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TRACTS

ON THE

NATURAL HISTORY

OF

ANIMALS AND VEGETABLES.

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OF

ANIMALS AND VEGETABLES,

TRANSLATED FROM THE ORIGINAL ITALIAN OF

(Lazzaro)

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BY

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WITH

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1803.

TO

THE RIGHT HONOURABLE

JAMES EARL OF HOPETOUN,

VISCOUNT AITHRY, BARON HOPE,

LORD LIEUTENANT OF LINLITHGOWSHIRE,

ETC. ETC. ETC.

THIS TRANSLATION IS INSCRIBED

BY

JOHN GRAHAM DALYELL.

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TRANSLATOR'S PREFACE.

The acknowledged fame of the celebrated author of theſe Tracts, and the importance of his numerous diſcoveries, are too well eſtabliſhed to require any additional confirmation here. His name has juſtly been inrolled with that of the moſt illuſtrious European phyſiologiſts: and his works will ever remain an emblem of genius and reſearch. All bear indiſputable evidence of profound inveſtigation into the phenomena of nature, and of the moſt comprehenſive and enlightened underſtanding. Probably the following treatiſes on the *Natural Hiſtory of Animals and Vegetables* will rank with the firſt of his productions; for the new, ſingular, and intereſting matter they contain will be an equal ſource of pleaſure and admiration to the philoſophic reader.

The unaccountable theory of ſpontaneous generation made great and rapid progreſs towards the middle of laſt century : it was even embraced by naturaliſts of no inconſiderable merit. Mr Needham, in ſupporting a ſyſtem which he firmly believed, attacked or miſconſtrued ſome of the author's principles, who, in his turn, retorted with uncommon ſeverity. This controverſy occupied a conſiderable portion of the firſt Tract, but part of it is here omitted ; for it is certainly needleſs to revive any thing concerning a doctrine now totally exploded. Whoever beholds the animalcula of infuſions, either with the naked eye or a microſcope, in my opinion, can demand no other evidence of their animation, which was the principal point in diſpute. All the other Tracts are preſerved entire as in the original.

Several notes are ſubjoined, chiefly in illuſtration of general points, and ſometimes in explanation of the text. But this is not a mode of writing to be recommended ; for it diſtracts the attention from the ſubject, and too frequent opportunities occur of making uſeleſs remarks. Perhaps it will always be preferable, if poſſible, to connect obſervations of this kind in ſuch a manner as to form a brief diſſertation, which is a much better way for a commentator to illuſtrate. The preliminary remarks will in a certain degree

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ſupply the place of other elucidations. But an ample field will be found for many more than are there.

I muſt acknowledge, that I have ſometimes been at a loſs to diſcover the exact ſpecies of ſeeds which the author uſed for infuſions. He ſeldom or never gives ſpecific names; and in Scotland little is yet known of Italian botanical ſynonyms. However, it is of leſs importance, as many different infuſions will produce the ſame animalcula. In my own obſervations on theſe ſingular animals, during ſeveral years, I believe I have diſcovered almoſt the whole which have been the ſubject of the author's conſideration. A few I have not found, or have been unable to recogniſe.

It will eaſily be perceived, that the Treatiſes on Animal Reproduction originally formed no part of the Tracts. Therefore the reader may reaſonably inquire, why he finds them here.—Such inveſtigations may properly conſtitute a ſixth ſubject, on which as great learning and ingenuity are beſtowed as on the reſt. This is a ſtudy which ſeems to have made leſs progreſs in Britain than on the Continent; there are few or no original experiments in Engliſh, and ſome of the following tracts are difficult to be procured: therefore it appears right to convert what may be obtained to uſe. There is complete evidence that a decapitated ſnail will acquire a new head, notwithſtanding the numerous diſcordant opinions and experiments. For this celebrated diſcovery we are indebted to Spallanzani, who has conſidered the various reproductions of animals more profoundly and comprehenſively than any other author: indeed, a very great portion of what is known concerning them we owe to him. The firſt Memoir on ſnails is entire; but the ſecond, where the author enters copiouſly into his own defence, is much abbreviated. It is true, all the matter is preſerved, but the keenneſs and redundancy, which ever attend controverſy, can give no pleaſure to thoſe whoſe more uſeful reſearches are directed to facts. For of what avail are opinions unleſs eſtabliſhed on facts? Even the foundeſt analogical reaſoning is too often to be diſtruſted.

In theſe Memoirs, ſeveral redundancies will occur, but it was impoſſible to avoid them. M. Bonnet's whole treatiſe may appear in this light. However, in one reſpect, it may be uſeful, namely, from the apparent correctneſs of the engravings. It is by figures chiefly that we are more eaſily enabled to underſtand ſuch intricate ſubjects of natural hiſtory; M. Bonnet's are more diverſified

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ſied than thoſe of the other two Memoirs; and as this Memoir was added to the laſt edition of the Tracts, independent of the excellence of the matter which it contains, the reader will not be diſpleaſed to ſee it reprinted. Although Spallanzani makes repeated alluſions to an extenſive work on animal reproductions, he never publiſhed any thing reſpecting the reproductions of water newts, except what is in his PRODROMO. The author of his *Literary Life* informs us, that he conſidered the defect had been fully ſupplied by M. Bonnet, and he therefore abandoned the deſign of publiſhing his Reſearches on Animal Reproductions;—a determination deeply to be regretted by every philoſopher. All that is contained in theſe volumes on this ſubject, was at firſt intended for a ſeparate publication.

It may ſafely be affirmed, that we are indebted to the friendſhip ſubſiſting between the two philoſophers for whatever portion of this work is written by M. Bonnet. Their eſteem was mutual. Spallanzani tranſlated *La Contemplation de la Nature* into Italian; and propoſed to tranſlate *La Palingeneſie* alſo. However, I believe, the Inquiſition oppoſed it: at leaſt, it is ſaid, that the tranſlation of the work was prohibited by this formidable tribunal.

The difference between the ſtile of Spallanzani and Bonnet is inconceivable. In general, that of the former is natural and perſpicuous: the meaning is eaſily comprehended, for it is in a manner analyſed. Bonnet, on the other hand, is commonly prolix, and very often obſcure; and it is ſometimes with extreme difficulty that his real ſentiments can be diſcovered. Several of his writings ſeem never to have undergone correction; and a literal tranſlation, even abridging the innumerable tautologies and redundancies, ſounds uncouth in Engliſh. Notwithſtanding theſe imperfections, he is unqueſtionably a great philoſopher; he has profoundly inveſtigated nature, and his authenticity is unchallenged.

Although Spallanzani was reputed the firſt phyſiologiſt of the age in which he lived, in one reſpect he was certainly moſt unfortunate. The truth of his experiments was diſputed; nay, his veracity itſelf was called in queſtion. Nor was this done by the ignorant, weak, or malignant, who are uſually the firſt to labour for the diſcovery of error, but by philoſophers of eſtabliſhed credit, learned and liberal. Not only has the author's own defence elucidated any doubt that attended his principles, but the lateſt experiments on the ſame abſtruſe and difficult ſubjects have

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tended to prove them true, and given them additional weight.

In his whole writings, the author uniformly teſtifies the utmoſt contempt of nomenclators. Yet we muſt allow that nothing is more uſeful than correct nomenclature; for it reſts both on deſcription and phyſiology. This ſecond qualification, it is true, has not met with ſufficient attention; and the extraordinary anxiety of moſt modern naturaliſts for claſſification, from external appearances, has occaſioned the neglect of real phyſiology; therefore, the conſequence has been continual alterations. Indeed, if both are to be obſerved, they will ſometimes be at variance with common underſtanding. Such as placing bats, whales, and dolphins, in the ſame claſs with mankind. Theſe arrangements, however juſt, are at firſt repugnant to received opinions; and many will feel the ſame repugnance at admitting meduſæ, actiniæ, ſnails, and animalcula, into the claſs of vermes.

It has often ſurpriſed me very much, that ſo few foreign works of high authority are tranſlated into Engliſh, and almoſt never until a conſiderable interval after publication; nor do I think that any good reaſon will eaſily be aſſigned. Aſſuredly it is not becauſe they are found preferable in the original, becaufe they are very feldom to be procured in that ftate; and this difficulty of procuring the foreign authors is undoubtedly very detrimental to the literature of the nation: for difcoveries and obfervations, well known on the continent, are frequently a long time dormant here. But the intercourfe of countries, and the rapid progrefs of civilization, will tend more and more to the diffufion of fcience.

PHYSIOLOGICAL REFLECTIONS

ON THE

NATURAL HISTORY

OF

ANIMALS AND VEGETABLES.

I. Animalcula of Infusions.—Even in this advanced ſtage of ſcience, we are intimately acquainted with the natural hiſtory of only a few of the larger animals which are daily in view. The particular age when they begin to generate is known; we can calculate the period of geſtation; we have learned what food is unſuitable to their nature; and ſometimes it may be diſcovered whether they are healthy or diſeaſed. Theſe general facts have been attained by reiterated and evident obſervation.—Yet of the ſecret vital phe- nomena,

nomena, and of the life of a ſavage animal, our knowledge is limited indeed.—The original cauſe of exiſtence is in the moſt profound obſcurity; we are in the utmoſt ignorance why the offspring of an animal aſſumes a determinate figure; why the increment of one part is more rapid than that of another; and why the ſize of the whole is at laſt confined within certain bounds. No ſatisfactory reaſon has hitherto been aſſigned, why the capacity of either ſex to generate does not ariſe until a certain age; for the very different duration of geſtation by different females, or the variety in the time of incubation. And although we contemplate the progreſs of a diſeaſe that ends in death, it is ſeldom that we can either tell what is the real origin of it, or the cauſe why exiſtence ceaſes. Moſt of the animals, which have been the ſubject of obſervation, we have endeavoured to domeſticate, but thoſe that roam in the deſert, dwell on lofty mountains, are hid in the earth, or concealed in the receſſes of the ocean, are hardly known to us by name.—Is there any wonder, therefore, that ſo many centuries have paſſed away, before the properties of beings, almoſt as minute as we conceive the particles of matter, have been inveſtigated?

The animalcula of infuſions conſtitute a claſs in the animal kingdom, on which the learning

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and ingenuity of philoſophers have been equally exerciſed. Their infinite multitudes, their varieties, the places of abode, and above every thing, their extreme minuteneſs, have all conſpired to retard our inquiries into their real nature. Let one conceive himſelf, in a moment, conveyed to a region where the properties, the figure, and motion of every animal are unknown, and he will be able to form ſome idea of what infuſion animalcula are. The amazing variety of figure beheld at once, and their motions, will firſt attract his attention. One is a long ſlender line, another is an eel or a ſerpent. Some are circular, elliptical, and globular, others cylindrical or triangular. One is a thin flat plate, another like a number of articulated reeds. Several have a long tail, almoſt inviſible, or the poſterior part is terminated by two ſtrong horns. One is like a funnel, another like a bell, and many cannot be referred to any object familiar to our ſenſes. Certain animalcula can change their figure at pleaſure. Sometimes they are extended to immoderate length, then contracted almoſt into nothing; ſometimes curved like a leech, ſtretched or coiled like a ſnake. At one moment an animalcule is inflated, and the next it is flaccid. Some are perfectly opaque; ſome ſcarcely viſible, from extreme tranſparency. Numbers have no apparent organs; and many are covered with tubercles or

briſtles.

briſtles. No leſs ſingular is the variety of motion peculiar to each. Several ſpecies ſwim with the velocity of an arrow; the eye can hardly follow them. Some drag the unwieldy body along by painful exertion, and others ſeem to perſiſt in perpetual reſt. An animalcule will revolve on its centre as the axis of motion, or the anterior part or head is made that axis by another. Some move by undulations, by leaps, or inceſſant gyration. In ſhort, there is no kind of animal motion, or any other mode of progreſſion, that is not practiſed by animalcula. The manner in which they propagate is as remarkable. In general, they produce eggs, many, a living fœtus, and others, like a few of the larger animals, both; ſome multiply by a part of the body detaching and becoming a new animal; ſome by a tranſverſe or longitudinal diviſion of the body, and others, by the mother burſting to allow her offſpring to come into the world.

Compared with the reſt of animated nature, the number of infuſion animalcula ſurpaſſes all belief: they are ſurely the moſt numerous. Next are worms, inſects, or fiſhes; amphibia and ſerpents, birds, quadrupeds; and laſt, is man. The ſpace he occupies on earth is ſmall, and the propagation of his ſpecies goes ſlowly on. The human female produces only one at a time, that

after

after a confiderable interval from birth; and but few during her whole exiftence. Many quadrupeds are fubject to fimilar laws; fome are more fertile, and their fecundity is little, if at all, inferior to that of certain birds, for they will produce ten or twenty at once. Several birds will breed frequently in a year, and have more than a fingle egg at a time. How prodigious is the difference, on defcending to fifhes, amphibia, reptiles, infects, and worms! Yet, among them, the numbers cannot be more different. According to naturalifts, a fcorpion will produce 65 young; a common fly will lay 144 eggs; a leech, 150; and a fpider, 170. I have feen a hydrachna produce 600 eggs, and a female moth 1100. A tortoife, it is faid, will lay 1000 eggs, and a frog 1100. A gall infect has laid 5000 eggs; a fhrimp 6000; and 10000 have been found in the ovary, or what is fuppofed that part, of an afcarides. One naturalift found above 12000 eggs in a lobfter; and another above 21000. An infect, very fimilar to an ant, has produced 80000 in a fingle day; and Leeuwenhoeck feems to compute four millions in a crab. Many fifhes, and thofe which in fome countries feldom occur, produce incredible numbers of eggs. Above 36000 have been counted in a herring, 38000 in a fmelt, 1000000 in a fole, 1130000 in a roach, 3000000 in a fpecies of fturgeon, 342000 in a carp,

carp, 383000 in a tench, 546000 in a mackrel, 992000 in a perch, and 1357000 in a flounder. But of all fiſhes hitherto diſcovered, the cod ſeems the moſt fertile. One naturaliſt computes that it produces more than 3686000 eggs; another, 9000000; and a third, 93444000. Here then are eleven fiſhes, which, probably in the courſe of one ſeaſon, will produce above thirteen millions of eggs; which is a number ſo aſtoniſhing and immenſe, that, without demonſtration, we could never believe it true.

Perhaps the innumerable multitude of animals in exiſtence is leſs influenced by the numbers produced at a time than by frequent and early propagation, by the hazard of deſtruction to which the young are expoſed, and alſo, it is poſſible, by ſome females being more numerous than males. Many inſects generate but once in their whole lives. It has been calculated, that two females, of the animals moſt loathſome and diſguſting to the human race, may ſee ten thouſand deſcendents in eight weeks; that above fourteen thouſand may, in the ſecond generation, come from a ſpider; and a common fleſh fly have ſeven hundred and forty thouſand young in the third month. None of theſe animals are at firſt very fertile, compared with others. But what are all their

their numbers in comparifon to invifible animalcula and vermiculi? One hundred and fifty millions have been computed in the milt of a fingle fifh.

How does it happen that the earth is not overrun by animals, and that they find food fufficient for the prefervation of life? In a day, an immenfe legion will fometimes arife, carrying famine and defolation along with it.—To preferve the requifite balance, there muft be deftruction proportioned to propagation; and the wifdom of Nature feems to have provided for it in a certain degree. The animal, its egg, or the young, are all liable to perifh; and the more ftages it has to pafs through before maturity, the greater hazard does it undergo. Many females produce thoufands of eggs, without any commerce with the male: it often happens that thefe are never fecundated, or a very fmall portion of them, if external fecundation fhould take place. The young, in their tender ftate, may either be deftroyed by the elements, or become a prey to thofe ftronger than themfelves. From their various metamorphofes, they are liable to perifh by difeafe, or from unfuitable fituations. Therefore, comparatively fpeaking, few come to maturity. The number of butterflies is very inconfiderable, in proportion

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to the eggs: few frogs are found near a pool which has been black with tadpoles. Let a leaf, or a particle of animal ſubſtance, fall into a little water, myriads of animalcula will be produced; but ſimple evaporation of the water, an event that muſt enſue, is inevitable deſtruction to the whole. Although the period of exiſtence is limited, few animals die a natural death. The war, which inceſſantly rages among the whole, is a never failing ſource of deſtruction. The ſtronger prey on the weaker; and theſe on animals that are leſs powerful than themſelves. The juſtice of Nature might be arraigned: Why are ſome provided with horns, tuſks, or poiſon, while others have no ſuch means of defence? Thouſands of animalcula are apparently but a ſimple veſicle, without viſible organs external or internal: they burſt on contact with the air alone. All this deſtruction is neceſſary; the earth would be overſtocked; and even thoſe animals, now living in amity, would make each other a prey. Death muſt thus be the indiſpenſible attendant on life, unleſs propagation were to ceaſe.

The ſtructure of ſome animalcula, ſuch as it appears to us, cannot be more ſimple; but the organization of many is certainly very complicated, no leſs ſo than that of animals a thouſand

times

times their ſize. Several naturaliſts have aſſumed the *ſimplicity of organization*, as the means of explaining different phenomena exhibited by animals. To me this ſeems moſt fallacious reaſoning. The organic ſtructure of one animal is ſimple, only when compared with another known animal. If it lives, it is perfect in its kind. Becauſe no heart, lungs, brain, or nerves, are viſible, we can ſeldom poſitively affirm either that theſe organs do not exiſt, or that there are no other parts which perform the functions belonging to the moſt important of them. Indeed, in the ſimple animals before us, they may be ſo diverſified, diſguiſed, and incorporated or partitioned into its ſubſtance, as, ſeparately, to be inviſible or irrecogniſible, but, collectively, capable of performing every vital function. Some may have leſs neceſſity for the important organs which we behold in the larger animals. A muſcular heart is not equally eſſential to all that even have one. In general they inſtantly die, when deprived of it ; but the life of ſeveral, thus mutilated, endures longer than that allotted to certain animals in their moſt perfect ſtate. The pulſation of one heart will ceaſe the moment that it is wounded ; another will beat long after being torn from the body. Some agents, deſtructive of life, will deſtroy its irritability ; others will produce no ſenſible effect. Penetration of the brain will often occaſion inſtant death; but various animals will live long, not only after the whole has been ſcooped from the cavity of the ſkull, but after the head itſelf is cut off.

In the larger animals, where diſſection may be uſed, it is in the power of the anatomiſt to lay every muſcle, tendon, and blood veſſel, bare. In thoſe ſo minute as infuſion animalcula, where, inſtead of each part, the entire whole is in general hardly viſible, the philoſopher muſt ſometimes be ſatisfied with the moſt rational inductions from what he beholds, and analogical reaſoning, if that is ever to be admitted.—The different functions of animalcula are probably performed by means analogous to thoſe of other animals. The courſe of ſome, which are without perceptible external organs, proportionally exceeds the ſwifteſt flight of birds; conſequently, they muſt have conſiderable ſtrength. Whether their motion proceeds from curvature, leaps, or undulations, it can only be effected by impulſe againſt the water; and we may reaſonably infer, that ſome muſcular movement is the principle, or that the ſtrength proceeds from ſomething correſponding to muſcles. The great ſource of motion among worms and aquatic animals, deſtitute of feet and fins, is undulation. In this manner is the progreſſion of all ſerpents, eels, and many other

other reptiles. No evident curvature is ſeen in the body of a ſnail; but in the belly are ſucceſſive undulations, by which its peculiar progreſſion is performed. It is more convincing, that animalcula have muſcular parts, and perhaps joints formed of membrane, or a ſimilar ſubſtance, when we can actually obſerve fins, feet, or fibrilli. Many which we ſuppoſe deprived of them, really are not ſo; and in others, they are ſo minute, ſlender, or tranſparent, or ſo much of the ſame colour as the fluid they inhabit, as ſeldom or never to be viſible. Indeed we often ſee particles, in an infuſion, carried along, at a diſtance from the animalcula, by ſome inviſible hair.

When an animal is called imperfect, we mean, that it wants organs with the uſe of which we are acquainted. External impreſſions are certainly the origin of ideas; and there is reaſon to believe, that mind originates entirely with the uſe that can be made of the ſenſes. Undoubtedly we can form no conception of any object, without the intervention of ſome of the ſenſes: and, although the mind may wander through all the immeaſurable field of imagination, ſtill it can invent no new idea that is without any chain or link to what is ſuggeſted by the ſenſes. Thus, if it is poſſible to conceive that a man might be born without any of the ſenſes, we cannot figure how he could have mind, or be capable of diſtinguiſhing objects. But there are many animals which want ſome of the known ſenſes, and are ſtill capable of volition, choice, and ſeveral of thoſe ſenſations which we aſcribe to the moſt perfect. It is not evident that they labour under much inconvenience, where certain organs are entirely wanting, or derive equivalent advantage, where they abound. The variety of organs is more eſſential than the number.—The flight of an inſect is as rapid with two wings as with four: viſion is as acute with two eyes as with eight: progreſſion is as quick with ſix or eight legs as with an hundred. Indeed it is difficult to ſay what is the uſe of ſuch a redundancy of organs, at leaſt, if we judge from ſimple appearances. Some animals, which have only ſix legs at firſt, acquire an additional pair every year of their lives. There is one ſpecies with eight legs, when full grown, in which the third pair is wanting at firſt; and another, with ſix pair originally, and a ſeventh is afterwards acquired.

More than five ſenſes may exiſt; nor is there any abſolute neceſſity for limitation to that number. In various animals, there may be others ſo complicated or uncommon, as to be totally incomprehenſible by the human mind; and they may

may be tranſmitted through channels very different from thoſe known to us for the tranſmiſſion of ſenſe. Hearing and ſight are very uncertain in whole claſſes of animals; the ears of very few inſects have been diſcovered; ſome molluſcæ and worms ſeem hardly to be ſenſible of ſound. In inſects, which form ſo great and beautiful a part of the creation, viſion is undoubtedly very obſcure. No organs of viſion are found in an infinity of worms; and thoſe that have eyes ſeem to make very little uſe of them. Although many inſects are ſenſible of the impreſſions of light, I doubt whether they are capable of the perception of objects, or guided towards them by viſion; even the wonderful operations of the bee are performed in utter darkneſs. There are ſome inſects, however, that are ſenſible of the preſence of adjacent objects, and actually recogniſe them as I have demonſtrated by experiment. One ſenſe may certainly ſupply the place of another, which we ſee in the larger animals. A bat, deprived of ſight, will traverſe the ſame courſe, and avoid the ſame obſtacles as before. Theſe creatures fly through openings, nay, diſcover new ones, and without any embarraſſment, paſs through them. Some naturaliſts thence concluded, that bats had a ſixth ſenſe; ſome from experiment aſſerted, they were guided by hearing; and others, that exceſſive delicacy of feeling rendered them ſenſible of

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the preſence and direction of objects. Innumerable inſtances prove, that one ſenſe, either in a natural ſtate, or well practiſed, may ſupply the place of another.

Thus, apparent ſimplicity of organization can neither be an argument for explaining the phenomena preſented by animalcula or the ſimilar claſſes of animals, ſuch as propagation by ſhoots or diviſion, reſuſcitation, reparation of the loſt parts, as has been ſuppoſed peculiar to certain animalcula. But with all this apparent ſimplicity, they poſſeſs every characteriſtic requiſite to entitle an animal to be called a living being. A ſlight inveſtigation of their nature is alone ſufficient to eſtabliſh the fact; for there is ſcarcely any phenomenon among known animals, that does not exiſt among them; and they exhibit many which ſeldom or never occur in any other race.

It is a prevalent opinion, that animalcula may be diſcovered in rain water with the microſcope, or in that of the pureſt fountain. This is an error. I have never once found them in the courſe of innumerable obſervations; and the great animalculiſt Muller himſelf ſays, they are very rare. That they may exiſt in theſe ſituations, is by no means impoſſible; they may be inviſible in air, and become

come viſible in water. Though imperceptible by our ſenſes, they may be ſuſpended in the air as their native element; and, if it was not for the exceſſive delicacy of their parts, and the difficulty of obſervation, they might be rendered viſible by the interpoſition of different mediums; —and here imagination may figure beings of another kind, which, by an interpoſing medium, may in the ſame manner be brought into view. Some animalcula of conſiderable ſize are ſeen with the utmoſt difficulty, from extreme tranſparency; it is only an accidental inflection, or an alteration of the uſual direction of the rays of light, that renders them viſible.

II. SEMINAL VERMICULI.—Three great and important points are to be conſidered in animated exiſtence; the mode of an animal's origin and introduction into the world; the duration of life; and the manner of it's death. What we daily behold, events and objects continually familiar to us, make little impreſſion; and, if there is any novelty at firſt, it ſoon wears away. If we reflect on the phenomena of nature, and inquire into the original cauſe of life, its preſervation and end, an infinite and unaccountable variety is preſented; and although we may be inclined to give phyſical reaſons in explication, or feel an internal conviction that none other will apply to certain cafes, it muft be acknowledged, that the boundlefs empire of nature has neither been completely explored, nor reduced within determinate limits by human underftanding, and perhaps never will.

Seminal vermiculi are a race of animals whofe origin, exiftence, and ufe, if they are of any, are all equally myfterious. Their origin is even more wonderful than that of the numerous other worms inhabiting the bodies by which they are nourifhed. Every thing contributes to the difficulty of inveftigating their nature. The termination of life, or the cruel mutilations that muft be employed to view them in their native abode; the ravages and diforder that fuch operations muft occafion; and their extreme minutenefs (for it has been computed that the diameter of fome does not exceed one hundredth part of the thicknefs of a human hair) render it furprifing how fo much of their hiftory has been difcovered. But the period is yet diftant, when every kind of propagation fhall be known. Two thoufand years ago, the generation of eels occupied the attention of naturalifts, and it is ftill obfcure.—The chief difficulty which attends our comprehending the origin of feminal vermiculi is their appearance only at a certain age. Where do they exift before this term arrives? The germ or primordium

primordium may conſtitute a part of the animal itſelf: it may remain entire, though incapable of expanſion, and not be liable to decay for a ſeries of years; then, when the fluid which it ſhould naturally inhabit is ſecreted, ſome of the numerous germs, which we may well ſuppoſe diſſeminated throughout the body, will be unfolded there, though the circumſtances neceſſary for their developement are ſeldom or never found elſewhere. In the ſame manner, it is not improbable, are the numerous worms inhabiting the human body produced. The egg is conveyed into the body, or tranſmitted by the parent; and, when in a ſuitable ſituation, it expands. Yet this reproduction is myſterious, when compared with that of other animals.

The ſame rules, with little variation, regulate the propagation of mankind, quadrupeds, and birds. Fiſhes and amphibia in general generate in a mode peculiar to themſelves. Inſects approach in ſome reſpects to the higher orders of animals; but deſcending to the innumerable and various claſſes of worms in all the branches, nothing can be more diverſified. Some divide into pieces, and each becomes a new animal; others ſend forth buds, which grow complete like the parent, and thus perpetuate the ſpecies; ſome produce living young; ſome eggs; and a few, both eggs and young.

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The theories of generation which theſe peculiarities have created are various; and all, even the moſt inconſiſtent and chimerical, have had partiſans. Perhaps the ſame difficulty does not attend a probable conjecture, not to go ſo far as *explanation*, of that generation, which is effected by means of a fluid and of eggs. When the female approaches maturity, and often long before it, ſubſtances reſembling eggs, or which really are ſo, appear; and in the male is ſecreted that particular fluid which is peopled by numberleſs vermiculi. Immediately after they were diſcovered, the charge of perpetuating animated beings was committed to them: it was generally believed that every animal originated from a vermicule. But it is now univerſally known that the foetus belongs to the mother alone; that it pre-exiſts fecundation; and lies dormant until it is called into exiſtence. Eggs have been rendered prolific; and animals, which require copulation to propagate their young, have been artificially fecundated, that is, without interpoſition of the male. It is even ſaid that this ſtrange experiment has ſucceeded in mankind. Still it is to be explained, why of twenty or thirty eggs, as in the human female, only one is impregnated at a time, and why ten or fifteen may be impregnated in a female quadruped; more eſpecially, if generation is effected by abſorption,

ſorption, as it is moſt natural to believe. Is it becauſe only one ovum is in a ſtate to be impregnated in the one caſe, and almoſt always more than one in the other? Whatever is the truth, it is evident that ſeminal vermiculi have no concern in generation, both becauſe it appears that ſome ſeminal fluids are entirely deſtitute of them, and not the leſs fecundative on that account; and although the whole are dead, their native element has loſt none of its prolific virtue. Since indiſputable obſervations prove, that various animals actually exiſt in the mother before fecundation, how do they originate there? Does every germ include another germ; that a ſmaller one; and in this manner involving ſucceſſive germs to infinity; ſo that a thouſand years ago, beings which have now paſſed through a thouſand generations, then exiſted, though the term of evolution could not arrive until a thouſand changes were undergone? Can we conceive, that at a time beyond the power of imagination to reach, a germ exiſted; that we ourſelves, the preſent generation of mankind, were in being, under any figure, however minute; that, until the maturity of the preceding generation, our bodies could never expand into perfect ſhape and organization.—Or, is it more rational to ſuppoſe, that at the age of puberty, ſomething is ſecreted by the mother, that there is ſome aſſimilation of parts which will form a fœ-

tus;

tus; that there is ſome generative power developed with age, which can produce a germ capable of being fecundated by the male. Diſcordant opinions like theſe have divided the moſt celebrated philoſophers; ingenious arguments have been given for each; and if infinite involution is the more generally adopted, perhaps it is becauſe the expanſion of parts in miniature is leſs repugnant to the mind than creation.—Yet changes certainly take place in the bodies of animals, which may almoſt be called creation. New parts are acquired; though theſe may be derived from other exiſting ſubſtances, ſtill they form a part of the body. The cartilages of a fœtus undoubtedly do not contain principles exactly the ſame as the bones of an adult. Solids may be involved in other ſolids; but it was never ſaid that fluids could be involved in fluids. The blood, the milk, the bile, urine, and the numerous other fluids in the human body, muſt be derived from other ſubſtances, becauſe the quantity of any one is ſo ſmall in the fœtus; not to name that prolific fluid which does not exiſt before a particular ſtate at which the body arrives. The ſolids muſt alſo acquire new parts; increment in ſize may be effected indeed, by expanſion alone, but no additional weight will be gained.—But this is entering on the profound theory of increment. Theſe facts bear ſome diſtant reſemblance to creation, notwithſtanding they are only the aſſimilation of one ſub-

ſtance

ſtance into the parts of another. Every ſyſtem of generation is accompanied by difficulties almoſt inſurmountable. We have penetrated ſome of the myſteries, but the veil is not entirely removed.

Though the germ belongs to the female alone, the male has an active ſhare in the expanſion and modification of its parts. The eyes, the voice, the colour, nay, the whole appearance, often reſemble the father much more than the mother. However, in conſidering this matter on a great ſcale, and neglecting the peculiarities of individuals, all animals have a greater reſemblance to the mother. The flat noſe, the woolly hair, and thick lips of a negro will be tranſmitted to his offspring by a European woman: and the high features, long hair, and light colour of a European father will be tranſmitted in ſome meaſure to the child of an African mother. The active part of the father is more conſpicuous in the generation of hybrids. In the offspring of the aſs and the mare, the goldfinch and canary, or the canary and linnet, ſome of the parts peculiar to the female are altered, while thoſe belonging to the father are preſerved almoſt entire. He communicates ſomething, whatever it may be, that awakens exiſtence in the germ, is aſſimilated into itſelf, and regulates the formation and appearance

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of its body. But the greateſt ſingularity is, that certain parts ſhould be received, and others rejected: that although the father is deprived of thoſe very parts which prove the offspring his, it will have them all. Notwithſtanding both parents ſhould be maimed or mutilated, the germ is perfect, and the embryo will exhibit none of their imperfections. What an immenſe fund for experiment and obſervation is here! Sometimes imperfections are hereditary; but theſe are rare and uncommon caſes: and it is much more remarkable when an exiſting imperfection paſſes one generation and affects the next. Deformity of the perſon is often tranſmitted to a ſucceeding race: we have known a vice in the conformation of the organs of hearing and ſpeech in a whole family, and ſometimes ſeveral children born blind. Theſe imperfections, on inveſtigation, are frequently found in the relations of the parents.

Conſidering the little progreſs of knowledge, and the rude hypotheſes of generation, it is not at all ſurpriſing that the origin of animals was aſcribed to ſeminal vermiculi when they were firſt diſcovered: and this would have acquired additional credit had it been known there were pores in the integuments of the germ or egg which vermiculi might penetrate.—Let us conſider an impregnated egg. It conſiſts of a tranſparent

ſparent viſcous fluid, ſurrounding a yellow liquid contained in another envelope; nothing more is viſible. If the egg is expoſed to heat, it is of no importance whence derived; if attaining a certain degree, appears an irregular vermicular figure, without any determinate parts. But this is the rudiments of the chicken. A red point next becomes viſible, which is the heart: then the eyes and the bill are unfolded: the embryo gradually increaſes; it grows a perfect animal, and burſts the ſhell. In a manner nearly analogous are the original evolution and increment of the human living fœtus. A tranſparent veſicle is detached from the ovary by impregnation; it is depoſited in the womb; all the parts of the human body appear; and what is in one and twenty days accompliſhed in the chicken ſucceeds in nine months: the perfect fœtus is formed. How this could be affected without any ſenſible primordium, confounded naturaliſts, (for then it had not been proved that the germ belongs to the female) and when they ſaw a living animal furniſhed by the male, they eagerly adopted it as the principle of exiſtence.

III. Death of Animals in Stagnant Air.—Long before philoſophical experiment, it was probably well known, that the life of an animal confined where there is no admiſſion of freſh air.

air, or where the circulation is impeded, will be deſtroyed; and that plants in ſuch a ſituation will droop and die.—Late reſearches in chemiſtry have diſcovered various inviſible fluids produced by different ſubſtances, which are fatal to the animals that reſpire them. Some are ſo deſtructive as to occaſion inſtant diſſolution; in others, an animal may linger long, though it will at laſt periſh under their deleterious influence. Nay, eminent philoſophers have fallen a victim to hazardous experiment on themſelves: and ſeveral have eſcaped after the moſt imminent danger.

So uſeful is atmoſpherical air to the conſervation of life, and ſo dependent is life on its influence, that none of the animals, with whoſe nature we are acquainted, can exiſt without it entirely: and if ſome do ſupport privation of air better than others, ſtill they languiſh, and at length will die. Thoſe inhabiting waters, and ſeldom come to the ſurface for reſpiration, periſh when the water is deprived of the pure air which it contains.

Atmoſpherical air is not a ſimple fluid; it is a combination of various fluids; ſome of which, in a decompoſed and ſeparate ſtate, are better calculated to ſupport life, at leaſt for a certain time, than the reſt, but none except itſelf has yet been found

found perfectly innoxious, nor any one equally beneficial. The principal ingredients are said to be azotic gas, oxygen, carbonic acid, and water. Azotic gas and carbonic acid are most pernicious to animals, and the former constitutes two-thirds of the whole. By respiration, a chemical process takes place, both in the system of the respiring animal and in the air. A change is produced in the blood, and animal heat is promoted. But the alteration that succeeds in the fluid respired is of greater importance, for it is that which occasions death in whatever manner it may operate. The oxygen, or pure vital air, is consumed; an addition is made to the carbonic acid gas, and almost all the azot is left. Such are the principal changes effected by respiration. When this goes on in the open air, the continual renewal of the pure parts, and the purification of those unfit for use, the emanations and combinations of what arise from the substances in which analogous operations are maintained, render it again capable of being respired without injury. When the vital air is consumed, and the noxious part remains behind, the pernicious effects immediately become visible. The manner in which death ensues, like most profound investigations, has divided the opinion of philosophers.

Various gaſes produce an alteration in the ſtate of the blood: experiment, it is true, has been chiefly directed to blood after proceeding from the body of the animal, and the difficulty of the ſubject has hitherto prevented a full conſideration of what are the principal phenomena on that which ſtill continues to flow during life. Whether or not the change there produced is ſufficient to deſtroy the animal, the operation of gaſes on the muſcular and nervous ſyſtem is very conſpicuous, and eſpecially on the former, perhaps, becauſe it may then be more eaſily recogniſed. Animals, at the ſame time, in ſome inſtances certainly die from abſolute ſuffocation; for the exceſſive irritation produced by gaſes will cloſe the entry to the lungs, and death enſue before one inſpiration is completed. The general operation is moſt probably on muſcular irritability and the nerves, more eſpecially when we reflect that the animals reſpiring ſo very little, as ſome are known to do, can not eſcape the pernicious conſequence of reſpired air. Yet we muſt admit, that if they reſpire at all, which it is moſt likely they do, by abſorbing air from water, if they are aquatic animals, they will alſo abſorb exhalations, and thus be deſtroyed.

Drowning and ſtrangling are equally fatal to life as ſuffocation in mephitic vapours; but the

irritability

irritability of the heart is much longer preferved in drowned and ftrangled animals than in thofe that have perifhed in gafes. Much light has been thrown on mufcular irritability fince the difcovery of Galvanifm; for by that means a fpark of life has often been found when it was thought totally extinct. The lofs of irritability depends in a great meafure on the nature of the fluid in which the animal has died. By fome, it will be altogether deftroyed, and others will only weaken its power. Drowning an animal fufpended by the feet, is entirely deftructive of irritability, alfo the vapour of charcoal and other gafes; but azotic gas and atmofpherical air only diminifh, they do not extinguifh it; and the heart of an animal, killed by its own refpiration, beats long afterwards. Thus the difference is very great both in the fuddennefs of death and the effect upon its body, according to the medium where an animal has perifhed. But it fhould be confidered, with the utmoft attention, whether any of the characteriftic fymptoms precede, accompany, or follow death.

From numerous experiments, it appears that death, in ftagnant air or in mephitic gafes, may proceed from fuffocation, from injury of the mufcular fibre, from affections of the nerves, or all three combined, or perhaps from the chemical change produced on the blood by refpiration being obftructed; but it is not evident that conclufions from experiments are coincident, or that the real caufe is fully eftablifhed.

IV. Animals Killed and Revived.—We are loft in aftonifhment while we contemplate the nature of Life: the deeper our inquiries go, the farther does the object feem to recede from us. We can account for the deftruction of life, when the reciprocal harmony of the parts of an animal is deranged: the functions of the moft important organs are interrupted, and death muft enfue; but how life is maintained is a profound myftery.---An animal originates, its parts unfold, and it vifibly lives. At firft, the interpofition of a parent is neceffary to adminifter the nutritive matter, for its own organs are too weak and imperfect. At length, they acquire fufficient power to receive nutriment; the animal is detached from its parent, and lives for itfelf. It gradually approaches to perfection, new parts are expanded, and thus may it remain for years with little or no fenfible alteration. Then the organs begin to evince perceptible fymptoms of decay: certain parts increafe, change, or diminifh: they become deranged, and incapable of performing their refpective offices: life grows feeble, the animal languifhes and dies; and what conftituted its perfonality

nality vaniſhes in air. Such is the termination of exiſtence by age: but how ſmall a portion of the animated creation attain the time! Often, during the vigour of youth, in the height of activity and ſenſation, the chain will be diſſolved without any external cauſe; the thread is cut; ſomething ſeems to depart; and the moſt beautiful work of nature haſtens into hideous corruption.

To affirm that a being, whoſe animation has been ſuſpended for an immoderate length of time, even for years, can, in virtue of certain conditions, be revived, has ſo ſingular and paradoxical an appearance, that reaſon finds it repugnant to admit the fact. But to produce an animal which has been ſtiff and motionleſs, withered, disfigured, and contracted; utterly incapable of any corporeal function, and the operation of its organs at an end; to produce this animal, and, by a particular treatment, to make it renovate every action that it could perform before; to ſee nutrition, digeſtion, and generation carried on, not only will it bear perfect conviction to the mind that it has come from a ſtate which, if it was not death, certainly cannot be called life, but that it again lives as completely as before its animation was ſuſpended or deſtroyed.---Some animals in the world enjoy this wonderful prerogative. They originate, arrive at maturity, and maintain the vital functions:

their native element is a fluid, and they are naturally moiſt ; but deprive them of this element, or allow them to dry, they become motionleſs, they contract, wither, and die. Thus may the animals remain one year or ten ; then let moiſture and the other requiſites be ſupplied, their members ſwell, life returns, and the animals become as lively and vigorous as ever.

It is reaſonable to expect that ſo remarkable a deviation from the courſe of nature ſhould be limited, and the privileged animals uncommonly few. Still they are not ſo rare as to prevent the truth of the phenomenon from being amply eſtabliſhed. Hitherto none of the larger animals have been found which are endowed with this ſingular property.—Nature, as if to veil that which is ſo fondly cheriſhed in idea by mankind, ſeems to have beſtowed it only on the moſt minute of her creatures.—The wheel animal, various microſcopic eels, and the ſloth, may all die and be revived. The exceſſive ſcarcity of the laſt has prevented naturaliſts from inveſtigating the utmoſt limits of its reſurgent faculties. No one, except Spallanzani, ſo far as I know, has ever been able to diſcover it. In this remote kingdom, where the ſtudy of animated exiſtence is yet in infancy, I have found three, perhaps four varieties of the ſloth, all evidently belonging to the ſame genus of this wonderful animal, or of a race

analogous ;

analogous; but whether poffeffing the fame prerogative I have not yet been able to afcertain. Of the fourth, I fpeak with diffidence; for, without long inveftigation, we muft hefitate in afcribing animality to a being whofe length does not equal the thicknefs of a human hair, which fcarcely ever exhibits perceptible motion, and is not once to be found in a thoufand obfervations.

