colour of the horns tends to violet, and is generally so in reproduced horns. It tends greatly to that of the nerve running through the flesh. The anterior part of another snail is seen, fig. 14. The mouth is not perceptible, but under the appearance of a little prominence, *b*. It seems awry.

I have in general, with Signor Spallanzani, remarked various conspicuous irregularities in the reproduction of the double parts of the same snail. I see a large horn, of only half, or of two thirds the length of the corresponding horn : at other times, this one is scarcely visible. I likewise see a small horn completely regenerated, while its fellow is barely perceptible, or not at all. I see a mouth, one lip of which is only half reproduced, while the other seems completely repaired.

I confine myself to these few examples : they will suffice to give an idea of the varieties presented by the regenerating head of the snail. It seems to me, that they infer the reproduction of one part to be independent of the reproduction of another : for, how can we reject this deduction, when we see one horn completely reproduced, while the rest are invisible, or only begin to grow ? This fact will not fail to be considered as most important in the theory of those admirable reproductions. But I will not touch upon it now, as I have endeavoured to guess at it in part 10. of the *Palin-genesie*.

I had, upon 12 May, decapitated thirty snails, of the same species, treating them precisely in the same manner as the first : more than two thirds have perished. Those still alive, regenerate with various degrees of slowness,

ness, presenting the same, or varieties analogous to those already described.

I shall observe upon this occasion, that the months of May and June, and the beginning of July, have been very fresh and rainy. On some days of the first week of July, Reaumur's thermometer, at sunrise, stood at 4, 5, and 6° above freezing^a.

At present, I will not enlarge the discussion of my experiments upon the reproductions of snails farther: I intend to resume the subject in another memoir. I think enough has been said to prove, that nothing is more certain than this wonderful reproduction. I know not what to say of the fruitless attempts of some philosophers, and particularly those of Mess. Adanson, Cotte, and Bomare. Perhaps they have too soon declared the state of their experiments, or they have taken for an equivocal, what was a real reproduction; or perhaps they have thought to be dead those snails still alive. In this case much patience is requisite, and, above all, to despair of nothing. I do not speak of the diversities which the difference of species might occasion, in the consequences of the experiments made by those celebrated persons; for I have reason to think, that among the great number of snails upon which they have operated, there were some of the same species

as

^a About 41, 43, and 46° of Fahrenheit's thermometer.

as mine. Neither do I speak of the diversities that might arise from the difference of climate; for the climate of Paris is very little different from ours. I therefore entreat those able naturalists not to be discouraged, and to examine again a subject so pregnant with new truths; a subject which cannot be too deeply investigated. They possess far more intelligence, talents and ability than are necessary to succeed in experiments of this nature; and I may predict the most complete success, if they will not be discouraged, and if they will proceed in the manner I have done.

M. Adanson wrote me concerning his own experiments, 30 July 1769.

“ I begin to have a philosophic doubt concerning the regeneration of the head, the horns, and the jaws of snails. My experiments, diversified to infinity for more than a year upon between fourteen and fifteen hundred snails of different species, have convinced me that my doubt has foundation. I have, as every one has had, reproductions, even very sudden ones, of horns, heads, lips, and other parts, but those were reproductions of parts which had not been completely cut away: for all the heads, (I say the real heads), all the horns, all the jaws, and the other parts that have been completely cut away, and only a quarter of a line from
“ the

“ the origin, never have exhibited any kind of
 “ reproduction, far less a regeneration. Let us
 “ be strict, and seek for reality. All those
 “ who have mutilated snails, and first Signor
 “ Spallanzani, seem to me to have been de-
 “ ceived : they have thought they had decol-
 “ lated the head, when the cap only has been
 “ cut off : they have believed they have cut
 “ away, or eradicated, the horns, or the jaws,
 “ while the origin of them always remained ;
 “ whence it is not wonderful there were re-
 “ productions. These, you will candidly ad-
 “ mit, are not reproductions, or rather rege-
 “ nerations, such as you, Mess. Trembley, and
 “ Reaumur, had seen in fresh-water worms,
 “ the polypus, and in the claws of crabs - - - -
 “ How many well credited operations have
 “ deceived persons, less familiarised than we
 “ are, with similar operations, and the anat-
 “ omy of shelled animals ! They have imagined
 “ they had cut off, completely below the ori-
 “ gin, so many heads, horns, and mouths,
 “ which they have, in every journal and peri-
 “ odical paper, so liberally regenerated ! I
 “ know well, how deficient we are in the
 “ greater part of nice experiments. Notwith-
 “ standing my great experience, I may almost
 “ presume to say dexterity, in the anatomy of
 “ the smallest animals, I always distrust my-
 “ self. For this reason, I have an hundred
 “ and

“ and an hundred times repeated the same ex-
 “ periments, before hazarding the results to
 “ the public. I have laboured the first, or a-
 “ mong the first, to corroborate all the expe-
 “ riments of Signor Spallanzani, and to make
 “ additions to what might have escaped him.
 “ I have operated upon a greater number of
 “ animals, and diversified my experiments
 “ more than any other person, to judge by
 “ all that has been read before the academy
 “ or printed, and I am the only one who has
 “ read nothing upon the subject which I in-
 “ vestigate with the greatest assiduity - - - - It
 “ is nearly the same with the reparation of the
 “ parts of newts, several species of frogs,
 “ toads, tadpoles, &c. I have seen sensible
 “ reproductions to the tails and feet partially
 “ cut away ; but no regeneration, when those
 “ parts were cut off close at the origin. Consi-
 “ der well my expressions, *regeneration* and
 “ *origin*, upon which your principles rest so
 “ much, and there is no actual regeneration :
 “ and I hope you will do the justice to my
 “ doubts, as to acknowledge with me, that
 “ Signor Spallanzani and his followers have
 “ too far extended their expressions of rege-
 “ nerations, which were only reproductions of
 “ portions of parts ” - - - - -