Whether an animal may actually die, and afterwards be revived, has been the fubject of much controverfy; equally fo as the fymptoms preceding and attending death. Putrefaction is, by common confent, regarded as the moft infallible fign of diffolution, though it will fometimes commence during life. But many animals dry up, and wither, and become as hard as wood without putrefaction ever appearing in the flighteft degree. Next to this, the want of irritability is confidered certain evidence of death. Yet the abfence of it will not always prove an animal dead; for life often remains when there is no fenfible irritability; and one ftimulant will awaken vitality, while the ufe of all others is vain.—In the fame manner as particular ftimulants are incapable of awakening dormant animation, neither can methods deftructive of it, in its utmoft vigour, in one inftance, affect it at all in another. Undoubtedly we fhould fuppofe the moft efficacious methods of deftroying animals, are depriving them of nutriment, or depriving them of ſome organs which ſeem moſt important in executing the vital functions. Whether life is in theſe ſituations extinguiſhed by the ſame means, or whether it is extinguiſhed in every caſe, by exciting a cauſe that will impair exiſting irritability, is left to greater phyſiologiſts. But until the principle of life is diſcovered, which, according to ſome, is the union and reciprocal action of the parts in reſiſting diſſolution; or conſiſts in the blood; in a particular aura, like the imaginary aura ſeminalis; in ſomething reſembling electricity; in irritability, or the exiſtence of nerves; all reaſoning on what affects its creation, preſervation, or deſtruction, muſt be unſatisfactory. One thing is certain, that the death of one part is often the neceſſary conſequence of that of another; that the death of the heart may occaſion the death of the brain; and the lungs will die when the heart is dead.

Mankind can ill ſupport the privation of food. It is true there are ſome wonderful ſtories of abſtinence, in the records handed down to our own time; however, theſe in general may be rather aſcribed to the love of impoſture, and that anxiety to deal in the marvellous, which ſo eminently characteriſed the darker ages. Still, abiding by authentic information, and truſting to veracious accounts of modern date, men have ſubſiſted on a quantity of food ſmall beyond belief; and have even exiſted

incredibly

incredibly long without any at all. Captain Bligh of the Bounty, ſailed near four thouſand miles in an open boat, reduced to the utmoſt extremity for want of proviſions : ſometimes a bird, not many ounces in weight, was the only food for ſeventeen people in a day. Fourteen men and women of the Juno, wrecked an the coaſt of Arracan, lived twenty-three days without a morſel of food. On the fifth day after the ſhipwreck, two people firſt died of want.

Animals, Redi obſerves, do not periſh from hunger ſo ſoon as is generally believed. The period of their death is very various. Houſe and field rats never lived with him three days ; capons lived ſeven, eight, or nine ; a civet cat lived ten ; wild pigeons, twelve and thirteen days ; an antelope, twenty, and a very large wild cat, the ſame time. A royal eagle ſurvived twenty-eight days without food. Buffon mentions one that lived five complete weeks. A badger lived a month, and ſeveral dogs thirty-ſix days.—When we conſider total abſtinence from food for ſuch a length of time as thirty-ſix days, it is truly wonderful that the animal could exiſt. But accounts ſtill more ſurpriſing are given by naturaliſts of undoubted credit. A crocodile will live two months without food. Leeuwenhoeck had a ſcorpion that lived three. A bear is ſaid to have

have lived fix, and vipers the fame time. However, allowances muft be made, if this happens in winter, during extreme cold, when the digeftive and fecretory powers are feeble, and if the animal, of whatever kind, is liable to torpor. A common garden fnail lived eight months on a pane of my window, nor do I know that it was then dead. Redi had a cameleon that lived eight months without food, and vipers ten. Vaillant had a fpider that lived as long; nay, its ftrength was at that time fufficient to kill another, put under the receiver where it was kept, as large as itfelf, and quite vigorous. According to different authors, fome of thefe animals, fuffering long abftinence, have not become much emaciated. Mr John Hunter inclofed a toad between two ftone flower pots; it lived fourteen months, and was then as lively as ever. M. Sue quotes inftances of the fame animals living eighteen months without either food or refpiration, becaufe they were clofely fealed up in boxes or veffels. M. Heriffant confined three toads in a box, and then covered the whole with a coat of plafter: the box was opened in eighteen months, and one of the toads was ftill alive. Land tortoifes lived eighteen months with Redi; Muller kept hydrachnæ equally long. But Baker had a beetle that lived three complete years without food, and then efcaped; and Virey cites Plempius for

leeches

leeches ſurviving three years in water without any viſible food. This inſtance, and that of the hydrachnæ, indeed, are different from total abſtinence; for birds which drink ſeldom, and even carnivorous animals, have ſubſiſted on water alone for a very conſiderable time. Whether inviſible animalcula might be the aliment of thoſe animals, I cannot tell, but I have ſeen ſnails, which were kept months in water only, void very ſenſible excretions, and increaſe the ſize of their ſhells, though theſe continued uncommonly tranſparent, nor could the excretions be the remains of food in the ſtomach, for the ſnails had never ate; they were young; and I had bred them from the egg. However, this is little more ſurpriſing than that plants ſhould vegetate and attain conſiderable ſize in water, for both they and animals extract ſome nutritive part, which, by various decompoſitions, ſecretions, and aſſimilations, is converted into their own ſubſtance. Now, indeed, it is ſuppoſed that the excretions of plants are diſcovered.

If daily experience did not teſtify the reverſe, it might reaſonably be conceived that all the parts of an animal were of equal uſe to it; and that privation of one would be as injurious as privation of any other. But we are acquainted with ſome animals which can loſe a portion of the

the body, and be as healthy, lively, and vigorous, as before; and we continually witneſs this in the amputation of limbs. Nay, ſo far from mutilations being fatal, ſeveral of the loſt parts will be repaired, as the branches of a tree are renewed.

The moſt dreadful wounds, that imagination can figure, hardly ſeem to accelerate death. M. Riboud ſtuck different beetles through with pins; he cut and lacerated others in the ſevereſt manner; yet all lived nearly, if not quite, as long as thoſe that were entire. One, with a pin through the body as thick as its thigh, ſurvived fourteen days. I have ſeen a caterpillar ſtill alive, though ſhrunk to one third of its original ſize; and the body of a butterfly manifeſt animation, when the wings were dry and ſhrivelled up. I have ſeen a butterfly live a month after being ſtuck through with a pin, and after I conceived its life had been deſtroyed by the fumes of ſulphur; for ſuch cruel experiments were not purpoſely made. Leeuwenhoeck, I think, had a mite which lived eleven weeks ſtuck on a point before his microſcope. Vaillant, intending to preſerve a locuſt of the Cape of Good Hope, took out the inteſtines, and filled the abdomen with cotton, and then fixed it down by a pin through the thorax; yet, after five months, the animal ſtill moved its feet and antennæ.

In

In the beginning of November, Redi opened the ſkull of a land tortoiſe, and excavated the whole brain. The tortoiſe did not ſeem to ſuffer: it moved about as before, but groping its way; for the eyes ſoon ſhut after loſing the brain, and never opened again. A fleſhy integument formed, which covered the opening of the ſkull; and in this ſtate the animal lived until May, that is, ſix complete months. Spallanzani deprived four frogs of the brain: two lived till the fifth day. He alſo deprived three newts of the brain: they ſuffered violent convulſions; their eyes cloſed; they hardly moved from one place to another; and expired about the middle of the third day.

He cut the heart out of three newts: they took to flight, leapt, ſwam, and executed the ſame functions as before; however, all died in forty-eight hours. Four frogs, deprived of the heart, kept their eyes open, and preſerved the uſe of their limbs. They ſurvived thirty-ſix hours.

Privation of theſe organs, and even of thoſe far leſs important in the organic ſyſtem, would occaſion immediate death to innumerable animals. But it does not appear that the tenacity of life is uniformly dependent on the ſame principle, or abſolutely

abſolutely of the ſame kind ; at leaſt, we cannot affirm that it is entirely ſo.—Eels, it is well known, exhibit ſymptoms of life in the various divided parts. They are killed by daſhing against the ground. Oſbeck ſays, the greater dog fiſh will move about, though the head or tail is cut off ; nay, that it lives more than an hour after the inteſtines are taken out. Lyonet ſaw motion in the abdomen of a waſp, three days after ſeparation from the reſt ; and a caterpillar crawled about ſeveral days after the head was cut off. From theſe and other experiments, he remarks, that the ſoul of animals, if they have any, is extended over the whole body ; and every part ſeems capable of evincing conſciouſneſs and ſenſation. The headleſs caterpillar endeavoured to eſcape ; it had the ſame motions as before decollation. The anterior half of a divided waſp bit every thing preſented to it. The middle part of an earth worm, deprived of both ends, expeditiouſly moved away when touched. The ſame concluſions are made by Sue, in his Reſearches on Vitality. A decollated turkey ſeemed to have ſpontaneous motions. The body of a butterfly continued to fly as uſual, among the flowers, twenty minutes after the head was cut off. A decapitated beetle will advance over a table, groping its way, and recognize a precipice on approaching the edge. Reverſed on its back, it

will

will endeavour to recover the natural poſition equally as before. Similar motions are exhibited by decollated ſnails and other animals. Cuvier, indeed, an eminent naturaliſt, ſeems to conſider the head or brain as the only ſeat of ſenſe and reflection. But this can hardly be admitted, from the numerous inſtances which appear contradictory; and the reaſon of ſo prevalent an opinion, in all probability, ariſes becauſe the principal organs, for conveying external impreſſions, are uſually ſituated in the head. If more animals were ſubjected to experiments of this kind, and if it could be performed without ſuch terrible concuſſions of the brain and ſpinal marrow, we ſhould be more able to reaſon upon it. Labillardiere remarks, that the turtles of Waygiou, weighing above two hundred pounds, would continue to crawl ſeveral hours after being deprived of their heads. Redi decapitated four tortoiſes, and all the blood was diſcharged. On opening the body twelve days afterwards, the heart was perceived to beat. He decapitated another large tortoiſe; it lived twenty-three days; but Brouſſonet affirms, that this animal will ſurvive during two months without the head. Colonel Pringle decapitated ſeveral libellulæ. One lived above four months, and another nearly ſix; and a ſingular occurrence in his experiments was, that

thoſe

thoſe unhurt, which were kept in the ſame ſituation as thoſe decapitated, never ſurvived above a few days.

There are many other examples of animals living in a condition, where it is ſcarcely to be imagined how any principle of life could remain. Swammerdam had a larva that ſeemed to live as well in ſalt water as freſh, which Lyonet quotes; and Reaumur had another that lived twenty-four hours in ſpirit of wine. De-Geer, propoſing to kill a lobſter, put it in vinegar: in five hours, it was as lively and vigorous as ever; and, then being put in ſpirit of wine, it died in an hour.

I have purpoſely avoided illuſtrations from the animals that repair their loſt parts, at leaſt illuſtrations relative to them, and reproduce the head when cut off, as ſnails, leeches, and ſeveral worms; for there is a certain vegetative proceſs there going on. At the ſame time, the ſuſpenſion of moſt, or all the organic functions, during ſix or even twelve months that it takes to be renewed, renders it wonderful that the animal is not altogether deſtroyed.

All this violence is committed on animals in their utmoſt vigour, when pulſation is ſtrong, digeſtion powerful, and ſenſation exquiſite. The very

very ſhock would ſeem ſufficient to extinguiſh life. But exiſtence will continue two years, while an active and voracious caterpillar, which perhaps ate its own weight in a day, is ſhut up in the chryſalis, and incapable of feeding; perceptible motion is its only function; digeſtion is at an end. Motion, in other animals, is the firſt ſenſible function. The eggs of the common water ſnail are at firſt tranſparent globules; then a ſpeck appears, which is the fœtus. It continues to increaſe; and, long before burſting the integument, it moves about within the egg.

If the whole of theſe facts are conſidered, and due weight allowed to each, it may not appear ſo very far beyond belief, that all the vital functions of an animal may be ſuſpended without death for a long time, and that they may be again awakened when its organs are brought into a ſuitable ſituation. What is the ſtate of an unimpregnated germ?—Certainly it does not live: Though it proceeds from the mother even at the appointed time, it corrupts, unleſs the prolific liquid is applied. Let us conſider the conſequence. Is there a creation of life, or only the awakening of that which is dormant?—Aſſuredly the creation of life does enſue. The fecundative liquid alone can produce ſo wonderful an effect; it does not awaken dormant life, for their is no evidence, not even a diſtant probability, that any exiſts to be awakened. If life conſiſts in irritability, or without going ſo far, if it is true that it is not extinct, where irritability is manifeſted, what elſe is requiſite, than that the animal ſhall be put in a ſituation where its dormant irritability may be re-excited, that ſome ſtimulus, capable of awakening the ſuſceptibility of its muſcles, ſhall be applied? This, however, generally ſpeaking, is only a caſe of ſuſpended animation; undoubtedly, it is not death, which occaſions a moſt difficult and delicate diſtinction; for by admitting it, almoſt every criterion of real diſſolution is rejected. Suſpended animation may degenerate into death, without any evident external change. A man has been revived who was forty-five minutes in water; two minutes more, or a little accidental delay, might have rendered all efforts to reſuſcitate him ineffectual. The muſcles might have loſt that property which rendered them ſuſceptible of a certain ſtimulus. There may be, nay, it is likely, there are ſtimulants, with which we are unacquainted, that will rouſe the vital powers into action, after a much longer period than has hitherto been witneſſed. The condition of an animal ſuffocated in mephitic vapours, or half ſtrangled, is the ſame; life may be recalled by particular ſtimulants. When torpid from cold, the ſimple application of mo-

derate

derate heat will revive the animal. Where is the term at which death begins, when every ſpark of life is extinct, and all endeavours to reſtore ſuſpended animation will be ineffectual? Though the great phyſiologiſt of Berne fixes it in man when the irritability of the heart is deſtroyed for ever, we muſt neceſſarily admit that it is ſtill unknown. But that vigorous life may be ſuſpended for a long interval, is a fact not to be called in queſtion. Sleep is a temporary ſuſpenſion of the active faculties; ſtill all the organic functions are going on. Torpor is a greater ſuſpenſion. Some of the functions are interrupted, and others imperfectly executed. It bears the neareſt reſemblance to the ſtate of the reſurgent animals under our view, becauſe it may be protracted to an immoderate length by the ſimple continuance of cold. The life of inſects in the chryſalis, of birds and reptiles in the egg, may alſo be long protracted, or the production of the perfect animal accelerated. The chicken exiſts in the egg before incubation, but it depends entirely on heat whether it ſhall ever be unfolded.

Suſpenſion of animation, without becoming death, is leſs repugnant to the mind, when an animal is preſented that will feed voraciouſly, and then abſtain from food for eighteen months or three years. An animal that can loſe its blood, its heart, or its brain, without immediate death, and one that exhibits voluntary motions after decapitation, nay, which will live thus a year, and the head then grow to the place from which it was taken.

Socoloff remarks it as a ſingular circumſtance, that an inſect, immerſed in ſpirit of wine a quarter of an hour, will revive. He threw a number of flies, that had been accidentally drowned, among wood-aſhes, and was ſurpriſed to find them alive. He repeated the experiment, and the flies revived. He had equal ſucceſs with ſome ſmall beetles. The experiment was repeated five times within three hours on a ſmall ſpider, which ſo much weakened it at laſt, that it could ſcarcely recover. Common bugs revived, but they required to be longer among the wood-aſhes. Millipedes would not revive. Earth worms immerſed nineteen hours in oil, which is a fluid moſt deſtructive to all their race, revived when Spallanzani put them in humid earth. Dr Franklin affirms, that flies drowned in Madeira revived after ſix months. Mr Gough made a number of experiments on drowned inſects. None revived, if immerſed longer than two or three minutes, except the nut-weevil, which was in brandy ſeventeen hours. He quotes inſtances

of

of these animals living *torpid*, and not destroyed, though bottled up in a phial of brandy many weeks. It is rational to suppose that some animals may suffer very long immersion in water without the vital principle being extinguished; at least, not nearly so soon as has been supposed.

The success of naturalists in reviving the wheel animal has been very various. Muller never saw it recover after being two minutes dry, unless involved in some terrestrial substance. Fontana revived it after being dry two years. Virey thinks no organised being can dry without death, nor that the complete desiccation of animalcula can happen, unless to their destruction.—Is it possible to conceive, that any portion of humidity remains when an animal has been a-year out of water, its native element, and the whole organic functions interrupted, or that its own fluids are not contracted, hardened, and dried up. Yet, in addition to the instances of animals that have remained dry two, six, or twenty-seven years, and then come to life, it is confidently asserted that the hair worm will revive after long desiccation; and there is an account tolerably well authenticated, though I do not consider it absolute proof, of snails reviving when put in water after they had been kept in a cabinet sixteen years.

 From

From obſervations made by myſelf, I have great reaſon to ſuppoſe ſome ſpecies of ſnails are reſuſcitant, and that ſeveral infuſion animalcula enjoy the ſame privilege.—Plants are undoubtedly a kind of organiſed beings, whoſe life and propagation bear the neareſt reſemblance to thoſe of animals. In a late experiment, the water lentil revived thirty-three months after it was dried. Moſſes, it is ſaid, have revived after an hundred years deſiccation, and ſeeds preſerved their vegetative faculty two hundred years.

Thoſe who have been unſucceſsful in reviving reſurgent animals, have either neglected the neceſſary conditions, in the ſame manner as the uſe of one ſtimulant will excite vitality, while the want of it, or employment of another, will allow ſuſpended animation to terminate in actual death; or they have not attended to the proper ſpecies of animals. But we are ſafe to conclude, that the vital functions may be ſuſpended incredibly long, and the animal ſtill revive, and that there are animals which may exiſt years in complete deſiccation, without the principle of life being loſt. All this is only ſuſpended animation; it is not death. May there be ſuch a thing as a ſecond creation of life? If the life of the impregnated germ is created, perhaps the ſame creation, if ſuch an idea can

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be admitted, happens to the animals whofe animation we conceive to be only fufpended.—Should it actually be fo, their ftate is abfolute death.

Where is the immaterial and fentient principle during this long interval of corporeal repofe? Is it annihilated and again renewed, or does it exift totally independent of the ftate into which the material part of organization may be brought?

V. Origin of Mould.—The obfcurity which reigned over the origin of animated nature has been diffipated by the penetration of philofophers. Abfurd and contradictory ideas were formerly embraced by thofe who called themfelves learned. Adopting a hafty opinion from a curfory glance, and without time for inveftigation or judgment to difcriminate, they hurried on into error; and at length became fo deeply entangled, that to be extricated was impoffible, unlefs by totally abandoning received theories, and entering on experiment, which is the only infallible guide to truth.

The uniformity beheld in the generation of the larger animals, the mutual intercourfe of fexes, and particular periods of geftation, were facts fo evident and undeniable, that any doubt or queftion concerning their exiftence proved itfelf falfe. But it was different with animals more minute. Butterflies and bees appeared without any producing parent; myriads of wafps and flies were ſeen about the mud of rivers and putrid carcaſes; whence, then, did they originate? Some ſuppoſed them to come from one thing, ſome from another, from mud, lime, ſhells, or, as it would ſeem, whatever the animals were neareſt to at the time; but the great ſource of all was believed to be putrefaction; that, by general concurrence, was admitted to be the origin of inſects, and many other animated beings. It is inconceivable how long this opinion was maintained. Ocular demonſtration of the reverſe was hardly judged conviction, and treatiſes were even written in its ſupport. Others, more watchful of the progreſs of nature, ſaw an egg produced by an inſect; they kept it till a worm came forth; and preſerved the worm until, by a complete metamorphoſis, it changed into a winged animal. Thus was the real origin of an immenſe part of the creation aſcertained.

It is not ſo ſurpriſing that the ſpontaneous generation of plants alſo gained credit in the fulleſt extent. The ſeeds were either ſo minute or ſo difficult to be found, that they eluded the moſt accurate ſearch, and in this manner gave foundation to conclude that there were none. Now is it univerſally received, that the origin of every plant muſt be from ſome part of another plant; and

and it was probably the inability to diſcover that part, or to recogniſe the ſeeds, that allowed the belief of ſpontaneous generation. All this was applied to the very minute plants of mould. The ſeeds were inviſible, or not recogniſed : eminent philoſophers of modern date thought they had none, and advanced cogent arguments for ſpontaneous production of the plants. It is, indeed, very difficult to account for the exiſtence of ſeeds in certain ſituations, unleſs by admitting that, like the germs of animalcula, they are univerſally diſperſed, and, falling every where without any law, expand in thoſe ſituations only which are congenial to their nature. One of the niceſt points to be inveſtigated in the natural hiſtory of mould conſiſts in examining the places where ſeeds are found.

When a minnow or a leech dies under water, even ſeveral inches from the ſurface, and has ſeldom or never come there but for the purpoſe of reſpiration, it will ſoon be covered with a peculiar kind of long ſlender mould, while lying at the bottom, as I have repeatedly ſeen. Various ſpecies, ſome thicker, others more buſhy and ſhorter, grow on different animal or vegetable ſubſtances alſo under water : it then ſeems more luxuriant, which is a general remark to be made of

mould

mould where it imbibes moſt nutriment. Whence does this mould originate? Does the fiſh, or whatever it is, receive the ſeeds of the plant the moment it comes in contact with the air? Do they adhere to it without being waſhed off by the water, until the conditions neceſſary for their expanſion concur in its death and tendency to putrefaction?—Is it more reaſonable to ſuppoſe the ſeeds are diſperſed through the water, continually ready to be attached to the ſubſtances preſented, to expand and produce their plant. Philoſophic botaniſts may decide which of theſe opinions is the more probable. But it would rather appear, that the ſeeds of mould are diſperſed in water; for I have kept many aquatic animals, which I can hardly think came to the ſurface during a long time, and if they required air, perhaps extracted or received it from the ſurrounding water: and, after death, luxuriant mould grew upon them. The thickneſs of this aquatic mould has been conſiderable, but never equally ſo as mould growing in the open air, which, it may be, ariſes from the difference of ſpecies more than from privation of circulating air. There is no reaſon why mould ſhould not thrive in water juſt as well as many plants, whoſe natural aliment is there, and abſorb air in the ſame manner; becauſe there are facts which ſeem to prove the ex-

iſtence

iſtence of certain ſpecies of aquatic mould that are not to be found on terreſtrial ſubſtances.

Mould is ſometimes ſeen in nuts and other ſhell fruit, without any viſible opening. In this caſe, it has, perhaps, penetrated the parts of fructification in an early ſtage, or while the plant was in flower; for it is unlikely that the ſeeds, however minute, will penetrate a thick ſhell where pores do not abound. They may lie in the fruit till an opportunity occurs for their developement. Theſe examples, it is true, are attended with great difficulty; but there are certainly methods of explaining them, though ſtill undiſcovered, unleſs we recur to equivocal generation, which is abſurd.

Another theory may be conceived, which is the moſt probable of any. The ſeeds of mould may be diſſeminated in ſuch abundance as to enter into the compoſition of all animal and vegetable ſubſtances. They find a receptacle in the external pores, or enter within the body itſelf. If a ſlice of moiſtened bread is put under a very ſmall receiver, it will ſoon be covered with as many plants as would require millions of ſeeds for their origin. Six ſquare inches of flour and water boiled into paſte, and put under a receiver, not above eleven or twelve inches in capacity, have

ſhown

ſhown a vaſt variety of mould both in colour and ſpecies. Some long ſlender filaments crowned by a globule, ſome thick buſhes with fruit on every branch, ſome very ſhort, and conſiſting almoſt of duſt. Then in one place a green ſpot; in another, lead colour, blue, red, or yellow. Whether is it more probable, that all the ſeeds, producing this infinite variety, have fallen from the air of the receiver, that they are communicated by the moiſtening liquid, or form a part of the bread and flour; the laſt ſuppoſition is not the moſt difficult: it is the readieſt method of accounting for the extraordinary abundance and univerſal exiſtence of mould.

But wherefore all this anxiety for framing theories. Let us be better acquainted with the kingdoms of nature; and, by deep inveſtigation, endeavour to become intimately acquainted with the real properties of plants and animals.

To deſcribe the prodigious variety of mould would require a volume: it would be a methodical ſyſtem of botany. A fleece of long white mould will cover one ſubſtance; and it is ſometimes ſo ſhort as to have hardly any ſenſible length. The ſtalk of a few ſpecies is articulated like a ſtalk of corn. In general, it is ſtraight, ſlender, and tranſparent. Sometimes it conſiſts

of

of alternate angles, and at each is a globule; or it is intricate and confuſed below, but with fruit ſurrounding the extremities. The figure of the globule is various; ſome are ſpherical, ſome flattiſh, and others a perfect cone.

All mould certainly ought not to be claſſed among fungous plants. I have ſeen ſeveral ſpecies which rather appeared to belong to graſſes: and oftener than once, I have thought flowers were perceptible. The Cryptogamia formed an immenſe pit, into which every plant of obſcure origin, little known, or imperfectly deſcribed, was thrown. It only requires attention and experiment to diſcover the real properties of bodies, and thus to aſſign them their proper place. It appears indubitable that ſome kinds of mould belong to genera of plants whoſe larger ſpecies are well known; but there may alſo be many which exhibit peculiarities to be found in none other. The knowledge of this beautiful part of botany is in its infancy: diminutive objects are too apt to be conſidered as undeſerving of notice; but to the philoſopher all the works of nature are pleaſing and intereſting.

ANIMAL REPRODUCTION.—In all the field of natural hiſtory there is no ſubject more extraordinary than animal reproductions. Generation

neration itſelf is hardly ſo wonderful. There, indeed, by the laws of organization, the germ of an impregnated egg unfolds. Nutrition is ſupplied, and all the parts expand until the final term of increment arrives. This is an immutable ordination, and belongs to every animal alike that propagates by a foetus or eggs. If a limb is deſtroyed after attaining its full ſize, in numberleſs inſtances, it is loſt for ever; nay, the remaining trunk in general withers and decays.—But there are ſome animals ſingularly privileged by nature. Let a member be torn off; the wanting limb is in the ſame ſtate as before the act of the parent animals in generating and fecundating the germ: it is even leſs in exiſtence; for the original germ of the defective part is alſo deſtroyed. Yet, not only will a new generation enſue, the amputated portion be produced as complete and entire as what was taken away, but, unlike the firſt evolution, which was of all the parts, this will acquire the ſize, figure, and motion, peculiar to itſelf, independent of the reſt of the body.

In whatever lives there is a powerful tendency to vegetation, and heat is one great ſource of exiſtence. Animals are more lively, their increaſe is more rapid, and eggs more numerouſly hatched, according to the external heat that prevails; and the reverſe ſucceeds with cold. The ſame

ſame is an invariable rule with the reproductions of mutilated members.—Two general claſſes of animals are the cold and warm blooded. The latter always remain nearly at the ſame degree of heat, that is, commonly between 96° and 104°; though the ſurrounding medium ſhould be very far above or below theſe limits. Thoſe with cold blood are not far from the temperature of the ſurrounding medium; and if it increaſes to a great degree, or falls very low, a material change is produced on their ſyſtem. Theſe animals ſeem to enjoy ſeveral eminent prerogatives reſpecting their individual ſafety; and among others, that of reproduction. Few material parts of warm blooded animals are regenerated; while of ſome cold blooded animals, there are few that are not reproduced. But heat is equally eſſential to this regeneration as it is to propagation and excluſion. Although we divide animals into cold and warm, *cold* is but a relative expreſſion; for, in the total abſence of heat, life would be extinct; and we always ſee it deſtroyed by a conſiderable degree of cold.

Even in warm blooded animals, there are important reproductions continually going on, perhaps from the moment of birth until death. The fluids are continually waſted and repaired. The

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infenfible parts, the hair, the nails, the ſkin, are conſtantly falling off and produced again. Some parts regenerate only once or twice during life; others hardly ceaſe before the reproductive faculty is loſt for ever.

Several parts of the body are acquired after birth. Then the teeth of mankind ſeldom exiſt. In a few years, thoſe firſt expanding are loſt and replaced by others, which remain ſerviceable until decayed by time. Sometimes, at a very advanced age, a new ſet appears, even at an age which few live to attain; and theſe, whoſe *germs* have exiſted a century in the perſon, (if germs or organs are not created,) are as ſound and entire as in the moſt vigorous and youthful. The hair grows rapidly after birth; it is continually deſtroyed and conſtantly reproduced, and in ſuch a manner that it will frequently branch aſunder. This is the common courſe repeatedly in a year; but it is very remarkable, that, after ten or twelve years, it begins only then to grow on particular parts of the body: and after it ſprings, there ſeems to be very little reproduction. The nature and properties of this ſubſtance are as yet but imperfectly underſtood. The renewal of the ſkin is almoſt inſenſible; it comes off in minute ſcales. One inſtance occurred where the whole ſkin of both hands, up to the wriſt, was thrown

off

off like a pair of gloves, frequently in a year, without any material inconvenience attending the change.

Were it poſſible to aſcertain the fact, I ſhould ſuppoſe that many parts of the human body would reproduce. The ſides of a wound unite, and pieces of fleſh are always regenerated. There is much reaſon to think that ſome part of a mutilated limb might be reproduced, was it not from the methods practiſed in healing the wounds. Examples of human reproductions are numerous. An inſtance lately occurred of a child born with two thumbs on one hand, and each with a perfect nail. When the child was three years old, the ſupernumerary thumb was amputated; however, it grew again, bearing a nail. It was cut off a ſecond time, yet a third thumb like the firſt was reproduced.

The ends of fractured bones will grow; nay, entire bones have ſometimes been regenerated. The head, neck, and part of the os humeri, we are aſſured, have been repaired. The complete bone of the leg, between the joints, was extracted by an operation; it was afterwards fully repaired. The whole has been caſt off by expoliation, and a new bone grown in its place.

There is a remarkable ſympathy often beheld between ſome parts of animals that are continually reproducing, and thoſe which never regenerate. No part is more conſtantly reproduced than the beard; its progreſs is frequently perceptible in a few hours; but caſtration puts an end to it for ever. Many animals reproduce the horns every year; but caſtration of the animal without them prevents their growth: and it is ſaid, the caſtration of an adult male prevents them from falling again. This operation produces a ſingular effect on the frame: a viſible change enſues; the increment of certain parts terminates, while others extend to an immoderate ſize. Even the mind is materially affected.

Throughout moſt of the animal ſcale, there is a general reproduction of the parts which are of a ſimilar nature. The ſkin, nails, hair, feathers, horns, and ſcales, have a great analogy to each other. Some of theſe, which, in particular animals, are renewed by a partial and continual change, are loſt and repaired by others all at once. Serpents and caterpillars caſt the whole ſkin. A caterpillar will in a ſhort time reproduce ſix or ſeven,—within a few months. The lobſter annually throws off its ſhell, and acquires a new one; and, with the ſhell, it alſo loſes the ſtomach. The feathers of birds are changed once a year; but feveral males acquire a long and beautiful tail only during the feafon of their amours, which afterwards falls off. The hair of thofe quadrupeds that inhabit the northern climates grows in much greater quantity as the rigours of the feafon become more intenfe. The fcales of fifhes, according to fome naturalifts, are yearly renewed; but, in the opinion of others, they only acquire an additional ring. Certain horns alfo grow by an additional fheath periodically acquired.

In moft of the warm blooded animals, the inevitable confequence of mutilating important organs is death; even unfkilful treatment of thofe that appear of lefs confequence is generally attended with pernicious effects. The fhock fuftained, the pain they endure, and the lofs of blood, all combined, have a fatal tendency, which ends in total deftruction. The difference is incredible in the mutilations of animals which do not poffefs much internal heat. Not to fpeak of the polypus, which is well known to enjoy the property of living though cut in pieces, and each becoming a new and complete animal, there are fome other animals that enjoy prerogatives nearly as great; and many which furvive the utmoft violence committed on the body. The claws of lobfters torn off will grow again. The legs

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and tail cut off a water newt will be reproduced. Loſing the head is not a mortal mutilation. I have ſeen one of the *planariæ* regenerate a new head in fourteen or ſixteen days; and the ſevered head acquires a new body. Indeed, the reproductive faculties of this animal are almoſt equal to thoſe of the polypus, and perhaps will prove a more intereſting ſubject for experiment. Indiſputable evidence proves, that the ſnail will acquire a new head in place of the old one cut off, and that it regenerates almoſt every other part of the body. Certainly it is a moſt extraordinary property, to reproduce one of theſe organs commonly reputed the moſt important in the preſervation of animal life. But we have already ſeen, that thoſe organs which are of the utmoſt conſequence to one animal are not ſo to another; and that they may be wounded, mutilated, or deſtroyed, without death infallibly following. The reproductive faculty is not exhauſted by a ſingle regeneration: if the ſecond head is ſevered, a third will come in its place; and if this alſo is cut off, another will grow.

The ſea anemone, ſome ſpecies of which are ſeveral inches in diameter, and which realiſes the ancient fable, applied to another animal, of producing its young by the mouth, poſſeſſes the reproductive property in a degree little inferior to the polypus. If the earth worm is cut in pieces, each

will

will live and produce a new worm. Some ſingularities ariſe from theſe mutilations. If the two ends are cut off a worm, the middle piece will become a perfect animal; but the head will not germinate from what we ſhould ſuppoſe the natural place, that is, the anterior part; on the contrary, it proceeds from the portion that was next the original tail; and the new tail will grow from the anterior part of the trunk. It has been ſuppoſed, that the difficulty of eradicating that terrible ſcourge of mankind, the tenia, is owing to the prompt and ſudden reproduction of mutilated parts. Numbers of marine animals, ſuch as aſteriæ, meduſæ, even fiſhes, enjoy this property; and it is thought that it deſcends to the animalcula of infuſions. In ſhort, there is every reaſon to believe that it is widely diffuſed through the animal ſcale; and, if experiments could be made without endangering life, or inducing diſeaſe, that its empire would be found far more extenſive than poſſibly can be conceived.

The firſt obſervations on animal reproduction were readily credited, becauſe they related to parts without which it was known an animal might exiſt; but when ſuch obſervations were extended to the organs moſt material and important to life, and when the loſs of theſe daily teſtified that the animal inevitably periſhed, an unu-

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ſual degree of ſurpriſe and diſtruſt aroſe. When the head was ſevered from the body, either intentionally or by accident, every known animal was deſtroyed. Therefore, the aſſertion that a new head would grow in place of the old one, naturally met with diſcredit. Intelligent perſons reaſoned from what had happened, and not from what might enſue. Certainly we can hardly call their heſitation culpable : and it has tended to one good conſequence, namely, the eſtabliſhment of the fact without diſpute. But, in the proſecution of theſe experiments, diſtruſt will gradually wear away : when we ſee the fins, fleſh, bones, claws, feet, eyes, and jaws of an animal regenerated, and behold an animal ſurvive after being divided in pieces, it is not ſo repugnant to think that it may reproduce the head.

If there is any reaſon why certain animals enjoy this ſingular prerogative in its full extent, it is yet unknown. Thoſe hardy philoſophers, who are irreſiſtibly impelled to account for every phenomenon they behold, are continually deceived ; and in nothing is it more eminent than their arguments, once eſteemed plauſible, for the reproduction of loſt parts. Here there is an immenſe ſource of information to be laid open to phyſiologiſts, both with regard to the regeneration of important

important organs, and the diſcovery of facts reſpecting the mental powers. But in all experiments and reaſoning, it will be found that nature purſues a certain determinate courſe; if there are deviations from it, they are accidental; and the ſame uninterrupted and invariable line will be reſumed, whenever the cauſe obſtructing it is removed.

TRANSLATOR.

OBSERVATIONS AND *EXPERIMENTS*

ON THE

ANIMALCULA OF INFUSIONS.

CHAPTER I.

WHETHER, ACCORDING TO A NEW THEORY OF GENERATION, ANIMALCULA ARE PRODUCED BY A VEGETATIVE POWER IN MATTER. INFUSIONS AND INFUSED SUBSTANCES EXPOSED TO HEAT.

NOTHING is more common with philosophers who have invented any theory, or given a new form to one already eſtabliſhed, and univerſally known, than to republiſh it on ſome other occaſion, corrected, improved, or illuſtrated, with additional information. If we would review our diſcoveries, if we would examine them profoundly and with impartiality, we ſhould in general find defects unnoticed before, which ariſe from

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the want of connection in sentiment, from the want of a necessary and laudable perspicuity, or because they are discordant with more recent discoveries.

A certain vegetative power some have conceived to reside in matter, appropriated to the formation and regulation of organised existence; that by it are the numberless combinations of the animal machine effected; the operation of nutrition and perspiration, the variety of constitution, the animal appetites and dimensions of the human frame. By the same means has it been explained why a blind or a maimed person may have children vigorous and entire; because the vegetative power will restore to them the members defective in the parent.

Not only has it been supposed to be destined for the organization of matter in animated beings, but that it might change an animal to the vegetable state, and the vegetable again to an animal; that it acts on plants while living, and when dead regenerates them in new beings; these are the animalcula of infusions, which cannot strictly be called animals, but beings simply *vital*.

One proof adduced in support of this hypothesis, is derived from th. origin o animalcula. We are told they must either come from specific seeds, or be produced by the vegetative power; that

that the firſt cannot take place, becauſe they are found in cloſe veſſels ſubjected to the action of heat, equally as in open veſſels, whereas the included germs, if there were any, ought not to ſurvive. Therefore, they muſt originate from the vegetative power alone. Nothing has been omitted to obtain favourable arguments for this opinion, and to give it that clearneſs, elegance, and ſimplicity moſt likely to gain converts.

Nineteen veſſels, containing infuſed ſubſtances, were hermetically ſealed, and kept an hour in boiling water. Being opened at a proper time, not a ſingle animalcula was to be ſeen (1). To this experiment of mine, it was objected that the long continuance of heat had perhaps entirely deſtroyed the vegetative power of the infuſed ſubſtances, or materially injured the elaſticity of the air remaining included in the veſſels; thus, it was not ſurpriſing if animalcula did not appear.

To eſtimate the weight of theſe objections, I conceived an experiment apparently deciſive; which was, to make nineteen infuſions, and boil ſome of them a ſhort time, others longer, and the reſt very long. If it was founded, the number of animalcula would be leſs according to the duration of boiling, if not, the number would be alike in all caſes.

(1) Saggio di oſſervazioni microſcopiche.

Vegetable ſeeds, being the moſt fit for producing animalcula, were preferred to other ſubſtances, and thoſe that never failed to produce them though they had experienced the influence of heat. White kidney beans, vetches, buck-wheat, barley, maize, the ſeeds of mallows and beets were infuſed; and, that the experiment might be the more accurate, I endeavoured as much as poſſible to take each ſpecies of ſeed from the ſame plant. As the yolk of an egg in maceration abounds with animalcula, one was alſo infuſed.

Experiment has demonſtrated, that the heat of boiling water is not always the ſame, but greater, if the atmoſphere is heavier; and leſs, if lighter: therefore, water will acquire more heat at one time than another, which will be proportioned to the ſtate of the atmoſphere. In this, and my other experiments, the ſeven different kinds of ſeeds, and the yolk, were all boiled an equal time, that they might acquire the ſame degree of heat. Here the experiment was diverſified, by boiling a certain quantity of each infuſion half an hour; another quantity, an hour; a third, an hour and a half; and a fourth, two hours. Thus, four claſſes of infuſion, and the egg, could be formed. The ſame water, in which the ſeeds had boiled, was taken for the infuſions, and what had boiled half an hour alone taken for the

the ſeeds that had boiled half an hour. The like proportions of time were preſerved in the water for the other three claſſes of infuſions; that is, an hour, one and a half, and two hours.

Each of the four claſſes was marked with a different number, to avoid all hazard of confuſion or error: and, becauſe an equal temperature was moſt eſſential, all were depoſited in the ſame place. The veſſels, containing the infuſions, were not hermetically ſealed, but looſely ſtopped with corks; the only object of this examination being to diſcover, whether long protracted ebullition would prejudice or deſtroy the property of infuſed ſubſtances in producing animalcula; if it did, there would be no difference whether the veſſels were open or cloſe.

The examination of one, or of few drops, will often induce an obſerver to ſuppoſe the infuſion quite deſerted, or very thinly inhabited, while the obſervation of many drops proves it to be otherwiſe. I was not content with one drop only, but uniformly took a conſiderable number from each infuſion.

The ſurface of infuſions is generally covered with a gelatinous ſcum, thin at firſt, and eaſily broken, which, in proceſs of time, acquires conſiſtence. Here, animalcula are always moſt numerous, as may be ſeen by a method I have conſtantly practiſed, examining with a magnifier a portion placed in a ſtrong light.

Where the animalcula are minute, or rare, the thickneſs of the infuſion often prevents the obſerver from diſtinguiſhing whether any are there or not. It is then neceſſary to dilute the drops with water. Elſewhere it has been remarked, that diſtilled water was taken to make the infuſions; common water might introduce ſome latent animalcule (1). In the courſe of theſe obſervations and experiments, diſtilled water has alſo been employed for dilution, when required; and, for greater ſecurity, examined with a magnifier before being uſed. In particular caſes, the accidental concealment of a ſingle animalcule might vitiate the truth of the experiment.

I conceive it my duty to mention precautions ſo eſſential, and to put it in every individual's power to judge not only of the experiments and obſervations themſelves, but of the mode of conducting them in matters ſo nice and important.

On the 15 of September, I made thirty-two infuſions; and on the 23 examined them for the firſt time. Animalcula were in all; but the number and ſpecies different in each. In the maize infuſions, they were ſmaller, and proportionally more rare, according to the duration of boiling.

From

(1) Saggio o diſſertazione citat. Cap. 4.

From this it may ſeem, that although long continued heat had not prevented the production of animalcula, it had contributed to diminiſh the number, or alter the kind. But with the reſt of the infuſions it was otherwiſe: the kidney beans, vetches, barley, and mallow ſeeds, were in a better condition, after ſuſtaining the violent impreſſion of heat two hours, than thoſe that had been expoſed to it leſs. Let us enter on that detail which the ſubject merits.

In the infuſion of kidney beans, boiled two hours, were three ſpecies of animalcula; very large; middle ſized; and very ſmall. The figure of the firſt, partly umbellated and attached to long filaments dragged along in their progreſs; the ſecond were cylindrical; and the third, globular. All three were incredibly numerous.

In the infuſion boiled two hours, were animalcula of the largeſt and ſmalleſt claſs, but few in number; ſtill fewer, in that boiled an hour; and feweſt of all, in that boiled half an hour.

The infuſion of mallows, boiled two hours, produced middle ſized circular animalcula; and ſome very large, with the head extremity hooked. In two infuſions, boiled an hour, and an hour and a half, the number and ſpecies were the ſame: and though they might be ſurpaſſed by thoſe of the infuſions boiled two hours, ſtill they

 were

were much more numerous than in thoſe boiled half an hour.

In vetches, boiled half an hour, was an immenſe number of ſemicircular bell-ſhaped animalcula, all of conſiderable ſize, while in thoſe boiled an hour and a half, they were ſmall and rare. Some bell-ſhaped animalcula might be ſeen in an infuſion boiled an hour, but it gave the eye pain to diſcover a few, and theſe moſt minute, when it had boiled only half an hour.

Thoſe in a barley infuſion boiled two hours were numerous beyond deſcription, and large; part of an elliptic figure, others oblong (1). The infuſions boiled an hour and a half had but a moderate number of animalcula very minute; and ſome appeared when boiled half an hour.

There was no fixed rule with the remaining infuſions. In buck-wheat boiled an hour and a half were many more animalcula than in any other infuſions of it. This alſo happened in the egg and beet ſeed boiled an hour; but it is to be remarked, that fewer animalcula were in theſe two infuſions boiled half an hour than in any of the reſt.

Hitherto, the figure of theſe legions of animalcula has been curſorily alluded to. A circumſtantial

(1) Probably the author means different kinds of ellipſe. At the ſame time, there are animalcula, though few, nearly of an oblong figure. Moſt of the deſcriptions in all this Tract are ſo general, that it is difficult to aſcertain the exact ſpecies of animalcula.—TRANSLATOR.

ſtantial account is in my Diſſertations, and it will be ſpoken of more at large in the courſe of the Tract.

Thus, it is clearly evident, that long boiling of ſeed infuſions does not prevent the production of animalcula; and, notwithſtanding the maize does not ſeem to favour it, four infuſions ſtrongly corroborate the fact.

What is the cauſe that infuſions boiled leaſt have feweſt animalcula? I cannot think myſelf miſtaken in aſſigning the following reaſon. That animalcula ſhould appear, it is neceſſary that the macerating ſubſtances give ſome indication of the diſſolution of their parts; and, in proportion as diſſolution advances, at leaſt for a limited time, the number of animalcula will increaſe. The uniformity of this has been ſhewn in another place, and would be confirmed, was it requiſite, by further experiments and obſervations, in theſe new inquiries. Now, as ſeeds have boiled a ſhorter time, ſo are they leſs inveſted and penetrated by the diſſolving power of heat; therefore, when ſet apart to macerate, they are not ſo ſoon decompoſed as thoſe longer boiled. Thus, there is no occaſion for ſurpriſe if ſome infuſions ſwarm with animalcula while others have very few: And this I do believe the reaſon why, when two infuſions are made at the ſame time, one of unboiled, the other of boiled ſeeds, animalcula are frequently

quently obſerved much ſooner in the latter than in the former. A little boiling will not decompoſe vegetable ſeeds, for decompoſition is effected by ſlow and gradual maceration.

Some days after theſe experiments, the number of animalcula always became greater; and towards the middle of October increaſed ſo much, that each of the thirty-two infuſions was equally ſwarming. The only difference was in ſize, figure, and motion: I enjoyed this pleaſing microſcopic ſcene uninterrupted until the 10 of November; and it might have amuſed me longer had I continued to examine the infuſions.

It ought not to be omitted, that experiments exactly ſimilar were ſoon afterwards made with peaſe, lentils, beans, and hemp ſeed. Except in the beans, the reſult ſo far correſponded, that a greater number of animalcula appeared in the infuſions that had boiled moſt.

It is a fact eſtabliſhed by the univerſal concurrence of philoſophers, that, after water has come to the ſtate of ebullition, it cannot acquire a greater degree of heat, however much the action of the fire may be augmented, provided it can evaporate. Therefore, when I ſay the ſeeds boiled longeſt have acquired greater heat, I mean it to be underſtood in *time* and not *intenſity*, by ſuppoſing that the duration of boiling encreaſed the

the intenſity of heat the ſeeds would be expoſed to.

Recourſe was had to another experiment to learn whether an encreaſe of heat would obſtruct the production of animalcula. The eleven ſpecies of ſeeds were ſlowly heated in a coffee roaſter till they became pretty well roaſted, and eleven infuſions formed of them with water previouſly boiled as uſual. But this heat, ſo much more intenſe, neither prevented the origin of animalcula nor leſſened the number. They were rare at firſt; but about the middle of October, that is, twenty days after making the infuſions, the fluid was ſo full as abſolutely to appear animated.

The conſtancy of their appearing even here, excited my curioſity to augment the heat ſtill more. The ſeeds were burnt and ground the ſame as we burn and grind coffee. Of the duſt, which reſembled ſoot, I made as many infuſions as different kinds of ſeed: likewiſe, an infuſion was made of the yolk of an egg, which by the thermometer had ſuffered 279° of heat (1). What followed?

(1) The author uſed Reaumur's thermometer in all his experiments. As Fahrenheit's is the only thermometer uſed in this country, the degrees of heat are here reduced to his ſtandard, 2.25 of Fahrenheit being equal to 1° of Reaumur. The fractional parts of the former are not given, both becauſe experiments can ſeldom be made within

followed? Animalcula equally appeared in these infusions, only a little more time elapsed before they became so numerous, because the weather was colder; and they uniformly inhabit infusions sooner or later according to the temperature of the atmosphere.

Vegetable seeds were exposed to trials more severe: they were exposed to the greatest heat that can be excited by common fires, or fire augmented by art. Burning coals, and the flame of the blow pipe, were the two agents exercising their power on them. And, in the first place, I kept them on an iron plate above burning coals until entirely consumed by the violence of the flames, and converted to a dry cinder, which was reduced to powder, and as many infusions formed as there were seeds. A cinder was also made by the blow pipe, which, besides excessive aridity, had acquired considerable hardness. I must acknowledge I did not in the least expect to find animalcula in this new infusion. After viewing them once and again, hardly able to credit my eyes, I repeated the experiment twice. Some suspicion arose that the animalcula might come from the water used rather than the burnt seeds; therefore, on repeating the experiment, the same as

in parts of a degree, or thermometers to agree exactly, and because the difference here, where there is any, never exceeds .25 of 1°.—T.

as what formed the infuſions was put in other veſſels. Both times, however, they re-appeared in the burnt ſeeds, while not one was ſeen in the water.

Theſe facts fully convinced me, that vegetable ſeeds never fail to produce animalcula, though expoſed to any degree of heat; whence ariſes a direct concluſion, that the *vegetative power* is nothing but the work of imagination; and if no animalcula appear in veſſels hermetically ſealed and kept an hour in boiling water, their abſence muſt proceed from ſome other cauſe.

CHAP. II.

WHETHER THE INFLUENCE OF HEAT DIMINISHES THE ELASTICITY OF AIR INCLUDED IN VESSELS HERMETICALLY SEALED; AND IF IT WOULD BE AN OBSTACLE TO THE PRODUCTION OF ANIMALCULA.

THIS inquiry may be reduced to two heads: *Firſt*, By ſubjecting a given number of veſſels hermetically ſealed to heat ſo regulated that they might ſuffer different degrees, and obſerving if the production of animalcula is obſtructed or altogether

altogether prevented, by increaſing the heat. *Secondly*, Whether the increments of heat prove the diminiſhed elaſticity of the air.

For convenient examination of both, eleven veſſels, containing eleven kinds of the ſame vegetable ſeeds as before, were hermetically ſealed. But, to proceed with due caution, it was eſſential that the included air ſhould undergo no ſenſible rarefaction in ſealing with the blow pipe, and not loſe its elaſticity, which would aſſuredly happen if the veſſels were ſealed without any further preparation, by the flame ſurrounding and ſoftening the neck: for ſuch powerful heat, after communicating from the neck to the belly, could not but expel great part of the included air, whence the part remaining behind would become more or leſs rarefied, and more or leſs elaſtic accordingly. Indeed, when the hermetic ſeal is broken, after the veſſel cools, a faint hiſſing is almoſt always heard, which proceeds from the air eſcaping by the orifice: and that this is the fact is certain, by applying the flame of a candle near the ſeal; when broke, the flame is driven from the aperture, and ſometimes actually extinguiſhed. If the ſeal is broken when the veſſel is inverted in water, the water ſuddenly riſes above the level of what ſurrounds the veſſel; a moſt ſatisfactory proof of the internal air being more rarefied than the external. To avoid this inconvenience, the

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neck of the veſſel was drawn out at the lamp almoſt to a capillary tube: the ſmalleſt part was then inſtantaneouſly ſealed, ſo that the internal air could ſuffer no alteration, as was evident from no hiſſing being heard on breaking the veſſel.

After aſcertaining that the included air was of the ſame denſity as the atmoſpherical, it was neceſſary, before expoſing the veſſels to heat, to inveſtigate, whether ſimple incluſion of the ſeeds would obſtruct the production of animalcula: Was it ſo, it could be aſcribed neither to heat nor air, but the cloſeneſs of the veſſels alone. Other experiments had rendered me cautious here(1). They had taught me, 1. Infuſion animalcula are not produced in veſſels hermetically ſealed unleſs the veſſels are capacious; 2. They are not always produced; 3. They are never ſo numerous as in open veſſels. I now felt the neceſſity of ſuch circumſpection, and although it was uſed, two ſubſtances, kidney beans and peaſe, ceaſed to produce animalcula. The other nine ſeeds produced a moderate number. To theſe nine only, I confined myſelf, and ſubjected each to heat in the following manner. Nine veſſels, hermetically ſealed, containing ſeeds, were immerſed in boiling water half a minute; other nine, a minute; nine more, a minute and a half; and nine, two minutes. Thus I had thirty-ſix infuſions,

(1) Diſſert. Cap. 10.

fufions, the feeds in which were expofed to the heat of boiling water. To difcover the proper period for examining the fealed veffels, fimilar infufions were at the fame time made in open veffels; and, when animalcula abounded there, thofe fealed up were vifited. In eleven days, the nine open infufions being full of animalcula, it feemed fit to examine the others. On breaking the firft hermetic feal, a faint hiffing was heard, not unlike that mentioned above. Then it did occur to me that heat had truly injured the elafticity of the internal air, and excited me to obferve, with the utmoft attention, what happened on breaking the feal. The hiffing was obvious in all; but I foon difcovered it arofe from an oppofite caufe, namely, encreafed elafticity of the air. In the firft place, the flame of a candle was driven from the orifice; fecondly, on juft touching the fealed part with a file, it twice fprung more than a fpan from the veffel; in the third place, on making the infufion flow towards the feal, and then breaking it, the infufion violently fpouted out; fourthly, the feal being broken under water, inftead of running into the veffel, the water was forced away. Reflecting on the nature of macerating fubftances, I faw it could not be otherwife. Vegetable feeds are well known to contain a great quantity of air. During

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ing their diſſolution by heat it muſt be extricated, and in this way render the portion included more elaſtic. I do not deny, however, that this encreaſ-ed elaſticity may partly ariſe from an elaſtic fluid, diſcovered in vegetables, of a nature different from the atmoſpherical fluid.

To return to my microſcopic examination of the ſealed infuſions. It gave me great ſurpriſe to ſee heat, ſo very far inferior to that mentioned in the preceding chapter, had obſtructed the origin of our animalcula. Some infuſions were an abſolute deſart, and others reduced to ſuch poverty as only to afford ſo many wandering animalcula like points, and hardly perceptible (1). Let the reader figure two lakes; in one are fiſhes of every ſize, from a whale down to the ſmalleſt; while in the other are a few minute fiſhes, not larger than ants, and he will have a ſenſible idea of the animals appearing in the open infuſions and thoſe that appeared in the cloſe. I was particularly ſurpriſed how the heat of half a minute had been as injurious as the heat of two minutes. Thoſe inexpreſſibly

 minute

(1) *Monas Termo.* Muller, *Animalcula Infuſoria*, p. 1. Hauniae, 1786.—This is a ſyſtematic work on the animalcula of infuſions, where the indefatigable author has deſcribed 378 ſpecies of theſe ſingular beings. The reader will find a number of ſynonyms, preceding each deſcription, if the animalcula has come under the view of other authors.—T.

minute animalcula were produced from beans, vetches, buck-wheat, mallows, maize, and lentils. Whatever attention was beſtowed on the other three infuſions, I could never diſcern the leaſt indication of animation.

From this ſeries of experiments I concluded, that the heat of boiling water half a minute was fatal to large, middle ſized, and ſmall animalcula, which, to adopt the energetic expreſſion of my illuſtrious friend M. Bonnet, I ſhall term animalcula of the *higher claſs*; while the ſame heat, protracted two minutes, did not affect the infinitely minute animalcula, which I ſhall term the *lower claſs*. Here two problems occurred: Whether the continuance of boiling longer than two minutes would prevent the origin of the lower claſs; or, if diminiſhing the boiling leſs than half a minute, would admit the exiſtence of the higher claſs. As no two problems could be more important, I endeavoured to ſolve them in the following manner.

I began with the firſt; and, uſing the method already obſerved, kept veſſels with ſix kinds of ſeeds, producing the lower claſs of animalcula, in boiling water, ſome two minutes and a half, three minutes, three and a half, and four minutes.

The hermetical ſeal of twenty four veſſels was broken at a proper time; though the higher claſs of animalcula was wanting, it was not ſo with the

the lower; ſome were in all the infuſions. In general, the hiſſing was heard on breaking the ſeals, which proceeded from the violent efforts of the air, rendered more elaſtic, to eſcape, as the proofs already given convinced me, and an additional one occurred, than which nothing could be more deciſive. A ſmall barometer was included in a receiver full of common air; whenever the neck of a ſealed veſſel introduced by the top was broke, the mercury roſe in the barometer. To avoid redundancy, I may now remark, that condenſation of the internal air almoſt uniformly manifeſted itſelf in the other experiment with heat yet to be narrated.

Veſſels were immerſed ſeven minutes in boiling water; animalcula of the lower claſs appeared in all ſix; and they conſtantly originated though the infuſions remained twelve minutes in boiling water.

Perhaps it may be imagined that ſome optical illuſion deceived me; that I was induced to ſuppoſe the lower claſs of animalcula what aroſe only from the particles of the infuſed ſubſtances. That it might be the conſequence of decompoſition by gradual fermentation, or by their lubricity occaſioning locomotion on the leaſt ſhock or agitation; or of an active and volatile ſpirit penetrating and putting them in motion; from a great evaporation, or one leſs copious; from a powerful attraction or repulsion, by which the particles of the substances were forcibly attracted or repelled, or from any other accidental cause deceiving the eye. But in the same manner as these, and so many other fallacious appearances, may impose on him who is only beginning the difficult art of experiment and observation, so can they be properly appreciated by one well accustomed to microscopes, and who has made the history of these infinitely minute beings his long and particular study.

Notwithstanding the lower orders of animalcula are incomparably smaller than the others, they are not so very minute as not to differ in figure and size. But I shall not tire the reader with descriptions.

I would willingly have protracted the heat by continuing the immersion longer in boiling water, but the nature of the glass prevented it; for, after being a few minutes immersed, all the vessels burst in pieces, and of a sufficient number for my experiments I am sure two-thirds were lost. Therefore it became necessary to procure glass that would sustain heat better; which was effected by putting only a small quantity of water in the vessels with infusions. Omitting this precaution, I was certain to see them fly in pieces. It is needless to descend to minute details: the result of experiment proved, that boil-

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ing half an hour did not prevent the origin of the lower animalcula. Boiling three quarters, or a little leſs, rendered the whole infuſions ſterile.

We know the heat of boiling water is about 212°. The infuſions had evidently acquired this degree at leaſt, from their ebullition, while the ſurrounding water boiled. I ſay at leaſt, for philoſophers know, that water boiled in a cloſe veſſel acquires more heat than boiled in an open one.

The firſt problem being ſolved, which was to aſcertain how much longer than two minutes infuſions muſt boil to obſtruct the production of the lower animalcula. The ſecond, which was the inverſe of the firſt, remained for ſolution, how much within half a minute, boiling would admit the origin of the higher claſs. A ſecond watch was uſed; and the veſſels were immerſed a certain number of ſeconds, beginning with twenty-nine. In a word, boiling for a ſingle ſecond prevented the appearance of the higher claſs. Thus I had to take a leſs degree of heat, as 209°, 207°, 205°, 203°, until arriving at that which did not obſtruct their exiſtence. To be abſolutely ſure the heat had time to penetrate the infuſions, the water was gradually warmed, till attaining the requiſite degree, which was indicated by a ſmall thermometer alſo immerſed.

But to recede by ſuch ſmall differences would have been extremely tedious and troubleſome; and receding by great intervals, as for example, from 212° to 167°, 122°, might produce inaccuracy, and admit that animalcula had appeared, not only at that degree, but at one much inferior, therefore I thought of adopting a mean temperature, which would both diminiſh the labour, and liberate me from the imputation of an unexact obſerver. The reduction proceeded by 11°, deſcending from 200° to 189°, 178°, 167°; whence I had four claſſes of experiments correſponding with the numbers 200, 189, 178, 167. Each claſs had nine infuſions of the ſeeds before named, which made thirty-ſix veſſels, whoſe ſeals were broken, when the time neceſſary for producing animalcula had elapſed: but not a ſingle animalcule was in any one of the thirty-ſix veſſels. Whence I concluded, that none of the higher claſſes could appear at 167°, which is 45° leſs than the heat of boiling water. Continuing to deſcend by 11° from 156°, I came to 111°; whence I had five claſſes of infuſions, and forty-five veſſels.

My ſurpriſe has already been expreſſed at ſeeing ſuch abundance of animalcula of every deſcription, in ſubſtances openly infuſed, after expoſure to the violent flame of the blow-pipe: but it gave me no leſs aſtoniſhment not to find a ſingle a ſingle one of the higher claſs in infuſions hermetically ſealed, though they had ſuffered only the moderate heat of 111°.

This was done in the middle of July; and the thermometer in the ſhade ſtood at 88°. Eighteen veſſels were put to the teſt: nine ſuffered 99°, and nine 88°. No animalcula of the higher claſs were produced at 99°; but the whole nine infuſions produced them at 88°. Animalcula, the ſame in number and ſpecies, were in each veſſel, as in the cloſe infuſions not ſubjected to heat. From this fact, it was eaſily diſcovered, that the degree of heat fatal to theſe animals was between 99° and 88°; and I found it to be 95°: at 93°, a few of the higher claſs appeared; at 95°, only thoſe of the lower.

The method of opening the infuſions has been deſcribed towards the beginning of this chapter. When cloſe infuſions were made, I alſo formed open ones: and both being put in the ſame place, to have an equal temperature, when the open infuſions abounded with all ſorts of animalcula, I examined the cloſe. Such a plan always ſeemed the beſt; nevertheleſs, if diſappointed of ſeeing animalcula, I changed it oftener than once: ſometimes the veſſels were opened ſooner, ſometimes later; and though frequently delayed very long, the fact was uniform. In ſhort, I remain under the moſt abſolute conviction, that the non-exiſtence of theſe animalcula does not depend on the duration of time, but entirely on the action of the heat to which infuſions have been expoſed.

Before terminating the experiments before us, and making the reflections which they merit, let a word be ſaid, in paſſing, of the death of animalcula, after ſpeaking ſo largely of their birth. We have ſeen the lower claſs originate, in cloſe veſſels, at 212°; while the higher ſcarcely can at 93°. It would therefore appear, that, on expoſing both to heat, the lower claſs ſhould reſiſt it much better than the higher: however, the ſame degree that is fatal to the one, is fatal to the other; and both conſtantly die at 106°, or, at moſt, 108°.

Two important conſequences flow from theſe experiments: Firſt, The efficacy of heat in rendering cloſe infuſions barren of an infinity of animated beings. In open infuſions, are an incredible number and variety; while in the cloſe, ſubjected to the action of fire, one ſeeks in vain for an animal which he can call even the ſmalleſt in ſize. We cannot affirm, that ſimple incluſion occaſions ſuch devaſtation, ſince, in other caſes, it only diminiſhes the number: therefore, we muſt conclude, that heat truly does it. But how can it operate? Can we think it is by rendering infuſed ſubſtances unfit for producing animalcula?

animalcula? The inſufficiency of ſuch a ſuppoſition has already been ſeen. Neither can heat impair the elaſticity of the internal air: becauſe, from the precautions taken, there was not the leaſt difference between the ſtate of the internal air and the external; and attending to what happened on opening the veſſels, ſo far from that within being leſs elaſtic, it is even more ſo than the other. It is impoſſible to conceive the encreaſed elaſticity is prejudicial to the origin of our animalcula, as I have ſeen them where the air was condenſed twice or three times more than its natural ſtate. The concluſion will then ſubſiſt, that when the higher claſs does not appear, it is becauſe heat has vitiated or injured the productive principle. The force of this concluſion will afterwards be better comprehended. The ſecond conſequence is the inverſe of the firſt, and reſpects the conſtancy or rather certainty of animalcula appearing in boiled cloſe infuſions. And this reſult is no more favourable to the reaſon aſſigned for none originating in infuſions boiled an hour, becauſe too great heat had deſtroyed the vegetative power, or impaired the elaſticity of the air; nor with the time I ought to expoſe infuſions to heat, and ſtill ſee animalcula, which has been preſcribed to be as much as will deſtroy the eggs of the ſilk-worm-moth, that is, 135°, or 138°, or 140°, the ſame as we ſhall

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ſoon ſee that renders their eggs unfit for excluſion. But I have not only found the loweſt animalcula at that degree, but at 212° continued fully half an hour.

Theſe are the facts I have deemed it neceſſary to collect for eſtimating the weight of the two objections to my experiment: and we readily ſee how diſcordant they are. If, in the heat experiment mentioned in my Diſſertation, I found no motive inducing me to admit an imagined vegetative power, I have now the ſtrongeſt reaſons for rejecting it as inconſiſtent and chimerical. And as I could not then conceal my propenſity to believe, that infuſion animalcula originated from germs, neither do I heſitate here to ſay, propenſity has become perfect conviction(1). If the animalcula, in cloſe veſſels ſubjected to heat, do not originate from the vegetative power, I do not

(1) In the courſe of this work there are many alluſions to germs. The great diſpute concerning the generation of animated beings ſeems to reſolve into the queſtion, Whether there is a preorganized principle continually involved in another preorganized principle, and ſo on, by ſucceſſive involutions, from all eternity, and this, by the concurrence of peculiar circumſtances, expanding into the complete animal; or if it is more probable, that by the intercourſe of the ſexes, or otherwiſe, ſome change or creation is effected, which gives birth to a new animal or a wanting part: Both hypotheſis are attended with infinite difficulty.—T.

not ſee how we can aſcribe their origin to any thing but eggs, ſeeds, or preorganized corpuſcula, which we underſtand and diſtinguiſh by the generic name of germs. That ſuch is the real origin of animalcula will be directly proved, in the courſe of this Tract, by incontrovertible facts. An objection, it is true, here preſents itſelf, which impartiality will not permit me to conceal. Speaking of germs that develope into the loweſt order of animalcula, we muſt admit, that theſe germs have reſiſted the heat of boiling water, and that for three quarters of an hour; for it cannot be ſuppoſed they have paſſed from the air and inſinuated themſelves through the pores of glaſs, after cooling of the veſſels. Such ſuppoſitions, if not impoſſible, are certainly very difficult to be comprehended. However this ſhould rather be called a doubt or difficulty than a real objection; ſince, when well weighed, it may be reduced to the conſideration, whether we can conceive germs of animals in nature, whoſe extreme ſubtility permits their paſſage through glaſs, or whoſe conſtitution allows them to withſtand the heat of boiling water. As to the firſt hypotheſis, though I do not find it abſolutely repugnant, becauſe we know there are animals ſo very minute that their exiſtence never would have been credited but for the microſcope, I cannot admit it for the following reaſons. In the *firſt* place,

place, becaufe the fize of germs is proportioned to the fize of animalcula, as I have feen in more than one fpecies; and, on the other hand, the lower animalcula, confidered in themfelves, being of fome fenfible fize, fo fhould their germs be of fome fenfible fize, fuch, at leaft, as prevents them from penetrating glafs; particularly when we know, that other corpufcula apparently more minute, as the particles of air and water, of the moft acute and penetrating odour, cannot do it (1). In the *fecond* place, thefe animalcula are produced not only in glafs, but even in metal veffels fealed with metal, immerfed above half an hour in boiling water, as I have twice had occafion to experience, notwithftanding the greater narrownefs of the pores, or more irregular and tortuous pofition, made it impoffible to conceive the germs would penetrate the fides of the metal. Finally, Was the hypothefis true, animalcula of the lower clafs fhould originate equally well, whatever is the duration of boiling; for, in both cafes, the paffage of the germs through the fides of the veffel fhould be equally fuccefsful. On the contrary, not one appears after boiling three quarters of an hour.

Thus we are led to afcribe their origin to included germs, which for a limited time can refift the

(1) Academ. del Cim.

the influence of heat, but at length yield under it. As the higher claſs does not originate unleſs at a much more moderate degree of heat, there is a neceſſary inference, that the germs of this claſs are much ſooner affected than thoſe of the lower; whence we muſt conclude, that the amazing multitude of animalcula in the infuſions of open veſſels expoſed to boiling heat, and the intenſe flame of the blow pipe, do not appear becauſe their germs have reſiſted ſo great a degree of heat, but becauſe other germs have come to the infuſions after ceſſation of the heat.

But is there any proof, or any forcible argument, to remove or leſſen our natural repugnance to ſuppoſe that germs of the lower animalcula can reſiſt the heat of boiling water? To ſpeak of the germs or eggs of animals known to us, are there none of this nature? Undoubtedly, as far as our knowledge extends, we are unacquainted with any of that deſcription. Something has been ſaid on the ſubject in the ninth chapter of my Diſſertation, demonſtrating how the eggs of various inſects, as well as thoſe of birds, periſh at a degree of heat conſiderably leſs than that of boiling water. It is further ſhown, that this heat injures the ſeeds of plants, even thoſe with the hardeſt ſhell. A greater number of ſeeds and eggs, indeed, might have been ſubjected to experiment: and ſome may be found capable of ſtanding the

trial.

trial. With regard to ſeeds, hope ſhould not be abandoned from reading in Duhamel, that he ſucceeded in the germination of wheat which had ſuffered 234° in a ſtove ; and it is very probable this ſeed is not unique. So many analogies exiſting between ſeeds and eggs, I indulged the hope of finding ſomething ſimilar in the latter. Theſe facts were ſufficient inducements to make new experiments on ſeeds and eggs, to which I was additionally incited by the moſt ſingular phenomenon of the loweſt animalcula originating in boiled infuſions : and, in caſe eggs and ſeeds ſhould not withſtand the heat of boiling water, it would ſtill be uſeful to aſcertain what they could ſupport, by paſſing through various degrees to that which was fatal to them. But there was one particular inquiry not to be omitted, namely, whether animals and plants were more eaſily deſtroyed than their eggs and ſeeds, and in what proportion, the ſame as animalcula of the loweſt claſs can ſupport heat leſs than their germs. As all ſuch inveſtigations would greatly elucidate the preſent reſearches, I endeavoured to realize them by experiments, which will afford matter for the ſubſequent chapter.

CHAP.

CHAP. III.

EGGS AND ANIMALS, SEEDS AND PLANTS, EXPOSED TO DIFFERENT DEGREES OF HEAT.

In the month of May, I fiſhed up the eggs of frogs, which had a few hours before been depoſited in the water of ditches. The quantity was divided into equal portions, and each expoſed to a different intenſity of heat, in this manner. The eggs were completely immerſed in water, where I had put the ball of a thermometer. The veſſel was then placed on a ſlow fire; and, when the thermometer had attained the requiſite height, the eggs were taken out, and each portion put in a veſſel of cold water. I had ten veſſels, becauſe there were ten portions of eggs that had experienced different degrees of heat, as 111°, 122°, 133°, 144°, 155°, 167°, 178°, 189°, 200°, 212°.

The eggs that ſuffered 111°, 122°, 133°, produced young, but with ſome difference. Almoſt all thoſe at 111° were fertile; fewer produced at 122°, and the number extremely ſmall at 133°. The whole that were expoſed to greater heat became corrupted.

corrupted. The heat neither accelerated nor retarded the exclusion of the eggs, for tadpoles were hatched in the same time as in those not exposed, which had been reserved for the purpose of comparison.

Having ascertained the heat which the eggs of frogs could suffer without injury to the tadpoles, I had to examine what heat the tadpoles produced by them could bear; however, they were unable to resist as much, for all died at 111°.

These experiments were repeated on adult frogs. Although I had several species, I preferred those that had produced the eggs. They inhabited the ditches of plains, were rather small in size, and greenish on the back. Being put on the fire, they had all liberty in the water; they could swim at pleasure, and come to the surface to respire: but a covering prevented them from escaping. The whole perished at nearly about 111°.

I know there are frogs that live in warm springs, though the heat is greater than 111°. Thus, the illustrious Cocchi relates, they are not injured in the warm baths of Pisa, where it is 111° by Fahrenheit, which corresponds with 37° of Reaumur's thermometer (1). But we must either say they are of a different species, or, being long accustomed

(1) There seems to be some error here: 37° of Reaumur's thermometer corresponds with 115° of Fahrenheit. —T.

tomed to that degree, ſuffer no injury, though at firſt it would have been fatal to them. And we know, that men who can hardly endure the vapour bath ſix minutes, and are covered with profuſe perſpiration, can, in proceſs of time, remain fifteen minutes without any ſenſible inconvenience (1).

(1) Both man and animals can bear an incredible degree of heat without periſhing, and even without any ſenſible injury. Not that the living body will become heated to a high degree; it always preſerves a temperature near what it ſhould have in a natural ſtate. In a memoir on this ſubject, it is ſaid that a girl ſupported 284°, in an oven without inconvenience. Thoſe ſerving the oven bore 257° a quarter of an hour, and perhaps could have endured 212° half an hour. *Tillet ſur les chaleurs auquels les hommes ſont capables de reſiſter. Mem. de l'Acad. Roy.* 1764. Several perſons bore a room heated to 198°. 210°. 211°. The ſame perſons could juſt bear cooling ſpirits at 130°, cooling oil at 129°, and cooling quickſilver at 117°. They could not ſuffer the heat of water at 125°; *Philoſophical Tranſactions*, 1775, p. 117, 120. Different perſons at Liverpool bore the heat of an apartment at 224°; and Sir Charles Blagden bore one at 260°. *Philoſ. Tranſac.*

A dog has been in the heat of 236° without inconvenience; and a ſpecies of tænia has been found alive in a boiled carp.

In Ruſſia, the vapour bath is ſaid by Storck to be generally 133°. according to D'Auteroche 167°: and Acerbi obſerves, that thoſe in Finland are from 158° to 167°.---T.

I could have wifhed, when my experiments were made on other animals, to have alfo made them on their eggs; but it was not always as convenient for me to procure the latter. Thus when, at one time, I had abundance of larvæ of mufkitoes, water fleas (1), rat-tail worms (2), and other aquatic infects, I never could fucceed in finding the eggs whence they originated. However, it did not appear that my labour would be loft in making experiments on the animals. The nymphs and larvæ of mufkitoes (3) died at 111°; rat-tail worms and water fleas, at 106°; water newts and leeches died at 111°; the eels of vinegar, at 113°.

In my experiments on filk-worms, the caterpillars of elm butterflies, and the worms of the large flefh fly, I was more fuccefsful, as I had both the animals and their eggs. Until 93°, filk-worms did not appear affected; at 95°, and particularly

(1) No name is more incorrectly applied in general than *water flea*; almoft every fmall aquatic animal, even *fquillæ* and *gyrini*, have been fo defigned. It is moft likely the author means the *monoculi* of Linnæus. There is a complete fyftematic hiftory of thefe, and many other minute aquatic animals, by Muller, *Entomoftraca feu Infecta Teftacea*, 1785, 4*to*.—T.

(2) Reaumur fo denominates certain white worms from the refemblance of their tail to a rat's.

(3) It is uncertain whether the author may not mean tipulae.—T.

ticularly at 97°, they became reſtleſs; at 99°, they ceaſed to move; and at 108°, all had periſhed. The eggs producing theſe animals long reſiſted the influence of heat: at 88°, they produced the greateſt poſſible number of worms; at 99°, many, but fewer than before; and the number always diminiſhed as the heat encreaſed: at 144°, not one was fertile. The eggs and caterpillars of the elm butterfly perfectly correſponded with the ſilk-worms. It would be ſuperfluous to ſay more of them; and I paſs to my experiments on the large fly.

The ſpecies was that which depoſits the eggs on fleſh either putrid or tending to putrefy. Until 124°, a great many produced worms; at 135° and 138°, very few; and all were ſterile at 140°. The larvæ of theſe eggs, at 88°, began to be reſtleſs, and endeavoured to eſcape: their agitation encreaſed at the ſubſequent degrees; and at 108° all had periſhed. Full grown worms of the ſame kind died at 108° alſo; Changed to nymphs and flies: the latter bore the heat worſt of all; 99° degrees killed them. Flies came from nymphs at 104° and 106°; but none at 111°: having opened them, I found the heat had entirely dried them up.

And this much being ſaid with reſpect to animals, and their eggs, expoſed to different intenſities of heat; let us next narrate what happened to plants, and their ſeeds, in a ſimilar ſituation.

The ſeeds I uſed were grey peaſe, lentils, wheat, lintſeed, and trefoil: each, as uſual, was expoſed to a different degree, 167°, 178°, 189°, 201°, 212°, and then ſown in a ſmall diſtinct ſpace of earth, ſo prepared that each ſpace might contain an equal number of ſeeds.

Their germination was not injured by 167°: 178° began to be prejudicial; very few ſucceeded: at 189°, there were no more than eleven plants of trefoil, and only ten at 201°: of thoſe expoſed to 212°, only three germinated. Thus none of the ſeeds but trefoil could ſuſtain the heat of boiling water.

The five ſeeds had been ſubjected to heat by means of a ſand-bed. In a ſecond experiment, they were kept in water that was gradually heated, till as hot as required, in the ſame manner as before with ſeeds and eggs. Heat operated more powerfully on them here: at 167°, peaſe and trefoil germinated plentifully; but very little lintſeed, lentils, or wheat: at 189°, were only ſeven ſtalks of trefoil; and at 212° none.

My curioſity being ſatisfied with regard to ſeeds, I had ſtill to ſatisfy it concerning the plants ſprung from them. Growing plants, thirteen days old, were ſubjected to 167°, 178°, 189°, 201°, 212°, dipping the roots in water gradually warmed.

ed. Though immediately replanted in moiſtened earth, all died. As 167° was too powerful for theſe young plants, the heat was reduced to 156° and 144°; and this was not prejudicial to them, for the whole grew when replanted.

I had already ſubjected ſeeds to the influence of heat, but did not then think of doing the ſame to their plants. The ſeeds were beans, barley, white and black kidney beans, maize, vetches, parſley, ſpinage, turnips, beets, radiſhes, and mallows. They were heated to 167°, in ſand, after the manner above deſcribed; and all germinated. At 178°, ſome began to periſh; at 189° and 201°, very few ſucceeded; and at 212°, only one plant of kidney beans. The experiment was repeated at 201° and 212°, on all the ſeeds, but not one germinated.

My firſt experiment having proved, that trefoil had reſiſted heat better than the reſt, it occurred to me, that, it being the ſmalleſt ſeed, the ſize might perhaps concur towards the cauſe. Whether more heat could be ſuſtained as the ſeeds diminiſhed, could have been aſcertained by inſtituting a ſeries of experiments on a given number of vegetable ſeeds gradually decreaſing in ſize. But beans, and kidney beans, which are incomparably larger than trefoil ſeeds, had ſupported heat as well: and this induced me to abandon the idea, and ſpare myſelf uſeleſs trouble.

It may be proper here to recur to the feeds compofing feveral infufions, mentioned before, expofed to the heat of boiling water in veffels hermetically fealed. Two minutes immerfion admitted of full germination, but it ceafed if the heat continued longer: and the like happened in open veffels, only, thefe could fcarcely bear two minutes, and the others could bear a little more.

At firft fight, the experiments before us feem difcordant with the former, indicating the vegetative faculty is loft when feeds in water are expofed to 212°, or the heat of boiling water. By attending to the different modes of conducting the experiments, all inconfiftency difappears. In the former feries, the water was heated till beginning to boil; here, on the contrary, that in which either the fealed or open veffels were immerfed two minutes, did not give the leaft fign of ebullition, and the included water would have required at leaft four or five minutes longer for it; that the feeds vegetated is not furprifing, while thofe named before did not, fince the one fuffered more heat than the other.

Such have been my experiments on animals and their eggs, and on feeds and their plants; which, although not very numerous, feem to bring certain laws of nature in view, whence we derive fome elucidation of this fubject. We collect, in the firft place, that the eggs of the animals ex-

amined

amined were more able to withſtand heat than the animals themſelves. Tadpoles and frogs died at 111°, while their eggs became ſterile at 133°; and ſome even not ſo ſoon. Silk worms and the caterpillars of the elm butterfly died at 108°: their eggs did not produce at 133°. Large fleſh flies periſhed at 99°, their nymphs at 111°, their larvæ at 105°, and eggs at 140°. Secondly, There is much the ſame relation between plants and ſeeds as between animals and eggs. Some, as trefoil, beans, and kidney beans, are fertile, after having been expoſed to 212°, or the heat of boiling water, while their plants cannot ſupport 167°. Thirdly, The ſeeds of plants are more adapted for reſiſting the violence of heat than the eggs of animals. All the ſeeds my experiments were made on by dry heating germinated, though they had ſuffered 167°, and ſome 212°, but no egg was hatched after 144°. Laſtly, It is to be remarked, that heat is more noxious when acting along with water. None of the ſeeds in water at 212° afterwards germinated.

I am very far from pretending to give reaſons for all theſe reſults. I feel the difficulty of the enterpriſe, and at moſt ſhall only hazard ſome conjectural explanations, allowing whatever weight they merit, and permitting every one to think as he judges beſt. It may not be difficult, if we take the firſt appearance only, to comprehend why plants and animals do not ſuſtain heat ſo well as ſeeds and eggs; and it would ſeem, becauſe they receive the immediate impreſſions, which is not ſo when included in the ſeed or egg. But was the difference only a few degrees, which the animal could ſuſtain in the egg and out of it, and the ſame will apply to plants, this reaſon might be good; however, when that difference reaches 22°, nay 31° and more, who does not perceive the inſufficiency of it? Beſides, we ſhould neceſſarily have to admit, that the integuments of eggs, which in many inſects are but as points in matter, would be able to protect them againſt 22 or 31 additional degrees of heat, which is very improbable, when we conſider its extreme facility and activity in penetrating ſubſtances ſo pervious. Neither do I think the minuteneſs of the germ in the egg a ſatisfactory reaſon why it ſhould be leſs ſenſible of the impreſſions of heat; for, however ſmall it may be, the particles of heat are incomparably ſmaller, and they will therefore inveſt and penetrate it on all ſides, the ſame as they inveſt and penetrate it when developed. A complete refutation of this imaginary reaſoning is in the ninth chaper of my Diſſertation.

Before we are able to conceive why an animal in the egg is not ſo eaſily deſtroyed by heat as after it is produced, we muſt take an accurate view of what conſtitutes life in both theſe ſituations.

tions. But, if the life of animals in exiſtence is ſo little known, notwithſtanding all the efforts of modern phyſiology, much more obſcure muſt be the life of an animal concealed or concentrated in an egg. Certainly we may conclude, that the life of an animal in the egg is moſt feeble compared with that of the animal produced. During the firſt hours of incubation, the animation of the chicken is indicated only by the beating of the heart. Life before this is ſtill more feeble: it is a leſſer life, doubtleſs ſuch as that in the germs of inſects eggs previous to their having experienced the degree of heat neceſſary for excluſion. Can this moſt faint and feeble animation be any reaſon why it may endure more heat than after it is developed? Certain it is, that animals, when in a ſtate of very feeble life, which hardly merits the name of animation, do reſiſt external injuries with much greater impunity than when moſt vivacious (1). Thus, if we cut off the

(1) This certainly depends very much on the nature of the injury to which the germ or animal is expoſed, for the imperceptible agency of particular ſubſtances, whoſe nature we are little acquainted with, are noxious. Thus Michelotti incloſed a number of eggs in glaſs veſſels, ſome of which admitted the rays of light, and others excluded them. Few or none in the former were hatched; whence, from a ſeries of experiments, he concludes that light is prejudicial to the developement of all the germs of animals, and the ſame with reſpect to vegetable germs.—T.

the head of a frog, toad, lizard, ſnake, or viper, or take out the heart, or deprive them of ſome member during winter, while torpid with the cold, and apparently more dead than alive, they ſurvive the operation much longer than if they undergo it in ſummer, when in the vigour of life. I have often admired this fact; and that inſects immerſed in water in winter live longer than if immerſed in ſummer.

There is no doubt that the life of plants is weaker while included in ſeeds than after they are produced; and why may not this leſſer life, as with the germ of an animal in the egg, render them leſs ſenſible of the impreſſion of heat? In winter, when plants are ſurely leſs alive than in other ſeaſons, are they not leſs liable to periſh on being rooted up, wounded or mutilated, than on doing this during ſummer?

I ſhould not ſuppoſe that the reaſon why eggs are more unfit for ſupporting heat than ſeeds is from the greater ſoftneſs of the former, becauſe there are ſeeds not nearly ſo hard as the ſhell of an egg, and ſtill capable of ſupporting the heat of boiling water, as trefoil ſeeds, but from fluids being more abundant in the egg, by means of which heat has more influence in deſtroying the germ. Experiment renders it undoubted, that the fluids of eggs, and conſequently of their germs, are more abundant than in vegetable ſeeds.

That

That this excess of fluid contributes to destroy the germ more readily, appears to happen from the heat expanding the fluids, and putting them in motion; thus they must violently strike against the very subtile filaments of the germs, and occasion their rupture and destruction. This we have actually witnessed in the seeds that become sterile with less heat in water than if dry. For a similar reason does a piece of ice melt sooner in warm water than in air of equal temperature.