To the numerous experiments and doubts
 of my celebrated correspondent, I shall oppose
 only

only the letter written to me by the Abbé Spallanzani, cited at the beginning of this treatise, wherein he details the precautions he used, to avoid error. I sent a copy of this letter to M. Adanson; but it did not produce upon his mind the effect I expected; and he still persisted in his doubts, when he wrote me, 20 July 1775—"The various parts cut or taken away, not only from different species of snails, but also from several other aquatic animals, as frogs, toads, newts, have produced no organized reproduction to me, as the part cut did according to Signor Spallanzani. I have so much diversified the experiments which my friend Mr Needham, and some other observers of this rank, have witnessed, that we all esteem it certain, that whenever the operation has been complete, the reproduction is but a stump, that is, a mass of flesh unorganized, or differently organized: and, Signor Spallanzani should know, that the observations of our most celebrated anatomists have proved, that the reproduction of the tails of lizards, so common, although externally well formed, present no regular ossification as the rest, nor any vertebræ in the interior" - - - -

M. Adanson is, as we see, one of those philosophers who start difficulties upon facts, and who wish themselves to see prodigies again
and

and again, before admitting them to be true. I cannot blame such a degree of reserve; but I confess, that in this case it appears to be extreme, especially after evidence so strictly demonstrative as the Abbé Spallanzani has given of his discovery. May I therefore hope, that the experiments I now publish, will triumph over the incredulity of our learned academicians? Doubtless he will not suspect that I have only deprived the snails of the *cap*, to use his expression; for the head, so complete and so perfectly separated from the trunk, represented Fig. 1, will not permit the remainder of the least suspicion. I request M. Adanson to estimate all the details of my experiments, and to attend to the designs of my able artist, which represent so admirably well the regenerations which I have witnessed. I could with facility have extended them further; but I do not consider it as essential to the purpose I have in view. If it should be objected, that the snail, drawn from the life, Fig. 10. and 11, had not then touched the young vine and lettuce leaves with which it was supplied, I may answer, that it has given the most indisputable evidence of being provided with very good teeth: 27 July, it begun to eat the paper covering over the mouth of the vessel where it was confined, and voided some well formed excrements, the colour and consistence of which,

which, exactly like paper, indicated that they were the remains of it.

M. Adanson also doubted the reproduction of the limbs of the newt, so perfectly ascertained by the numerous experiments of Signor Spallanzani; the principal results of which, he mentioned in his interesting *Programma*, published 1768. In his letter of 20 July 1775, above transcribed, M. Adanson says, "When-
 " ever the operation upon the newt has been
 " complete, there has only appeared the stump
 " of a reproduction, that is, a mass of flesh
 " unorganized, or differently organized:" and he cites the testimony of Mr Needham, and some other observers. But, what will M. Adanson himself say, when I inform him, that this pretended stump, or imagined lump of unorganized flesh, is the member itself, perfectly formed, concealed under those deceitful appearances, and which has been completely developed before my eyes, as I had formerly seen the developement of the heads and tails of those aquatic worms I multiplied by cutting in pieces? I have actually in my cabinet, newts completely repaired, of which I shall publish a history in a future memoir, accompanied by excellent drawings. Our celebrated academician has therefore been precipitate in his opinion, when he thought he only suspended it. He has decided, that the newt reproduces

duces but a stump, while this stump was the member itself, where there was nothing essential defective, and had only to acquire the size of that which it replaces.

M. Adanson was thus deceived concerning newts, as concerning snails; and the mistakes of such a naturalist, are a good lesson to those who have neither his knowledge nor his ability. I am persuaded that he will acknowledge his error; for I know him to be a sincere friend to truth, and I have not to fear that he will reproach me for having laid it open in this little treatise.

M. De Bomare, no less a friend to truth, and whose experiments had been as unsuccessful as those of M. Adanson, was, in consequence, equally incredulous. I had referred him to the same letter of the Italian observer, printed in the *Avant Coureur*, 30 October 1769; and he wrote to me 5 November 1775—" You
 " ask me, why I have not answered one of the
 " articles of a former letter, concerning the
 " reproduction of the head of the snail. I as-
 " sure you, that all the experiments I have at-
 " tempted upon this subject, seem adverse to
 " those of the Abbé Spallanzani. You will
 " see at the article *Limaçon* of my Dictionary,
 " edit. 1776, what I have said upon this sub-
 " ject, which I before mentioned in 1768."

I shall transcribe from the *Dictionnaire d'Histoire*

Naturelle, the passage to which I am referred by M. de Bomare.

“ I acknowledge that, not being able to credit the reproduction; while at the Chateau de Chantilly during Autumn 1768, I made many experiments upon the subject, which I have communicated to the public: the result follows. Of fifty-two land snails, whose heads I cut off, (all, whenever they felt the cutting edge of the instrument, suddenly and very powerfully contracted themselves: the section finished, the part retiring precipitately into the shell, appeared wrinkled and contracted, like the extremity of the rectum of a hen), nine were in motion for twenty-four hours, and only those where an imperfect cut was made upon the neck, between the large horns and the organs of generation, the edge of the knife being so blunt, that I had evidently seen all the horns retract and retire into the interior of the animal. I remarked, that in this way I had only cut away the skin and the jaw of the snails, so that in ten or twelve days they proceeded from the shells, crawling about with mutilated horns. The snails from which I had cut only the diagonal half of the head, displayed only two horns; but those which I had suddenly decapitated entirely (which were by far the most numerous) all died in a few
“ days,

“ days, excepting two which remained alive
 “ five months fixed to a wall, and died in
 “ Spring, without any indication of the repro-
 “ duction of a head. I have taken other snails,
 “ and made a longitudinal incision in the head,
 “ between the four horns. Nature employed
 “ more than a month in reuniting the two
 “ parts, the animals appearing very languid.
 “ I repeated those experiments 1769, and all
 “ were unsuccessful. Many persons have writ-
 “ ten to me from different countries, that
 “ their attempts have been precisely similar to
 “ mine.”

It is singular, that I have succeeded with on-
 ly a dozen of snails, while M. de Bomare has
 failed with more than fifty, and M. Adan-
 son with more than fourteen hundred. But it
 is possible that those gentlemen were too anxi-
 ous to believe their experiments failed, or they
 did not pay sufficient attention to the progress
 of the regeneration, always more or less tardy,
 more or less disguised, and of consequence more
 or less difficult to recognise.