But let us leave these intricate researches, which are in some measure foreign to our inquiries, and compare the results concerning seeds and eggs with those concerning infusion animalcula. If we mean to assume a standard of the heat which the germs of the lowest class of animalcula can support, from that which eggs withstand, we cannot be divested of a natural repugnance to suppose them capable of enduring boiling water, when eggs are incapable of doing near so much. If, instead of comparing the germs with eggs, we compare them with vegetable seeds, our repugnance is wonderfully diminished; for, besides Duhamel's wheat, we have seen other seeds, as trefoil, beans, and kidney beans, resist heat as great. However, in pursuing this analogy, we incline more to compare them with the germs of eggs than seeds. At the same time, there are eggs that may most properly be com-

pared

pared to ſeeds, ſince, like them, after becoming dry, and remaining long in that ſtate, they are excluded on application of moiſture. Such are thoſe of certain pennated polypi, diſcovered by M. Trembley(1). Why may not the germs of the loweſt claſs of animalcula be of this kind? The poſſibility becomes probability, and this advances a ſtep ſtill farther, by our finding that the germs or ovula of ſimilar races of infuſion animalcula, poſſeſs the qualities nearly of vegetable ſeeds and the Trembleyan polypi.

If the example of vegetable ſeeds reſiſting boiling heat would induce us to believe that the germs of our animalcula might do the ſame, the ſuppoſition is ſingularly ſtrengthened by other arguments, the moſt immediate and direct, deduced from the animals and eggs themſelves. Duhamel obſerved, that a beetle, which feeds on grain, did not die at the heat of boiling water; and Schaeffer found one ſpecies of caterpillar that ſupported as much. The aſſertion of ſuch celebrated naturaliſts deſerves all credit.

If from animals inhabiting climates ſo temperate as ours, we paſs to the conſideration of thoſe that live in the warmeſt regions, and confiding in the moſt credible hiſtories, they certainly multiply, and are moſt numerous, notwithſtanding the exceſſive heat. Apamea and the Cape of

(1) Bonnet, Corps Organiſes, Tom. 2.

of Good Hope abound with animals of every ſize and figure, though the thermometer, in the ſhade, riſes to 111° (1). Equally abundant is Carolina, where it alſo riſes in the ſhade to 122°, and higher. It has been demonſtrated, that the direct heat of the ſun is twice that of the ſhade, and ſometimes thrice in the hotteſt countries, therefore this heat in Apamea and the Cape of Good Hope ſhould be at leaſt 189°, and in Carolina will exceed 212° (2). If animals live at ſuch heat as

(1) Haller, Phyſiolog. T. 2. It is not clear what country the author means by Apamea; ſeveral regions have that name.—T.

(2) Aſſuredly this is an error; for it is very much to be doubted, nay, I incline to deny altogether, that in any part of the globe the heat of the ſolar rays is nearly double that of the ſhade. By the few experiments made in hot countries, the difference is not many degrees. In Scotland, I have ſeen a thermometer, in an ordinary ſituation, expoſed to the ſun, riſe to twice the heat of the ſhade. But that was from reflection, and the heat that the ſurrounding ſubſtances had acquired. It has aſcended to 118° or 120°, which was from the ſame cauſe, as experiments demonſtrated.

The author is not the only perſon who ſuppoſes the direct heat of the ſun is twice that of the ſhade. Haller, an illuſtrious phyſiologiſt, and other naturaliſts, think it may even be more. At Montpellier, he ſays, it has been ſo great as to roaſt an egg, *Phyſiologia, tom. II. p.* 32. which would be between 150° and 160° at leaſt.

At

as in Carolina, will ſurpaſs that of boiling water, and their eggs ſuffer no injury; and if there are animals

At Benares, when the thermometer in the ſhade ſtood at 100°, it roſe only to 110° in the ſun. When it ſtood in the ſun-ſhine at 113° and 118°, expoſed to a hot weſterly wind, it was 104° and 110° in the ſhade, *Philoſophical Tranſactions*, 1793.—In Caffraria, when the heat was 102° in the ſhade, it was 106° in the ſun, *Barrow's Travels in Africa*.—At Goree, in the ſhade, it was 86°, in the ſun 99°, *Memoire, Journal de Phyſique*, 1788, *p.* 224. *Annales de Chimie*.—At Gondar, in Abyſſinia, the thermometer roſe from 81° or 82°, in the ſhade, to 113° in the ſun; it is not improbable from ſome diſturbing cauſe, *Bruce's Travels*.—But the moſt ſurpriſing accounts of ſolar heat are that in Paris 1793, while the thermometer in the ſhade was 94°, it ſtood at 144° in the ſun, *Annales de Chimie, tom.* 18.—At Montpellier, in the year 1705, in the ſhade it was about 100°; expoſed to the rays of the ſun twenty-eight minutes, it roſe to 212°, the heat of boiling water, *Memoires de l'Academie Royale*, 1706, *p.* 12. 13.—I cannot avoid ſuſpecting that the concluſions from both theſe inſtances have been erroneous; that the heat has, in the former caſe, been owing to ſome ſurrounding objects, or the reflection of a neighbouring wall, and in the latter caſe that the inſtrument has been imperfect. Thermometers it has been thought were then open at the top; but it was certainly otherwiſe in general. However in each of theſe caſes, the heat might be very great. There is much reaſon to believe, that, except the obſervations made at ſea, moſt of all we have are erroneous.

The heat of many climates would be to us almoſt intolerable. The greateſt cuſtom can hardly reconcile the inhabitants of northern regions to the burning heat of the ſouthern.

animals in our temperate climates that can alſo ſuſtain it, what difficulty is there in admitting, that

ern. In a warm country, the negroes could not ſleep for cold when the thermometer was at 68°. *Park's Travels.*—The heat in Cayenne is from 70° to 93°. *Prelong, Memoire.* In the town of Batavia in Java, while the laſt Chineſe embaſſy was there, between 88° and 92°, *Macartney's Embaſſy*, v. 1. *p.* 251.—During Perouſe's voyage, the thermometers, when higheſt, ſtood at 95°, *Voyages.*—They were in the ſhips; and it is to be remarked that cold is greater at ſea. The greateſt heat in Japan, while Thunberg was there, was 98°, *Travels.*—In one excurſion in the Cape of Good Hope, it was 100°, *Sparman's Voyages.*—At Aleppo, 101°, *Ruſſel's Natural Hiſtory of Aleppo.*—At Fort George, on the coaſt of Coromandel, the thermometer ſtood at 104°, *Philoſophical Tranſactions*, 1780. It was equally high in Paris 1720 and 1793, *Kirwan on the Temperature of different latitudes, p.* 75. *Annales de Chimie, Tom.* 18. In Goree, the heat was from 104° to 106°, *Prelong, Memoire.* —In the interior of the Cape of Good Hope, 108°, *Barrow's Travels.*—At Pekin, 1773, from 108° to 110°, *Kirwan, p.* 93, *from Mem. de Scavan. Etrang.*—At Pondicherry, 1769, the heat was from 111° to 117°, *Gentil Voyage dans les mers de L'Inde, tom.* 1. *p.* 490. 495. 505. Browne one day ſaw the thermometer at 116°, *Travels in Egypt and Syria.* Bruce ſaw it ſtand at 114° in Sennaar, and 119° at Chendi. M. Monneron told Prelong, that at Maufulipatam, he had ſeen the thermometer at 118°, in the ſhade: and an officer aſſured him, that he had ſeen it riſe to 131° at Podor. During a voyage there, the thermometer, in the cabin of a veſſel, ſtood at 133°, *Adanſon Hiſtoire Naturelle du Senegal, p.* 81. And this is the great-

eſt

that the germs of animalcula of a fimilar conftitution may exift. In confirmation of all this, I may

eft degree of heat in the fhade that I have yet met with well authenticated.

Although the direct rays of the fun are not fo powerful as the author fuppofes, the heat which various fubftances, from their peculiar nature, receive from them, is greater than can eafily be imagined: and there may actually be countries where the heat of certain fpots does equal that of boiling water. We read of the fand fo hot that the feet can hardly be borne upon it. In Senegal, it was 168°; and Adanfon fuppofes, that the thermometer might have rofe higher had the tube admitted; and eggs hardened in it, *Hift. Natur. p.* 131. At Marfeilles, it is faid, Dr Raymond found the earth heated to 170°, *Kirwan on different Temperatures, from Mem. de la Societ. Med. de Paris*, 1778. But it depends entirely on the fubftance what heat it will receive. The fand of Goree, which confifts of broken fhells, was heated only to 113°, *Prelong, Memoire;* and we have feen that the thermometer in the fhade rifes almoft as high.

Some perfons have imagined, I know, that if the heat was as great as the author admits, all the rivers would boil. This by no means follows, for one fubftance is not only more eafily heated than another, but will receive more and retain it longer; and at Marfeilles where the earth was heated to 170°, the fea was only 45°. The like has been found in other inftances, and is daily evinced in water in particular.

From all thefe facts, we are warranted to conclude that there may be places in the world, where in particular fituations, and aided by collateral circumftances, fome animal eggs or vegetable feeds may be expofed to heat not inferior

I may relate an obſervation by M. Sonnerat, correſpondent of the Academy of Sciences, concerning the heat of certain waters in Luçon, one of the Philippine Iſlands. They were ſo hot he could not bear his hand in them, and the thermometer roſe to 187°. Yet, to his great aſtoniſhment, were fiſhes ſwimming there (1).

I am conſtrained, by philoſophical ſincerity, now to think otherwiſe of the germs of certain infuſion animalcula than I did at the time of publiſhing my Diſſertation, when it did appear to me poſſible their germs in general could reſiſt the heat of boiling water. This was a deduction from vegetable ſeeds and eggs periſhing at that heat: but the facts narrated here, which were then unknown to me, have induced me to alter my opinion.

Though the germs ſo often referred to are not deſtroyed, at leaſt for ſome time, by boiling heat, the animalcula thence produced periſh at 108°, a degree remarkably inferior. This has been already obſerved, and not without ſurpriſe, but none will remain on bringing the example of plants and

inferior to that of boiling water; it may even ſurpaſs it; but it is a very different conſideration whether theſe eggs and ſeeds will not loſe their fertility; eggs undoubtedly will when they are in that ſtate, if they have the various parts commonly aſcribed to an egg.—T.

(1) Obſervations ſur la Phyſique, par M. Rozier, tom. 3.

and animals into consideration, as they can sustain so much less heat than seeds or eggs. However, the rule has an exception in the germs of the highest animalcula; for, whatever may be the cause, they can support less heat than the animals themselves. The animalcula die at 108°, but the germs are not developed after 95°. We are under the necessity of admitting, therefore, that the nature of the germs of the higher and lower classes, is very different relative to their faculty of resisting heat; which is fully coincident with all I have said of vegetable seeds, and what succeeds in eggs. Pease, lentils, wheat and lintseed, for the most part, became sterile at 189°; trefoil vegetated at 212°; and M. Duhamel's wheat at 234°: and although the difference has not been so sensible in the eggs of the animals mentioned above, it is sufficiently perceptible in those of another kind. The eggs deposited by certain butterflies on the under side of leaves, as well as those that some insects deposit to a northern aspect, perish at 79°. Twenty degrees more will hatch the eggs of other insects; and that heat even seems necessary for their exclusion. Such are those inserted by asili in the hard hide of oxen, cows, or bulls; of particular flies, that insinuate them into the nose or frontal sinus of sheep, goats, or deer; and of others, which

deposit

deposit them in the rectum of the horse (1). The same may be said of several species of worms breeding in the human body, and in calves, where the heat is about 99°.

If there is so much similarity in the powers of animalculas germs, and the eggs of other animals, in resisting heat, there is still more relation between animalcula and the animals themselves; for the same heat is fatal to both, or they die at degrees not much different.

Though these connections between germs and eggs, between infusion animalcula and other animals, afford additional conviction, that all the operations here are according to the known and ordinary laws of nature, without recurring to imaginary forces, still we want further information to acquire more particular, more enlarged, and more correct notions of a class of beings, which their wonderful minuteness has removed to such a distance from us. Yet our curiosity is singularly excited concerning them, from the famous systems of generation to which they have given rise, by their mysterious mode of reproduction, and the uncommon qualities connecting them with the rest of animated nature. 'Here 'another universe begins,' says M. BONNET, 'of which we the Columbus and Vespucius have 'but distinguished the shores, and describe them

(1) Vallisneri.

' not unlike the manner of the firſt American ' navigators.'(1). After Mr Needham, I have attempted to make little excurſions in this univerſe. I have endeavoured to penetrate the Continent, to view the inhabitants, and have not failed to give naturaliſts a faithful account on my return. But from new excurſions, and by exploring the country with greater diligence and leiſure, I perceive my narrative is very ſuperficial to what may be given. This is what I have begun to communicate to the reader in the preceding chapters, and which ſhall be proſecuted in the ſubſequent.

What above all ſhould be inveſtigated is the nature of the inhabitants of this microſcopic world. The nature of an object is diſcovered from its properties, that is, its relation to other beings. The more analogies there are, the greater ſcope is there for compariſon; and the more compariſons we can make, our knowledge of it acquires greater extent. My principal purpoſe in theſe new reſearches ſhould therefore be, to inſtitute the greateſt poſſible number of compariſons between animalcula and other animals. I have already made ſome experiments on them with heat, and I ſhall now proceed to ſpeak of others; and firſt of what is directly the reverſe of heat, namely, the influence of cold.

CHAP.

(1) Corps Organiſes, T. 2.

CHAP. IV.

INFUSION ANIMALCULA AND THEIR GERMS EXPOSED TO VARIOUS DEGREES OF COLD.

Animalcula were tranſported from the heat of the atmoſphere to the cold of an ice-houſe. It muſt have been a ſevere change, in the heats of Auguſt, to be removed from 84° to 36°. The only alteration I could perceive was ſome relaxation of motion; but they did not ſeem to ſuffer farther, though they remained there ſeveral days.

The experiment was diverſified by expoſing them to the cold of freezing, which I did by burying the veſſels of infuſions in ice. Conſiderable part of the animalcula died on the fourth day: of twenty-two infuſions, thoſe of ſeven only were alive. Theſe ſeven were kept buried in ice, and viſited from time to time. In eleven days, the animalcula of two had periſhed, but thoſe of the other five were ſtill living at the end of two months; nay, one ſpecies ſeemed more numerous. Beſides the ſeven infuſions already full of animalcula, two, which were yet ſterile, from being lately made, had at the ſame time been put among the ice. In ſome days, I know not how many, they produced a legion of moſt minute animalcula.

In the courſe of the following winter, animalcula were expoſed to new trials, and the reſult correſponded with that formerly obtained. Though under the freezing point, the infuſions continued fluid, from the vegetable oil they contained, and not a particle of ice was to be ſeen: yet the animalcula of many died, except ſome more robuſt ſpecies; on which I determined, for that reaſon, to make further experiments. During this winter, I put the animalcula that the cold had not been able to kill without the window in an exceſſively cold day. The thermometer fell to 19°; and the infuſions, hitherto preſerving their fluidity, were covered with a thin cruſt of ice. Breaking this cruſt, and applying ſome particles to the microſcope, in the parts not completely hardened, I ſaw animalcula ſtill alive, immerſed in little caverns of ice (1): but in the portions abſolutely frozen and dry, they were dead and motionleſs; nor did they revive after melting the ice. Where the water was perfectly fluid, the animals were quite vivacious (2).

This

(1) The author means, he took the pieces when beginning to freeze; for water expoſed at 19° would very ſoon become a ſolid lump of ice.—T.

(2) The illuſtrious Muller of Copenhagen has met with ſome

This was not enough. I was eager to ſee what happened as the water gradually froze.—A large drop of infuſion being prepared, it was adjuſted to the microſcope. The circumference, that is, the thinneſt part, froze firſt. The animalcula retreated from the edge to the interior of the fluid. As the freezing advanced, they ſtill receded, until collected in a maſs in the middle of the drop, where it was yet fluid. When this alſo froze, the life and motion of the animalcula ceaſed.

On repeating the experiment, they again fled to the centre, and died there as the drop hardened into ice. Two other glaſſes being filled with ſimilar infuſions, they took an hour to freeze; and this infinite army of animalcula had ſo concen-

ſome ſpecies that have ſurvived the congelation of infuſions;—a fact which it has not been my fortune to witneſs: and I muſt ſuppoſe, he convinced himſelf the infuſions were perfectly frozen. Quaedam (*i. e.* Animalia Infuſoria) rigorem frigoris ſuſtinent aquaque gelu ſoluta, eodem numero, vigoreque priſtino circumnatant, alia gelu affecta periere. Thus does he expreſs himſelf, in his treatiſe on Infuſion Animalcula, Leipſic, 1773, 1774. I regret that this work did not come to my knowledge until too late to uſe it in the text of my manuſcript, which was already tranſcribed. The loſs can only be repaired by notes; particularly as the author and myſelf have frequently remarked the ſame facts, or diſcuſſed analogous problems.

trated in the middle, that very few were frozen in the reſt of the infuſion.

What I have hitherto related proves that theſe ſpecies of animalcula do not periſh at 13° under freezing: but do they periſh becauſe cold has deſtroyed them, or becauſe the infuſions have loſt their fluidity, for I have uniformly found, when infuſions dried up, the animalcula were irrecoverably dead? The matter was dubious, nor could it be elucidated unleſs by farther experiment. It was neceſſary to augment the cold below 19°, and at the ſame time prevent the fluid from freezing. Both the one and the other were eaſily accompliſhed, by means of artificial cold, or a mixture of ſalt, ſnow, and common water, in which animalcula that had died at 19° were put. It is well known to philoſophers, that water does not loſe its fluidity at 20°, nay, at 21° below freezing, if at perfect reſt, which is attained by incluſion in a veſſel, and removal from all external motion. Thus I diſcovered that 19° had not been fatal to the animalcula but the freezing of the infuſions, ſince at 12° all were alive and ſwimming about, though with much leſs velocity than uſual. Some ſpecies could ſupport no more, for they died at 12°, though the water was not frozen, but began to be covered with a thin film. Two ſpecies ſtill ſurvived, and perhaps, or even without perhaps,

might

might have ſupported a greater degree, had I been able to keep the water longer fluid (1).

The germs of the animalcula were likewiſe expoſed to cold. New infuſions being made, and the veſſels hermetically ſealed, I expoſed them to moſt intenſe cold produced by a mixture of finely pounded ſalt and ſnow; the thermometer fell 2° below 0. The infuſions were frozen ſo hard as to require above half an hour to melt where the temperature of the air they were removed to ſurpaſſed the temperate degree (2). But the germs had not ſuffered the ſmalleſt injury, as all the infuſions, though conſtantly remaining in veſſels hermetically ſealed, produced every ſpecies of animalcula at the proper time.

Little is ſaid of cold in my Diſſertation, but it has been obſerved, that the cold of ſnow, or, which is the ſame, that of freezing, killed infuſion animalcula. It is confirmed by the facts ſtated here: and we ſee in addition, that all animalcula do not yield to that degree, but ſome can bear 14° and others more. Theſe things completely quadrate with

(1) Dr Blagden cooled boiled diſtilled water to 20° without freezing. He does not ſeem to conſider that keeping it at perfect reſt is the cauſe of fluidity. *Philoſophical Tranſactions*, 1788.—T.

(2) The temperate degree in Reaumur's thermometer. I can neither procure one of them marked with the temperate degree; nor can I diſcover from the Author's own memoirs what it is. Probably it may be between 51° and 56° of Fahrenheit.—T.

with the animals bearing the greateſt analogy and the neareſt to animalcula, I mean inſects. Some races do not die at 11° below 0, while others periſh at 10°, or at moſt at 7°. Many cannot ſupport ſimple freezing, and others ceaſe to live at cold far inferior (1).

There is this difference between infuſion animalcula and inſects expoſed to cold; the former are ſufficiently lively to preſerve the action of their members; the latter at freezing, and ſome before it, loſe all vivacity, and aſſume the appearance of dead bodies. However, there are a few inſects which, in this reſpect, may be compared to animalcula; beſides the podura of Linnaeus, which inhabits the ſnows of Sweden (2), I have ſeen the eels of vinegar retain motion at an intenſe cold. Vinegar does not freeze ſo ſoon as water; ſome kinds did not freeze at 16°; others more ſpirituous at 7°; and the anguillae always moved while it remained fluid. Eels, like animalcula, inſenſibly become motionleſs on encreaſing the cold; they ſtill move while a thin cruſt covers the vinegar; but the freezing being augmented, motion ceaſes, and they are extended in a ſtraight line, or in one a little curved. If ſudden aid is brought, by melting the ice, they will certainly recover; but if the ice is allowed to harden more, melting will not bring them to life (3).

The

(1) Reaumur, Memoires ſur les Inſectes, tom. 2. 5.

(2) Fauna Suecica.

(3) Dr Power obſerves, that the eels of vinegar may be frozen

The relation between inſects and animalcula extends to the originating principles of both. Intenſe cold neither deſtroys the germs of animalcula nor the eggs of inſects. The year 1709 is celebrated for its rigour, and the fatal effects it had on plants and animals. The thermometer fell to 1°. Who can believe, exclaims Boerhaave, that the ſeverity of this winter did not deſtroy the eggs of inſects, eſpecially thoſe expoſed to its influence, in the open fields, on the naked earth, or the branches of trees? Yet, when the ſpring had temperated the air, theſe eggs produced as after the mildeſt winters (1).

I

frozen and thawed ſeveral times, and they will ſtill be as lively as ever. Fiſcher has ſeen a ſpecies of taenia reſiſt freezing eight days, and the hydatis celluloſa has ſurvived it as long; *Virey ſur les Vers*. Muller expoſed a glaſs veſſel of water containing various minute animals to the cold of freezing for twenty-four hours. The ice was then melted, and the bodies of the animals appeared dead during twenty-four hours longer that they were examined. But on the following morning he ſaw the *Cypris piloſa*, a ſmall ſhelled inſect, and the *Cyclops quadricornis*, which is one of the monoculi, as lively as ever both males and females. Some ſmall water beetles alſo recovered. *Entomoſtraca ſeu Inſecta teſtacea quae in aquis reperiuntur*, p. 5, 6.—T.

(1) Since that period, there have been winters more ſevere. In France, during December 1788, the thermometer fell conſiderably lower, and in ſeveral other temperate European climates. There is a memoir on the ſubject alſo containing ſome judicious remarks on thermometers by M. Gauſſen.—*Memoires de la Societe des Sciences Phyſiques de Lauſanne, tom.* 3.—T.

I have expofed eggs to a more rigorous trial than the winter 1709. Thofe of feveral infects and among others of the filk-worm moth, and elm butterfly, were inclofed in a glafs veffel, and buried five hours in a mixture of ice and fal gem; the thermometer fell 6° below o. In the middle of the following fpring, however, caterpillars came from all the eggs, and at the fame time as from thofe that had fuffered no cold.

In the following year, I fubjected them to an experiment ftill more hazardous. A mixture of ice and fal gem, with the fuming fpirit of nitre, reduced the thermometer 22° below o, that is 23° lower than in 1709. They were not injured, as I had evident proof by their being hatched.

Combining all thefe facts, we conclude that cold is lefs noxious to germs and eggs than to animalcula and infects. Germs in general can fupport 2° below o: whereas of animalcula, fome die at freezing, and fome about 20°. The eggs of many infects continue fertile at 22° below o, while the infects die at 16° and 14°. This I have feen in filk-moths eggs, and thofe of the elm butterfly; and although there are caterpillars and chryfalids able to refift great cold, I have uniformly found it to be in a lefs degree than their eggs. What can be the caufe of fuch a difference? The queftion has already been agitated when fpeaking of heat; and in the fame

manner

manner as infufion animalcula and infects can refift cold lefs than their germs, fo do they lefs refift heat. A caufe for the difference has alfo been attempted to be affigned, and what has been faid will apply to the prefent cafe, which the reader may fee by refuming the paffage. There is ftill a more obvious caufe: infects killed at 16° and 14° are fo penetrated and frozen by the cold, that their members do not yield to the preffure of the finger, and feem perfect ice under the knife. This does not happen to eggs, though fubjected to a much greater intenfity. Their humours remain fluid, even at the greateft cold, as may be feen by crufhing them with the nail. Perhaps this is derived from conftituent fpirituous or oleaginous parts, or from fome principle adapted to abate the power of cold (1). If eggs do not freeze, it is probable the included embryos do not freeze. Is there any thing wonderful, therefore, that they then furvive cold which is fatal to them when produced? Probably for the fame reafon, (and I fee no objection

(1) To underftand this in its full extent would be erroneous; for an egg will freeze by a great degree of cold: at the fame time, there feems to be a living principle which enables it to fupport cold without deftruction; and when once that principle is deftroyed, cold more eafily operates. An egg was froze by the cold of 0; after thawing, it froze feven minutes and a half fooner. A new laid egg took half an hour to freeze in 15° and 17°; but when thawed, it froze at 25° in half the time. *Hunter on the Animal Oeconomy.*—T.

tion that can apply) animalcula concentrated in the germ, ſupport a degree of cold they are incapable of when developed.

Before terminating the chapter, ſome reflections ſhould be made on the ſmaller ſpecies of animalcula which originate at freezing,—a phenomenon not remarked in my Diſſertation, when occaſionally ſpeaking of the ſeaſon moſt fit for their production, either becauſe I had not adverted to the fact, or had never obſerved it. We muſt admit, that the germs of theſe moſt minute animalcula expand when no eggs will produce; for there is no inſtance of any being hatched at the freezing point. But there is nothing ſingular in it, if we conſider what is the temperature we denominate *freezing*. The ancients believed it the greateſt poſſible degree of cold: modern experiment has demonſtrated how much it may be augmented, either naturally or by art; and the facts we juſt now relate are an inconteſtible proof. They ſhew, that the cold of freezing is never ſeparate from a conſiderable portion of heat. Can we deſire any thing more convincing? If the ball of a thermometer is transferred from a mixture of ſnow and ſalt to plain ſnow, it will riſe from 22° or 27° below zero to the freezing point again. Is not this a clear indication of the thermometer paſſing from a cold to a warm ſituation, or, to ſpeak more philoſophically, from a place

place where there is lefs heat to one where there is more. If the temperature of freezing retains a portion of abfolute heat, Why fhould it not develope the germs of the moft minute animalcula? It is needlefs to fay we are unacquainted with any fpecies of eggs that may be hatched by fo little heat; had we never feen any but thofe of birds which require 104°, we fhould undoubtedly fuppofe all others require the fame. A little initiation into the ftudy of minute animals teaches how many kinds produce at heat infinitely lefs: Such are the eggs of butterflies and many other infects, of frogs, toads, lizards, tortoifes, down to fome, as thofe of toads, which I have feen produce at 45°. If thefe eggs hatch at 59° lefs than thofe of birds require, what repugnance will there be to fuppofe that at 13 degrees lefs, or the cold of freezing, may hatch the eggs of other animals? Nor fhould it furprife me to be told of animals, whofe eggs would produce at much greater cold, after knowing there are plants, beings fo fimilar to animals, and many of them, which, amidft the rigours of winter, flourifh, are impregnated and fructify, as winter aconitum, liverwort, (*epatica nobile*) narciffus, black hellebore, terreftrial moffes, and corallines (1).

Among

(1) It is uncertain what the Author means by Corallines.—T.

Among the germs producing infuſion animalcula, there is one ſpecies that does not periſh at boiling heat, whence the ſmalleſt animalcula originate, or, as we have termed them, of the loweſt claſs. The reſemblance in minuteneſs of ſuch animalcula, and thoſe originating at freezing, made me ſuſpect, that the germs reſiſting boiling might be the ſame with thoſe expanding at freezing. Rigorous experiment was neceſſary to aſcertain whether it was actually ſo. While ſeveral new infuſions were buried in ſnow, others hermetically ſealed were expoſed to boiling heat; and I examined both at a proper time, but I never could diſcover any ſenſible difference in the figure, ſize, organization, or motions of the reſpective animalcula; whence I think there is ſufficient reaſon to conclude they are identically the ſame ſpecies. The identity of the animalcula eſtabliſhes the identity of the germs. Two moſt ſingular properties, therefore, exiſt in theſe minute animated beings; one the power of reſiſting the heat of boiling water; the other the peculiarity of originating at freezing.

CHAP.

CHAP. V.

MORE ACCURATE AND EXTENSIVE CONSIDERATION OF THE EFFECT OF HEAT AND COLD ON ANIMALS.

HEAT and cold, we have ſeen, are two agents deſtructive of animals, when extended to a certain degree. We have alſo obſerved, that all ſpecies do not periſh at the ſame intenſity; but ſome can ſupport more, and ſome leſs, according to the hardineſs of their conſtitutions. All this, however, has been viewed only on a narrow ſcale, and in a limited number of animals, and even thoſe occupying the loweſt rank in the ſcale of animation. Let us generaliſe our ideas, and conſider the facts more at large. Let us run over the different claſſes and orders of animated beings, beginning with man, the moſt noble, the moſt ſublime, and moſt perfect of all.—— Such conſiderations will afford an agreeable interlude, and leſſen the ennui which attends the ſameneſs of a ſubject.

Though man, like other animals, being ſubject to phyſical laws, muſt neceſſarily be liable to periſh from exceſs of heat or cold, he can ſuſtain a degree of heat that might be ſuppoſed inſupportable.

portable. Coinciding with Boerhaave's ſentiments, it is commonly believed, we cannot exiſt in an atmoſphere warmer than blood-heat. So that illuſtrious philoſopher concluded, becauſe he ſaw certain birds and quadrupeds die at 149°, which is 50° more than human blood (1). This opinion is ill founded; ſince there are countries inhabited where the atmoſpherical heat is greater than that of our bodies. Thus, in Apamea and the Cape of Good Hope, it is 113° in the ſhade (2); to which the natives muſt be expoſed. In Carolina, it ſurpaſſes that of the human body; for the thermometer falls when taken from the air in the ſhade and put in a perſon's mouth (3). In warm baths, we are ſometimes ſubjected to more heat than in the hotteſt climates. Certain waters are 113°, and others ſo much as 120° (4).

There is the ſame ſimilarity in the facts related of cold to thoſe related of heat. Boerhaave thought the utmoſt degree of cold that could happen was zero in Fahrenheit's thermometer, or $14\frac{1}{2}$° below freezing by Reaumur's; at which, he remarks, men, animals, and vegetables ſoon periſh. Experience proves that, in different parts of the globe, cold is greater. According to what the Pariſian academicians relate, in

(1) Chemia. Tom. 1. (2) Cap. 4.
(3) Haller, Phyſiolog. T. 2. (4) Haller, ibid.

in ſome winters at Peterſburgh, the thermometer fell 29°, and once 33° below o (1). The cold at Quebec exceeded it, for the thermometer fell to 42° (2). That at Torneao, obſerved by Maupertuis, was ſtill greater, as it fell to 51° (3). But this which appears extreme, compared with what we witneſs, cannot bear compariſon with that ſometimes felt in many parts of Siberia, as Tomſk, Kirenga, Jeniſeik, where the thermometer has been ſeen at 90°, 128°, and even 178° below o (4).

Such dreadful cold, we cannot deny, was pernicious, nay fatal. In Peterſburgh, at—29°, the face could not be kept uncovered above half a minute (5): and at Torneao, where the thermometer fell—51°, thoſe expoſed to the air felt the breaſt as if lacerated. Nor is it uncommon for the inhabitants of theſe cold climates to loſe ſome member, as a leg or an arm, during winter (6). Similar and more terrible are the effects in Siberia: yet, in other parts of the earth, cold is perhaps more intenſe. Such may Captain Middleton have experienced in Hudſon's Bay, as he has communicated to the London Royal

 Society.

(1) Chem. ibid.
(2) Hiſtoire de L'Academie Royale de Sciences, 1749.
(3) Voyage au Cercle Polaire.
(4) Hiſt. de l'Acad. Roy. (5) Ibid.
(6) Maupertuis, ibid.

Society.—All the liquors, not excepting brandy, froze within their houſes, and the beds in their apartments were covered with a coating of ice three inches thick,—though the walls of the dwellings, where they had buried themſelves, were ſtone, and two feet thick,—the windows very ſmall, and cloſed with ſtrong boards moſt part of the day,—and they had great fires continually burning. The Dutch ſuffered equal cold in Nova Zembla; where the rigour of the weather was ſuch, that, in a cloſe hut, and with a conſtant fire, it was with great difficulty they could keep their feet from freezing. Their cloaths were always covered with ice; and their wine, though very ſtrong, was dealt out in lumps of ice (1).

The

(1) Here the author's deductions are from erroneous experiments made by others; becauſe cold ſo very intenſe has never been witneſſed by modern philoſophers. It is not impoſſible, indeed, that there are countries where it is really as great, but they are yet unexplored.

It was long believed, that no natural degree of cold exiſted which would congeal mercury. It was artificially frozen, and the thermometer fell ſeveral hundred degrees below the freezing point. Therefore, when travellers in the northern regions ſaw it ſtand at—100° or 200°, they concluded that to be the real degree of cold: but the mercury in the thermometer was already frozen, and rapidly contracting into leſs bulk. The freezing point of mercury, it is now aſcertained, is under—40°: but if the cold exceeds

The effects of cold to that degree too clearly demonstrate, that it would be fatal to the human race, if unprotected against it. I do not mean,

 it

exceeds this very little, it will descend the scale some hundred degrees. In an experiment, where the cold was only a few degrees more than 40, the thermometer immediately fell to—450° and 490°.—*Cavendish's Observations on Mr Hutchin's Experiments, Philos. Transac.* 1783, p. 23, 25.

The greatest cold that Acerbi observed in his travels in the north, was—13°. Mackenzie experienced 16°; but his thermometer was then broken.—*Travels over the N. Continent of America.* The greatest cold at Montreal in North America was—16°. 18°.—*Philosophical Transactions.* That experienced by Captain Cartwright, on the coast of Labrador,—25°.—*Journal of a Residence on the Coast of Labrador.* On the same coast, by M. de la Trobe,—30°.—*Philos. Trans.* 1782, p. 198. The cold at Petersburgh and Moscow has been from—30° to 39°.—*Acta Petropolitana, Nov. et Vet. var. loc.* Mercury has froze at Prince of Wales Fort, Hudson's Bay, and Albany Fort.—*Philos. Trans.* 1793, p. 368. 1770. M. Patrin suffered—35° in Siberia; and quicksilver froze.—*Journal de Physique,* 1791, p. 88. But even these degrees have been far surpassed at Oustioug Velikoi, in the government of Vologhda, lat. 60°. 50. N. where the thermometer fell 83° below Zero, in December 1787; and, in January the same year, so low as 103°.

The sensations excited by such excessive cold are inconceivable to us, who live in temperate climates. "I can-"not express the pain of respiration at 35°," exclaims M. Patrin: "boiling oil seems to fill the lungs. Even in the "closest it will be ſo abſolutely, but only relative to the condition of the perſons expoſed. With regard to what was ſuffered by Middleton and the Dutch in Nova Zembla; ſhut up in huts, they led a quiet and ſedentary life, which undoubtedly promotes the action of cold; I do not think myſelf miſtaken in affirming, that, well cloathed, and taking plenty of exerciſe in an open country, they might have braved as intenſe cold without danger. In the winter nights of our temperate climate, it is ſometimes much more than freezing, and one expoſed to it without any motion would really die; but preſerving ſufficient motion, we might ſuffer a greater degree. Thus, the Paris academicians, though accuſtomed to a climate

"cloſeſt carriages, one is ſuffocated by this piercing cold." Its other effects are no leſs terrible: rocks and trees are ſplit with reports like cannon. There is a perfect calm, and ſo thick a miſt prevails, that nothing can be diſtinguiſhed a few paces diſtant. Magpies, crows, and ſparrows fall dead to the earth; nor are quadrupeds and the human race ſecure from danger.

The pernicious conſequence of cold is viſible in all animated nature. Plants are neither ſo abundant nor luxuriant as in the warmer regions. Animals are fewer; they are leſs diverſified; and, in general, their ſize is ſmaller. Something is wanting to expand the organic ſyſtem; nay, to create that beauty and variety which are found in the genial temperature of the South, and which it is ſo pleaſing to behold.—T.

climate as temperate as ours, began their aſtronomical obſervations amidſt the woods or mountains near Torneao, before the ſnow lay deep upon them, and the intenſe cold had not encreaſed to---51°, as it afterwards did; however, it was ſuch, that all liquors excepting brandy froze, and a veſſel could not be taken from the mouth without drawing blood, for the froſt had glued it to the lips.

The ſavages of the moſt northern climates continue to hunt during the coldeſt weather; and ſo true is it, that motion alone preſerves life, when any misfortue threatens deſtruction, they accelerate death by reſt(1). There can be no better proof of the efficacy of motion againſt cold than the narrative of the Dutch who wintered at Spitzbergen, a country ſituated in 78° of north latitude, and by common conſent allowed to be the coldeſt in the world. Thoſe who had ſhut themſelves up in a hut, in the beginning of the ſeaſon, died one after another. The cold was ſo exceſſive that no fire could warm them; whereas thoſe who had gone into the open air, and employed themſelves in the chace, in carrying wood, or any other corporal exerciſe, preſerved health and vigour (2).

E 4 From

(1) Boerhaave, Praelectiones. Haller, Phyſ. tom. 2.

(2) The ſame has invariably been proved by the accounts of thoſe unfortunate perſons condemned to winter

From what has been ſaid may be judged the variety of heat and cold that man can ſuffer, beginning with heat equalling or ſurpaſſing blood heat, and deſcending to the horrors of cold ſo far exceeding freezing, which ſhews that man is not neceſſitated by nature to inhabit certain determinate parts of the globe, but to live, multiply, and exerciſe his ſovereignty over all, without finding an obſtacle in climate. It is otherwiſe with quadrupeds. They are diſperſed over the earth; ſo that ſome are limited to warm climates, ſome to temperate, and ſome to cold; nor has any ſpecies yet been found adapted to live in all indifferently. The lion, elephant, tiger, leopard, and panther, inhabit only the warmeſt regions; when tranſported to the temperate, they become incapable of propagating, and ſoon periſh in the cold. Although the domeſtic animals, ſo uſeful to us, are not injured by warmer climates, they cannot live in colder. Such are the horſe, ox, and 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 live in temperate climates. At leaſt this has been

ter in ſuch inhoſpitable regions. Air and exerciſe always preſerved health, while inactivity uniformly ſubjected them to diſeaſe. However the death of a great many ſeems to have enſued from improper food.—T.

been found with the rein-deer; its naturalization has often been attempted in France and Germany, but inſtead of multiplying they always periſhed(1).

The law reſtraining quadrupeds to their native countries is liable to modifications; for there are ſome that can exiſt and multiply in temperate though originally from warm climates. The rabbit and guinea-pig are inſtances of the former; the beaver and lynx of the latter(2).

Birds may in this inveſtigation be conſidered as divided into two claſſes: Of thoſe inhabiting cold, temperate or warm countries, ſome do not wander far from their native place, or do not change their climate. Others have no fixed abode, but change according to the different ſeaſons, being neceſſitated to it either by the ſcantineſs

(1) Buffon Hiſtoire Naturelle, tom. 14.

(2) It is very true, that the animals which have been removed all at once from cold to warm climates, and the reverſe, have ceaſed to propagate their ſpecies. However, there are ſeveral exceptions, and more will be known in proportion as natural hiſtory continues to be further cultivated. Perhaps the ceſſation of multiplying their ſpecies ariſes from the great and ſudden change: but if one generation was removed two degrees farther north or ſouth, propagation would moſt probably continue. If their progeny were removed two degrees more, and ſo proceeding by ſhort diſtances, every generation, there is great reaſon to ſuppoſe, would continue fertile, and the animal be naturalized in any climate.—T.

nefs of food, or from inability to bear the winter, or even a flight degree of cold.

We have faid that Boerhaave thought 149° immediately killed certain birds and quadrupeds. Surely 50° above blood heat is very great, and cannot but be intolerable to many races of animals; yet we muft acknowledge it may be borne, and that it is fo by many fpecies inhabiting the torrid zone, and other very hot regions. And it appears to me, that we fhould reafon on heat which birds and the reft of animals can fupport as we reafon on the cold. As thofe in the northern climates can fuftain exceffive cold, fo can thofe in the fouthern fuftain exceffive heat.

It is eafy to afcertain the greateft cold that either cetaceous or fquamous fifhes fuffer: it will always equal that of the water which they are in, confequently in frefh water lefs than freezing, elfe it would not continue fluid. Thofe inhabiting falt waters, as the fea, will fuffer a little more. Thus fifhes are fecured againft the rigour of cold to which innumerable animals are fo much expofed. No lefs are they fheltered from the burning heat of the atmofphere, excepting thofe living in fhallow waters, and on that account more or lefs fubjected to the influence of the air from the predominating feafons or the climate.

From fome obfervations, we know there are carp living in warm fprings that experience blood heat

heat (1). I took ſeveral river carp for experiment. When the water they were in had been heated to 106°, they exhibited no ſign of uneaſineſs. At 109°, they began to ſtruggle, and died at 116°. Experiments were made on other fiſhes, as eels, tench, lampreys, but none could bear ſo much heat. Whence, by analogy, thoſe inhabiting warm ſprings ſhould ſupport greater heat: and of this the fiſhes we have juſt mentioned afford ample evidence (2).

But of all known animals, reptiles and inſects ſtand in greateſt dread of cold, and ſeek heat the moſt. The heat of the ſun may be called their ſoul. Then they are full of ſenſation and motion; and as that luminary is more powerful, ſo do their activity, vivacity, and boldneſs increaſe. The venomous kinds, as ſnakes and ſcorpions, are more formidable, and their poiſon more dangerous. But cold produces an oppoſite effect. Innumerable inſects periſh on the approach of winter, and moſt of both them and reptiles that ſurvive would encounter the ſame hazard, if not protected againſt it. In temperate climates, all ſeek a ſafe retreat when winter comes. Some, as ſcorpions, and many ſpecies of flies, retire to the rents of walls, or under the tiles of houſes; others are concealed in the midſt of ſtones, the clifts

(1) Haller, Phyſiol. Tom. 2. (2) Ibid. Tom. 4.

clifts of trees, or holes in their trunks, as vipers, ſnakes, cantharides. Some are ſecured in the caverns of mountains, in ſubterraneous abodes and the cellars we make, as ſpiders, flies, muſkitoes, naked ſnails, and beetles. Others find a genial warmth in dunghills during the rigour of winter. The bottom of waters, and the bowels of the earth above all, give ſhelter and retreat to moſt reptiles and inſects. Yet, in the whole of theſe aſylums, though ſufficiently defended from the fatal effects of cold, they ſuffer from its influence in the moſt ſenſible manner. Their limbs become torpid, and they remain in a lethargic ſlumber the whole winter.