I have named another valuable naturalist,
 who has not been more fortunate than Mess.
 Adanson and De Bomare : I speak of F. Cotte,
 curate of Montmorency. He has narrated his
 unfruitful attempts in a letter to the Abbé Ro-
 zier, published in the *Journal de Physique*, May
 1774. He denominates the reproductions of

the heads of snails, *imaginary*. He says, that from 1768 until 1774, he decapitated a great number of snails : that almost the whole died soon after the operation, which had been performed with a sharp knife, not by drawing, but by a single blow : and he concludes with three consequences, which he assures us were attendant upon all his experiments and observations. 1. He has observed that snails have the property of contracting themselves very suddenly, to protect the head from the instrument, in such a manner as to escape with the loss of only part of the horns, or, at most, the skin of the head. 2. When it happens that the head is actually cut off, it is not reproduced ; at least, he declares that he has never seen reproductions, not even of the parts of horns cut away. 3. Snails can live a very long time without eating, and without the head.

I know not whether Mess. Adanson, de Bomare, and Cotte, have continued their experiments, or what has been the consequence. But I can say, that I am not the only naturalist who has succeeded in corroborating the discovery of Signor Spallanzani : that has been already done by the celebrated Signora Bassi of Bologna, by Mess. Lavoisier and Schæffer. M. Senebier, pastor and librarian of our Republic, who has given public testimony of his skill in philosophy and natural history, has had
the

the same success with myself in experiments upon snails. I would here transcribe what he has written to me concerning them, did he not inform me that he has sent his observations to the Abbé Rozier, to be published in his *Journal*.

Although the head of the snail is a very complicated little machine, as I have elsewhere shown, by the structure ^a; it is indubitable, that the gelatinous quality of the flesh greatly promotes its wonderful reproduction. I have enlarged upon this remark, when treating of the polypus. However, I do not mean that one should infer that all gelatinous animals, and all animals in their first state of jelly, may reproduce themselves, or repair the loss of their members, as the polypus and the snail. Experiment alone can discover the limits of this admirable property: and, what we have already learned concerning the extent of its dominion, should excite naturalists to diversify to the utmost their attempts upon a subject so fertile in wonders. I cannot too much exhort them to despair of nothing, not even in the most uncommon experiments.

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^a *Palingenésie Philosophique*, part. 9. New edit. of *La Contemplation de la Nature*, part. 3. chap. 21. note 4. 5.

MEMOIR II.

HAVING myself beheld some of the wonders which the regeneration of the head of the snail presents, I wished to know what M. Adanson thought of the results of my experiments ; and I requested him to read the preceding treatise which I had published in the *Journal de Physique*, September 1777. He did so, and wrote me 10 January 1778.

“ I have read your excellent memoir upon
 “ the reproduction of the head of the snail,
 “ with all the attention it deserves ; and the
 “ perusal only tends to confirm me more and
 “ more, of what a course of experiments, con-
 “ tinued nearly ten years, since 1768 until
 “ the present day, upon several thousand
 “ snails, has informed me, namely, that the
 “ integral parts of the snail we speak of, whe-
 “ ther the head entire, the eye, or the ocu-
 “ lated horn, the upper jaw radically extir-
 “ pated or cut below the origin, are not re-
 “ produced, either under the same form, or
 “ with the same organization as before. You
 “ have decollated two or three dozens of snails,
 “ 8 and 12 May 1777. I believe it. You
 “ cut

“ cut them as I did myself, two lines below
 “ the origin ; that is, towards the opening of
 “ the parts of generation. You have, as I
 “ have done, taken from each severed head,
 “ the upper jaw entire, and the oculated horn
 “ entire ; and then, at the end of two or three
 “ months, in June and July, you have seen
 “ a third of the snails reproduce themselves,
 “ even eleven of the twelve first decapitated,
 “ a complete head, with the eye-horns, the
 “ upper jaw formed like a horse shoe, with
 “ the ferrated teeth. You must permit me
 “ still to retain my philosophic doubt con-
 “ cerning the three last assertions, until you
 “ have repeated the following experiments up-
 “ on which it is founded ; experiments which
 “ have served to confirm the accuracy of my
 “ operations, and certain proofs that my snails
 “ were completely, and not apparently, decapi-
 “ tated. To have the same certainty, take
 “ the greatest number of snails you please,
 “ (that is, hundreds, to provide against the
 “ great mortality that will follow), not of
 “ the small species, called the Lacquey, which
 “ you have made use of, and which is most
 “ deceiving, from its great lubricity and agili-
 “ ty in evading the knife, an agility propor-
 “ tioned to its smallness, which must have de-
 “ ceived you ; at least, that happened to me
 “ in my first attempts, which obliged me to

“ relinquish it : take, I say, the large yellow-
 “ ish snail of the vine, named Pomatia, or ra-
 “ ther the brown garden snail, called by us
 “ the Gardener, which is almost as large, and
 “ the most common of all : after having kept
 “ the whole, immersed one or two days more
 “ or less in water, under a press, to diminish
 “ their vivacity and lubricity, tear away the
 “ upper jaw, which is formed like a horse
 “ shoe, and edged with five or six teeth, the
 “ lower palate, which is a membrane dentated
 “ like the tongue of a cat or a file, and era-
 “ dicate the two large oculated horns, using
 “ for these last little pincers, with thread or
 “ flax, to take away any edge, and preserve
 “ them from slipping, or indeed pressing the
 “ neck of the animal with two fingers, to era-
 “ dicate the jaws : avail yourself of this forced
 “ situation, to cut away, with a fine and sharp
 “ botanical scalpel, the two oculated horns,
 “ with the bulb below the eyes ; or only one
 “ of the horns, to have an object for compari-
 “ son : cut the head entirely off others, ob-
 “ serving whether the severed heads have the
 “ jaws and eyes complete : preserve the heads,
 “ jaws, and eyes, to be certain that you have
 “ as many as snails operated upon. The snails,
 “ thus deprived of teeth, eyes, or heads, will
 “ for the most part live six months, even one
 “ or two years, without eating ; they gradual-
 “ ly

“ ly become emaciated unto perfect extinction:
 “ if, during this time, they recover new eyes,
 “ new jaws, a new head, which I have never
 “ had the good fortune to see in those identi-
 “ fied after the operation : if this experiment,
 “ made with all the precautions I have taken,
 “ and which I believe it necessary to use, suc-
 “ ceeds in your hands, and in those of the
 “ Abbé Spallanzani, I shall esteem it a fact,
 “ that the parts entirely separated are repro-
 “ duced in those animals. But take care,
 “ would you desire to make those contradictory
 “ experiments, without which you cannot be
 “ certain of the real reproduction of a jaw, an
 “ eye, or a head? I abridge the subject, because
 “ the consequences deducible are in my letter
 “ 30 July 1769, which I thank you for having
 “ reminded me of in your treatise - - - -

“ I pass to newts. I have not yet been able
 “ to procure the perusal of your memoir : but,
 “ excepting the tail, to judge by the observa-
 “ tions I have made upon that of lizards, it
 “ does not seem to be capable of reproducing
 “ osseous vertebræ ; and although I have
 “ only had reproductions of stumps of the
 “ feet cut off several species of those animals
 “ and of frogs, because I have not been able
 “ to prosecute my experiments with so much
 “ conveniency, or so long, upon them as up-
 “ on snails, I very firmly believe the possibili-
 “ ty

“ ty of the reproduction of the fingers and of
“ their bones, whenever the anterior or poste-
“ rior part of the arm is not cut off.”

I want words to express all the surprise excited in me, by this letter of my learned correspondent ; and I doubt not, that the reader will partake it along with me. What M. Adanson desires me to do, is precisely that which would occasion the failure of the experiment : for how could one tear out, or eradicate with pincers, the different parts of a snail, without causing the greatest disorder within ? How, in this way, is it possible to succeed in eradicating all the parts ? And, supposing it to be possible, would not we endanger the sources of reparation ? Is it not sufficient, that I am certain, by the most attentive examination of the heads I have cut from my snails, that they contain all the parts that characterise a head, such as the four horns, the mouth, the jaws, &c. ? Was it necessary to cut out the large horns with a botanic scalpel, in order to be sure that the snail would produce new ones ? Was it not enough that I had seen, and many times seen again, the origin and progress of the new oculated horns, that I had seen the new eye and the optic nerve appear the first in this wonderful reproduction ? It is improper here to encrease, as M. Adanson has done, the agility with which the snail retracts its head the moment

ment it is touched by the instrument ; for the agility is not to such a degree as to prevent one, with a little address, from performing complete decapitation. I can even say with truth, that I have very seldom failed to effect it, at least when I have taken the precautions mentioned in my memoir.

M. Adanson seems to reproach me with having employed snails of too small a size : he says, the small species I have used must have deceived me, from their great agility in eluding the edge of the knife. Nevertheless I can affirm, that I have operated upon the small species with as much facility as upon the middle-sized, and even upon the largest. But, we exaggerate the quickness of snails in saving their head from the instrument ; for, an abstinence several days, and the water in which they are immersed, weaken them more or less, and to a certain degree diminish the celerity of their motions. Besides, if I made use of small snails, it was only because I reasonably presumed, that the wonderful reproduction I wished to behold would be performed more easily, or in shorter time, than in the largest snails. I have not neglected also to operate upon those of the largest species, and I shall relate the effect.

To terminate the answer to the objections of our celebrated Pyrrhonian, I shall here sub-
join

join an extract from a letter I wrote to him,
21 January 1778.

“ If it was in my power, my dear and illustrious friend, I would make the experiments you desire upon snails. But in truth, I do not think that any thing can be done more strictly demonstrative, than what has been so well executed by my friend Signor Spallanzani, and what he narrated at length in the letter of 11 September 1769, a copy of which I sent you, but upon the subject you have never replied a single word.

“ What is required? To ascertain whether the head is completely cut off, and whether the head reproduced has all the organs of the natural head. What must be done, to ascertain these two facts? It is necessary to dissect, with care, the head cut off; to examine the interior with attention; and to be convinced that the head contains all the organs belonging to it: then, it is necessary to dissect the reproduced head with the same care; to be sure, by an exact examination of the interior, whether it actually contains all the parts belonging to the head of the snail. This has been frequently done by Signor Spallanzani: and I now ask you, whether there is any foundation, in strict rhetoric, for doubting an experiment made with similar precautions? Yet you write me, 30 July
“ 1769,

“ 1769, that the Abbé Spallanzani must be
 “ deceived ; that he thought the whole head
 “ was cut off, when the cap only was taken a-
 “ way. You persist in the same assertion, 20
 “ July 1775. Certainly you have not attend-
 “ ed to the letter of the Reggian observer, to
 “ which I referred you. Permit me to refer
 “ you to it again.

“ I have set apart all the severed heads : I
 “ have observed the whole with attention : I
 “ have seen the two large horns with their eyes,
 “ the small horns, the mouth, the lips, &c. I
 “ have then seen new horns protruded ; I have
 “ seen the eyes of the horns, the optic nerve of
 “ the eyes : I have seen a new mouth, new lips,
 “ and new teeth appear in the snails, in the
 “ same snails whose former heads I had set a-
 “ part : I have seen snails that gnawed with
 “ their new teeth the covering of a vessel, and
 “ I have seen them void excrements, in which
 “ was found the matter they had consumed.
 “ What more would you desire, my worthy
 “ friend ? And how can you write me, after so
 “ many united proofs, that you still retain your
 “ philosophic doubt ? Can such a doubt, ex-
 “ tended so far, and challenging the most ac-
 “ curate, reiterated, and demonstrative experi-
 “ ments, be termed *truly philosophical* ?

“ Consider, that the Abbé Spallanzani and
 “ I, are not the only observers who have our-
 “ selves

“ selves beheld the wonders seen in the repro-
 “ duction of the snail. The celebrated Signora
 “ Baffi, Mess. Lavoisier, Schæffer, Muller, &c.
 “ have seen and described them also. Are you
 “ willing to think that all those observers have
 “ been imposed upon, they who have given such
 “ ample evidence of their ability and accuracy?