But among quadrupeds, birds, nay, perhaps, among fiſhes, there are ſome that experience a kind of lethargic torpidity not unlike that of reptiles and inſects. To ſay nothing of frogs, toads, lizards, and the like, among quadrupeds which dwell in water or in the earth all the winter, hedgehogs, land tortoiſes, ſeveral rats, the marmot and dormouſe, are alſo overcome by lethargy. Some in ſociety, and ſome in ſolitude, conceal themſelves in the trunks of trees, or are hid in the earth. Cold has the ſame influence on bats; they are found ſtiff and motionleſs in hollow trees, or the rents of walls, or hanging to the vaults of ſubterraneous caverns.

Some

Some birds are alſo ſubject to torpidity. At the end of ſummer, hundreds collect, cluſter together, and plunge into water, where they remain the whole winter in heaps, and ſhrunk within themſelves. The learned reader already anticipates that I ſpeak of ſwallows. The fact is too well circumſtantiated, too well authenticated, for any one to be hardy enough to call it in queſtion. Many reſpectable and credible perſons declare, they have not only beheld flocks of ſwallows collect and plunge into pools on the approach of winter, but oftener than once have ſeen cluſters of them taken out of water, and even from beneath ice. The doubt therefore is, whether the ſwallows of which theſe reſpectable authorities ſpeak are ours, that is, thoſe conſtructing an earthen neſt in our houſes, and reſiding with us during ſummer, or whether they are ſtrange ſwallows, by which I mean a bird ſimilar in colour, figure, and ſize, but of a different nature and ſpecies. For many years I have endeavoured to ſolve this queſtion. Experiment has taught me, that animals which are torpid in winter become lethargic alſo in other ſeaſons, if ſubjected to the requiſite degree of cold; ſo that, on expoſing a frog, a dormouſe (*ſorcio moſcardino*) or a lizard, to the cold of freezing, in ſummer, while moſt vivacious, it ſoon becomes torpid, and is in a ſtate of torpor till the cold ceaſes. Suppoſing the ſwallows of

our

our country were thofe drawn torpid out of water, and from beneath ice, I imagined they might become torpid and motionlefs if fubjected to the fame degree of cold, and thought of expofing fome to the temperature of an ice-houfe, gradually bringing them from warmer atmofpheres, as of a cave or an apartment adjoining to the ice-houfe; for, during the month of Auguft, to carry them all at once to fuch cold might be too fudden a change. But all the fwallows in the adjoining chamber died in three hours, without my being able to difcover whether they had firft fallen into a lethargy. The cold was not great, as the thermometer ftood at 43°. Other fwallows had the fame fate. Whence I may conclude, that the fwallows found in water or under ice are of a fpecies fpecifically different from ours, becaufe they perifh at a fmall degree of cold (1). This

(1) Since the author wrote this, he made the following experiments on fwallows, which may tend in fome meafure to elucidate the natural hiftory of thefe birds, which has hitherto been fo obfcure.

'August 23. 1792, the thermometer being at 76°, four houfe fwallows, (hirundo domeftica) confined in a glafs veffel, were buried in fnow. In an hour, they had not fuffered in the leaft, and flew about the apartment. They were returned to the veffel, and the cold increafed. In

This experiment, which is mentioned in my Annotations

124 minutes, the thermometer ſtood at 9°; they were very feeble, but kept their eyes open: they moved when touched, and endeavoured to eſcape. The thermometer fell no more. In 60 minutes, ſome ſigns of animation were indicated by two, but the others appeared dead. However, this was only aſphixy, for the heat of the atmoſphere reanimated them, and in 68 minutes they had recovered their natural vivacity.

'Of two ſwallows expoſed to 1°, one died in ten, the other in fifteen minutes.

'Two window ſwallows (hirundo urbica) expoſed to 2° and 3° of cold, died in 21 and 40 minutes.

'Six martins (hirundo apus) were expoſed to 8° for three hours. The firſt hour they were reſtleſs; the ſecond their motions leſs frequent; the third they ſeemed motionleſs though without lethargy. Their eyes were open, and they moved on being touched. Expoſed to the atmoſphere at 74°, they recovered.

'In another experiment with the ſame martins, the thermometer fell to 3°; one died in ſix minutes, two more in twenty-five; the other three appeared dead, but revived on expoſure to the atmoſphere an hour: however, they died irrecoverably when returned to the veſſel ten minutes longer.

'Some naturaliſts think, that the bank ſwallow (*hirundo riparia*) conceals itſelf in holes during winter; and the error aſcribing the ſame to window ſwallows, ariſes from confounding notations on *La Contemplation de la Nature*, a work

confounding the one with the other. Montbeillard conceives, that inſtinct may induce them to conceal themſelves in the earth during winter.

'Achard, coming down the Rhine in 1763, procured ſome birds, evidently ſwallows, quite torpid and inanimate, drawn from their ſandy holes. Being put next his ſkin, they recovered, and flew away. But this obſervation only proves, that there exiſt ſwallows ſubject to real lethargy without determining what ſpecies. Therefore, notwithſtanding all that has been written on it, the queſtion is ſtill dubious. In two years, I opened above fifty ſwallows holes on the Banks of the Po, without finding any thing but their neſt or its remains, which demonſtrates that the owners had gone to winter in other climates. This recalls an obſervation by Collinſon, who did the ſame, and he diſcovered nothing.

'On the 15 of June four ſwallows confined in a glaſs veſſel were immerſed in a mixture of ſoda and ice. The thermometer fell to freezing without affecting them; I confined them again, and the thermometer fell to 10°. In twenty minutes they were taken out. They could ſcarcely move, or ſtretch their wings; their eyes were ſhut, nevertheleſs they gradually recovered, and in half an hour flew about the apartment.

'The experiment was repeated; they lived thirty minutes at the ſame degree. Put on their backs on a table, they were motionleſs at firſt; then after many efforts they recovered work tranflated by me (1), I find confirmed by M. De Buffon, in his firft volume on birds, publifhed

covered their natural pofition, and walked about the chamber, but had not ftrength to fly. As the animal functions returned, refpiration gradually became more fenfible and quicker; the eyes opened, motion and life were re-acquired. In three hours the fwallows could fly.

'They were again expofed to the cold of 0 twenty minutes. Two expired; the other two revived in five hours, but were unable to ufe their wings. Is this torpor a real lethargy fimilar to what is moft improperly called the fleep of many animals? Immobility of the body, almoft extinguifhed refpiration, fufpenfion of the fenfes, and recovery of them are ftrong prefumptions. But the fame fymptoms may attend real afphixy, fimilar to that of animals immerfed a certain time in water, or brought within the fphere of fome mephitic gas: an afphixy effentially different from lethargic fleep, which may be continued feveral months without injuring the exiftence of the animal, while the other will foon deprive it of life.

'To afcertain this point, I entered with greater eagernefs on new experiments, as they afforded me an opportunity of correcting an error, when formerly fpeaking incidentally of the lethargy of fwallows. In one of my notes to *La Contemplation de la Nature*, I faid, feveral fwallows which were kept in an apartment adjoining to an ice-houfe, where

(1) Firft printed 1769, 1770. The experiments were made five years before.

lished 1770; where he obſerves, that, with the ſame deſign, he confined ſeveral ſwallows in an ice-houſe,

where the thermometer ſtood at 43°, died without becoming lethargic; whence I concluded, that theſe birds were incapable of ſupporting cold. In my *Opuſcoli di Fiſica Animale e Vegetabile*, I repeated the ſame remark: the fact was true, but I deduced a falſe conſequence; for they bear a much greater degree without inconvenience.

'Several ſpecies were ſhut up in wicker baſkets, covered with wax-cloth to preſerve the humidity, and buried in ſnow. In twelve hours, they were ſtill vivacious, and crept cloſe together to ſecure themſelves as much as poſſible againſt the cold. The next twelve hours, they were in the ſame ſtate. Two could fly languidly about the ice-houſe. In thirty-five hours four were dead, two houſe ſwallows, a bank ſwallow, and a martin; the reſt were ſo weak, that they could neither ſtand nor fly, neither did they make any effort to eſcape. Still, theſe ſymptoms rather indicated infirmity than lethargy: their eyes were not ſhut, and they reſembled dying birds in every thing. Not one was alive in ten hours more. All periſhed in 48 hours in another experiment.

'It was neceſſary to cover the baſkets with wax-cloth, for two ſwallows, expoſed without this precaution, died in two hours and a half, and were as wet as if they had been drenched in water; which has certainly been the caſe with thoſe mentioned in my note on *La Contemplation*, as I recollect perfectly well that they were very humid.

'Nine

ice-houſe, but without ever ſeeing them become torpid; and they uniformly periſhed on remaining expoſed any conſiderable time. He thence concludes it impoſſible that this bird becomes

lethargic

'Nine more were confined in an ice-houſe: they languiſhed without becoming lethargic, inſenſibly loſt their ſtrength, and died in 41 hours. Theſe animals did not periſh from hunger, for others lived without food three days, three and a half, and five days, while the moſt vigorous in the ice-houſe lived only 48 hours. Thus, the acceleration of death can only be aſcribed to privation of heat.'—A.

Theſe experiments very much elucidate the nature of ſwallows in one reſpect, but they are not quite ſatisfactory. The author ſeems to think that intenſe cold does not actually produce lethargy. If this is the caſe, it muſt be admitted, that all the ſwallows named here leave Britain in autumn and return in ſpring; and that we have no proof as yet, that any birds taken torpid from the earth, caverns, rocks, or walls, were really ſwallows.

It is not evident whether the author retains his opinion in the text, that ſwallows have been drawn torpid from water. This has been ſupported by learned men. It is admitted in a late tract by Fabricius on the winter ſleep of animals.

All the experiments on ſwallows are related in the author's hiſtory of theſe birds. Neither that, nor his hiſtory of Owls and Eels, in the ſame volume, are yet tranſlated into Engliſh, ſo far as I know.—T.

lethargic in winter; ſo much the more, as he learns from M. Adanſon, that common ſwallows conſtantly appear in Senegal during autumn, and diſappear in ſpring: and he conceives the European ſwallows, and thoſe ſubject to lethargy, of different ſpecies, though they have hitherto been eſteemed the ſame.

Neither are ſome wanting in the immenſe tribe of fiſhes on which cold produces a ſimilar effect. Tench, if we may credit Pechlin, as quoted by Haller, are of this deſcription (1). In the beginning of winter, he has obſerved them bury themſelves in the mud, juſt as we have ſeen many reptiles and inſects do in the earth. Speaking in general terms, fiſhes are a claſs of animals enjoying the privilege of preſerving action and vivacity, however cold the atmoſphere may be, not only becauſe fluid water can never acquire a great portion of cold, but becauſe they may retire ſtill deeper and deeper whatever is the cold it acquires (2).

Whence

(1) Ad Praelection. Boerhaav. T. 4. Haller, Phyſiol. T. 5.

(2) The poſſibility of fiſhes becoming torpid has been doubted. A few experiments which I have made on ſeveral ſmall fiſhes ſeemed to indicate torpor. When expoſed to conſiderable cold, though leſs than freezing, they ſunk motionleſs to the bottom of a veſſel, and immediately

Whence does it happen that almoſt all reptiles and all inſects, at a certain degree of cold, loſe their whole vigour; their motion ceaſes, and they aſſume the appearance of death; while man, and the moſt part of quadrupeds and birds, at an infinitely greater degree, retain their original ſtrength and vivacity? What can be the proximate, the immediate cauſe of this apparent death in the former animals, and that it is not ſo in the latter? No one that I know of, before M. de Buffon, ſet himſelf ſeriouſly to conſider this ſingular phenomenon. According to him, the animals that become torpid are cold blooded. Such had he found the greater dormouſe (1), the common dormouſe, land hedgehogs, and bats, which of themſelves have no internal heat, and only that of the atmoſphere. Their blood refrigerates in proportion as the atmoſphere refrigerates, which cannot take place with warm blooded animals from their internal principle of heat. Torpidity muſt enſue from this refrigeration, becauſe the uſe of the ſenſes and limbs is

 loſt;

ly revived on removal to a milder atmoſphere. Some, however, preſerved a languid motion while the water was about 32°. La Cepede appears to conſider fiſhes, in general, ſubject to a degree of torpor; and he ſays, the deeper the lethargy, the leſs of their ſubſtance is loſt.—*Hiſtoire des Poiſſons*, T. 1. Diſcours, p. 132. 133.—T.

(1) By him called Lerots.

loſt, and becauſe the blood probably circulates in the larger veſſels only. Such does this author ſuppoſe the immediate cauſe of torpidity in theſe four animals; and he extends it to marmots, and all others ſubject to torpor, as he is convinced their blood is cold (1).

I could have wiſhed that ſo plauſible an hypotheſis had been true; but I have never found it accord with facts. In the firſt place, it is not the caſe that every animal becoming torpid has cold blood. Hedgehogs, marmots, and bats certainly are not ſo. Haller, who has diſſected ſeveral hedgehogs, ſays poſitively, he has always found their blood warm. Liſter, Robinſon, and Lanciſi affirm the ſame (2). I moſt fully aſſent to the opinion of theſe illuſtrious phyſiologiſts. The blood of three hedgehogs diſſected by me was warm; ſo have I found that of bats. M. de Buffon's method was uſed to aſcertain it. He introduced the ball of a ſmall thermometer into the body of dormice by the mouth. He never ſaw the fluid riſe: On the contrary, it ſunk ſometimes one and ſometimes two degrees; an evident ſign that the blood was cold. But whenever I introduced the thermometer into the mouth of hedge-hogs and bats, the fluid roſe to 99°, and even 102°, if kept eight or ten minutes, which demonſtrates

(1) Hiſtoire Naturelle, Tom. 16. 17.

(2) Phyſ. Tom. 2.

demonstrates the heat of both to be the same as our own (1.)

As it was not then in my power to have marmots at pleasure, I desired a much respected friend, who could easily obtain them, to undertake similar experiments (2). He did so; and the result proved, that marmots are not cold blooded, as M. De Buffon had supposed, but are endowed with an internal principle of heat, equalling that of other animals. He was positively assured of it, by keeping the thermometer in the axilla of two marmots. The heat of one raised it to 90° in eight minutes, 20° above the temperature of

 the

(1) Hunter found the heat of a dormouse 80° and 85°, when the heat of the atmosphere was 50° or 60°; when the animal was lively, 91° or 93°. A thermometer introduced within the body and applied to the pelvis rose to 99°, the atmosphere at 66°. The mouse being put an hour in an atmosphere of 13°, it rose to 83°.

In hedge-hogs, he observes, Mr Jenner found the heat 48°, when they were torpid or the atmosphere 44°, and the heat only 30° in an atmosphere of 26°. But exposed to this cold two days, the heat in the rectum was 93°. So far from being torpid a hedge-hog was lively, and the bed on which it lay felt warm. Whence he concludes that excessive cold rouses the vital powers.—*Observations on the Animal Economy*—T.

(2) Sig. Giannambrogio of Milan an able chemist, already well known in the republic of letters, by a most elaborate

rhe atmofphere at the time; and in fifteen minutes, that of the other raifed it to 93° (1).

Some

laborate and learned differtation on Fox-tail, (Covetta) and ammunition bread.

(1) It will be obferved, that, in the courfe of this work, much is faid of cold and warm blooded animals, and fome important conclufions thence deduced. Notwithftanding all that has been written on animal heat, the caufe is undoubtedly very obfcure. Experiments and reafoning have neither fufficiently coincided, nor been fo generally adopted, as to entitle us to form pofitive opinions on the fubject, more efpecially when philofophers do not agree whether the living body has the property of producing heat or cold.

Amphibia, reptiles, fifhes, infects and worms, are generally believed to have cold blood. However, there are experiments on all but the laft that feem to indicate a principle of internal heat independent of the atmofphere; fo that the thermometer will afcend when removed from the open air into their bodies. Yet many naturalifts deny that they have any principle of heat; and think the temperature of their bodies muft always be exactly the fame as that of the furrounding medium. But, in my opinion, fufficient accuracy in experiment has been neglected, and due attention has not been paid to the ftate of the medium where the animals previoufly were. A few experiments which I have made on the fubject, even with thofe precautions, have been difcordant; one thing however

Some time afterwards I ſucceeded in procuring two marmots.—The experiments made on them completely correſponded with thoſe of my learned friend. In the open air, the thermometer ſtood at 66°; when introduced within the throat, it roſe to 102°: therefore the fact, with reſpect to all theſe animals, cannot be more deciſive (1).

How, in contradiction to the facts before us, which admit of no reply, can Buffon's aſſertion ſubſiſt, which is in expreſs terms—that he has found the blood of hedgehogs and bats cold? Without ſuppoſing his animals of a different ſpecies from mine, and much leſs not to allow his

ever is certain, namely, that all inſects are not cold; and although the heat of one ſingly is not ſenſible, the heat of a number collected is very conſiderable. I kept a thermometer in a bee hive; it ſometimes ſtood between 90° and 98°. The height did not ſeem entirely regulated by the temperature of the atmoſphere, for it has been 80° and 90° in the hive, while 55° and 51° in the ſhade. When 62° in the ſhade, it has been as low as 82° in the hive. A number of bees collected do not become torpid during winter. When the atmoſphere was at 25°, which is cold weather in Scotland, I reverſed a hive, and introduced a thermometer among the bees, it immediately aſcended to 71°, and would perhaps have roſe higher.—T.

(1) I believe that the author afterwards made a ſeries of inveſtigations concerning marmots, but I have not been able to procure the work.—T.

his experiments credit, there is an eafy method of conciliating the differences: It is, That the French naturalift has made his experiments in winter, when the animals are deprived of fenfe and motion, and actually are not different from cold-blooded animals, becaufe inclemency of the feafon has exhaufted every principle of heat. Experiment had taught me, as reafon itfelf would do, that neither bats nor hedgehogs become lethargic unlefs their internal heat is diminifhed.

From the whole it is evident, that, notwithftanding M. De Buffon's idea is ill founded, it is indubitable that refrigeration of the blood takes place in all animals experiencing lethargic fleep. Shall we hence conclude the lethargy an immediate confequence of this refrigeration? Let us confider the incipient torpor of an animal.—The influence of cold begins: it acts not only on the exterior, but alfo internally. Application of the thermometer will not allow me to doubt it; and evinces, that the action of the cold is equally communicated to the fanguinous fluid and the folids. Yet I am left in doubt, whether torpidity proceeds from refrigeration of the blood, of the folids, or of both. I try to analize the fact; and reflect whether there is any animal, among thofe becoming torpid, which, after privation of its whole blood, will, for a confiderable time, preferve its original vivacity and vigour.

Such

Such a one may perhaps afcertain the truth. If, after being deprived of the blood, this animal does not become torpid on expofure to cold, then the fole caufe of torpidity lies in the refrigeration of the blood: if torpor does enfue, we cannot recur to the refrigeration of the blood, but to that of the folids, or to the influence that cold has on them. An animal of this kind is not only poffible, but it exifts,—there are feveral, as, frogs, toads, tree frogs, and water newts. In my experiments I have obferved, that all the blood being difcharged from the opened heart or divided aorta, thefe animals ftill leap for many hours, run, dive in water, and fwim to the top, retain a lively fenfe of fight and feeling; in a word, continue the exercife of every corporal function that they had before (1). I now repeated the experiments, and began with frogs.—Several very vivacious were buried in fnow; part were untouched, and part of the number deprived of the blood, by endeavouring to evacuate it completely from the heart and large veffels. In eight or ten minutes, fome were examined: the blooded and thofe entire were exactly in the fame ftate, that is, half dead, and not attempting to efcape, though at liberty. In fifteen minutes, I drew

(1) Thefe experiments are fpoken of in my work, De Fenomeni della Circolazione.

drew others from the ſnow, ſome entire, and others deprived of blood; they appeared motionleſs, and as if frozen. All were replaced in the ſnow, and in a few hours removed to a warm ſituation. I attentively conſidered what happened. By degrees, the contracted ones ſtretched, their eyes opened, they ſhook themſelves, began to leap and eſcape. Being again conſigned to the ſnow, and taken out after a certain interval, all exhibited the ſame phenomena. There was not the ſmalleſt difference at whatever ſeaſon the experiments were made. I found a remarkable correſpondence between tree frogs, toads, and water newts: the whole became lethargic in the ſame manner, by the cold of ſnow,—and returned to their former animation when removed from it.

The coincidence of theſe facts obliges me to ſay, that the failure of ſenſe and motion does not ariſe from refrigeration of the blood, for it cannot take place where there is none, nor from the relaxed circulation of this fluid, but depends entirely on the ſolids, which, being powerfully attacked by cold, are in a condition very different from the natural ſtate. What the new condition is, may be diſcovered from the phenomena of lethargic animals. They appear contracted, the muſcles have no longer their natural ſoftneſs and pliancy, but become hard and withered.

Thus

Thus there is abſolute proof that the muſcular fibre acquires great rigidity, and it is ſuch as muſt materially prejudice its irritability: This is evident from the moſt active ſtimulants not occaſioning the ſlighteſt contraction or corrugation. Irritability is commonly believed the origin and principle of life:. when it is ſo much injured in the animals of which we ſpeak, it muſt occaſion that lethargy, that ſimilitude to death which is manifeſted.

If this is the real and immediate cauſe of torpor in theſe animals, I cannot ſee any reaſon why it ſhould not extend to all others ſubject to torpidity. It is impoſſible, indeed, to deprive the warm blooded animals, liable to torpor, of what Buffon ſuppoſes the efficient means, for their nature does not admit of them living without blood. But their muſcular rigidity, alſo, is certain; and it renders them inſenſible to every ſtimulus while in the lethargic ſlumber. I have ſeen it in bats. I ſprinkled them with ſalt, bathed them with warm water, pricked them with needles, and laid open the pectoral muſcle, methods moſt powerful to excite irritability, but all were ineffectual, while they were oppreſſed by profound lethargy. The electric ſpark, ſo fit above all ſtimulants to awaken irritability, was equally ineffectual. If the irritability of warm blooded animals is ſuſpended by means of cold the ſame as that of the cold blooded, and if the ceſſation of this power, as far as appears

pears to me, is the only and immediate caufe of lethargic fleep in the latter, I do not fee why it may not alfo be extended to the former.

The fame degree of cold does not occafion torpidity in all the animals fubject to it. Very little is neceffary for fome; others require confiderably more; and others an exceffive degree (1). What we defign temperate, which is fo mild to our fenfations, occafions torpidity in dormice; a little more affects bees, fnakes, vipers, and many fpecies of bats. What affects frogs, toads, newts, and others, approaches freezing, but this is far from operating on marmots, as they require 11 degrees below freezing (2). The difference

(1) Reaumur Memoires fur les Infectes.

(2) At different feafons, however, the fame cold feems to produce different effects. On the 18 of July, when the heat is generally between 60° and 70°, I cooled the water with fix Hydrachnae down to 38°. They all funk to the bottom, and remained completely torpid. In general, they yielded to 42° or 40°, except one, called, by Muller, a variety of the Papillator (but apparently conftituting a particular fpecies in Scotland), which refifted 38° a long time, and then funk down along with the reft. All revived at a moderate heat.

On the 5 of December, I took five hydrachnae from water at about 44°, and put them into water at 36°. They became more languid. The thermometer gradually ference of cold required to occasion torpor, must arise from the different nature of the muscular fibre, which in some animals is more susceptible of it than in others. If the power of cold is encreased, torpor degenerates into death.

The reason assigned by physiologists for the death of man and animals from cold, is certainly very plausible. The contraction of the cutaneous vessels forces a reflux of the blood to the internal parts of the body; whence is occasioned the insensibility and stiffness of our fingers and the extreme paleness of the body. Cold becoming more intense, the internal and large vessels contract, and the reflux of blood is greater; but those of the brain, being better defended by the cranium from the injuries of the air, are not so liable to contract. Blood flows copiously in the

ly fell to 32°; and a crust of ice, a quarter of an inch thick, formed on the top. Still the animals could move, though languidly, and a very faint motion was perceptible in the legs of one. Thus the water remained some hours. In twenty hours after the experiment began, the thermometer stood at 36°. None of the hydrachnae were torpid.

Experiments were at the same time made on several other aquatic animals. Squillae and gyrini did not become torpid at 32°. Perhaps this subject deserves further consideration.—T.

the arteries, covered and protected from the atmosphere, while the jugular veins being contracted, it is with difficulty returned to the heart. A sensible relaxation in the circulation will ensue, which, encreasing as the intenseness of the cold advances, will end in rest, and the animal will die.

In the northern countries, it is not unusual that a *coup de froid* kills men on the spot. The cause has been supposed nearly the same. The lungs, exposed to the immediate action of the cold air, are suddenly contracted, and impede the passage of the blood from the right to the left ventricle of the heart. Thus, according to those authors, death proceeds from obstructed circulation (1).

I am fully convinced this may be the real reason why numberless animals die, that is, all those necessitated to perish when the circulation is stopped; but there are many that live, at least some time, when the circulation of the fluids is suspended, or even when they are entirely taken away. The death of animals by cold must therefore

(1) According to the most authentic accounts, when men are exposed to intense cold, an irresistible desire to sleep ensues, which ends in death. Thus, it is not improbable that men may be in a degree of torpor and revive. —T.

fore be aſcribed to ſome other cauſe than obſtructed circulation (1).

To diſcover the immediate cauſe of death, I made obſervations on animals killed by cold ſimilar to thoſe I had made on animals becoming lethargic at a ſmall degree. The phenomena attending death are theſe. Rigidity of the muſcles gradually encreaſes until the whole body hardens and freezes. Freezing firſt appears at the extremities, whence it extends to the centre. If taken to thaw in milder air, the parts acquire their former pliancy, but the animal will not revive. Its death is in conſequence of having been frozen, but we cannot ſay it is from freezing of the blood: firſt, from the reaſons above given; ſecondly, becauſe ſeveral animals being expoſed to cold, ſome deprived of blood, and others untouched, all died in the ſame time. Thus, death of this kind proceeds from the ſolids being frozen. At a certain degree of cold, the muſcles grow rigid, and the irritable power is deſtroyed; thence proceeds their apparent death. Cold more intenſe freezes the muſcles; freezing deſtroys the power of

(1) In my work *De fenomeni della Circolazione*, I have demonſtrated, that many animals live a conſiderable time after privation of the whole blood, alſo when circulation is ſuſpended, by tying up the aorta. I have ſince obſerved this in reptiles, as vipers, ſerpents, eels, &c.

of irritability, and real death is the consequence. The muscular fibre is contracted by cold; the liquid, rendering it moist and pliant, is inspissated; but freezing vitiates the use of this fluid, by changing it into so many icicles, whose sharp and cutting points lacerate the finest and most delicate parts of the fibre. The muscular flesh is then discovered to be full of these icicles; and, when one attempts to twist or bend it, fracture ensues, as of a friable substance.

CHAP. VI.

INFUSION ANIMALCULA EXPOSED TO VARIOUS ODOURS AND LIQUORS; TO ELECTRICITY AND A VACUUM.

CERTAIN odours are to insects the most virulent poison. Such has Reaumur found the oil of turpentine, and the fumes of tobacco. The odour of camphor, according to Menghini, has the same effect; and its vapours are still more efficacious when burnt (1). In my investigation, I proposed to institute the greatest possible number of comparisons between known animals and those so remarkable as the animalcula of infusions, the better to penetrate their origin, nature, and

(1) Commentar. Acad. Bonon. T. 3.

and properties, which induced me to refolve not to neglect trying the effect of odours upon them. I began with that of camphor: the refult was precifely the fame as has been obferved in infects. The vapours of this refin occafioned fudden agitation and difcompofure in the animalcula: they endeavoured to retreat from the malignant fumes, by retiring deep into the infufions. If the vapour was rare, they were long of dying; but when denfe, it immediately became fatal. The odour of the oil of turpentine killed them, but not fo foon as camphor. The fumes of tobacco were not fatal for fome hours: but thofe of fulphur, inftantaneoufly (1).

The liquids ufed in my experiments were chiefly oleaginous, becaufe thefe are mortal to infects: they were no lefs fo to animalcula. I omit the fpirituous or corrofive, which killed them in a moment; as alfo falt water, vinegar, ink, brandy, and fpirit of wine.

We could hardly believe that human urine produces animalcula, after ftanding a few days, as Hartfoeker has obferved, if the fame phenomenon was not daily obferved in vinegar, which is full of microfcopic eels, though a fluid equally deftructive to animalcula as urine. Repeating

 the

(1) Odours penetrate deep into water, but to a very different degree: Some will traverfe feveral inches, others not one. There are a few curious experiments on the fubject. *Senebier Phyfiologie Vegetale.* Tom. 5. chap. 6.—T.

the experiment, I found it perfectly true. A dark cineritious pellicle covers the furface of urine that has been fome time at reft; and here are the animalcula generated. They are of a roundifh figure, and in fize like animated points (1). The fame race of animalcula always continues in urine, kept feveral months; but no new fpecies ever appears. One might fufpect, that they are produced after the urine has loft its acrid and corrofive principle: but, befides retaining the characteriftics of real urine, it is ftill fatal to other animalcula; nor do thofe of ftale urine die when put into that which is recent. Therefore this fpecies muft be effentially different from common infufion animalcula.

It is well known, that the electric fhock kills animals; and as they are fmaller, fo are they more eafily killed. A battery ten feet fquare with difficulty kills a cat or a dog (2); but very few feet is enough for a pigeon; lefs for a goldfinch or canary; and thus diminifhing as the animals are fmaller. It appeared that a fhock of moderate ftrength would deftroy animalcula; but not being poffeffed of an electrical machine, I availed myfelf of the affiftance of Sig. Pietro Mofcati, then my colleague in the royal univerfity of Pavia, who, in

(1) Leeuwenhoeck thinks he faw animalcula in the recent urine of a mare, *De ortu et defluvio capillorum.*—T.

(2) Prieftley. Hiftory of Electricity.

in addition to ſkill in electrical experiments, had a machine of conſiderable power. He was ſo obliging as frequently to comply with my philoſophical requeſt, and ſubject infuſions full of animalcula to the electric ſhock. They were invulnerable, and as lively after receiving the ſhock as before; nor was there any difference, though two, three, or more ſparks were drawn from the infuſions. The weakneſs of the ſhock cannot be objected, as two or three killed a leech, newt, or ſimilar ſmall animal.

I was accuſtomed to communicate the reſult of my experiments to M. Bonnet before publiſhing them, becauſe this illuſtrious naturaliſt ſeemed to wiſh to partake of my little diſcoveries. Among the reſt, I mentioned that which Dr Moſcati had made. He anſwered, that he had ſhown M. de Sauſſure my letters, who had repeated our electrical experiments, but with oppoſite effects, which he aſcribes to the great humidity of the air of Lombardy preventing electricity from being ſo powerful as at Geneva. He adds, that M. de Sauſſure would himſelf inform me of his reſults, which he did very ſoon; and I tranſcribe them here, ſuch as M. Bonnet ſent to me, in the following letter (1):

G 3 "*My*

(1) M. Bonnet afterwards publiſhed a collection of his letters to his learned correſpondents. I have collated all that are quoted here with thoſe in that collection, and corrected any errors or inaccuracies.—T.

' *My Solitude*, 15 *February* 1772.

' It was but lately, my celebrated friend, that ' M. de Sauſſure tranſmitted me his experiments ' on our animalcula. Certain of the pleaſure ' you will derive, I do not delay a moment in ' ſending them. You will judge of my opinion ' by your own; and I have no doubt will be as ' well ſatisfied. This is a ſubject equally new ' and curious for the meditation of philoſophers; ' nor is it to be queſtioned, that in future we ſhall ' be able to diverſify and extend this new kind ' of *phyſico-electricity*. But a plan muſt firſt be ' pointed out; and there is no ſmall merit in ' opening up the unknown ſources of truth, ' which experiment is to accompliſh. I can- ' not retard the pleaſure you will have in M. de ' Sauſſure's letter.

" *Geneva*, 8 *February* 1772.

" I return, Sir, with a thouſand thanks, the " letters you had the goodneſs to ſend. I have " peruſed both with extreme ſatisfaction, but re- " gret that you tranſmitted my letter on the " *tranſparency of germs* to Sig. Spallanzani, for " it is not worthy of that honour, and ſtill leſs of " your eulogium on it (1). Behold where you " have conveyed that trifling epiſtle; being in " your's, it will alſo be publiſhed by Signior Spal- " lanzani,

(1) Vide M. Bonnet's ſecond letter at the end of this Tract.

"lanzani, though I never thought it would have
"been printed, which it little deſerves.

"At another time, I have told you, Sir, but it
"cannot be repeated too often, my extreme plea-
"ſure in peruſing the beautiful ſeries of Signior
"Spallanzani's experiments and obſervations.
"He is fit for your friend and fellow labourer.
"With him are found that order, that analyſis,
"that juſt and rigorous logic, of which your own
"writings afford an example.

"You know that I alſo have been occupied
"with infuſion animalcula; and you yourſelf
"have honoured me by inſerting ſome of my re-
"ſults in the republication of your Palingeneſie.
"It has pleaſed me to obſerve, that my experi-
"ments entirely coincide with Signior Spallan-
"zani's obſervations.

"I had attempted, as he did, to repeat
"Mr Needham's ſingular experiment, which
"conſiſts in introducing the halves of corn
"pickles into ſlices of clay, that they might ger-
"minate at the ſurface of water. I alſo ſaw ani-
"malcula originate as in common infuſions; but
"neither diſcovered thoſe zoophytes, nor vege-
"table roots producing animalcula, which Mr
"Needham had ſeen rather with the eyes of an
"imagination heated by the love of theory, than
"with the calm ſenſes of a philoſopher.

"I had ſeen the minute round heads which "terminate the filaments of mould burſt when "moiſtened, and diſcharge a globular duſt. I had "communicated it to Baron Haller, who ſpeaks "of it in the article *Mucor*, in the new edition "of his hiſtory of Swiſs Plants; but I had nei-"ther witneſſed nor ſuſpected the ſurpriſing in-"deſtructibility of this duſt, which Signior Spal-"lanzani juſtly ſuppoſes the ſeed of the plant.

"I had long endeavoured to kill infuſion ani-"malcula by electricity, and without better ſuc-"ceſs than Sig. Moſcati and Spallanzani; but "more exact obſervations have at laſt given op-"poſite reſults. You may communicate them "to the latter if you think proper.

"Some drops of rice infuſion, full of animal-"cula, were put on a glaſs ſlider four inches "long and one broad, with the rounded point "of a quill, and the drops drawn out ſo as to "form an uninterrupted line from one extremity "of the glaſs to the other. When the ſlider was "applied to the machine, ſo that the electric "fluid paſſed continually, and without ſhocks "and ſparks, they moved about, and did every "thing as uſual. In general, I have obſerved, "that ſimple electricity, that is without ſhocks "and ſparks, never produced the leaſt effect; "but, when the ſlider was ſo diſpoſed that a

"ſtrong

" ſtrong ſpark ſuddenly paſſed from one end to
" the other, the whole were killed almoſt inſtan-
" taneouſly, and any that ſurvived died very
" ſoon. The Leyden phial was not required; a
" ſpark from the conductor, without any more
" apparatus, was ſufficient.

" I was curious to learn what happened at the
" moment of the ſhock, and diſpoſed my ſlider
" ſo as to obſerve the animalcula. After a vio-
" lent ſhock, they were always in agitation.
" Some were immediately reduced to granuli, a
" mode of death to which you know theſe ani-
" mals are very ſubject: Polypi, which in the
" manner of multiplication reſemble them ſo
" much, often periſh in the ſame way. The ani-
" malcula remaining entire, revolved a few ſe-
" conds in the liquid, then ſtopped at the bot-
" tom, and died on the ſpot, without any change
" of figure.

" The ſpark is fatal, though they are in a
" greater quantity of water. I filled a glaſs tube
" two lines in diameter and four inches long with
" water full of animalcula. Five or ſix very
" ſtrong ſparks drawn through it killed them all.
" But the conſequence was different, on taking
" tubes four or five lines in diameter. The
" electric fluid, diſperſed in ſo great a ſpace, is
" not of that denſity to lacerate the body of ani-
" malcula.

" One

" One fact has to me appeared moſt ſingu-
" lar. You know, Sir, that the ſparks, which
" we wiſh to direct through a ſubſtance, often
" glide over the ſurface rather than penetrate it,
" even where the ſubſtance is very permeable by
" electricity. Matters may be ſo arranged, that
" ſuch a phenomenon will infallibly ſucceed; and
" I have frequently prepared a baſon of water ſo
" as the ſparks would paſs over a ſurface a foot
" long without penetrating the water. This I
" found to have the ſame effect on animalcula as
" when the ſparks paſſed through the water it-
" ſelf. While my eye was applied to the microſ-
" cope, the moment ſuperficial ſparks were
" drawn, I ſaw the whole animalcula in agita-
" tion; ſome reduced to grains, and the reſt im-
" mediately die.

" Do not imagine the poſſibility of my being
" deceived, by ſuppoſing the ſpark glided over
" the ſurface, when it in reality penetrated the
" water, for the difference is too ſenſible. That
" which glides appears moſt brilliant over the
" whole ſurface; that which penetrates paſſes
" without being ſeen. Perhaps you will ſay one
" portion of the electric fluid paſſes within while
" the remainder paſſes without. Doubtleſs this
" may be; was it ſo, it ſeems that ſuch a divi-
" ſion ſhould weaken the ſpark; whereas it ap-
" pears more brilliant and ſonorous than uſual.

" Theſe

"These superficial sparks do not go deep: "they have no effect on animalcula swimming "five or six lines from the surface: only few are "killed, which certainly are near the top when "the spark passes: the rest remain vivacious and "well. At this depth, a very strong shock, such "as is capable of melting an inch and a half of "iron wire one twelfth of an inch in diameter, "has not the least influence on them.

"These, Sir, are the results of my most interest-"ing experiments on electricity operating on ani-"malcula. I wish they may satisfy you and Sig. "Spallanzani, if communicated to him, or that "you will tell me what more I ought to do. Two "circumstances should be mentioned; one that "the experiments were made on the infusion "animalcula of wheat, hempseed, and maize, "and the result has always been uniform; the "other, that the animalcula were of the largest "size that infusions produce."

M. de Saussure's experiments besides being ingeniously conceived, and happily executed, appear decisive; and they induced me to suppose, some unforeseen accident had opposed Sig. Moscati's; and perhaps that the excessive humidity of the air of Pavia, as the Genevese professor had thought might be the cause, particularly as our experiments were made in winter; I wished to repeat them in a more favourable season, but that

that became impoſſible, as Sig. Moſcati was ſoon afterwards re-eſtabliſhed in Milan, his native place. However, in two years, having procured an excellent machine, I repeated them myſelf, and the conſequence could not correſpond more with M. de Sauſſure's reſults.

The animalcula were firſt expoſed to the diſcharge of the Franklinian battery. Upon it was put a little ſpot of pitch, with a very ſmall hole through the centre, full of infuſion; the ſpark was drawn through this hole. Not one animalcula of thouſands in the liquid ſurvived the ſhock a moment; many were wounded and reduced by the electric vapour, and many appeared untouched. The ſhock was diminiſhed by charging the battery leſs, but the effect was the ſame. The quantity of liquid expoſed to the ſhock was encreaſed, by drawing a right line on the ſpot, two thirds of an inch long, and two lines broad, proceeding from the central hole. Then the ſhock was tranſmitted through the whole fluid. It was a real thunderbolt to the animalcula; all immediately died. If the breadth of the line was encreaſed but not the length, a change enſued. So long as only two lines broad, none eſcaped, but when more than that, the animalcula either were not injured or did not die for ſome time. Thoſe within the limits of two lines were ſtunned, and continually revolved on themſelves; the vertigin-

ous

ous motion diminifhed by degrees, and in a quarter of an hour entirely ceafed. Thofe not fo near as two lines furvived longer; the activity and livelinefs of the moft diftant evinced they were not effected by the electric fluid. If, inftead of encreafing the breadth of the liquid more than two lines, it was produced from the centre of the fpot, fo as to reach the circumference, which made full five inches in length, the fhock equally killed the animalcula throughout the whole. Such were my experiments with Franklin's battery.

Let us now fpeak of a fimple fpark drawn from the conductor. Here I ufed the fame fpot of pitch which was put on the conductor. The fpark drawn through the central hole feemed more brilliant and fonorous. The central hole alone was filled with fluid, or a little channel was added of various dimenfions on the fpot. Every time the fpark was drawn through the hole, all the animalcula perifhed, but three or four were required to kill them in the little channel.

Many fubftances are better conductors of the electric fluid than water; and where the fhock was weak, I was unable to direct it through the channel, efpecially if very long and narrow. But the fluid did penetrate and act upon it, as appeared from the crackling when the conducting rod was applied; and this trifling electricity was

fufficient

ſufficient to kill animalcula, which I could not have believed.

To learn whether the electricity diſſipated by metal points, fixed to conductors, would deſtroy animalcula, I applied a drop of infuſion to one, and found it to be ſo only if the electric fluid paſſed for ſome time from the point.

It was evident, in ſhort, that every ſhock, however feeble, was always fatal. But ſimple electricity, that is which operates ſilently, had no effect, as M. de Sauſſure alſo obſerved. As to the kind of animalcula, I can freely affirm that experiment has been omitted on none of the vaſt variety, and electricity has been alike fatal to all.

The perfect coincidence of my experiments with M. de Sauſſure's leads me to publiſh them; but the friendſhip I had always borne to Sig. Moſcati, impoſed the duty of firſt inquiring whether he had repeated our experiments as he promiſed, when I communicated them to the Geneveſe naturaliſt. I tranſcribe his anſwer, as he ſeemed to wiſh it ſhould be publiſhed. It both proves that he kept his promiſe, and obtained new reſults, which cannot but do great credit to truth.