“ With respect to the reproductions of newts,
 “ you tell me, you firmly believe the possibility
 “ of the reproduction of the fingers and their
 “ bones, so long as the anterior or posterior
 “ part of the arm is not cut away. I am sorry
 “ that you should have written this, before per-
 “ using my memoir upon the *Reproductions of*
 “ *Newts*. There you would have seen, that I
 “ have cut away the fingers, the hands, the
 “ fore-arm, whole arms, feet, legs, and thighs,
 “ complete; and that all those members were
 “ perfectly reproduced by the animal. This
 “ would have induced you to bestow more con-
 “ fidence in the beautiful discoveries of Abbé
 “ Spallanzani upon snails and newts. My me-
 “ moir was printed in Rozier's *Journal*, for last
 “ November; and I wonder that you, who re-
 “ side in the same place where it is printed,
 “ have not procured it. The figures added to
 “ the memoir are very accurate, but the designs
 “ were superior to the engravings. It will
 “ present you with facts which I hope in a short
 “ time

“time you will not oppose. See, therefore, and
“believe.”

The letter written to me by Signor Spallanzani upon the mode of operating, and to which M. Adanson had not paid the attention it deserved, is so well adapted to convince naturalists of the reality of the ingenious discovery of which it treats, that I cannot dispense with transcribing it here, as the best refutation that may be opposed to the detractors of this discovery, and as a model of the method that one ought to pursue in researches of this nature.

“Modena, 11 September 1769.

“I thank you, Monsieur, for the intelligence you have given me concerning snails.
“Considering the different results of naturalists, and particularly of French naturalists. I
“am of opinion, that beside the little dexterity in the art of experiment, the diversity of
“the species of the snails upon which they
“have tried to repeat my experiments, has in
“a considerable degree occasioned a difference in the result of their experiments. I
“am certain, that all the snails of Modena
“reproduce more or less; but I do not warrant the reproduction of foreign snails: some
“among them perhaps do not possess this resource. Upon this point you will see greater detail in the preface I shall prefix to my
“Italian

“ Italian translation of your *Contemplation de la*
“ *Nature*, which will appear this year. It is
“ very probable, that the snails which have ex-
“ ercised the industry of the learned anony-
“ mous Frenchman of whom you speak, are of
“ the number of those where the property of
“ reproduction does not reside in the highest
“ degree. I might say the same of the snails
“ upon which M. de Bomare and F. Cotte
“ have operated. But does it thence follow,
“ that I am deceived? To say I am, would be
“ a rash opinion, to call it no more. If any
“ one attempts to confute me, I will try to de-
“ fend myself; and my very circumstantial de-
“ tails, and those of my friends, will prove
“ that I am not deceived.

“ You obligingly ask, whether the severed
“ head truly contained all the organs pertain-
“ ing to the head of a snail? To answer this
“ important question, I will mention what I
“ did in making the experiment. When I
“ saw that snails enjoyed the prerogative of
“ reproducing themselves, I began to dissect
“ them, on purpose to obtain a perfect know-
“ ledge of the anatomy. I wished to make
“ myself master of all the organs of which
“ the head was composed. M. Lyonet’s ana-
“ tomical work has always been my model;
“ and I was provided with all his apparatus.
“ I killed in water the snail I proposed to dis-
“ sect:

“fect : it then proceeds from the shell, the
 “four horns are displayed, and it dies in this
 “position, which is the most favourable for
 “dissection. It is by this trivial expedient
 “that I have been able to convince myself,
 “that the head cut off truly contained all the
 “parts Swammerdam has described in his treatise upon snails.

“It was only after having studied well the
 “structure of the head, and having ascertained
 “the situation of each part, that I begun
 “to mutilate the animal ; and I proceeded in
 “this manner :—Before cutting off the head
 “of the snail, I waited until it had come out
 “of the shell, and the horns were completely
 “displayed : then the operation succeeded
 “wonderfully well : and it often happens
 “that the severed head will keep the horns
 “nearly as much extended as before the operation, only they sink down and appear
 “feeble : the head itself contracts or concentrates very much. I have soaked it in water,
 “and in some hours it dilates, and becomes
 “soft ; in which state it is easy for me to
 “anatomize it.

“I begun the dissection close to the cut ;
 “and after dividing the integuments, I had the
 “pleasure to observe very distinctly the separation or distribution of ten nerves, proceeding to the eyes and the other parts of

“ the head : I observed as distinctly the distribution of the nerves of the œsophagus, and the muscles serving for the different motions of the head. Sometimes, instead of the nerves, I examined the whole or part of the brain of the severed head.

“ Continuing the dissection, and extending it farther, it was easy for me to trace the different parts, even to the places where they were inserted in the head. Without the least difficulty I found the throat of the animal, its tongue, lips, mouth, teeth, the four horns, with their nerves, muscles, and other appendages. If I chose, I could retract the horns into the head : I had but to pull the ends of the divided muscles.

“ These are, my dear correspondent, the most remarkable parts that presented themselves to my view in the severed head. I say the most remarkable, for I discovered many other less important, of which I shall speak in my large work.

“ I now ask you, If this assemblage of all the parts composing the severed head ; if this assemblage, which I have seen and reviewed an hundred times, is simply the integument of the head, or part of this integument, as the French observers, whose opinions or experiments you have communicated to me, imagine : is it not the most satisfactory

“ tisfactory evidence, that I have been under
 “ no illusion, and that this severed head con-
 “ tained complete all the organs which com-
 “ pose it ?

“ I practised nearly the same method with
 “ regard to the head reproduced. It would be
 “ superfluous to give you a detail of the or-
 “ gans composing it. I could only repeat,
 “ word for word, what I have just said of those
 “ we discover in the original head cut off. It
 “ is true, that, oftener than once, this decol-
 “ lation gives place to various monstrosities
 “ of the parts reproduced : but this does not
 “ affect the essence of my discovery.

“ I have taken care to measure the severed
 “ head, and to compare it with that repro-
 “ duced. I have taken many other precau-
 “ tions, which I now pass in silence, but shall
 “ mention them at large in my work.

“ I may flatter myself, that my treatise up-
 “ on the reproductions of snails, will be so
 “ rich in experiments, and that those experi-
 “ ments will be so accurately described, and
 “ so well detailed, as to convince the most ob-
 “ stinate infidels.”