'In your laſt letter, you inquire whether I 'have repeated the experiments which we made 'ſome years ago on electrifying animalcula,

'which

'which were then neither injured nor killed: 'More than once they have been repeated, with 'different and even oppofite refults, which I have 'alfo difcovered did not arife from the weaknefs 'of my machine, fince fulminating electricity is 'not neceffary to kill them, but to the method 'employed. When we made the experiments, 'we ufed a little brafs cup; and, towards its cen-'tre, endeavoured to direct the fhock of the jar 'where the cup was fixed. We never killed one 'animalcule by this method; nor was I more 'fuccefsful on repeating the experiment alone, at 'the moft favourable periods for electricity. But 'as you informed me that M. de Sauffure, whofe 'merit and accuracy in experiment I know and 'highly efteem, had feen them die; and as it had 'alfo appeared, that the fpark, inftead of com-'ing from the liquid with the animalcula, efcap-'ed from the circumference and fides of the vef-'fel, I began to fufpect that, inftead of paffing 'through the fluid and ftriking the animalcula, 'it came directly from the metallic cup to the 'conductor, gliding over the furface of the in-'fufion. I therefore changed the plan. On a 'polifhed cryftal plate, well wiped, I put a little 'hollow piece of wax, and faftened two brafs 'wires, with obtufe points, from two oppofite 'extremities near the furface; one communicat-'ed within, and the other without, the jar. This

'apparatus

'apparatus was placed before a compound microſ-' cope, made by Cuff, which his Excellency 'the Conte di Firmian had preſented to me. 'The hollow was filled with liquid, containing 'lively and vigorous animalcula; and I kept my 'eye intent on them, while another perſon work-'ed the machine: I ſucceeded thus in killing 'them with a ſhock of no great ſtrength, and 'with a ſmall jar. The animalcula ſuffered ei-'ther by receiving the ſhock or being in the vi-'cinity when it paſſed; thoſe at the bottom of 'the hollow remaining alive. One thing I re-'collect to have particularly remarked, that the 'animalcula killed aſſumed a briſtly appearance, 'like a microſcopic ſponge, and were more 'opaque than the reſt; from the ſuperficial aſ-'perities occaſioned by the ſhock, they ſeemed 'larger than when alive. I was in this manner 'convinced of the truth of the experiment; and 'being afterwards engaged in other matters, as 'I ſtill am, thought no more of it. Excuſe the 'brevity of the recital and the dryneſs of the ſub-'ject; it does not ariſe from indifference to ſuch 'agreeable ſtudies, but the neceſſity which you 'well know I am under of applying to ſubjects 'leſs intereſting. I have the honour to be, with 'the utmoſt eſteem and friendſhip, your moſt de-'voted, obliged ſervant and friend,

'*Milan*, 6 *Jan.* 1774. PIETRO MOSCATI.'

I have ſtill to ſpeak of animalcula included in a vacuum, which is the laſt ſubject that I propoſed to diſcuſs in this chapter.—The conſequences were different according to the difference of ſpecies; a vacuum was very ſoon fatal to ſome, and others lived in it very long. Let us enter on detail.

Several ſmall glaſs tubes, cloſe at one end, were filled with various infuſions. The tubes were very ſmall, and the glaſs extremely thin, ſo that applying the magnifier I might ſee what paſſed within when appended to the inſide of a receiver. Some open tubes, full of the ſame infuſions, were kept in reſerve for the neceſſary compariſons. Sixteen days privation of air did not injure the animalcula; on the twentieth day, they began to die; and on the twenty-fourth, all were dead. Thoſe in the open air ſtill ſurvived, otherwiſe one might ſay the natural term of life was expired (1).

The experiments were repeated on more infuſions of a different kind. Of ſome the animalcula lived a month in vacuo, and even thirty-five days: thoſe of others died in fourteen, eleven, and eight days: and ſome lived only two. The infuſions mentioned in my Diſſertation were ſimilar

(1) Nearly a century ago, Derham obſerves, that he kept the animalcula of pepper water twenty-four hours alive in vacuo. Some naturaliſts have erroneouſly cited this paſſage, as if the animalcula lived a month. *Phyſico-Theology.*—T.

milar to theſe; their animalcula died in about two days (1).

The nature of certain animals is wonderful. They continue their ordinary corporal functions in vacuo a conſiderable time. Vipers and ſnakes will creep, and leeches ſwim in fluids. Some inſects feed, and others perform, the work of generation (2): Such is the nature of animalcula. In a vacuum, they preſerve their wonted motions, aſcending or ſinking, darting to the ſurface of the infuſion, and diving into its deeps, or driving before them the floating particles on which they feed. I ſhall afterwards ſpeak of their ſingular modes of propagation; and this alſo ſucceeds for ſeveral days in vacuo. In proceſs of time, and according to the ſtrength of the animalcula, motion relaxes and ends in death. It ſometimes happens, but rarely, that, being taken from the receiver, and left expoſed to the open air, they revive.

Theſe experiments have confirmed two obſervations in my Diſſertation; the uſual ſterility of infuſions in vacuo; and their fertility when the air was only rarified. No animal or vegetable ſubſtance macerated in vacuo ever produced a ſingle animalcule: the reverſe uniformly happened, on leaving a portion of air in the receiver. As much as keeps thirteen inches of mercury in equilibrio

(1) Capitolo, 10. (2) Sperienze del Cimento.

equilibrio is moſt advantageous for them to originate.

Similar phenomena have occurred with the eggs of animals. I have repeatedly put thoſe of terreſtrial and aquatic inſects in the receiver of an air-pump, but none were ever hatched, though they had all the requiſites neceſſary except air.

From theſe and other analogous facts is deduced the neceſſity of air for the developement of every animated being. While the animal is concentrated in the egg, it enjoys the beneficial influence of air by an infinity of minute pores throughout the egg, which have not eſcaped the notice of naturaliſts: when liberated from it, or the involucrum by which it was concealed in the womb of the mother, it receives the benefit of the air by other more evident ways. An immenſe number of animals reſpire by the mouth, and many by apertures in the ſides of the body, by the extremity of the abdomen, or by other parts. The breath enters the mouth of numerous channels at the ſurface, which conduct the air by ramifications into the interior of the body. The animalcula of infuſions, notwithſtanding their apparent ſimplicity of ſtructure, exhibit an organ which we are ſtrongly induced to conceive is intended for reſpiration. And in this caſe, they more than ever ſtand in need of the aerial fluid, which is evident by depriving them of it. There are fome that die very foon after privation of air, others after a longer interval, and the length of it is according to the nature of the animalcula. A fparrow, a nightingale, and in general other birds, quickly die in vacuo. A lizard, a frog, or a reptile, remain fome time alive; infects, ufually much longer. As there are diftinctions among infufion animalcula, there is a difference in their ability to fupport a vacuum; and thofe that do fo longeft, feem of all animals, the moft capable of living without air, at leaft we are unacquainted with any fpecies that exift a month in that ftate, as we have feen fome animalcula do. Though they can long fupport privation of air, they at laft fink under it and die, which is proved by the animalcula living in the open air above two months. And this confirms the general rule, that all animals require air.

I know there are inftances given of fome which are faid to have lived without this element; fuch as the famous ftories of frogs found alive in the middle of the hardeft fubftances, and living toads difcovered in the centre of ftones, or of entire trees, where not a particle of air could penetrate into their hidden receffes (1). But I am alfo aware, that fuch hiftories are more the object of the admiration than belief of perfons who have made any progrefs in experimental philofophy; becaufe they

(1) Melanges d'Hiftoire Naturelle.

they are not corroborated by that authenticity which is eſſential in a caſe ſo ſtrange and paradoxical, more eſpecially as the pulmonary ſtructure manifeſts that they are adapted for reſpiration. Therefore, until facts are produced to the contrary, more credible and better proved, we have ſufficient reaſon to aſſert, there is no living animal in nature, limiting ourſelves to thoſe already known, that can exiſt without the benefit of air (1).

(1) There are numerous accounts, unqueſtionably very ſingular, of animals found alive in ſolid maſſes; and ſeveral intelligent and reputable perſons have gone ſo far as to affirm that they have ſeen them. It is very difficult to conceive how an animal formed for breathing can live deprived of air: but it ſeems little leſs difficult to reject the teſtimony of the moſt creditable men Every day, we diſcover new ſingularities, which would be abſolutely incredible without authentic information. This is a fact in which more than uſual evidence is required; for without that, nay, without ocular demonſtration, we find it incomprehenſible. Yet I can never underſtand why the toad is almoſt always ſelected for theſe wonderful preſervations, and ſeldom any other animal. There is an extract from a late Memoir on the ſubject by Murhard, *Philoſophical Magazine*, vol. 3.—T.

CHAP. VII.

SINGULAR MODE IN WHICH MANY SPECIES OF ANIMALCULA PROPAGATE.

If we obferve two animalcula united, the firft idea which arifes is, that they are occupied in the work of generation; and we cannot avoid it, though the animals exciting the idea are infinitely minute, becaufe uniform example proves this to be the ufual pofition of animals for propagating the fpecies. Hence the actual copulation of animalcula has been fuppofed from feeing them united in pairs. Such is the opinion of Ellis, and the celebrated Father Beccaria, as he informs me in a letter, written many years ago, concerning my firft obfervations on infufion animalcula. The whole is here tranfcribed, as it is particular on this phenomenon, and alludes to other important points.

Turin, 11 *September* 1765.

' If your excellent experiments required any ' fupport from the teftimony of another, I could ' afford it: for, twelve years ago, when the Duke ' of Savoy called me to fee Mr Needham's ex-' periments on microfcopic animals, I thought it ' my 'my duty to prefent him a long treatife with this 'motto, *Si parva licet componere magnis*,—in 'which I demonftrated, from analogy, the fallacy 'of the opinion advanced; and then pointed out, 'that it was not the confequence of experiment. 'Befides, I employed moft of my leifure hours, 'for two years, in experiments on what feemed 'fo interefting a fubject; and fucceeded in dif'covering, 1. How infufions diffolved the fixed 'falts of the fubftances, carrying them to the 'edge and diffipating the volatile part, as is evi'dent by the tafte and fmell, and leaving a gela'tinous matter well adapted to collect and feed 'animalcula; 2. That animalcula have a proper 'internal and fpontaneous motion, in addition to 'the characteriftics of avoiding obftacles, chang'ing their direction, and paffing above them; alfo, 'the two following, thus defcribed in my Treatife, '*Lucem refugiunt, paulo vividiorem, putrem ma'teriam appetunt, quafi ut vefcantur.* A fingular 'fact, relative to the multiplication of animal'cula, cannot have efcaped your penetration. I 'have often remarked, that at full fize they 'feemed in copulation. Two animalcula are fre'quently feen at the circumference of a drop of 'putrid matter, one fupported by a particle or 'joined to it, or, to fpeak more certainly and 'adhering to appearances, in contact with it, and 'continually vibrating or ofcillating in the direc-

'tion of a ſtraight line uniting the centres of the 'two bodies; and the oſcillation is particularly 'manifeſted by the motion of ſome internal parts 'near the line of direction. But ſome reſerve 'has probably induced you, as well as myſelf, 'to be ſilent concerning this moſt ſimple obſer-'vation.'

In my anſwer to the polite and learned letter of this celebrated philoſopher, I had the honour to inform him, that I had ſeveral times ſeen the phenomenon of two animalcula united, which was expreſsly mentioned in my Journals,—and had even ſketched the figure; but, to confeſs the truth, although it did appear that this union might be a real copulation, I could not reſolve to advance it in my Diſſertation from the fear of being deceived. Animalcula are a part of the creation as yet little known to philoſophers; and it is eaſy to be miſtaken by applying our ideas of large animals to them. Thus was I induced to leave the matter in obſcurity; and only deſirous that ſome more fortunate or more acute obſerver would promote the intereſting ſubject, on which my poor abilities had thrown any additional illuſtration. Happily my wiſhes were not vain. My obſervations came into the hands of M. De Sauſſure, who, among other phenomena, having fallen on this ſuppoſed copulation, made it the ſubject of long, nice, and minute diſcuſſion; and

and he at laſt diſcovered, that it was not the copulation of two animals, but one animal which multiplied by a diviſion into two parts. His obſervation was communicated to M. Bonnet, who tranſmitted it to me.

My Solitude, 27 *January* 1770.

'I have hazarded ſome conjectures on infuſion 'animalcula, and their mode of multiplication, 'in the firſt volume of my *Conſiderations ſur les* '*Corps Organisès*, chapter 3.; and it is there 'ſaid, "Let us prefer conjectures founded on ob-"ſervation or experiment.—Let us compare theſe "animalcula to polypi, and other inſects, multi-"plying by ſections.—Let us ſuppoſe they pro-"pagate by natural diviſion, ſimilar or analogous "to the cluſter polypus; or, by breaking or ſe-"parating with extreme facility, like the freſh "water anguillae, ſpoken of in my *Traité* "*d'Inſectologie*, Obſervat. 21. part 2. By ſuch "ſuppoſitions, we may explain the chief pheno-"mena preſented by animalcula—that ſingular "diminution of ſize and encreaſe of number."

'I do acknowledge, that I had no great hopes 'that theſe conjectures would one day be veri-'fied, nor was I very ſanguine in their favour. 'Animalcula are ſo minute, that it was not eaſy 'to preſume the myſtery of their reproduction 'would be inveiled. But it is now accompliſhed; 'and we owe it to a naturaliſt, who, although

'experienced

'experienced in the rare and uncommon art of 'interrogating nature, is restrained by modesty 'from making his discoveries known, lest they 'may not be sufficiently comprehended. A work 'containing excellent observations on the *Petals* '*of Flowers*, a subject but little understood, has 'already spread his name among the limited 'number of his equals. It is evident I mean M. 'de Saussure, who, at an age when men only be-'gin to think, already fills one of our philosophi-'cal chairs with credit. His affectionate attach-'ment to me, which is merited but by a recipro-'cal sensation, would not permit him to let me 'remain ignorant of his discoveries concerning the 'mode of animalcula propagating. These are re-'lated at large in the following letter, which well 'deserves the attention of observers (1).

" *Geneva*, 25 *September* 1769.

" You have great reason, Sir, to suppose, that " animalcula of infusions may multiply by con-" tinual division and subdivision like polypi. You " state it only as a suspicion; however, my ob-" servations, on many species of these singular ani-" mals, convince me that we may regard it as a " fact. Animalcula of a roundish form, without " beak

(1) This and M. de Saussure's letter are inserted in the republication of La Palingenesie.

"beak or hook, divide tranverſely in two. A "contraction appears at the middle, which gra-"dually increaſes until the two parts are attach-"ed only by a thread : the animal, or rather two "animals, make violent efforts to complete the "ſeparation ; and, after it is effected, ſeem ſtun-"ned for ſome ſeconds. They afterwards begin "to traverſe the liquid as the entire animal did.

"You will eaſily conceive, that, in the firſt "movements of their new life, they are ſmaller "than the animal they compoſed, each, in reality, "is only one half, but it ſoon acquires the ſize "of the whole, and, in its turn, divides into "animalcula that rapidly become equal to it.

"Mr Needham has done me the honour to "commend this obſervation in his notes on the "tranſlation of Signor Spallanzani's excellent "performance ; and employs it in ſupport of his "theory, which is, that the ſmalleſt animalcula of "infuſions, thoſe that ſeem points before the moſt "powerful microſcopes, are produced by continu-"al diviſions and ſubdiviſions of the large ſpecies. "Undoubtedly, in four years which have elapſed "ſince I communicated this obſervation, he muſt "have forgot that I conſtantly obſerved the "parts of the divided animalcula in a ſhort time "became as large as the whole to which they "had belonged. Therefore, in their propaga-"tion, we find the ſame conſtancy and unifor-"mity that is ſeen in the reſt of nature. Per-

"haps

" haps I did not inſiſt with Mr Needham on this " peculiarity; perhaps I did not inform him " that, to remove every poſſibility of doubt, I " put a ſingle animalcule in a drop of water; " that it divided in two before my eyes; that " next morning theſe two had become five, and, " during the day, ſixty; and on the third day " were ſo numerous, that it was impoſſible to " count them; and all, except thoſe juſt produc- " ed, of equal ſize to the animalcule from which " they came.

" When, for the firſt time, you ſee the animal " dividing, you will think it the copulation " of two. I was completely deceived; and " thought, like Micromegas, that I had caught " nature in the fact: nor was I undeceived " until one had, in the ſpace of twenty mi- " nutes, ſucceſſively paſſed through all the de- " grees from the moſt imperceptible contraction " to full ſeparation (1).

" What is moſt remarkable in the inſtinct of " theſe animals is, when they obſerve or diſcover " two

(1) Muller ingenuouſly acknowledges, that the firſt animalcula he had ſeen united ſeemed to him in copulation: and this ſeductive phenomenon may impoſe on any one; whence Father Beccaria's innocent miſtake was not ſurpriſing: and there is no doubt that he would have diſcovered the truth, if his reſearches on electricity, which do him ſo much honour, had allowed him time to proſecute the obſervation.

" two on the point of ſeparation, and exerting " themſelves to attain it, they precipitate them- " ſelves between them as if to aſſiſt in break- " ing the connecting ligament. Nor can we " view this act as only fortuitous, ſince they are " generally careful to avoid one another, and " never ſtrike together whatever the rapidity of " their courſe may be.

" Another ſpecies, found in hempſeed infu- " ſions, with a beak or hook before, alſo multi- " ply by diviſion, but in a more ſingular man- " ner. When going to divide, the animalcule " ſeeks a convenient place at the bottom of the " infuſion, commonly that ſemi-tranſparent kind " of mucilage which forms in hemp infuſions. " After ſearching and examining various places, " it at laſt fixes on one. The body, which is " naturally long, contracts, the curved beak is " retracted or concealed, and the animal aſ- " ſumes a ſpherical form: it next inſenſibly be- " gins to revolve on itſelf, ſo that the centre of " motion is fixed, and the ſphere never changes " its place. The motion is performed with the " moſt perfect regularity, but the direction of " rotation is conſtantly changing, ſo that the ro- " tation may be firſt from right to left, then " from before, and next from left to right. And " all theſe changes are imperceptibly performed, " without the animalcule or rotatory machine

" changing

" changing its place. At length the motion ac-
" celerates; and at the point where the ſphere
" ſeemed motionleſs, two croſs diviſions begin to
" be viſible, exactly like the huſk of a cheſnut
" ready to burſt. In a little longer, the animal
" appears agitated, and making great exertions,
" and at laſt divides into four, the ſame as the
" producing animalcule, but ſmaller. Theſe
" grow larger, and each divides into four, which,
" in their turn, increaſe. I could ſee no end to
" the ſubdiviſions; the young always became
" equal to their parents, if we may uſe the word
" parent in this ſingular mode of generation."

M. Bonnet adds the following words: 'In
' the laſt ſpecies of animalcula is another analogy
' with cluſter polypi evident: We know that
' theſe create a little vortex in the water, which
' precipitates the food towards their mouth. Our
' animalcula perform a ſimilar operation, and
' ſurely with the ſame intent.'

In this new courſe of experiments, I have had the advantage of examining M. de Sauſſure's diſcovery, to verify and extend it, and find, that beſides the ſpecies he obſerved, there are many others which propagate by a natural diviſion, but often in the moſt ſingular and unaccountable manner. We may begin with the ſimpleſt; and firſt with the tranſverſe diviſion, being that mentioned by the Geneveſe profeſſor. It ſucceeds

ceeds both in ſpherical and elliptical animalcula, and in ſome other ſpecies without either beak or hook. For the purpoſe of correct obſervation, I iſolate an animalcule in a watch-glaſs. If the weather is warm, traces of contraction are viſible about the middle of the two ſides; it inſenſibly advances; and the animalcule ſomewhat reſembles a blown bladder tied tight acroſs. It ſtill ſwims about, darting its head among the particles of matter, if any are in the glaſs. The contraction continues increaſing; and the animalcule is at laſt changed into two ſpherules touching in one point, Plate 1. Fig. 1, A B C. Theſe connected ſpheres continue moving as the entire animal did, but they often ſtop. The poſterior ſpherule ſeems to be carried on by the weight of the anterior, and appears to have no ſpontaneous motion of its own, but what is neceſſary for ſeparation from its companion. This at length is done, and of one animalcula, two are formed. At firſt, they are apparently unable to move; however, each ſoon reſumes the velocity of the original whole. The ſpherules in time acquire the ſize of the entire animal.

Though all the ſpecies dividing tranſverſely ſeparate into two equal halves, theſe parts are not uniformly ſpherical, but more or leſs elliptic when very near diviſion: Nor are the new animalcula always torpid and inactive, for they often

ten retain the former velocity of the compoſing whole. It particularly merits obſervation, that the ſize of the two animals, while actually dividing, is ſo much augmented, that each is almoſt equal to the original, of which I have had undoubted evidence, by comparing each portion with one entire of the ſame ſize and ſpecies as that divided. The animalcula from theſe diviſions being alſo iſolated, ſimilar young are continually produced by diviſions and ſubdiviſions.

Among thoſe dividing tranſverſely are ſome generations, (like the elliptic kind, pointed before, ſometimes originating in wheat infuſions, and rather large) whoſe anterior part is provided with ſhort fibrilli in conſtant motion. The vortex aſcribed by M. De Sauſſure to the ſecond ſpecies of his animalcula, is certainly produced by this motion (1); but acute viſion and a powerful magnifier are neceſſary to diſcover the vortex and fibrilli (2). Neither the vibration nor vortex

(1) Muller diſputes the correctneſs of this obſervation. Vortex, quem animalculum, cujus meminit, claris. Sauſſure ex infuſione cannabis, partitioni intentum ciet, non, uti autumat illuſtris Spallanzani, vibratione pilorum, cum iis careat *Kolpodæque* generis ſit, ſed totius corporis agitationi debetur, *Animal. Infus.* p. 246.—T.

(2) There are few microſcopic objects ſo difficult to diſcern accurately as the vibrating fibrilli of ſome animalcula. Their extreme minuteneſs, their continual motion and

vortex is interrupted, during the divifion of the animalcule; and both continue when it is finifhed. After feparation, the pofterior part acquires the filaments; and, in a fhort time, alfo produces a vortex.

I have counted fourteen fpecies of animalcula multiplying in this manner: only two merit defcription. We fee a kind of circular animalcula, above the middle fize, in infufions of bearded wheat. From the circumference of the body arife a circle of minute protracted points fimilar to very flender cones, and in the quickeft motion. This animalcule and its points are mentioned in my Differtation; but for want of the neceffary obfervations, I was uncertain of what ufe they might be (1). Now, I do not think myfelf deceived in fuppofing that they ferve for fwimming, as the fins and limbs of fo many other aquatic animals. This is deduced from two reafons; firft, becaufe the points are at reft while the animal is tranquil, from their motion when it moves, and the accelerated vibration when at its greateft velocity. Secondly, if, by any accident, the number of points is diminifhed, the ani-

and removal from the focus of the microfcope, render it a nice and delicate matter to bring them diftinctly into view.—T.

(1) Capitol. 2.

mal no longer moves, or does ſo very ſlowly. Propagation is operated by a tranſverſe diviſion in two. The ſeparation is ſlow, and attended with one ſingularity, that, before being fully accompliſhed, each portion is as large as the whole, and, in the place of diviſion, has acquired points ſimilar to thoſe of the old animal, but ſhorter.

The other ſpecies, which muſt not be overlooked, is found in an infuſion of marſh-lentil, and is ſometimes ſo large as to be viſible without the microſcope. By filling a thin ſided chryſtal tube, and placing it in the ſun, the animalcula are ſo conſpicuous to the obſerver's eye, that the ſucceſſive diviſions may be eaſily ſeen. Other elliptic animalcula are obſerved with the contraction ſcarcely begun, ſome with it far advanced, and others with it almoſt completed. The multiplication is ſo abundant, that a ſingle animalcule, at certain times at leaſt, will, in a few days, people a whole infuſion.

I have ſtill to ſpeak of longitudinal diviſion, for that is alſo a way in which animalcula propagate. Thoſe with the filament, already mentioned, divide longitudinally; but the eaſier to ununderſtand how it is effected, we muſt firſt deſcribe the animalcule. On preſenting a drop of infuſion to the microſcope, animalcula are ſeen among the vegetable fragments, ſome attached to particles of matter, and others wandering free-

ly

ly about in the drop. The filament proceeds from the poſterior part of the animal; and although its natural poſition is in a right line, it often contracts ſuddenly into a ſpiral whoſe volutes approach ſo near as to touch: in a moment they recede; the ſpiral unfolds, and reſumes the ſtraight line. While unfolding, or already ſtretched, if a gentle motion is given to the drop, the filament becomes a ſpiral. If its extremity is fixed, any contraction towards the ſpiral forces the animalcule rapidly to the fixed point; when free, it approaches the animalcule. This it frequently does, almoſt periodically. It is of a pearl colour, and of extreme ſlenderneſs, at leaſt compared with the animalcule; the length equals it, and is ſometimes more. The figure reſembles an onion or bulb: to the extremity is attached the filament as the roots originate; thence it was named *the bulb-animalcule* (1). A circular row of filaments proceed from around a hollow. Theſe extremely ſlender fibrilli are in a conſtant vibratory motion, which occaſions a whirlpool in the fluid, abſorbing the ſmalleſt adjacent particles, and ſometimes very minute animalcula. As the bodies gradually approach, the motion becomes more rapid. Attending

 carefully

(1) This is the Vorticella Hians of Muller. *Animalcula Infuſoria*, p. 321.—T.

carefully to the fibrilli, it is not difficult to account for the operation. After the largeſt ſubſtances enter the hole or hollow of the animalcule, they are rejected, but the ſmalleſt remain; and there is every reaſon to believe they penetrate the body by ſome inviſible channel. The intent of the operation is in all likelihood for the animals nutriment and preſervation; the vibrating fibrilli cauſe a vortex; the vortex draws in the floating particles; and the animalcule ſelects either the moſt delicate or what ſuits it beſt for food.

Beſides the periodical motions we have aſcribed to the filaments, there are others peculiar to the animalcule. Whenever the filament contracts, the animalcule alſo contracts, ſuddenly concealing the hole and fibres within its body, and aſſumes the figure of a ſpherule, D. pl. 1. fig. 2: In a few ſeconds, the filament is extended, and the animalcule becomes like a pear, E; then its ordinary ſhape outlined F, and finiſhed G. The fibrilli and hole re-appear; the vortices recommence when their motion begins, for there is an entire ceſſation while the animalcule remains contracted within itſelf.

I firſt ſaw theſe animalcula dividing in two in an infuſion of white kidney beans boiled two hours. The anterior part of one ſeemed languid, which induced me to ſuſpéct it was going to divide. Two miſhapen animalcula, attached

by

by many points, appeared: each had its appropriate fibrilli and consequent vortex. Besides the usual contraction and extension, the animalcula were full of agitations and contorsions, and always separating more from each other, changing their mutual position, until the two holes and vortices became diametrically opposite.—The separation advanced; and in half an hour from the first observation, they were connected only by a point. The filament, which during the whole time of dividing periodically contracted and extended, was no longer common to both animalcula, but belonged to one whose only motion was vibrating the fibrilli, retracting them, and extending itself. On the contrary, the other animalcule was occupied with bending into different forms, in contorsions, and revolving on its own axis. At last it separated from its companion, began to swim in the liquid, and very soon left the field of the microscope.

This observation was a rule for experiment on many more of the same species; and I uniformly obtained the same results by isolating them in watch-glasses. A little cleft was seen at the anterior part of the animalcule, dividing the hole asunder. The cleft encreased; the vortex became double; and each portion acquired the rude figure of an animalcule. The separation advanced; the shape grew more perfect; and division being almoſt completed, they were tranſmuted into two entire and well formed animalcula. One remained attached to the filament, and, in a ſhort time, became as large as the whole, and, by new diviſions, gave birth to new animals. The other had no filament; it rapidly traverſed the fluid, contracted, extended, and an appendage ſoon budded from the poſterior part, which was the rudiments of the filament. With this the animalcule fixed itſelf to ſome ſurrounding ſubſtances: the filament lengthened, and the animalcule began to divide again. In fig. 3. pl. 1. are the various degrees of diviſion.

Theſe animalcula ſometimes periſh when iſolated in diſtilled water; and the like may be ſaid of all that divide: however they often divide and ſubdivide, ſtill their glaſſes are never populous; but the numbers encreaſe exceedingly, if portions of vegetable matter are mixed with the diſtilled water. Privation of food in the one caſe, and abundance in the other, is undoubtedly the cauſe of this difference.

Bulb animalcula not only inhabit boiled but alſo unboiled infuſions of kidney beans, and many other legumes, as lentils, beans, and peaſe. Nothing more is required for the convenient obſervation of their propagation than to macerate a few particles of ſeeds. In two or three days, if the experiment is made in ſummer, ſome animalcula

cula are ſeen attached by the filament to minute fragments in the infuſion; and they will divide before the obſerver. The number fixed by the filaments is proportioned to the number of diviſions about to take place.

The ſame legumes produce another ſpecies of animalcula, alſo multiplying by longitudinal diviſion, and preſenting phenomena ſimilar except in two facts: *firſt*, The fibrilli are not in the cavity, but on its lips; *ſecondly*, The figure reſembles a monopetalous flower. The body divides exactly in two.

There is alſo a ſpecies conſiderably larger, which propagates by a little fragment detaching itſelf obliquely from the body. This animalcule is ſometimes found in an infuſion of beets. It is ſpherical, and has a filament which is not endowed with the ſingular motions of the other two ſpecies, nor is the body ſubject to thoſe mutations of figure. The multiplication begins with a ſmall portion inſenſibly detaching itſelf from the body, near to the origin of the filament, and it is in continual motion, Plate 1. fig. 4. H. When ſeparated, it ſwims actively through the fluid; and although ſmaller than one-twelfth of the whole, it becomes equal to it in leſs than a day. Then it begins to propagate in the ſame manner.

As I have repeatedly ſpoke of iſolating animalcula, or the method uſed of confining one in a watch-glaſs, on purpoſe to obſerve the ſucceſſive degrees of diviſion, the reader will naturally be earneſt to learn the mode employed, much more ſo if he is accuſtomed to ſuch matters, and knows the extreme difficulty of obtaining one alone in a drop of infuſion, however ſmall. Sauſſure himſelf ſtates it as exceedingly difficult, and that by dint of patience he ſucceeded in confining one in a drop of water; and I muſt confeſs it was a great labour before I fell on a ready method. A drop of infuſion is conveyed into a watch-glaſs with the point of a pen; it is of no conſequence although abounding with animalcula: a drop of water is put two or three lines from the firſt, and they are made to communicate by a little channel formed by drawing out the circumference of the drops. The animalcula are not ſlow in traverſing the channel, and arrive one after another in the drop of water. Obſerving this paſſage with a magnifier, whenever I ſee an animalcule enter the water, I cut off the communication with a hair pencil: thus impriſoning a ſingle animalcule. If more than one are to be confined, it is eaſy to allow any number to enter the drop. The infuſion being then taken away, only one remains in the watch-glaſs, or more if I chooſe it.

I ſhall here ſpeak of an objection ſtarted by Mr Ellis, leſs becauſe it merits refutation than becauſe it ſhould not be entirely overlooked. His opinion is, that the diviſion of animalcula is not a natural mode of propagation, but the effect of accident, and that it is occaſioned by accidental ſhocks, from ſtriking againſt each other. This opinion he deduces from two reaſons; firſt, from the proportion of animalcula dividing to thoſe that do not, which is ſcarcely as one to fifty; and, ſecondly, from obſerving young in the body of adults, and within the young ſome ſtill younger (1).

It was unfortunate that M. de Sauſſure's diſcovery had not been publiſhed when this learned naturaliſt compoſed his Memoir. Had he ſeen his obſervations, and what has ſince been obſerved by me, it is no arrogance to affirm, that he would have deeply penetrated into experimental reſearch on infuſion animalcula. He would alſo have perceived that the ſhocks and ſtriking together are perfectly imaginary. In the end of my Diſſertation, their anxiety to avoid one another, and different obſtacles, are mentioned in expreſs terms. The like is remarked by two excellent naturaliſts, Father Beccaria and M. de Sauſſure; and, in my new enquiries, I have had opportunities of ſeeing the fact confirmed a thouſand times. Therefore, it

(1) Philoſophical Tranſactions.

it is falſe that the diviſion of animalcula is the effect of mutual ſhocks: and if that ſpecies, which the Geneveſe naturaliſt mentions, ſeems to prove the reality of the ſhocks, it is not at the beginning, or when farther advanced, but when the diviſion is almoſt at an end, and then only, that any ſhocks are given by the companion animalcula, and when the dividing ones are exerting themſelves to ſeparate. Beſides, their inſtinct to aſſiſt in ſeparating appears peculiar to this ſpecies. I have never witneſſed any thing ſimilar in the numerous kinds examined. But the *experimentum crucis* againſt Ellis's objection is, that animalcula, iſolated in glaſſes, multiply by diviſion as the reſt, though they can experience no ſhocks from others.

Had my reſpectable colleague continued to ſtudy animalcula, he would have perceived the inſufficiency of the proportions he has given from the immenſe numbers in actual diviſion. Frequently among innumerable multitudes traverſing an infuſion, there has hardly been one that did not exhibit ſigns of diviſion. But I can comprehend what has miſled Ellis. By conſtant obſervation, I find that this mode of multiplication has determinate periods; at one time, it cannot be more general; at another, it is rarer; and now it is not to be ſeen at all. Apparently, his obſervations were made when the

propa-

propagation was about to end; thence was a proportion aſſumed which he ſuppoſes general. I cannot think myſelf deceived in conceiving this has miſled the author, as he mentions having diſcovered the young and their deſcendants in the bodies of the old animalcula. Many of theſe animals appear to be tranſparent folliculi, with veſicles or grains internally ſtrewed here and there, very often including others ſmaller. The firſt time of obſerving animalcula, we are eaſily perſuaded that theſe veſicles or granuli are young. Many perſons preſent at my obſervations were of this opinion; and I cannot deny that I was of the number. But in truth they are not animals, which I can affirm from the moſt certain and indubitable proofs. Several animalcula were iſolated in a watch-glaſs; and that they might be all in the field of the microſcope after the number had increaſed, a little water was always left in the glaſs. Thus ſome individuals might be ſelected and recogniſed. The granuli themſelves aided me to this, becauſe they are ſeldom or never in the ſame poſition in one animalcula as in another: ſo it was eaſy to obſerve whether they actually underwent any change. But the number never diminiſhed; they remained invariably the ſame during the whole period of my examination; and at laſt had increaſed amazingly. Thus the granuli have no part in the

the propagation; and we muſt admit that they are intended for ſome other uſe, though to us unknown. The whole ſubſtance of the arm polypi, which multiply by diviſion, is alſo granulated; and M. Trembley has demonſtrated, that theſe grains have no ſhare in the multiplication.

In the extenſive empire of animalcula, naturaliſts are acquainted with one ſpecies only, as far as I know, which multiplies in the manner Ellis deſcribes. This is the celebrated *volvox*, apparently ſo named from revolving on itſelf in its progreſſive motion, firſt diſcovered by Leeuwenhoeck, and then found by other naturaliſts. From the great tranſparency, like moſt animalcula, the internal ſtructure is clearly ſeen; and ſome obſervers have already diſcovered the young within even to the fifth generation. In my long obſervations on infuſions, I have found two particularly abounding with the volvox, hempſeed and tremella. There are often many in the putrid water of dunghills. Theſe animals are originally very ſmall, but grow ſo large as to be perceptible by the naked eye. They are of a greeniſh yellow colour, a globular ſhape, of a very tranſparent membranaceous ſubſtance, and ſtrewed with the moſt minute globules within. Three volvoxes of different ſizes are repreſented, Plate 1. fig. 5. Examined with a very powerful magnifier, theſe globules are diſcovered to be ſo

many

many volvoxes infinitely more minute; and each provided with its diaphanous membrane. I have been able to difcern the third generation, but never the other two, though I had recourfe to magnifiers of the higheft power. Perhaps it was not my fortune to recognize them, or they were not vifible in the volvoxes examined, from a difference in the fpecies and fize between them and thofe obferved by other naturalifts (1). It is undoubted that globules within globules are fo many generations included within each other; for when my volvoxes had attained a certain maturity, the fmaller globuli began to move within the membrane, detached themfelves from it, left the mother, and fwam in the infufion, revolving on their axis, and in this manner paffing along according to the mode peculiar to thefe animals. When all had come forth, the common membrane, or mother, burft, and diffolved; and having loft all motion, I loft fight of it alfo. In the meantime, the new volvoxes increafed, as the included globules likewife did; thefe began to move, the common membrane burft, and they fwam about in the infufion like the former. By ifolation

(1) Muller, who defcribes feveral fpecies, has feen only the young and its offspring in the body of the mother, and that but in a fingle fpecies which he calls *volvox globator*.

isolation in glasses, I came to have the thirteenth generation in succession.

Here I must be allowed to make a digression. One of the strongest objections to the theory of germs, is the great difficulty of conceiving the successive envelopement of animals in animals, and plants in plants. It has been attempted to obviate this objection, by stating, that it is more adapted to startle the imagination than to confound reason, which admits of the infinite divisibility of matter, and examples favourable to envelopement have been adduced to weaken it. One egg has oftener than once been found within another; and some osseous parts of one fœtus included in another fœtus (1). The butterfly is first included in the shell of the chrysalis, and the chrysalis in the skin of the caterpillar. In vegetable seeds are found the rudiments of the future plant; and the fourth generation has been seen in a hyacinth root (2). The volvox affords a new and beautiful instance of envelopement; the eye has been able to see the thirteenth generation: probably that is not the last. I cannot speak otherwise, since nothing but time was wanting to investigate whether further developement would appear. But the naturalist is invited to extend this most important observation.

Baker,

(1) Histoire de L'Acad. Roy. 1742, 1746.

(2) Bonnet, Corps Organises, tom. 1.

Baker, in his Treatife *The Microfcope made Eafy*, fpeaking of the minute and innumerable creatures inhabiting waters, mentions a race of animalcula difcovered by Leeuwenhoeck in the marfh lentil, remarkable for a long tail, with which it attaches itfelf to the roots of the plant, and a hollow like a bell in the anterior part of the body; it is alfo characterized by a fpontaneous motion, contracting and extending the body and tail at pleafure.

Thefe fingularities, fo analogous to thofe of my bulb animalcula, excited the defire of feeking for what was to me a new kind of animal, to learn whether its multiplication was by natural divifion. But as it often happens, the more one feeks a thing the lefs does he find it; and when leaft thinking of it he difcovers it, or rather it feems to find him; fo was it with Leeuwenhoeck's animalcula. When I gave myfelf much trouble and folicitude I never was able to difcover them; and they at length appeared when I was occupied in matters entirely different. Intently confidering fome tadpoles about the roots of marfh lentils, which had been put into a vafe of water to feed them, and the direct rays of the fun falling on the water, I faw the roots very diftinctly, and diftinguifhed one from the reft by a light fpot of fhining white, furrounding it about the middle of the length. This peculiarity did not make the fmalleft impreffion

preffion on my mind at firft, but it was foon evident that it difappeared in a few feconds, and in a little appeared again, which feemed to be periodical. While the tint was vifible, I gently took away the radicle; it fuddenly vanifhed; but, the fhock having abated, unexpectedly re-appeared. The fingularity of the phenomenon recalled to my memory the animalcula of the marfh lentil. Examining the fpot more narrowly, I faw, with extreme pleafure, it was a group of more than fifty tails of the animalcula, the extremity of which was fixed to a lentil root. Thefe animalcula refembled the bulb fpecies, not only in extenfion and contraction of the body and tail, but in generating a vortex and directing the floating particles into the hollow or bell, by means of a circle of filaments or points, proceeding from the edge of the bell, Pl. 1. fig. 6. As this fpecies is much larger than the bulb animalcula, fo are the points and vortex proportionally larger. If the bell was wide open, which happened when the animal was extended, it feemed to terminate in the body by a little central hole, I. I tranfported this family of animalcula with the lentil root into a watch-glafs, for more convenient examination. They remained feveral days without appearing to multiply. At length all perifhed; the animalcula were untwined; motion ceafing,

ing, the filaments ceaſed to move, and then the tails (1).

It may well be ſuppoſed, that ſimilar groupes were eagerly ſought on the ſame plants; but in vain. However in ſix days, I had the ſatisfaction to ſee a new ſpot formed on one of the roots, I ſay formed, for it certainly was not there before; as it was much larger, the animalcula were more numerous. They and their tails performed the wonted reciprocal extenſions and contractions even when untouched, and the water at perfect reſt; theſe motions diminiſhed or augmented the ſpot. The whole could not be brought into the field of the microſcope from their prodigious number, I therefore took away a conſiderable part, and, excluding a large half, reſerved a portion for examination, adapted to the capacity of the inſtrument. New ſingularities occurred. The portion repreſented a tree in miniature; numberleſs branches, dividing into ſmaller ones, proceeded from the trunk; theſe into others ſucceſſively leſs; and each of the ſmalleſt bore a bell animalcule at the extremity. No ſcene could be more uncommon or more agreeable. Every three or four ſeconds the trunk unexpectedly contracted towards the lentil root, and inſtantaneouſly

(1) Muller calls this the *Vorticella Convallaria*. Animal. Infuſ. p. 315.—T.

drew in all the branches, twigs, and animalcula, but in a moment reſtored the tree complete, with all its animals, to the original ſtate. The reader will eaſily comprehend, that a vegetable is not meant under the appellation *tree*, becauſe it is evident from itſelf, that it is an *entire animal*, which cannot be better figured than by the representation of a tree. As each animalcule formed its own vortex, and there being above an hundred, the appearance of ſo many whirlpools at once preſented a moſt ſingular and intereſting ſpectacle, eſpecially when highly magnified by the ſolar microſcope.

I detached the ſhrub from the lentil root, by cutting through the trunk. The ſcene changed, but to one equally pleaſing. The animals, branches, and twigs, no longer approached the ſtem, but the ſtem, twigs, and branches were ſuddenly carried away by the animals; and at this inſtant all the whirlpools diſappeared. Amidſt theſe alternatives, the animalcula, no longer fixed to the root by their trunk, ſwam ſlowly through the fluid, drawing along the plant and its branches; and, while this common motion continued, the various parts of the plant alternately approached and receded from the animalcula (1).

Having

(1) I cannot affirm that I perfectly comprehend the Author's deſcription.—T.