The reader may now judge, if I had any
 foundation for reproaching the excess of M.
 Adanson's pyrrhonism concerning the discovery
 upon snails. It is in truth most singular, that
 he persists in his doubts, after the perusal of a
 letter

letter so strictly demonstrative as that I have just transcribed. How many physical facts are admitted by philosophers, and by M. Adanson himself, which are not better attested than that of which we speak! Shall I say more? M. Adanson still retained his doubts, 9 October 1779, as I learned from himself, during a visit he then paid me, after a journey made for the recovery of his health. At that time, I had not snails in complete reproduction; but I had the satisfaction of convincing him, by the testimony of his own eyes, of the reality of the wonders presented by the reproduction of the members of the water newt. I shewed him newts in various stages of reproduction. I shewed him arms, hands, thighs, legs, feet, perfectly well formed. He yielded to so many accumulated proofs, and was convinced that what he had erroneously supposed simple stumps, were in fact real members, which would be completely regenerated.

I return at present to the experiments I have made upon the reproductions of the head of snails. Those whose progress I mentioned in the first part of my treatise, died before finishing the reparation of the head. They became much emaciated, and assumed a transparency that is unnatural to snails in the country. One, whose anterior part is represented fig. 12, reproduced only one of the large horns, about a
line

line in length, but much thicker than the large horns are when they begin to extend. This singular horn, which seemed to be formed like a sort of spindle, had two eyes, *yy*, very distinct, and each eye had its optic nerve. Fig. 15. represents the whole magnified, where, slightly shaded, the part reproduced is indicated, which is always of a clearer colour than the original flesh. The horn being closely examined, it was easy to discover that it was formed by the union of two horns, which were ingrafted in each other, as in a manner by uniting. There was no trace of a mouth, nor of the small horns, perceptible in this snail: how then can a complete decapitation be doubted?

I resumed those experiments upon different species of snails, Spring 1778. Their various reproductions presented varieties similar or analogous to those I had observed in the snails decapitated the preceding year. There was among those varieties, one like that of which I have just spoken. Two eyes at the extremity of the left horn, were very distinctly seen in a snail that had begun to reproduce the mouth and the two large horns, fig. 16. The anterior part of the snail is here represented larger than life.

I continued to attend my snails during the whole course of the year 1778. The progress
of

of regeneration was as usual very unequal, and none repaired the whole head.

I decapitated twenty-four snails of the same species 26 May 1780, confining them in vessels after the operation. Several reached the summit of the vessel, and attached their shells to the sides, or to the paper covering over the mouth. The greater part shut up the shell with a very thin operculum, at different degrees of depth within.

Having immersed some of the decapitated snails in water, 9 September 1780, that I might judge of the state of the reproduction, there were two among them that presented a remarkable monstrosity. In one, I observed only one large horn, very like that of fig. 15, and evidently appeared to be formed as if by the union of two horns. At the extremity, were two small shining black eyes, each provided with an optic nerve perfectly visible through the transparent flesh. This monstrous horn, which was about a line long, appeared thicker in proportion than that of fig. 15. It greatly resembled a spindle, being cut even at the extremity, and being all nearly of the same thickness. But it was different from the other, by a more important distinction. On the left side was seen, a little under the eye, a very minute tubercle, which seemed to be a second horn growing out of the large horn. In this snail,

snail, I sought in vain for the parts constituting the mouth. I could discover no trace of it: nor was there the least vestige of the two small horns.

The second snail presented a monstrosity of another kind: only one large horn had protruded. At the extremity I thought I could perceive three black eyes, but so close to each other, that they seemed confounded together. Fig. 17. 18. the figures are magnified. Upon the superior part of the horn were seen, very distinctly, three parallel optic nerves, only one of which proceeded to the three minute eyes. Under this horn, and at a little distance from the origin, a very small one is discovered, which seems only beginning to expand. In this snail, as in the former, no mark of the parts forming the mouth were visible.

I observed again, both with the naked eye and a magnifier, the two snails, 25 October. They had made a very sensible progress. Upon this day, only eight of my snails remained alive: all, of that species whose shell is yellow or yellowish. The other sixteen had perished; some sooner, some later.

I immersed in water the six snails whose reproduction I had not yet examined. The greater part had made but little progress, and shewed only the origin of a single horn. One alone had protruded two eye-horns, at least a
line

line and a third in length ; the optic nerves so large or so visible, that they seemed to darken the greatest part of the horns ; but the small horns, the lips, or the other parts of the mouth, did not yet appear. This snail persisted in remaining in its shell, although immersed in water more than two hours. Suspecting it to be dead, I had taken it out, and, only after the lapse of several hours, it agreeably surprized me, by proceeding from its shell of its own accord, and displaying to my eyes its new productions.

In another Memoir, I will give the history of my eight snails.

After making experiments upon snails of the smallest species, it was proper to make them also upon those of the largest species. This I began to put in execution 24 May 1780, upon twelve of the largest snails of our country. We may judge of their size, compared with that of the snails which have been the subject of this and the preceding Memoir, when I say the diameter of the opening of the shell of those was at least nineteen lines, and that of the shell of these but four or five.

In some weeks, the half of my snails died, and exhaled an excessive foetid odour. Upon 13 August, I immersed the six surviving snails. They proceeded from the shells, and I saw that the large wound was perfectly cicatrized, but could perceive no indication of reproduction.

Towards

Towards the middle of October, other two snails died. Upon the 18, I immerfed the four remaining snails in water, where they were more than three hours without appearing. I fcratched with my nail the laft volute of the fpiral of the fhell in vain : all my attempts were fruitless, and the snails obftinately concealed the anterior part : they were then taken from the water, and fhut up in their vef-fel. I will ingenuoufly confefs, that I had little hope that the snails had made new productions. What then was my furprife, when they next morning proceeded from their fhells of their own accord, and exhibited unequivocal evidence of reproduction, and even of reproduction pretty far advanced ! One, which begun to repair the head, had two large horns about a line in length, fig. 19. The left horn, *c*, was thicker than the other, had two diftinct and black eyes at the extremity. The right horn, on the contrary, appeared to have none. I could as yet difcover no indication of the regeneration of the fmall horns, and of the mouth.

Another snail, fig. 20, had likewise begun to reproduce two large horns, nearly of the fame fize as thofe of the preceding : but the right horn appeared monftrous ; it feemed to terminate by three fmall foft points, which are drawn from the life.

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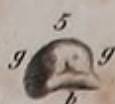
The reproduction of the head of a third snail was announced by four or five very short protuberances, where the large left horn only could be distinguished, presenting three black points, or three very minute eyes. The whole is magnified, fig. 21. The three eyes, *a*, *b*, *c* : two, instead of being situated at the extremity of the horn, are situated upon the side: the eye, *a*, is more apparent than the eye, *b*; the third, *c*, is a little below the other two. It was impossible for me to see the optic nerves through the flesh: the reproduction had still made but little progress. Under the horn are seen two protuberances, *p*, the nature of which cannot yet be known.

I shall continue my observations upon those large snails, and give the sequel in another Memoir.

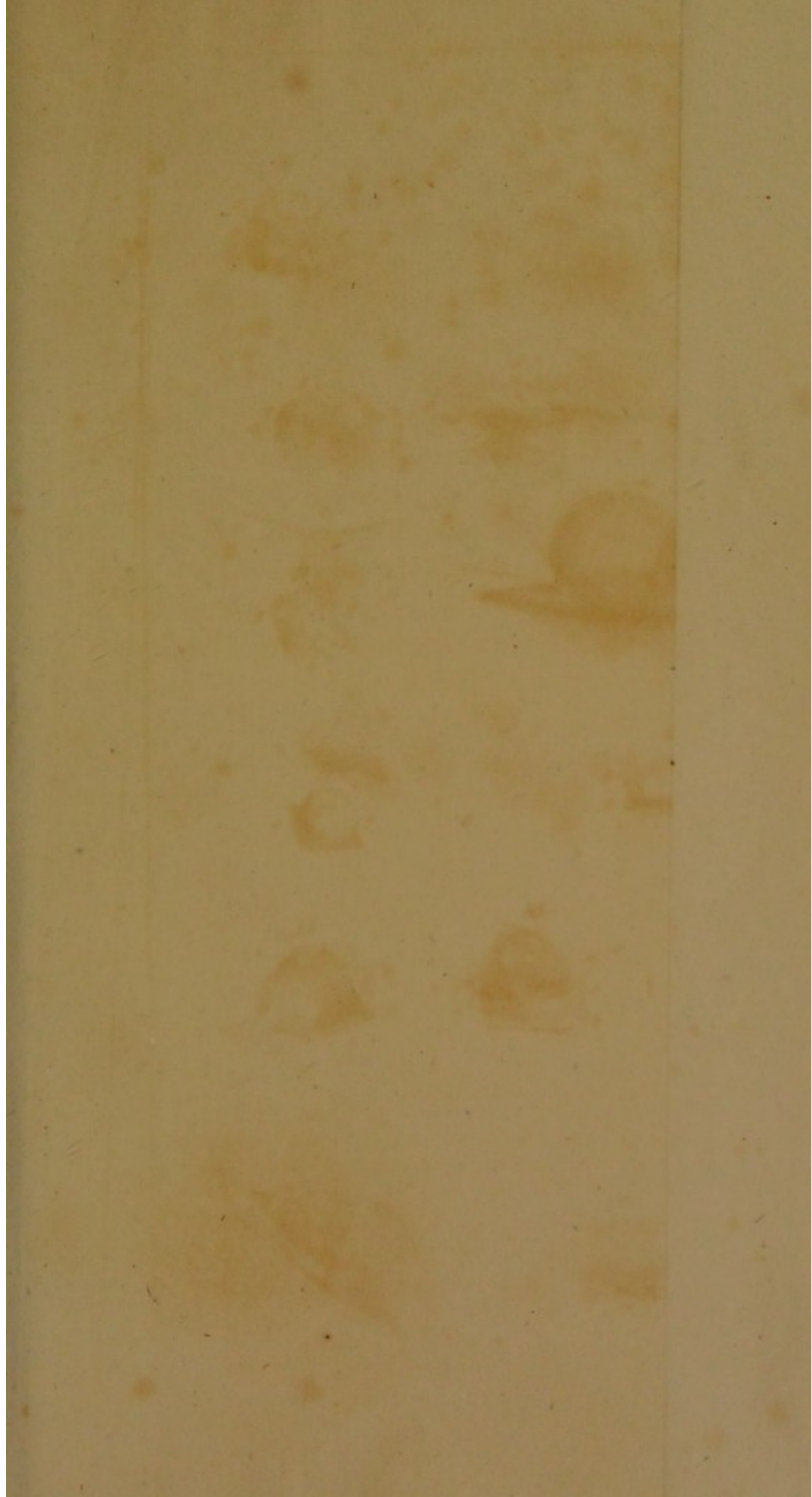
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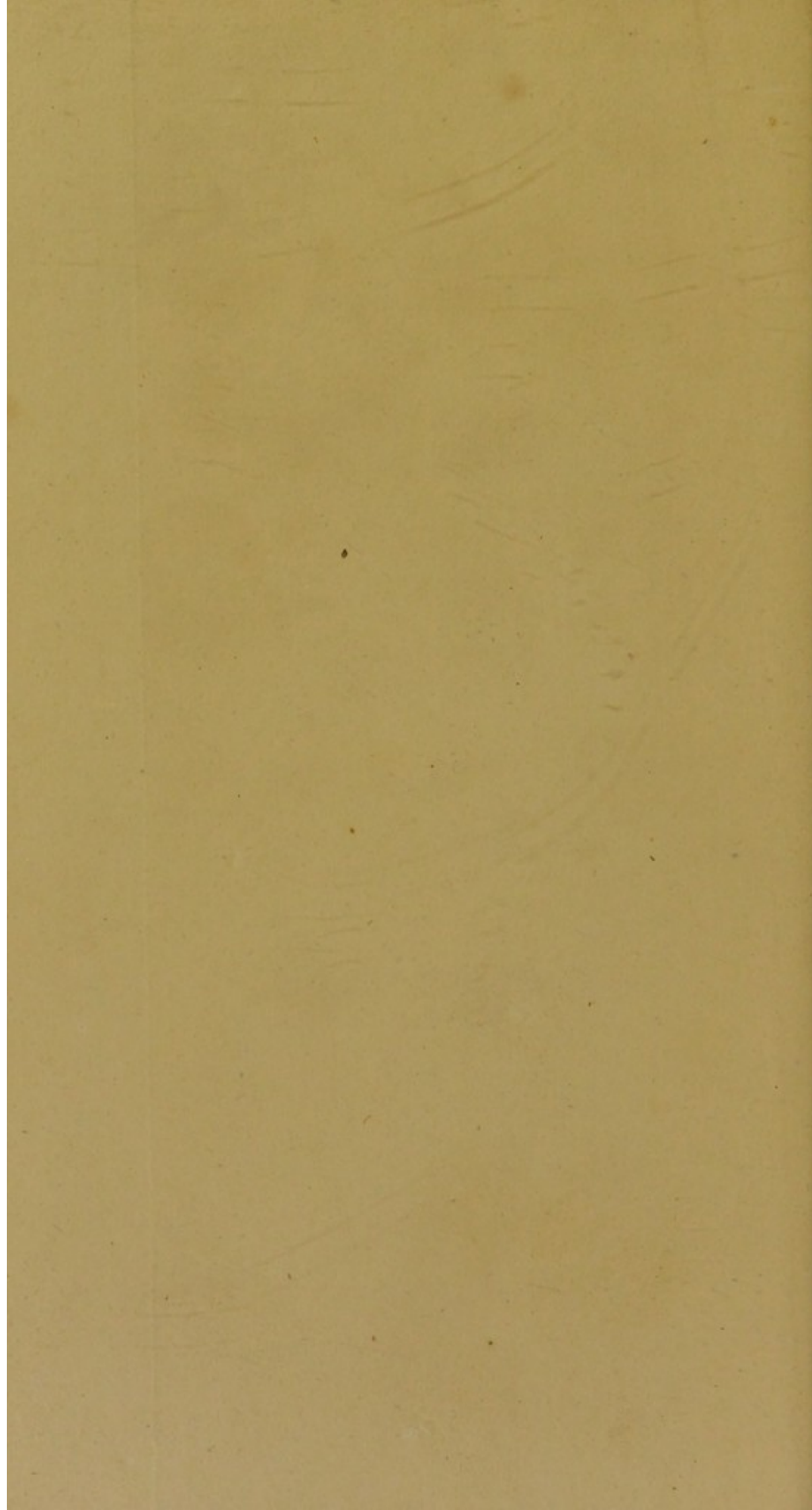
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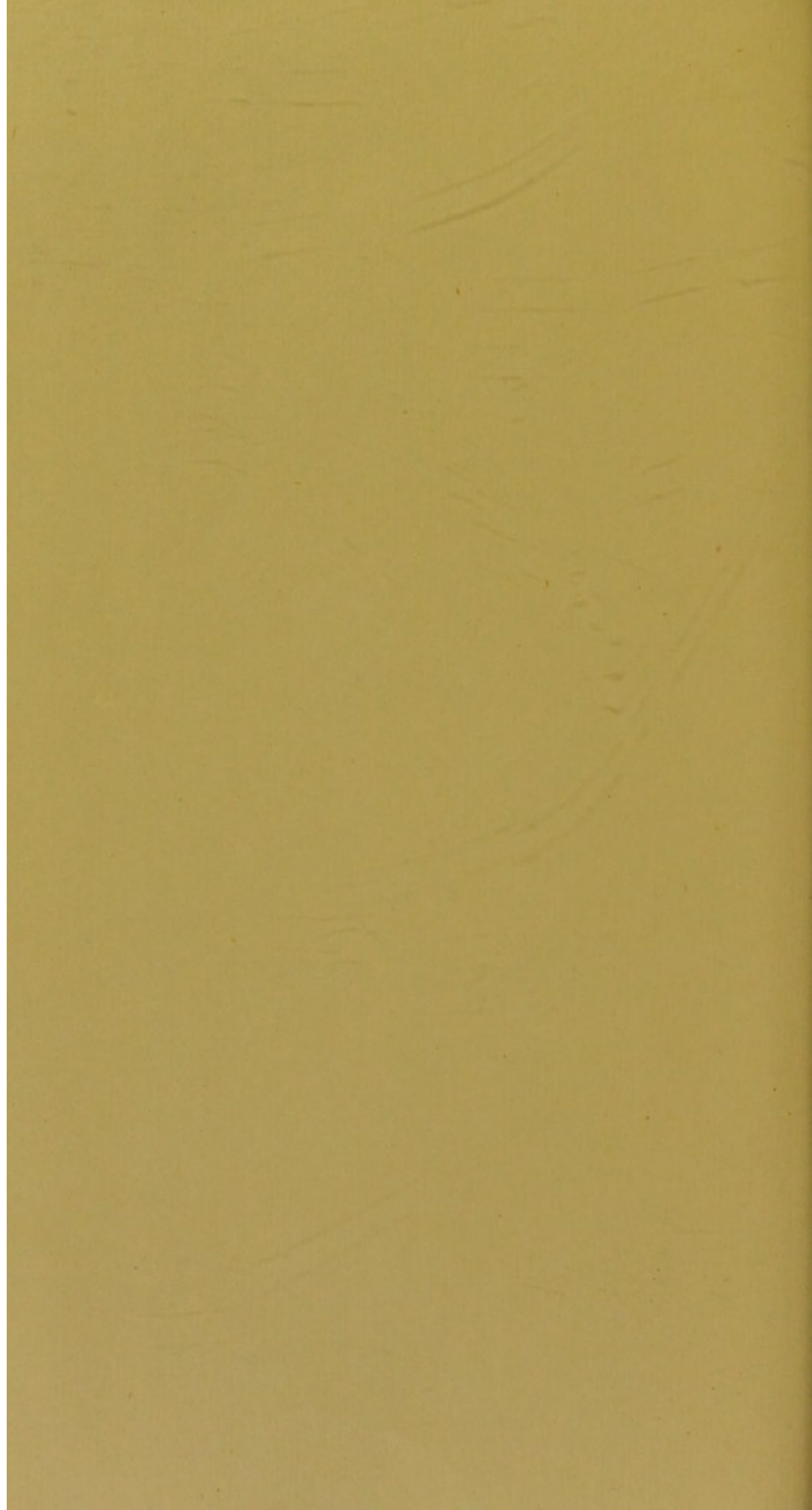
















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