Having left the plant thus in the glaſs, I examined it next day; all was in the ſame ſtate, except that inſtead of one animalcule as before, proceeding from the extremity of each branch, there were two, Plate 1. fig. 7. K. And thoſe yet ſingle were marked with a very fine furrow, L. The novelty attracted my attention; and it was ſoon perceptible that the furrow indicated an incipient diviſion; each in a ſhort time became double. Then I began to underſtand how ſo many animalcula appeared double on one pedicle; it was a propagation from diviſion. I cannot ſay whether the origin of the branches, to which they are attached, ſeparate in the ſame manner; my obſervations on that ſubject have not been ſufficient; but the animalcula were in pairs, and thoſe at firſt almoſt in contact, in half a day, were far aſunder, and had attained their complete ſize, K. L. fig. 7. plate 1. Further, I can affirm, that from each old branch two new ones budded; and the reproduced animals were implanted on their ſummit, K. Theſe attained the neceſſary ſize, divided as the parent, and remained to terminate new branches or twigs; whence the multiplication of branches was in proportion to that of animalcula, and both continued multiplying many days (1).

(1) Muller ſeems to conſider this a diſtinct ſpecies from the former, and names it *Vorticella Pyraria*. The definition

During this the branches of the ſhrub had ſo much extended, and become enlarged, that the circumference was triple. But the ſupervening death of the animalcula occaſioned that of the plant: They began to fall from the branches as fruit falls from the tree; and as they gradually ſeparated, the motive faculty was deſtroyed. Spontaneous extenſion and contraction were no longer ſeen, the vibration of the fibrilli at the mouth of the bell, nor the conſequent vortex. Every ſign of animation was gone; each animalcule became miſhapen, and was deſtroyed. The tree lived while it had animalcula; after that it neither lived nor vegetated; there was not the leaſt indication of ſpontaneous motion. Such was the fate of half the ſpot which I had taken from the marſh lentil root and put in a watch-glaſs.

I could now ſee the generation of theſe animal ſhrubs. Although the animalcula often died where they were produced and vegetated, that is, at

nition is, Vorticella compoſita inverſe conica pedunculo ramoſo. The other is defined; Vorticella ſimplex campanulata pedunculo retortili. It is not in my power to decide whether they are really different or not. The deſcription of all theſe complicated animals will be much better underſtood by conſulting the figures in Muller's work, Plate 44. 45 46.—T.

at the extremities of the branches, it was not uncommon to obſerve ſome ſwimming in the water, but always adhering to the limb or branch, ſince we thus term it. If the branch accidentally touch a lentil root, it immediately faſtens, and gives exiſtence to a tree bearing as many bell animalcula as there are branches to ſupport them. The animal attached to the root ſoon divides in two, then into four, eight, ſixteen, thirty-two: While theſe diviſions or propagations are going on, the origin and multiplication of branches and twigs, bearing animalcula at the extremities, alſo advance; and all the branches and twigs are immediately or mediately connected with the ſtem fixed to the lentil root, already much thicker and longer; which ſtem, properly ſpeaking, is preciſely the trunk of the microſcopic tree. Here I ſhall remark, in paſſing, that the animals, beſides fixing and propagating on the marſh lentil, alſo breed on other ſubſtances, as fragments of wood, ſtraws, leaves of graſs, and even on the ſides of the veſſels, provided they always remain in water.

This ſpecies, whoſe mode of reproduction Leeuwenhoeck could never divine, and was unknown to Baker, is a polypus much analogous to M. Trembley's *polype a maſſe*. The reſemblance is evident from that eminent naturaliſt's deſcription of the ſpecies which M. Bonnet calls *polypes a*

 pennaches,

pennaches, (polipi a fiocco). Beſides, being cluſtered together like the fungi of rivulets, bellſhaped, and producing a vortex, which draws particles to the mouth of the animalcule for food, and multiplying by a longitudinal diviſion, they are alſo attached to little twigs; theſe to larger, and the larger to the common ſtem; and all the various branches, as well as the ſtem, animated by a moſt remarkable motion of contraction and extenſion. Still they are different from M. Trembley's, for his produce a vortex, not by points, of which they are deſtitute, but by moving the lips of the bell; and, before diviſion, by loſing the bell ſhape and aſſuming that of a roundiſh corpuſculum: nor are they endowed with that contraction and conſequent elongation: they divide into unequal parts, and the vortex ceaſes during diviſion: finally, the contraction and extenſion of the branches is not natural and periodical, as in our animalcula, but the effect of conſtraint or accident, when the water is moved.

All the longitudinal diviſions yet ſpoken of have commenced at the anterior part of the animalcule, that is the part before when it advances, and where the opening of a mouth may in many be perceived. But the diviſion of other animalcula begins at the part exactly oppoſite, or behind. My obſervations here were too late, and

when

when I had no draughtſman to deſign them, therefore I muſt content myſelf with a ſimple deſcription. One ſpecies repreſents an infinitely minute hedgehog, or rather ſea hedgehog, being of a ſpherical figure, and the whole ſurface covered with long pointed prickles. The anterior part is diſtinguiſhed by advancing firſt, and producing the uſual vortex, by vibrating the ſpines: the reſt are in conſtant agitation when the animal advances. Another ſpecies reſembles the ſegment of a ſphere or a hemiſphere, and is entirely covered with ſpines: thoſe on the convex part ſerve for fins: others, appropriated to form the vortex, are ſituated on the ſection or plane of the hemiſphere, which is always the anterior part of the animal. All are diſunited; and their ſeparation ſeemed to ſmooth the animal's body, which can move any number of ſpines at a time. According to the number in motion, its activity, ſlowneſs, and even the vortex is greater. Theſe two ſpecies which commonly inhabit the tremella, and are of a coloſſal ſize compared with many other infuſion animalcula, divide longitudinally, but the diviſion begins at the poſterior part. A very faint cleft was ſeen there as uſual, which extended more on the animal's body, and at length divided it into two portions exactly equal. They were not, as eaſily might be imagined, two halves only, but, before the diviſion finiſhed, two complete animalcula equalling the ſize of the whole. The vortex never diſcontinues during diviſion: minute ſpines proceed from the cleft as it gradually advances, which, encreaſing in length and thickneſs, in ſome time are as large as the old. Two well formed hedgehogs are produced by diviſion of the firſt ſpecies; and two hemiſpheres armed with ſpines by that of the ſecond. A conſiderable period is required for completing the ſeparation.

Theſe being the moſt ſingular modes of longitudinal diviſion, I have thought it of conſequence to enter on ſome detail. Many ſimilar propagations leſs worthy of recital are omitted; and I prepare to relate new methods of multiplication by diviſion of the body into parts. A ſingularity is very frequently to be ſeen in an infuſion of tremella. Two minute pellets, attached together by many continued points, traverſe the fluid in an irregular courſe, Pl. 1. fig. 8. M. We cannot be miſtaken if we ſuppoſe this an animal preparing to divide: in fact it is ſo; but one would be egregiouſly deceived if he formed an opinion of what was to happen here. Judging by other animalcula, we ſhould imagine it had hardly begun, and that the cleft would encreaſe till the animalcula remained attached only by a point. It is otherwiſe; for in the twinkling of an eye, one pellet ſeparates from the other in ſpite of the

apparent

apparent ſtrong adheſion. Each having attained the full ſize, a faint contraction appears, which is the origin of two pellets ſimilar to the firſt, that in their turn ſeparate. Thus do the animals propagate (1).

Groupes of different round corpuſcula are often ſeen in infuſions of vegetable ſubſtances. Sometimes the group conſiſts of four diſtinct corpuſcles; ſometimes of five or more: and the corpuſcles are commonly different, according to the difference of the groupes, Fig. 8. N. pl. 1. It cannot be denied theſe groupes are real infuſion animalcula: they poſſeſs every characteriſtic; but how are they reproduced? One corpuſcle is detached after another from the cluſter, which is at laſt divided into as many portions as there were compoſing animalcula; and theſe begin to traverſe the infuſion with much greater velocity than the reſpective groupes to which they belonged. It might be objected, that I am ſtating contradictions, and that the groupes are perhaps the caſual or intentional cluſtering of animalcula, which ſeparate in a given time, and thus occaſion theſe apparent diviſions. I had recourſe to a deciſive experiment, iſolating ſome animalcula in a watch-glaſs the moment they ſeparated from the cluſter. When the ſolitary ones had acquired the ſize of their original groupes, furrows

(1) Monas uva. Muller, An. Inf.—T.

rows were perceptible on various parts of the body, which by little and little was transmuted into a new cluster, perfectly similar to the old. The new group then decomposed into other corpuscles or animalcula, which in size and number were equal to the former. I had the same results from experiments on three different clusters: and we can make no conclusion, but that this is a new mode of a real and actual division.

But the most surprising and singular multiplication is that of certain animated globules, often rolling along like pellets at the bottom of marsh lentil infusions, and visible by the naked eye. They are composed of external tumours, which are so many animalcula, situated above one another, and ready to escape, Pl. 1. fig. 8. N M. Let the reader figure a body almost spherical, formed of concentric strata, each of which is an aggregate of animalcula,—and he will have a sensible idea of these globules. The animalcula composing the exterior, or first stratum, separate from the body, and swim in the infusion: then is the second stratum, composed of similar animalcula, laid open. When all the first have departed, these separate from the body; and the third appears. This also vanishes by the departure of the animalcula that formed it. There are even a fourth and a fifth stratum, and others within to the last in the centre; so that the whole globe,

globe, from the circumference to the centre, is decompofed into a fwarm of animalcula. The compofing globe, I have obferved, has no motion but rolling in the fluid; however, the animalcula, when detached, fwim with the utmoft rapidity. Their exceffive abundance renders it impoffible to number them; but, without exaggeration, each globe confifts of a hundred (1).

It might be fufpected that thefe globules are compofed of many animalcula, at firft feparate, but afterwards collected together. I have had evident proof of the reverfe. While the ftrata decompofed, I feized fome detached animalcula, and immediately ifolated them. Each (which did not equal one hundredth part of the globe in fize) was as large as the whole in a few days. Their motion relaxed in proportion as they grew; fo that when full grown, or complete globes, they had only a rolling progreffion after the manner of thefe animals. The exterior ftratum was originally fmooth: it afterwards became unequal, and covered with tumours. Thefe were as many diftinct animalcula, which, in future, feparated from the globe to traverfe the fluid. The animalcula

(1) Very much analogous to this, and the former, is the Gonium Pectorale. It confifts of fixteen globular animalcula, invefted by a common membrane.—The membrane burfts; and each animalcule becomes the parent of fixteen young.—T.

malcula of the ſecond ſtratum did the ſame, as alſo thoſe of the remaining ſtrata, until the globe was entirely decompoſed. This experiment was made on ſeven animalcula from different ſtrata; and all ſeven afforded me as many globes.

Theſe are the different generations of animalcula propagating by diviſion, in the way hitherto explained; and which are in reality *polypi* that we will name *infuſion*, or, more properly, *microſcopic*, to uſe a general expreſſion, as their kingdom is not bounded by the narrow confines of infuſions. I have, at various times, examined the water of ditches, dunghills, ſtanks, and pools; fountain, ſnow, and rain water; thermal and medicinal water, both of mountains and plains; and I can affirm, that I have found all more or leſs abounding with minute polypi of infinite variety. If the multitude is ſuch, that a drop of water contains hundreds, nay thouſands, as experiment proves, every one may conceive the number inexpreſſibly immenſe, which ſhould be contained in the receſſes of all the waters ſo amply diſtributed over the ſurface of the globe (1).

It

(1) The number will riſe above all belief, if to the polypi of freſh water we add thoſe of the ſea; for, by Muller's obſervations, the ſea abounds in animalcula peculiar to itſelf.—A.

After the publication of theſe Tracts, the author ſeems

to

It muſt be remarked, that there are appointed times for theſe minute animals to originate, and be deſtroyed, as with other creatures that multiply to exceſs. Nature has with the wiſeſt proviſion deſtined, that when one ſpecies begins to be exceſſively numerous, it is reduced, either from the greater part of the individuals periſhing by diſeaſe, or a violent death by the voracity of other animalcula: for it is a perpetual and inviolable law with numberleſs animals, that one lives on another, and mutual deſtruction preſerves each ſpecies. The ſame law is maintained in the originating and deſtruction of our animalcula. An infuſion ſwarming to day will, in a few days,

to have extended his reſearches to marine *Animalcula Infuſoria*. 'The ſalt water, in which vegetable ſubſtances 'macerate and diſſolve, contains numerous microſcopic be'ings. Thoſe places in particular, where it is ſtagnant and 'ſo ſhallow that ſubaquatic plants die and are decompoſed, 'ſwarm with theſe minute animals. The ſame phenomenon 'happens in ſea water kept in veſſels, with vegetable ſub'ſtances diſſolving. But what are the laws of nature regu'lating marine infuſion animalcula? Are they the ſame 'with thoſe to which the animalcula of freſh water are ſub'ject? Some of theſe propagate by the natural diviſion of 'the body; others are viviparous, and ſome are oviparous. '—Marine animalcula propagate exactly in this manner.' *Lettera Relativa a Diverſe Produzione Marine*.—T.

days, have almoſt none; and, although thouſands periſh by a natural death, immenſe numbers are a prey to the larger animalcula. Sig. Abate Corti has before me obſerved ſome kinds carry on the moſt deſtructive war. The ingenious method which a cetaceous fiſh, called by the northern nations the great whale, uſes to take herrings is well known: Having driven ſhoals of them into a bay or ſtrait, a blow is given with its tail, ſo as to occaſion a whirlpool of vaſt extent and great rapidity, which draws in the herrings; the ſea monſter then preſenting its enormous mouth and tremendous jaws, the herrings are precipitated down the throat, and its ſtomach is ſoon filled. The carnivorous infuſion animalcula, of which we treat, alſo create a vortex in the fluid by their vibrating fibrilli; but they are under no neceſſity of confining the animalcula in narrow limits. If abounding in infuſions, they have only to keep their mouths open ready to ingulph them: if rare, they trace them out, and ſwallow them up. So voracious are they as to feed till they appear much larger: then the purſuit is no longer intereſting: the animals become indolent and ſluggiſh. On the contrary, if reduced to abſtinence ſome time in diſtilled water, they are full of ſpirit, and eagerly devour the minute animalcula ſupplied. The tranſparency of their bodies al-

lows

lows us to fee the animalcula, whofe motion continues after being fwallowed (1).

All the divifions may be feen in every feafon, even the coldeft and moft rigorous. Heat as much promotes it as it is retarded by cold: and we may affert, that the time required for divifion is nearly in proportion to the heat of the atmofphere. In the middle of winter, it takes many hours: in fpring and autumn, it is fooner performed: and finifhes very foon in fummer, efpecially if great heats prevail. Sometimes lefs than a quarter of an hour is then fufficient from the beginning to entire completion. This is one chief reafon why fummer infufions are much fooner peopled than winter ones.

Whoever wifhes to employ himfelf with thefe curious obfervations, and the fingular modes of multipli-

(1) It is fingular, that Muller fhould deny that animalcula prey on each other. Some fpecies he fays prefer being among the particles of duft, animal and vegetable fragments, and feem to take pleafure in gnawing them; but he can eafily fuppofe, that water alone may be their only nutriment, as he has feen the life of large animals, fuch as Hydrachnae or Entomoftraca, fupported by water, *Praefat.* p. 12, 13. However, he gives the figure of an animalcule containing one devoured, p. 165.—T.

multiplication by divifion, and is unwilling to fix the eye too long on the microfcope, fhould prefer fummer, if he does not chufe to have recourfe to a ftove, which my experiments prove operates equally well.

CHAP. VIII.

SEVERAL INFUSION ANIMALCULA ARE OVIPAROUS: SOME VIVIPAROUS: AND ALL HERMAPHRODITES IN THE STRICTEST SENSE.

In my microfcopic refearches, I have obferved, that many fpecies of animalcula become extremely numerous in a very fhort time, without evincing any figns of divifion. How, then, do they propagate? Shall we fay it is by inftantaneous divifion, and on that account not eafily perceptible: or that it is effected in any other manner? Experiment, the only method of diffipating doubt, has fhown us that this propagation was not the confequence of divifion, but from eggs, and fometimes minute foetufes; for I have actually found many kinds of animalcula oviparous and fome viviparous. Such an affertion is nothing unlefs fupported by convincing and decifive evidence. The

The reader ought ſo much the more to deſire conviction, as this is poſitively denied by Meſſrs Needham and De Buffon, who exclude univocal generation from infuſions entirely. Thus it becomes neceſſary to deſcend to circumſtantiate details; at the ſame time preſerving due attention to brevity.

One oviparous kind of the largeſt ſize among animalcula is found in rice infuſions. It much reſembles the figure of a kidney bean, except that one extremity is curved into a ſharp beak, Plate 1. fig. 9. O. (1). Having ſeen the wonderful multiplication of this ſpecies, without being able to diſcover whence it aroſe, I thought of recurring to iſolation, which on many occaſions had been ſo uſeful an expedient. One was, therefore, put in the uſual glaſſes with a little water, which, for ſecurity of containing no animalcula, had been a long time boiled. In ſeven hours, the animalcule was not alone, it had a companion. The new gueſt was ſo like the old one, it was impoſſible to diſtinguiſh them. I had no reaſon to ſuppoſe that it came from without, or was produced by the infuſion. When the animalcule was iſolated, equal portions of the ſame boiled infuſions were

(1) The Kolpoda cucullus of Muller: It has from 8 to 24 pellucid globules within, which he thinks are the offspring; in the young animals, none are to be ſeen. *Anim. Infuſ.* 103, 104.—T.

put into ſeven different glaſſes, for the purpoſe of comparing what might happen in glaſſes where there were originally no animalcula, and where there was one. But none either of one ſpecies or another ever appeared in the ſeven glaſſes: and I thought myſelf right in concluding, that the ſecond animalcule derived its origin from the firſt. This might be in various ways; whether the firſt had produced the ſecond alive, or laid an egg from which it came, or by dividing in two. More frequent examination of the glaſſes was required for diſcovering the truth. In half an hour, I found ſomething new: two minute pellets at the bottom of the glaſs, P Q. Plate I. fig. 9. One was oval; it moved from time to time; and in moving changed its place. Alternate motion and reſt continued an hour and a third: then it was more frequent and entirely local, the pellet beginning to ſwim ſlowly through the fluid. After the lapſe of ſome time, its motion was as conſiderable as that of the two animalcula. This, its equality in ſize to them, being pointed at the extremity, and apparently compoſed of the the ſame vaſcular ſubſtance, evinced it to be an animalcule of the ſame nature, expanding by degrees, and now become moſt active. While the elliptical pellet preſented theſe phenomena, the round one O exhibited others. Within were included a leſſer ſphere difficult to be obſerved, and

which

which I perhaps ſhould not have noticed had it not been for a gentle revolving motion upon itſelf, while the including ſphere was tranquil. After various revolutions, the ſhell burſt, and the leſſer ſphere eſcaped: the envelope was reduced to a wrinkled irregular ſubſtance. The ſpherule extended, and grew ſmall at one end to form the curved beak; and it began to ſwim: thus aſſuming all the characteriſtics of an animal which the other had. The origin of theſe animalcula is, therefore, from an egg, repreſented by the ſhell or involucrum.

But the conjecture required further and more deciſive proofs to become an eſtabliſhed fact; and ſuch I ſoon obtained. The glaſs being left in that ſtate on the evening of 15 June, next morning there were more than forty-five animalcula, all exactly reſembling the firſt which was iſolated. At the bottom a number of pellets appeared, part round, part elliptical. With my eye intent on the round, I perceived they did not become elongated, as the pellet mentioned above: one after another burſt, and as many inactive miſhapen animalcula came out, afterwards growing into complete figures full of action. When the ſmaller became larger, they did the ſame. Theſe ſubſtances were undoubtedly eggs; ſtill it was to be elucidated whether they had proceeded from the animalcule, which was more than probable. For abſolute

 conviction,

conviction, it was neceſſary to ſee them proceed from its body, which appeared difficult, not ſo much from the rapidity of its courſe, as becauſe it left the field of the microſcope every moment, from too great abundance of fluid. The fitteſt method was confining ſome in a very ſmall quantity of water, that they might be conſtantly in view. I did ſo; and the ſucceſs was ſooner than I could have expected; ſince, in ſcarcely a quarter of an hour after confinement, one was delivered before me of a round corpuſcle ſimilar to the former: and it having opened, gave birth to one of the uſual animalcula, firſt round, then oval, then diminiſhing into a curved beak, and commencing motion in the glaſs, as had happened to its other companions. More eggs were produced: I counted eleven that had proceeded from the poſterior part of the iſolated animalcule, producing an equal number of young. I might have counted more had not ſuch very minute obſervations exhauſted my patience. From the whole, it is clear that theſe animalcula are oviparous, and their mode of propagation by eggs.

This particular detail will render ſuperfluous what I ſhould have had to ſay on many other ſpecies alſo oviparous. I can only aſſure the reader, that by ſcrupulouſly practiſing ſuch a plan, each ſpecies has laid eggs which produced animalcula ſimilar to the mother. Some of theſe animalcula, round or cylindrical, originate in infusions of radish and camomile seeds, beans, and buck-wheat.

Let us next treat of viviparous animalcula, of which I have found two species, both carnivorous. We distinctly see the animalcula absorbed by a great vortex, passing down the œsophagus into a little bag, and thence into a larger, apparently serving for a stomach. Each animalcule has a long tail forked at the extremity, by which it can attach itself to the adjacent substances. Two oval bodies project from each side of the tail, and above them two smaller, resembling narrow leaves, Pl. i. fig. 10. R. It is easy to suppose these four bodies integral parts of the animalcule; and the two leaves are actually so; but the other two are real animalcula. We not only perceive them move, but, examined by a powerful magnifier, they are evidently two living animals, resembling the large one, to which they are attached, but confined and contracted within themselves. If kept in view, they gradually expand, are emancipated from the mother, and begin to swim. The opacity of this kind prevented me from seeing the fœtus before it issued from the body. After an animalcule has attained maturity, two young ones are seen where the tail originates. I have never discovered more or less than two in all the animalcula I have examined. In other animalcula three are to be ſeen, but them I judged of a different ſpecies, becauſe the leaves were wanting, and the interior ſeemed ſomewhat different, Fig. 10. S. Theſe two kinds of animalcula are commonly among the tremella of ditches.

Is copulation required for the propagation of their race by oviparous and viviparous animalcula? If I ſaid that I had once beheld a real copulation, ever ſince I ſtudied infuſions, it would be advancing what is directly oppoſite to truth. But, adhering to the principles of ſtrict logic, from which the naturaliſt ſhould never deviate, no legitimate conſequence can thence be deduced that they do not copulate. Like that of other animals, copulation might be inſtantaneous, and therefore eſcape obſervation. It was poſſible that the eggs of oviparous animalcula might be fecundated after excluſion from the body of the mother, the ſame as thoſe of frogs and toads: therefore I had to attain the truth, by obviating every poſſibility of the contrary, which was accompliſhed in the following manner. Having put the egg of an animalcule in a watch-glaſs, I concluded, if the animalcule from this iſolated egg produced a fertile egg, there would be no need for copulation: if a ſterile one, that more than one individual was requiſite for propagating the ſpecies, that is, copulation was eſſential. But the truth is, as many animalcula were produced as

eggs

eggs laid by the folitary animalcule, which fucceeded with all the fpecies I examined (1).

A fimilar method was purfued with viviparous animalcula, by taking feveral young, one by one, from the parent before being fully developed, and ifolating them to prevent all fufpicion of mutual intercourfe. Each ifolated animalcule, in due time, became parent of other two, that is, of two, fpeaking of the firft fpecies, and three, fpeaking of the fecond. And thefe young afterwards had defcendants (2).

Thefe two genera of oviparous and viviparous animalcula are, therefore, hermaphrodites in the

(1) Some authors indeed deny the copulation of animalcula. Roffredi fays he has been long acquainted with microfcopic animalcula, and known the frivolity of their pretended copulations: immediately afterwards, however, he defcribes that of the eels of blighted corn. Several inftances are related by Muller. The copulation of the Paramæcium Aurelia has continued two hours, *Anim. Infus.* p. 58. The Trichoda Aurantia, Prifma, Lynceus, copulate; as do the Vorticella Hamata, and Crateriformis, p. 185. 188. 226. 278. 279. 280. He has feen what he calls both a tranfverfe and longitudinal copulation of the Trichoda, Ignita, and Charon, p. 186. 230.—T.

(2) The Vorticella Nafuta propagates both by producing living fetufes, and by the divifion of its own body into four parts, *Muller*, p. 269. In this it fomewhat refembles other animals that produce both eggs and young.—T.

ſtricteſt ſenſe. When we find infuſion animalcula propagating by diviſion to be ſuch, ſince iſolation does not prejudice their multiplication, it may readily be ſeen how far abſolute hermaphrodiſm extends in the animated world, though formerly limited to few ſpecies.

This diſcovery tends greatly to elucidate a difficult queſtion concerning the original inhabitants of infuſions. Some time after an infuſion is made, it will be ſwarming with animalcula, though the utmoſt precaution is uſed againſt any one being concealed; and for greater ſecurity it is boiled ſeveral hours. I aſk, How do the original founders of the future moſt numerous inhabitants come there? I can conceive only two ways; they muſt either have pre-exiſted in the infuſion mixed with it, or they muſt have come there by means of germs. The firſt opinion cannot be adopted; for had they pre-exiſted in the infuſion, we are obliged to admit that they would never die when out of a fluid, or that they revive when reſtored to one, as the wheel animal and ſome other animals do. But experiments without number have demonſtrated to me, that the inhabitants of infuſions die irrecoverably on evaporation of the liquids (1). Thus there is a neceſſity for

(1) Muller, beſides quoting Wriſberg's experiments, and mine, ſays he has obſerved the ſame. Decantatus infuſoriorum

for recurring to the ſecond mode, namely, to ſome germ or ovulum paſſing from the air into the infuſion, and becoming the origin and ſource of ſo numerous a race of creatures. Such an inference acquires

fuſoriorum vere demortuorum, Vibrionem anguillulam ſi excipias in vitam reditus mihi ſeſe nullo experimento probavit, nec acutiſſimis obſervatoribus, Spallanzani et Wriſberg ſucceſſit, neque quomodo eadem reviviſcant perſpicio cum corpora plerorumque poſt exhalatam aquam rumpi, et in moleculas efflari manifeſte video.—A.

Here Muller evidently ſpeaks of complete death. In the ſecond edition of the work alluded to, the *Animalcula Infuſoria*, ſome additional remarks are ſubjoined to theſe, which the author has not proceeded to quote.—"But a drop of water being ſupplied, before complete rupture of the parts, motion and life will return; though, from the violence the animal has ſuffered, a degree of languor will for ſome time, or always, remain. If the animalcule is defended from the injuries of the air, by means of any particles of duſt or ſand caſually in the infuſion, and the humidity not entirely exhauſted, it will recover. Some are deſtroyed and totally diſſolved by ſimple contact of the air. I have ſeen ſome decompoſed on approaching the edge of a drop; and even others, amidſt the rapidity of their courſe, I have ſeen diſſolve in a moment."

Perhaps all animals are ſubject to inſtantaneous death. It is frequently found in inſects. But the apparent death of animalcula will often proceed from inability to move the members, except in water.—T.

acquires more force and perſuaſion when ſupported by facts. I allowed the fluid about animalcular eggs to fail, ſo that they remained quite dry ten days; then they were reſtored to their native liquid:—beſides being revived, they were ſoon hatched. From this there is no difficulty in conceiving why animalcula originate in infuſions where there are originally none; eſpecially by reflecting on the immenſe abundance diſſeminated through the air and on terreſtrial ſubſtances; and conſidering the innumerable animalcula inhabiting the waters of the globe.

Every fluid is not equally favourable to the expanſion of animalcular eggs. Pure water alone is unfit for it: hence it is no longer a myſtery, why in it, and much more in diſtilled water, we hardly ever ſee animalcula. On the other hand, they always originate in the water where vegetable ſeeds are macerated. I have found no fluids better adapted to the production of eggs than thoſe where infuſed ſeeds began to corrupt. The appearance of animalcula ſhews, that incipient putrefaction creates qualities in the decompoſing materials fit for developement of the egg: for ſuch is the tenor of nature, that eggs are not excluded wherever they happen to be, or in every caſe, but only in ſuitable ſituations, and by means of certain determinate conditions.

I have

I have particularly examined whether animalcula were ſpecifically different, according to the difference of the ſeeds infuſed and whether each had its peculiar ſpecies. Here I have found no uniformity. Certain ſpecies only have been found in particular kinds of vegetables; but it often happens otherwiſe. Both at different times and different places is there a variety in the animalcula of the ſame infuſion: and it is not uncommon in two infuſions of ſeeds, taken from the ſame plant, made at the ſame time, and kept in the ſame ſituation; a fact which well coincides with the vaſt variety of animalcular eggs diſſeminated in the air, and falling every where without any law.

If we can affirm that all the ſpecies, multiplying without any apparent diviſion, do ſo by means of ſome pre-organiſed principle, as is moſt credible, it muſt be allowed they form a moſt intereſting part of our animalcula. The other claſs, propagating by diviſion, and thence called microſcopic polypi, preſent ſomething ſtill more intereſting. What can we think of their origin in infuſions? Doubtleſs they alſo proceed from ſome pre-organiſed principle: but is that a ſeed, an egg, or other analogous corpuſcle? If facts are demanded, I acknowledge we have none; as theſe polypi die when deprived of the fluid, nor do they revive when it is reſtored, we cannot believe they fall from the air. I have no ſenſible

evidence

evidence of their originating from a pre-organiſed principle, ſince it has never been diſcovered by me; but, adhering to eſtabliſhed facts, this opinion is to be embraced: for, if the polypi, firſt ſeen in infuſions, are not produced by plaſtic or vegetative powers, which ſo many facts have proved chimerical, and cannot be the ſame that fall from the air into the infuſion, it is moſt rational to infer, that they proceed from ſome germ, or pre-organiſed principle, whatever it may be called. It is of no importance whether the germ or ſeminal principle is inviſible, or reproduction of the polypi effected by means of diviſion; becauſe, with reſpect to the firſt, we know that we ought not always to conclude on the non-exiſtence of a thing which we do not perceive; and in the caſe before us, the germs may be either too tranſparent or too minute to fall under our ſenſes; and with reſpect to the ſecond, this is not the only polypus which multiplies by germs or eggs, as ſome others do the ſame.

I have ſuppoſed that the germs whence animalcula originate come from the air; and this appears moſt reaſonable from the ſupport of undoubted facts, which I ſhall briefly enumerate (1).

Sixteen

(1) Muller alſo thinks it probable that animalcula and their eggs come from the air. Praefat. p. 22, 297, 298. —T.

Sixteen large equal ſized glaſs veſſels were ſelected and divided into four claſſes. Four were hermetically ſealed; four ſtopped with wooden ſtoppers, well fitted; four with cotton; and the remaining four left open. By this means the external air had no communication with ſome; very little with others, with the third claſs more, and as free as poſſible with the reſt. Every four contained infuſions of hemp-ſeed, rice, lentils, and peaſe; and were boiled a full hour in the vaſes before being cloſed up. I began the experiments 11 May, and viſited the vaſes 5 June. In each were two ſpecies of animalcula, large and ſmall; but the four open infuſions were ſo full and crowded, that they ſeemed to teem with life; with the cotton ſtoppers, they were about a third ſcarcer; and the animalcula ſtill fewer in the veſſels with wooden ſtoppers; in thoſe hermetically ſealed were feweſt of all.

The eſſence of the experiment was the ſame on taking maize, wheat, and barley for infuſion. Inſtead of uſing ſtoppers, I covered ſome of the infuſions with nut or olive oil; and this new obſtacle further diminiſhed the number of animalcula.

The immediate conſequence reſulting from theſe facts is, that animalcula are more numerous in proportion to the communication of the infuſions with the external air. From what we ſee, their origin is either from germs brought by the

the external air to the veſſels, or, being mixed with the infuſions, it concurs to aid the expanſion of them. That ſuch germs may be partly mixed with infuſions, and the air promote their evolution, I have no difficulty in believing. But the facts hitherto related, evidently demonſtrate that the air ſerves as a vehicle to them; and as it is impoſſible in this caſe to recur to thoſe of the infuſion, which ſhould have been deſtroyed by boiling for an hour, we are under the neceſſity of referring to thoſe of the air. This fluid entering more freely and copiouſly, ſhould convey a much greater number of germs into the open veſſels, and of conſequence the population of the infuſions ſhould be greater. The reverſe will happen, where little air enters and penetrates with more difficulty, as when the veſſels are ſtopped with wood. The volume of air included in the veſſels hermetically ſealed, will produce the animalcula appearing there, but few in compariſon to thoſe in open veſſels, on account of the rareneſs of the producing germs, which are proportioned to the ſmall quantity of air that is never renewed.

CHAP.

CHAP. IX.

ANIMALCULA POSSESS THE REAL AND CHARACTERISTIC MARKS OF ANIMALITY.

THE exiſtence of an immaterial and ſentient principle in animals, reſts on the analogy between their organization and operations compared with the organization and operations of man. Many who have had recourſe to this kind of analogy, though profound metaphyſicians, have not been naturaliſts enough to examine it as it ought to be. Surely they have not taken the animal progreſſion in its full extent, nor deſcended to a juſt and rigorous analyſis, which would have demonſtrated the inefficacy of analogical reaſoning in many links of the animal chain. Without any intention of combating their laudable ideas, let us take a view of them; and firſt of the animal organization. It cannot be denied, that the mechanical ſtructure of numberleſs animals correſponds entirely or in the greater part with that of man. Not to name the oran-outang ſo ſimilar to us, as differing only in the privation of reaſon, quadrupeds and birds in this reſpect could not approach nearer to the human ſpecies. The ſame organs for digeſtion, reſpiration, circulation, ſecretion;

cretion; the ſame ramifications of nerves from the ſpinal marrow, the origin of this from the brain, and the ſimilarity of its conſiſtence; the ſame meandring of veins and arteries, producing innumerable rivers and rivulets through the whole body, conveying life and nutriment every where. No difference is perceptible in the action of the muſcles, ligaments, teguments, cartilages, or tendons: the ſame variety in the nature, the motions, and offices of the bones. Some long, ſome bent, ſome curved into an arch. The hardneſs vies with that of ſtone in ſome: in others, the pliancy is equal to cartilages. Some are hollow and filled with marrow; others ſolid and maſſy throughout. Certain bones conſiſt of a ſingle piece, while various parts connected together form others. Laſtly, all theſe animals have the ſame number of ſenſes, and the organs of them ſituated in the ſame parts of the body, and conſtructed as ours. But it has pleaſed nature to diverſify the figure of theſe animated machines: ſometimes arming them with tuſks, horns, nails, or claws: ſometimes clothing them with ſcales, adorning them with feathers, or covering them with a hard hide; diminiſhing the anterior part of ſome into a pointed beak, a ſlender ſnout, or a long and monſtrous trunk; or enlarging it to form a hideous head, frightful to behold, or exciting pleaſure by its reſemblance to our

our own. This ingenious creatrix has formed the body of ſome ſo as to convey an idea of lightneſs and grace; while others diſplay a ſlothful inactivity: one is contracted within itſelf, and apparently only of a ſingle piece; another extended beyond all bounds; and a third moſt exactly proportioned. In a word, there are as many varieties among birds and quadrupeds as their forms are different from that of man; yet in every one is there the narroweſt reſemblance in the eſſential part of organization.

Analogical reaſoning applied to theſe two races of animals cannot be ſtronger or more convincing; but how is it weakened by deſcending the animal ſcale to fiſhes, reptiles, inſects, and at laſt is totally loſt. Let us attend a moment to the ſtructure of inſects. Not only do the bones, blood, heart, and other viſcera diſappear, but we cannot diſcover either veins or arteries. A longitudinal veſſel from one extremity to the other is ſeen, in which flows a liquid generally tranſparent. Although the nervous ſyſtem is maintained entire, there is no brain, at leaſt nothing properly ſo: and their reſpiratory organs much more reſemble thoſe of plants than thoſe of the larger animals. Deſcending the animal ſcale ſtill lower, every ſemblance of organs is loſt, and the whole body of the animal is reduced to the moſt ſimple ſtructure imaginable. Many polypi are but an elongated ſacculus covered with tubercles: Many aquatic animals are ſimply of a membranaceous or vaſcular texture. Many marine zoophytes are only a kind of jelly. The organization of theſe animals has not the ſmalleſt relation to that of man; plants themſelves may be ſaid to reſemble him more, becauſe we find ſap veſſels, utricles, and tracheae in them (1).

The

(1) Every ſmall animal was formerly called an inſect, and is ſo ſtill, by incorrect writers. The ſub-diviſions of animated nature muſt become more numerous, in proportion as ſcience advances, and peculiar diſtinctive properties are diſcovered. A great family has been ſeparated from proper Entomology, and called *cruſtacea*: but another, much more immenſe, has been removed farther under the name *Vermes*; and additional changes are made by every new writer. But it will be long before they are univerſally obſerved, particularly in imperfect inſects and worms.

It was generally believed, that none of the animals denominated inſects had any brain, and very few ventured to diſpute the fact; and after they did ſo, the reverſe was pertinaciouſly maintained. *Haller* lays it down as a general rule, that all animals, having a head and eyes, muſt alſo have a brain and ſpinal marrow: he thinks, neither eyes without a brain, nor a brain without eyes, exiſt in any animal; likewiſe, that all thoſe with a brain and ſpinal marrow muſt alſo have nerves, *Phyſiologia, Tom.* 4. p. 2. 155. Fabricius ſays, inſects have only the rudiments of a

brain,

The degradation in the organic ſtructure of animals is alſo viſible in their operations. Theſe, in many ſpecies, nearly approach to thoſe of man. Such are the operations of quadrupeds in general; but more eſpecially of the elephant, ape, and beaver. Thoſe of birds, likewiſe, bear much analogy to ours: their ingenuity in conſtructing neſts; the diverſity of note to expreſs the various affections of hatred, fear, pleaſure and pain; the provident ſagacity of many, in changing their climate according to the change of ſeaſons; the facility of inſtructing birds of prey for the chace: all

brain, *Entomologia Syſtematica, Tom.* 1. In one treatiſe, the great comparative anatomiſt Cuvier affirms, they have no brain properly ſo called, but only a ſpinal marrow, ſwelling into knots and tubercles at intervals, from which the nerves proceed, *Tableau de L'hiſtoire Naturelle des Animaux.* However, in his late work on Comparative Anatomy, he deſcribes the brain of many inſects divided into two lobes, and ſending forth nerves. The nervous ſyſtem of the various genera of worms is much more obſcure. According to Virey, Cuvier, and others, they have no brain, but ganglia on a nervous cord. The latter remarks. 'Ganglia nearly equal being uniformly diſtributed 'on a cord, extending through the whole length of the 'body, ſeems deſigned to furniſh each ſegment with a 'brain peculiar to itſelf.' Neither brain nor nerves have yet been diſcovered in the actiniæ, meduſæ, polypi, and many more.—T.

all are qualities proving what I advance. But this analogy exifts no more, when we come to fifhes, reptiles, and infects. It is true, that among the laft are many diftinguifhed by their operations: whether confidered by their anxiety for felf-prefervation, purfuing what is ufeful, and avoiding what is noxious; whether we confider their mutual anxiety for propagating the fpecies, or fingular folicitude for their young, placing them in fuitable fituations, and providing them with food until they need maternal affiftance no longer. We all know the ingenuity of bees, the fagacity of the leaf-moth (*tignuola delle foglie*), the induftry of the ant-lion and fpider, the ferocity of the hornet, or the ingenious cruelty of ichneumons. But the operations of numberlefs other animals are reduced fimply to feizing and fwallowing their prey, as the arm-polypus; or to open and fhut their fhells, as many teftacea; or imbibing nutriment by an immenfe number of mouths on the furface of the body, as many marine animal plants.

By this hafty glance at the animal fcale, we arrive in the degradation at a race of beings, which, to judge of their ftructure and operations compared with thofe of man, we fhould be more inclined to deprive of a fentient mind than to beftow one upon them. Behold how much analogical reafoning is enfeebled in the intermediate

claffes,

classes, and totally lost in the lowest, though appearing so evident and conclusive in the higher degrees! Can we thence assert that animals occupying the lowest rank bear the name of animals improperly, from being apparently deprived of an immaterial and sentient principle? This has already been suspected by Bonnet: he who, both as a profound metaphysician and a most able naturalist, has considered the gradual progression of beings so well. After supposing, in the *Corps Organises* and the *Contemplation*, that the polypus is a real animal, and, on this supposition, explaining the most embarrassing phenomena in his *Palingenesie*, he does not hesitate to hazard a mechanical explanation, by considering the polypus as an animal simply vital, or endowed with irritability alone; and suspects there may be other animals similar from the simplicity of their structure or operations. Needham goes farther: All animals that repair their parts, lost either by amputation or by natural division, are, according to him, animals *simply vital*, in which he places the immense kingdom of infusion animalcula, since, by M. de Saussure's discovery, they propagate by division (1). But he is less inclined to exclude

(1) There is such an immense variety of animalcula, that it is very difficult to say what class they belong to. Some late writers class them among worms, and some among

exclude them from the rank of animals from too great ſimplicity in ſtructure or operations, than his inability to conceive how an organiſed being reproducing by diviſion can have a ſoul.

That beings ſimply animated, or animals whoſe life conſiſts only in irritability of the parts, are poſſible, I can eaſily believe; eſpecially when ſpeaking of thoſe whoſe actions are few and little varied, and that by this hypotheſis the gradations of organiſed beings is better united by connecting the animal and vegetable kingdoms by means of ſuch ſimply vital or irritable beings, inferior to an animal, and ſuperior to a plant. That it is poſſible infuſion animalcula may be of the number, I offer nothing againſt: nor will any thing in the leaſt diſadvantageous to it be ſeen in what has yet been ſaid in this Tract. Earneſt, however, to reduce poſſibilities to facts, I am much more inclined to judge them real and actual animals than beings ſimply vital or irritable. There is foundation for my opinion, becauſe an aſſemblage of qualities is recogniſed in them ſufficient to conſtitute the qualities of abſolute animality. I have had occaſion to remark ſome of theſe qualities in my Diſſertation, ſuch as the exertions of animalcula

mong zoophytes. If our microſcopes could diſcover their internal ſtructure, it is moſt probable that many would be removed far from both.—T.

cula to avoid each other and the obſtacles to their courſe; ſuddenly changing their direction, taking an oppoſite one; and paſſing inſtantaneouſly from reſt to motion, without any external impulſe; eagerly darting to particles of the infuſed ſubſtances; inceſſantly revolving on themſelves, without a change of place, their courſe againſt the current, and crowding into ſhallow parts of the fluid (1). Neither have other characteriſtics,

(1) M. Guettard, in a work which, from the prodigious multiplicity and variety of matter, might be called a treatiſe *de omnibus rebus et quibuſdam aliis*, is convinced that infuſion animalcula are only the farinaceous veſicles of ſeeds put in motion by external cauſes: and with this conviction does he judge it proper to diſcuſs the qualities I mention; all which he eſteems inſufficient to prove the animation of animalcula. To adopt his mode of reaſoning, it would be a matter of very great doubt whether horſes or elephants were real animals, though poſſeſſing the ſame qualities. But the moſt wonderful circumſtance is, that the author is perfectly innocent of all theſe matters; and evidently ſhows that he has never ſeen a ſingle infuſion animalcule in his life. It would be loſing time to demonſtrate the frivolity of his arguments, fit for the ignorant only. The reader may conſult the author himſelf; and that he may not think me exaggerating, he is referred to Muller, who, without the leaſt connection with me, either by friendſhip or literary intercourſe, undertakes my defence,

 or

racteriſtics, forcibly corroborating their animality, failed to occur in the compoſition of this work; partly deduced from various accidents to which they are ſubject, like other animals, when put in ſimilar ſituations.

For the reader's convenience, I ſhall bring my different experiments briefly into view: from the recapitulation, he will be more enabled to underſtand the uſe of compariſon, which, as he already knows, conſtitutes no ſmall portion of the work.

Too great heat deſtroys animal life: 111° is fatal to the tadpoles of frogs, and to frogs themſelves,

or rather the defence of truth. Impreſſo huc uſque libello in manus venit folium 30 novel. Lett. Gottingenſ. 1772, ubi clariſſ. Guettard animalcula infuſoria meras veſciculas farinaceas arguere indicatur. Accerſito libro (memoires ſur differentes parties des Sciences et Arts, tom. 2. Paris 1770) avidiſſimeque, quae de his agunt perlectis, et quaſi devoratis, vultu tamen continuo ſubridente non potui, non admirari doctiſſimi viri temeritatem, argumentis, quae ſolo ingenio debentur (veſtigium enim obſervationis ullius infuſorii ab ipſo inſtitutae nullum extat) tentandi refutationem eorum, quae meris obſervationibus innituntur. Nec abſque apparenti ſucceſſu, licet enim meliora clariſſ. Spallanzani argumenta pro animalitate infuſoriorum pugnantia in aream producat, cuilibet lectori, obſervationum aeque ignaro, ac ipſe, fucum facit, quem tamen unaquaeque infuſoriorum contemplatio diſpellet. In re enim naturali non ingenio, ſed obſervatione vivitur.

ſelves, the nymphs and larvae of muſkitoes, and to water newts: 108° kills ſilk-worms and the larvae of the blue fleſh fly: 106° kills leeches, rat-tailed worms, and water fleas: and animalcula die at about the ſame degree of heat, that is 106°, 108°, 111°.

All animalcula are not alike affected by cold. Some die at freezing, or a degree not much greater; others ſurvive at 10°. Thus is it with inſects. Winter deſtroys moſt of them, but many brave its rigour, and ſome retain the uſe of their members, as is ſeen in various ſpecies of infuſion animalcula. During ſummer, I have often frozen water in a concave glaſs where different little inſects ſwam. Freezing began at the circumference, and formed a wreath of ice: but the inſects never remained to be impriſoned in it; they retreated to the interior where the water was yet fluid, and, as freezing advanced, collected in the centre of the glaſs, where they periſhed on complete induration of the fluid. Infuſion animalcula exhibit preciſely the ſame phenomena.

The odours and liquors that are a virulent poiſon to inſects, are the ſame to animalcula. Such is the odour of camphor, the fumes of turpentine, ſulphur, and tobacco. Oleaginous, ſpiritous, and ſaline liquors are equally deſtructive. The electric ſpark is a real thunderbolt to both. Agents ſlowly deſtructive of infuſion animalcula are

are likewiſe fatal to inſects, as the vacuum of an air pump.

The motions of animalcula concur in proving their animality. They are not the ſame in all, but different, and produced by different means peculiar to each ſpecies. Many move in infuſions only by undulating the body, as eels do in ſwimming. That undulation is not ſimilar in every one; for ſome form a few ſlight curvatures, others deep and numerous. The figure is formed in a moment by ſome, by others ſlowly and gradually. The arms, points, and fibrils, proceeding from the extremity of the body, are inſtruments for many animalcula to ſwim: ſome are long, ſome ſhort, ſome ſtrike the water often, others ſeldom, and the reſt with various degrees of velocity. There are animalcula whoſe motion is very languid, and there are ſome that move moſt rapidly. Some move at intervals; the motion of others is perpetual: they never ſeem to reſt. I have ſeen one ſpecies whoſe poſterior fibres, diſengaged and ſeparated far aſunder, folding together in an inſtant, darted the animal to a conſiderable diſtance, like an arrow from a bow. The courſe of ſeveral ſpecies never deviates from a ſtraight line; others continually pitch up and down like a veſſel at ſea. Some whirl like tops or balls on themſelves, without moving from the ſpot; while others have a progreſſion during this

rotatory

rotatory motion. In ſhort, there is no race whatever which, on diligent examination, does not exhibit motions peculiar to itſelf.

If to all this we join the artifice evinced in forming a vortiginous current to entrap their prey: their ferocious purſuit of the ſmaller animalcula, their indifference when the ſtomach is full, and greedineſs after them when hungry. If we conſider all the qualities relative to their nature, motion, and properties, not ſingle and disjoined, but collected and united in the ſame ſubject, it is impoſſible not to admit one of two things: either that an infinity of beings recogniſed by all the world as real animals are not actually ſo; or, if they are, ſuch alſo muſt be the beings found in infuſions.

If we reſume the uſual mode of analogy, which is the only ſupport of accurate judgment, whether a ſentient principle reſides in animals, and compare the various operations of infuſion animalcula with thoſe of the largeſt animals and ourſelves, we ſhall not find them ſo diſtant or different as not to correſpond in ſeveral particulars. Beſides the organization of many animalcula being ſo ſimple as to appear nothing but an aggregate of granuli inveſted with a ſkin, and completely included in it: in ſome we ſee an aſſemblage of parts for the moſt oppoſite uſes, ſuch are fibrilli for a vortex, fins for ſwimming, a mouth,

mouth, an oeſophagus, a ſtomach, which ſeems to have a periſtaltic motion, agitating the included aliments. I ought to add another organ I have diſcovered in this new courſe of obſervations, which I ſuſpect is deſtined for reſpiration. It conſiſts of two ſtars, with a very minute globe in the centre, and ſituated, as one may ſay, in the foci of elliptic animalcula of the largeſt or middle ſize, Plate 1. fig. 11. T. T. Whether the animalcule moves or not, the ſtars are always in alternate and regular motion. Every three or four ſeconds the minute central globules ſwell like a bladder to three or four times their natural ſize, and then fall: the inflation and eflation are performed very ſlowly. The ſame is done by the rays of the ſtars, except that inflation of the globes empties the rays, and inflation of the rays empties the globules. During this alternative, a long narrow ellipſe is obſerved, in the largeſt animalcula, on the ſide between the two ſtars, in continual motion U. (1).

Under this conviction, that the animalcula of infuſions are real animals, in addition to the full concurrence of paſt and preſent obſervers, except M. de Buffon, Mr Needham, and a few of their partiſans, it gives me inexpreſſible pleaſure to

(1) It is the opinion of ſeveral naturaliſts, that animalcula have no organs for reſpiration, and live without air.—T.

to see myself joined by a naturalist, whose authority, though standing single, I should not hesitate to oppose to that of all Europe. I speak of M. de Reaumur, that is of one who, in the study of the obscure kingdom of minute animals, indisputably holds the first rank among the naturalists of the age. In letters to M. Trembley and Bonnet, he thus expresses himself on the theories of Needham and de Buffon; and, with respect to the first, he says, 'my object was to 'verify observations that had given rise to such 'strange ideas of the generation of animals. Different infusions have been my deepest study; and 'I not only find the imaginary organic molecules 'real animals, but that they are similar in generation to others. That these animals, according 'to the new theory, always become smaller and 'smaller, I have found absolutely false; on the 'contrary, all here proceeds by the ordinary rules, 'those originally small at length becoming larger.' (1).

This celebrated person expresses himself as decisively to M. Bonnet, signifying that he had repeated the experiments on the INSECTS of infusions; that he had examined them most attentively, and for whole hours; and had discovered what had imposed on those who supposed them simple globules in motion.

The

(1) Corps Organisés, tom. 1.

The firſt extract confirms what M. de Sauſſure and myſelf remarked of the erroneous idea which had induced the belief of the ſmaller infuſion animalcula being generated by others larger, and theſe by ſome of ſtill a larger ſize, according to M. Needham and de Buffon's ſentiments, who have undoubtedly been miſled by a fact very ſeductive in appearance. It often happens that the whole animalcula of an infuſion are of the largeſt ſize. By an invariable law, the life of animalcula has a determinate period; therefore the largeſt periſh in a certain time. Frequently, when they begin to diminiſh, a ſmaller ſpecies is generated, and theſe are ſucceeded by ſome ſtill ſmaller; laſt of all comes a colony of leſs ſize than any of the whole. One accuſtomed to explore nature, and to have nothing but her operations in view, will ſoon perceive there is no relation of parent and offspring among the ſucceſſive generations. But whoever diſdains the trouble of analyſing natural phenomena to the utmoſt, and fancies an hypotheſis, that the ſmaller races proceed from the larger, will readily diſcover it in every ſucceſſive colony of a different ſize.

If, from the reaſons adduced, we are conſtrained to conſider infuſion animalcula real animals, what can we anſwer to M. Needham, who conceives himſelf obliged to ſuppoſe them machines ſimply

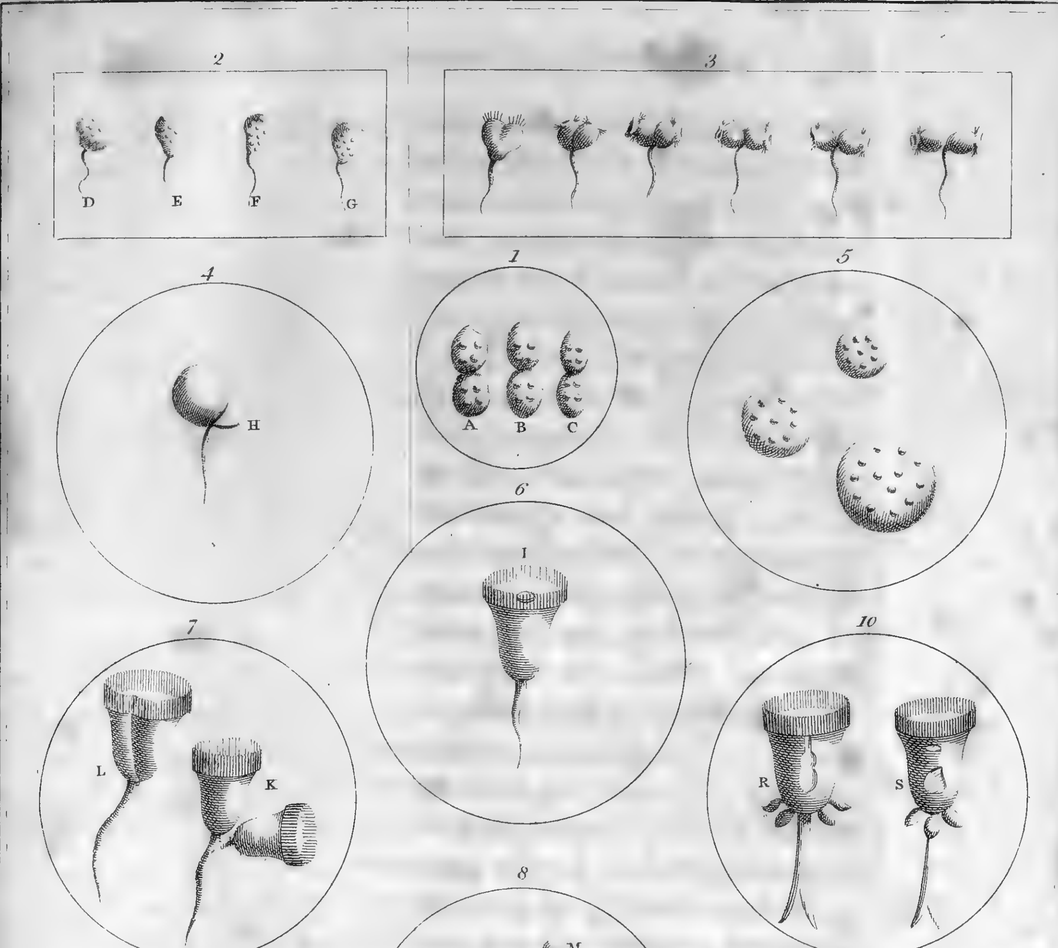
vital,

vital, and from the ſingular cauſe, that they propagate by diviſion? Firſt, I ſay the author draws a general concluſion from particular facts, as he aſſumes a general propoſition, that all animalcula propagate by diviſion. But many others multiply without it. The objection therefore will affect only thoſe of the firſt ſpecies; and far from not admitting a plauſible anſwer, it had formerly been advanced by the partiſans of *automatiſm*, when a diſcovery was made, that the ſections of the polypus became complete animals, as may be ſeen in Bonnet's *Corps organiſés*; which work, if Needham had taken the trouble to peruſe, would have prevented him from publiſhing his objections; becauſe if in abſtruſe and obſcure matters one is contented with probability, as a wiſe and rational philoſopher ought to be, there he would have found enough to his ſatisfaction. Therefore I adhere to M. Bonnet's principles, not only becauſe ingenious but juſt; and by their means we can comprehend and explain how the divided parts of an animalcule are transformed into animated and ſentient beings. The fact may be elucidated by an animal many million times larger than infuſion animalcula, by the earth worm. Every ſegment becomes a new whole regenerating in itſelf the parts deficient, and among others the head and tail (1). The reproduction of theſe parts,

(1) Prodromo ſopra le Reproduzioni Animali. In Modena, 1768.

parts, and the ſame may be ſaid of the reſt, is probably by means of two germs, one deſtined to develope into a tail, the other into a head. The ſoul of the worm when entire reſided in the head, admitting in general that it reſides there in animals. It will alſo reſide in the ſame part of the regenerated worm, either becauſe God has created a new mind, or, as appears more philoſophic, becauſe this mind pre-exiſted in the germ, and only required evolution to be called into exiſtence. Behold how the ſections of a worm are reproduced into new and ſentient worms. This with the due proportions may be transferred to infuſion animalcula, propagating by natural diviſions. Theſe, as far as yet known, may properly be reduced to three kinds, the *tranſverſe*, *longitudinal*, and *anomalous*, or irregular. By the tranſverſe, the animal ſeparates into two parts, one the anterior, the other the poſterior. As in the anterior the head remains entire, conſequently the ſoul, that *ſelf*, that perſonality by which a being may be called animated, will alſo remain entire. The queſtion reſts on the poſterior part alone. The progreſs here is the encreaſe of this ſection, until it becomes equal to the whole animal; it aſſumes the figure peculiar to the animal's head, whether pointed, curved, obtuſe, or bell-ſhaped, and if the animal is of the number that produce whirlpools, the points generating

a vortex



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vortex begin to protrude. Therefore I have good reaſon to believe that a new head is developed, and conſequently that this *whole* begins to be animated by a new ſentient principle.

The theory eaſily applies to longitudinal diviſion, ſince it is undoubted that the ſoul reſides in one of the lateral portions, as it reſides in the anterior part, where tranſverſe diviſion takes place, at the ſame time it is certain that the other lateral portion will be fully renewed, as the poſterior part is in tranſverſe diviſions. Therefore, if this portion expands to form a real animated and ſentient being, it is rational to ſuppoſe the ſame will happen to the other.

The like may be ſaid of the anomalous or irregular diviſion, by which I mean the diviſion of an animal into more than two parts, without being referrible to either the longitudinal or tranſverſe. Into whatever number of parts it divides, each, in acquiring the ſize and figure of the whole, will acquire that perſonality which conſtitutes it a real animal. One part only in theſe irregular diviſions does not require the developement of a new ſoul, that is, what formed the original head, as is moſt evident.

TWO EPISTOLARY DISSERTATIONS ON INFUSION ANIMALCULA ADDRESSED TO THE AUTHOR BY THE CELEBRATED M. BONNET OF GENEVA.

ARGUMENT.

MANY of the reſults in theſe tracts had at different times been communicated to M. Bonnet, eſpecially in two long letters of 20 December 1770, and 15 September 1771. The anſwer to the latter will be found in the Tract on Seminal Vermiculi, and that to the other is the firſt of the following. As theſe two letters particularly compriſe the reſults on infuſion animalcula, it has been judged proper to ſubjoin them to the tract. What the reader has previouſly peruſed will enable him to underſtand them properly.

According to M. Bonnet's deſire, ſome annotations have been made by the author, where he felt himſelf neceſſitated to be of a different opinion. And he has been the more induced to it as he knew M. Bonnet was ſincere. Theſe letters, eſpecially the ſecond, would afford a ſtriking inſtance of the facility with which this great philoſopher abandons his opinions, when inconſiſtent with facts, or leſs probable than thoſe of another, if his other works did not already demonſtrate it.

LETTER

LETTER I.

My Solitude, 17 January 1771.

Three of your letters, my illustrious friend, are before me; the first dated 23 November, the second 20 December, and the third 6 January. I owe you a long answer, especially to the second, for it is an exuberant folio replete with new facts, on which it is impossible to bestow too much reflection. How much your interesting details have delighted me cannot be described, nor could I resolve to engross the whole to myself. Mess. Trembley and De Saussure have participated in the pleasure, and both have been equally satisfied; they desire many compliments, with most sincere wishes to you. Their applause was certain; and having traversed these unknown regions, they are the best judges of your discoveries. We all three coincide respecting your letter, and join in the just eulogiums which your ingenuity, accuracy, and correct reasoning merit so well. Some ideas, excited by that interesting epistle, were communicated to these intelligent observers, and appeared to give them satisfaction. I could have wished that theirs had been communicated in return, but M. Trembley delays until nature speaks in more intelligible language; and M. de Saussure till he has repeated the experiments. Therefore I shall alone run over the principal articles of your curious dissertation, for such undoubtedly is the immense letter you have taken the trouble to write, and for which accept a thousand and a thousand acknowledgements. It has been perused with the pen in my hand, and a corrected extract made, that nothing essential might escape, and that I might be the better enabled to comply with your request. It is only discharging my heavy debt to your friendship.

I. Your distribution of infusions into classes, distinguished by the time of ebullition, has been most judicious. By excellent experiments, we are now assured that two hours boiling does not prevent the production of animalcula; we have even reason to admit that the population of infusions is generally proportioned to the duration of ebullition, and the longer it is continued, the more do animalcula encrease (1). Here then is enough to pulverise

(1) The meaning of my proposition is: although the least boiled infusions had originally few animalcula, compared with those that had boiled more, in time they had immense numbers. This greater abundance is naturally explicable by the increasing dissolution of the infused seeds, because dissolution is a condition most necessary for the population of infusions. M. Bonnet supposes that the additional animalcula may arise from more of them, or their germs, falling into the infusions. I have shewn that it is as little probable that animalcula fall from the air, as it

pulverise all the sophisms of our obstinate Epigenesist. The infusions, at first rarely inhabited, became more populous in time, and you ascribe it to the gradual dissolution of the infused matter. As the vessels continued open, one might say the additional numbers depended on the seeds of animalcula, or on animalcula themselves precipitated from the air, perhaps being attracted by the penetrating odour of the infusion. I do not hesitate to make these suggestions: you wish it, and yourself discover many which are similar, in your investigations of nature.

N 3 II.

it is certain their germs do so. Whence I have no doubt that the animalcula, whose numbers increase with the lapse of time, partly originate from new germs precipitated into the vessels. However it cannot be thought, that the accession of these germs alone is equal to the additional population, otherwise an equal quantity having fallen into the infusions that had boiled much, as into those that had not, there is no reason why the animalcula of the first should be abundant in a few days, and those of the second only after an interval of many. A difference so sensible must depend on some secret condition, and I can see none other than decomposition of the infused substances which takes place as much sooner in the infusions boiled much, as later in those that have boiled little.

II. It was already important to ſee thouſands of animalcula in every kind of infuſion boiled two hours; but ſubſtances were expoſed to a trial ſtill more ſevere, by roaſting them in metallic cups, and then forming powders of which you compoſed infuſions with boiled water. All ſwarmed with animalcula of every ſize and deſcription. After this, how can we refuſe our aſſent to the general concluſions which you deduce from ſuch deciſive experiments? How can we refuſe to agree, that the *vegetative* or *productive* power of our friend the Epigeneſiſt is a perfect chimera? He objects that too great heat in your firſt experiments might have deſtroyed the productive power of the matter infuſed; yet, when expoſed to much greater heat, it was ſtill inhabited by numerous animated beings (1). If the obſtinacy of our friend is not invincible, he will yield to ſuch evidence.

III. It ſeems rigorouſly demonſtrated by your experiments, that animalcula appear in ſubſtances included in veſſels hermetically ſealed, and expoſed ten minutes to the influence of boiling water before incluſion. But the reaſon why as many animalcula are not exhibited in cloſe as in open veſſels, we may infer to be, becauſe the exceſs in the latter ariſes from the ſeeds of animalcula and ani-

(1) It is already ſeen how much heat was increaſed.

animalcula themſelves precipitated from the external air. Perhaps the communication of the ſubſtances with the air may alſo facilitate their diſſolution, and the generation of animalcula in conſequence.

IV. By the cleareſt experiments, you have happily refuted an important objection, that the altered ſtate of the air in the veſſels had prejudiced the vegetative power of ſubſtances infuſed; but animalcula continued to appear in veſſels hermetically ſealed expoſed to boiling heat, ſome from half a minute to two minutes, others from ſix minutes to twelve (1). The ſmalleſt animalcula only originate, and none of the largeſt or middle ſized. Thus it ſeems ſufficiently proved, that thoſe of the higher claſs cannot originate or expand in ſubſtances ſubjected to ſimilar experiments. A fact which may be the ſubject of the moſt profound meditation is demonſtrated: the ſmaller the animalcula are, the leſs injurious is heat to their generation or developement. I ſhall ſoon return to this. If the higher claſſes are not ſeen in veſſels hermetically ſealed, and expoſed half a minute to boiling heat, cannot we thence conclude, that all the animalcula you have ſeen ſo numerous in infuſions boiled from half an

 hour

(1) Ebullition above twelve minutes has not obſtructed the production of the ſmalleſt animalcula.

hour to two hours, that theſe animalcula, I ſay, or part of them, may come from the external air, from that in the veſſels or the ſeeds attached to their ſides, or from all three? This concluſion ſeems the more probable concerning the higher claſſes. Indeed, if we ſuppoſe that they or their ſeeds lodged in the infuſed matter, there is no reaſon why they ſhould not appear in veſſels hermetically ſealed, and expoſed to the heat of boiling water, if that degree was not prejudicial to their appearance. You have proved that they are ſtill ſeen in ſealed veſſels of infuſions, which have not been expoſed to heat. The higher claſs, therefore, did not pre-exiſt in the infuſed matter: But I do not thence mean to inſinuate, that they or their germs could not pre-exiſt in it, for animal and vegetable ſubſtances are probably covered with them. I only mean, that theſe animalcula, or their germs, are probably deſtroyed by boiling the ſubſtances where they are lodged. Are you not ſurpriſed, my dear friend, that I do not ſay *certainly* deſtroyed? But I dare not make ſuch a poſitive aſſertion concerning beings ſo little known. Is it not poſſible, that the heat of boiling water, or any other of equal or even greater degree, produces no effect but deſiccation of animalcula or their germs, and thus reduces them to a ſtate analogous to that of pennated polypi's eggs, which may be kept dry ſeveral months, as

I have obſerved, Article 317 of the *Corps Organisés?* After having boiled veſſels hermetically ſealed with various infuſed matter, I wiſh you would let it cool in the ſame veſſels, and make obſervations to diſcover whether the animalcula gradually appear. This ſimple experiment may be very inſtructive (1).

V. By your letter, I perceive that the animalcula of what you term the *mediate and higheſt claſs*, which are here more briefly deſigned of the *higher*

(1) My anſwer to M. Bonnet is thus expreſſed: 'The 'experiment which you, my illuſtrious friend, propoſed, 'had already been made in part, although the ſole object 'at the time was to examine whether veſſels hermetically 'ſealed, and expoſed to the influence of heat, would afford 'more or fewer animalcula in proportion as I delayed to 'obſerve them. Therefore, on 26 September 1770, eleven 'veſſels were boiled, and the ſeals broke 9 October; 'they contained only the moſt minute animalcula: five 'more were opened October 13: they had none but the 'ſame animalcula. Thus the prolongation of time had no 'influence on the production of the largeſt animalcula.' Brevity prevents me from relating another experiment, where the effect was ſimilar. We have already ſeen, that protraction of time did not favour the production of the largeſt animalcula, or higheſt claſs, in veſſels hermetically ſealed and expoſed to heat. I think there is reaſon to conclude, that the heat of boiling water really deſtroys the germs of the higher claſſes.

higher orders, I perceive, I ſay, that theſe animalcula cannot expand at 174°, but want of time prevented the extenſion of your reſearches on this point. It would be very deſirable to aſcertain the degree, or to come near it, at which they may be developed (1); and it would be uſeful to aſcertain how much cold they can ſuſtain. All this would have ſome tendency to elucidate the ſingular conſtitution of theſe living beings, and afford us compariſons and inductions which might throw ſome light on ſo obſcure a part of the animal kingdom. The evolution of animals is evidently proportioned to the heat neceſſary for putting their fluids in motion, and for extenſion of their veſſels. The earlieſt plants are apparently thoſe whoſe liquids are put in motion by the leaſt degree of heat, and whoſe veſſels afford but little reſiſtance to gentle impreſſions of their fluids. The life of many inſects may be abridged or prolonged by keeping them in cold or warm ſituations, *Corps Organiſés, Art.* 167; and we know there are inſects that can ſupport the cold of 14 or 15° of Reaumur's thermometer, and remain alive though complete-

ly

(1) By experiments afterwards inſtituted, I was able to fix the preciſe degree.

ly frozen (1). To the celebrated Reaumur are we indebted for our knowledge of this part of the animal economy. In January 1767, I repeated the curious experiments with frozen inſects. Chryſalids of the beautiful cabbage caterpillar were expoſed to 12 or 13° (2); they ſeemed completely frozen; and, when dropt into a china veſſel, ſounded like a ſtone. But they were not dead; and, towards the middle of May, the butterfly appeared, nor was the transformation later than of others of the ſame ſpecies that had paſſed winter and part of ſpring on the ſtove of my apartment. Infuſion animalcula might in this way preſent much more ſurpriſing facts: we have only to invent experiments fit to diſcover them. The ſubject is too intereſting not to excite the curioſity of a naturaliſt as intelligent as you (3).

VI.

(1) M. Bonnet means theſe degrees below freezing; becauſe the ſcale of Reaumur's thermometer begins at the freezing point, or 32 of Fahrenheit's; therefore 14 will be nearly 0, and 15 about two below 0 of Fahrenheit's.—T.

(2) About 5 and 3 of Fahrenheit.—T.

(3) My anſwer to M. Bonnet informed him that I had anticipated the experiment he ſuggeſts; but that the communication was reſerved until I had obtained enough of facts. Theſe are detailed in the Tract.

VI. I now come to that article of your letter which has given me the moſt agreeable ſurpriſe, and affords moſt ample matter for reflection. You have completely proved that the ſmalleſt animalcula, or thoſe I denominate the *loweſt claſs*, originate and expand in infuſions expoſed from half a minute to twelve minutes to the effects of boiling water, in veſſels hermetically ſealed; while it is at the ſame time demonſtrated that the animalcula themſelves periſhed at 106 and 108°.

Here undoubtedly is a moſt important fact which philoſophers would never have ſuſpected without deep meditation on the nature of germs, and their analogy with the elements. This excellent diſcovery has given me much pleaſure: it ſeems to corroborate my ſentiments concerning germs; and the reflections which it has excited ſhall be ſubmitted to your opinion.

You know, my dear friend, that the more diaphanous a body is, the leſs is it heated by the rays of the ſun; and the greater the number of pores, the more open and direct, the leſs do the rays act on their ſides. The celebrated Bouguer reaſonably attributes the exceſſive cold felt on the higheſt mountains to the extreme rarity of the air admitting too free paſſage to the ſolar rays for them to make any ſenſible impreſſion on this fluid. It is eaſy to conceive that ſome bodies may exiſt, ſo thin, homogeneous, and perfectly diaphanous,

nous, that light or heat may traverſe them without impreſſion. Even the denſeſt and moſt opaque ſubſtances become tranſparent when divided into lamina extremely thin. Gold is a moſt remarkable inſtance (1). May not the germs of animalcula of the loweſt claſs be among thoſe ſubſtances ſo thin and tranſparent that heat may traverſe them without alteration? Let this idea be proſecuted farther: it merits conſideration.

The animal and vegetable kingdom firſt appear under the form of a whitiſh jelly more or leſs tranſparent. Such is the original form of the majeſtic oak and the powerful rhinoceros: in the beginning they are but a drop of jelly, and ſtill leſs. Were we permitted to aſcend higher in the animal and vegetable origin, we ſhould find them much more tranſparent. We are acquainted with

(1) So far as I know, it can only be ſaid that gold is not abſolutely opaque; for brought to the greateſt degree of thinneſs, $\frac{1}{180000}$ part of an inch, it appears of a green colour. Poſſibly other metals might tranſmit light if they were ſufficiently malleable to bring their pores into a ſtraight line; for it is hardly to be ſuppoſed that any ſubſtance is without pores, or does not conſiſt of component parts, until we arrive at the atoms of matter, if there is ſuch a thing. But it is a very different queſtion whether the pores of ſome ſubſtances may not be ſmaller than the particles of light, however minute theſe are ſuppoſed to be.—T.

with insects which are so all their lives, and continue gelatinous during existence. Such are those belonging to the singular and numerous family of polypi: also the animalcula of infusions. How many insects should be transparent in their primitive state in the state of a germ! It is, indeed, a most remarkable fact, and on which sufficient attention is not bestowed, that all animals and vegetables are, in their original state, nearly of the same consistency, and those which, like the oak and rhinoceros, afterwards acquire the greatest solidity, have at first no more than the polypus. By what wonderful mechanism does nature bring them to the degree of consistence and opacity proper to the species? Here obscurity thickens more and more. Only the rudiments of the profound theory of increment are known. These rudiments I have attempted to trace in *Part* II. *Palingenesie Philosophique*, and shown the philosophic naturalist in what manner he may throw more light on this important subject. The principles, by which I many years ago attempted to give a reason for increment, are in some measure confirmed by Nature herself. M. Herissant has been her interpreter; and his excellent experiments on the growth of the bones and of marine bodies have greatly strengthened the probability of my ideas. He has communicated them to us; and I have had the satisfaction of doing justice to

his

his labours. M. David of the Royal Academy of Sciences of Rouen, an excellent anatomiſt, and well known to the public by his various works, has juſt publiſhed a treatiſe on the increment of the fœtus, where he inclines to adopt my principles, and eſtabliſh them by new facts. On that ſubject, he has communicated information which gives me ſo much the greater pleaſure, as it ſupports the opinion of M. Heriſſant, and as I had not indulged ſuch hopes of approaching ſo near the great work of Nature. But this treatiſe I have not yet procured.

Increment ſuppoſes nutrition, and this the incorporation of numerous heterogeneous ſub ſtnces, terrene, oleaginous, ſaline, and others. All theſe are aſſimilated to the organic texture by its own interpoſition. It is the chief inſtrument of aſſimilation, conſequently of the almoſt infinite partial ſecretions which are operated in all the organic points of this texture, and on which increment and ſolidity finally depend. At firſt the blood of a chicken is colourleſs; it next becomes white, then yellow, and laſtly red. It colours and thickens only by the introduction of terrene aliments; therefore it ſhould loſe the primitive tranſparency in proportion as the animal grows. The gradual incorporation of foreign particles ſhould obſtruct the pores more, and ſhut the avenues to light.

What

What happens ſo much at large in the chicken probably paſſes in infinite miniature among our infuſion animalcula. They feed; they expand by nutrition; and the more nutriment they take, the more is their tranſparency leſſened, but it is never entirely deſtroyed. Their delicate veſſels do not admit particles ſufficiently groſs or homogeneous to accompliſh this. Thoſe aſſimilated are proportioned to its extreme fineneſs (1).

While animalcula of the lower orders remain in their original ſtate of germs, their tranſparency is probably ſo complete that light or heat traverſes them without impreſſion. It may be poſſible that the germs are ſo ſmall as to admit only one or two rays of light. But when expanſion commences, they begin to aſſimilate foreign particles with themſelves. The aſſociation of theſe with the elements of the texture tends more or leſs to diminiſh the tranſparency, conſequently to allow more influence to light or heat. It is nearly the ſame as with air, which, being condenſed

(1) The parts of many animals are never expanded by nutrition. There is hardly an inſect known that grows after its laſt metamorphoſis; and if any do ſo, their increment is too ſudden to ariſe from nutrition. A caterpillar feeds voraciouſly on leaves: Its ſize rapidly increaſes: it changes to a butterfly, which lives on the pureſt nectar of flowers; but increment is at an end. —T.

condenſed and loaded with foreign atoms, becomes ſuſceptible of a greater degree of heat by the influence of light or fire.

In this way, my eſteemed friend, do I conceive the germs of the lower animalcula can withſtand the heat of ebullition, and how the animalcula themſelves periſh at 106° and 108°. Nutriment gradually changes the original conſtitution of animalcula, and incorporation of a quantity of foreign matter gives an efficacy to heat which it could not previouſly have. The nutritious atoms becoming conſtituent parts of the minute organic whole by incorporation, heat cannot but have ſuch influence upon them as, to a certain degree, to affect the vital functions. Perhaps animalcula may exiſt in the atmoſphere ſo perfectly diaphanous, and feed on ſubſtances ſo rare, that they may paſs through the fire without periſhing. Theſe would be ſylphs and gnomes a little leſs fabulous than thoſe of poets: Your animalcula of the loweſt claſs are gnomes yet more real.

All this has much analogy with what I have ſaid, in the firſt five parts of the Palingeneſie, on the little *ethereal* body which I have conſidered the real *abode of the ſoul*, and which, according to my ideas, has been rendered capable of triumphing over the effects of time and the elements to preſerve the *perſonality* of the animal, and one day reſtore it under another form. It is ſingular that direct experiment has already brought animalcula into view which reſiſt the heat of ebullition. This is at leaſt a ſmall preſumption in favour of my hypotheſis on the future reſtitution of animals. It may remain to expoſe the germs of theſe animalcula, or rather the ſubſtances where they lodge, to fire. Let us deſpair of nothing; but let us inveſtigate Nature farther. You underſtand how to interrogate her as ſhe ought to be, and to you will ſhe make her moſt favourable reſponſes. You will not preſume, apparently, that fire may be the natural element of a race of animalcula, much leſs will you think with M. Robinet that it is only an aggregate of animalcula. You limit yourſelf with inquiring, by judicious experiments, what degree of heat animalcula, in the ſtate of germs, can ſupport without deſtruction.

I greatly approve of your extending theſe experiments to the eggs of inſects about which we know ſo little (1). Thoſe of many butterflies, beetles, or flies, may afford ample ſcope for obtaining evidence fit to ſtimulate the ſcrutinizer of Nature. Reaumur's experiments, ſatisfactorily proving that one may prolong the duration of the embryo's life in the egg, are directly

(1) Theſe have been detailed.

ly the inverſe of thoſe you propoſe on the eggs of inſects. The life of the embryo in the egg evidently differs much from that of the animal developed. Therefore it is a law of the animal economy, that the germ of the embryo can ſupport ſituations or accidents deſtructive of the animal evolved. Were we able to compare theſe two lives exactly, much light would be thrown on the experiments which we ſhould make of this nature, and the effects that would enſue.

But how far is our phyſiological knowledge from extending to that! We know the germ or embryo only by ſome of the moſt prominent, I ſhould ſay the groſſeſt features; and we are ignorant of its principal connection with the various parts ſurrounding it in the egg. The little of the whole that we know only reſpects the chicken. How can we hope that the light of obſervation will one day penetrate the ſecret organization of an inſect's egg infinitely minute when compared with that of a hen? What a profound abyſs to us is the egg of an inſect; but what ſubject in natural hiſtory, philoſophy, or metaphyſics, does not preſent ſimilar abyſſes? The ſage will ſelect the leaſt profound, and conſider them with modeſt and reſpectful reſerve.

I have juſt re-peruſed the eighth and ninth chapters of your firſt Diſſertation on Animalcula, which appeared 1765. In page 117, you reason plausibly, without suspecting that experiment made by yourself would one day be adverse to it. You undertake to refute the author of *Lettres a un Americain*, who thinks, without any direct proof, that heat of great intensity is requisite for the production of certain animalcula, and express yourself thus. 'It is enough that we view the 'fine and delicate texture composing the inter- 'nal mechanism of the animal, which may be 'considered an assemblage of the slenderest silk- 'en filaments, and we shall easily be sensible 'what ravages and destruction an irregular intes- 'tine heat would cause among them: If we see 'this heat, in warming the fluid, kill adult and 'vigorous animalcula, so much the more will it 'affect them in their integuments while yet weak 'and tender.' Nature itself evinces, that it is the germs or integuments of animals which support the heat of boiling water, whereas the animalcula themselves perish at 106° and 108°. This instance, like many others, proves how the most specious arguments may become deceitful in philosophy, and that we should distrust conclusions merely rational in subjects of natural history and philosophy. Thirty years ago, if one had asked the most acute philosopher, or the most expert anatomist, Whether he conceived that an animal could be multiplied by cutting it

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in pieces, or even by mincing it down? Do you think, my worthy correſpondent, that this philoſopher, or anatomiſt, would not have advanced an hundred good reaſons to prove the impoſſibility of the fact; and would any anatomiſt have been found attempting to combat his reaſons? What would have happened had we aſked that anatomiſt, If he thought an animal could exiſt which might be turned outſide-in, like a glove, without ceaſing to live, to grow, and to multiply? Nothing is fitter than ſuch diſcoveries to inſpire us with diffidence of our own opinions, and to create the moſt exalted ideas of the infinite variety of Nature's operations. This reflection, equally moral as logical, has often occupied a place in my writings. I have ſtrongly endeavoured to inculcate it into the minds of my readers. While compoſing parts 12 and 13 of *La Palingeneſie*, it engroſſed me much; and I attempted to point out the bounds and the natural imperfections of our knowledge. All that is ſaid on this fertile ſubject is little in compariſon to what a writer more enlightened and intelligent than me could have ſaid. But I have mentioned enough for my principal object, and for the reflection of thoſe readers who are capable of ſuch meditations.

Perhaps you know, that the Abbé de Lignac was the author of theſe *Lettres a un Americain*,

 whom

whom you refute from a criticism on M. Needham. It appears that M. de Reaumur valued this work of his friend the Abbé; he sent me a copy with many requests to read it. His method does not please me: His criticism on M. Needham and de Buffon is loose and injudicious. Several points, however, are well founded; but he is neither philosopher nor observer enough to treat such matters skilfully. His theological sentiments often seem to spoil his philosophical. Some years afterwards, this same Abbé de Lignac undertook the refutation of some metaphysical works, and particularly the *Essai de Psychologie*, whose author you may one day enquire for of me. I can well assure you that the refuter has never understood this essay: all his objections are false; he censures the author every where; and what is still more strange, he puts a confession of faith in his mouth, the most absurd and opposite to the author's real sentiments. He prints it in Italic characters, as if the psychologist's own expressions. The title of this refutation, *Le temoignage du sens intime et de l'experience, opposé a la foi profane et ridicule des Fatalistes modernes*, 3 *tom.* 12. So he classes the psychologist among the modern fatalists, whose creed is profane and ridiculous. I have not understood that this absurd psychologist ever thought of answering his keen and inconsiderate adversary. Doubtless he has conceived that

that his time would have been misspent; and that his *Essai* would give no offence to those who could penetrate into the abstract points which serve as its basis. What could he have replied to a critic who forgot himself so much, as in some measure to become an accomplice of the detestable parricide Damiens? Those acquainted with the psychologist, knew that he has fully forgiven the monstrous errors of his critic. I also know it. How odious is it to attempt refuting an author by attacking consequences which may have any latitude according to the will of the critic! (1).

(1) Oftener than once I have felt myself inclined to reproach M. de Bonnet with some of his ablest metaphysical opinions being adopted from an *Essai de Psychologie*, by an anonymous author. His keen defence of the work, his very partial expressions in that defence, the *Psychologie* itself, altogether induce me to suppose him the author: Although I conceive the whole, I cannot explain myself further. But my readers may be assured, that the author of the *Psychologie* will not revenge the plagiaries with which some persons have reproached the Genevese naturalist, because, to my certain knowledge, he did not commit any; and if he had, he would have openly acknowledged them. I also know, that he is very far from adopting all the ideas in the *Essai de Psychologie*. He has even impugned

I return to the original transparency of organised beings, concerning which your lower orders of animalcula have given me scope for reflection. In my *Considerations sur les Corps Organisés, Part* 1. *ch.* 9. you have seen the accurate discoveries of Haller, and the different consequences that seem to flow directly from them. These discoveries, which have gone far to perfect our knowledge of generation, form a series of facts, which I have ranged in a certain order, to present them with more precision and regularity to the mind. The second fact is thus expressed:—'The solid 'parts of the chicken are at first fluid: the fluid 'gradually inspissates and becomes a jelly: the 'bones themselves successively pass through the 'fluid and gelatinous state. On the seventh day 'of incubation, the cartilages are still gelatinous: 'on the eighth, the brain is but a transparent 'water, and is undoubtedly organized. Meantime the fœtus already moves its members. 'The vessels having become larger, admit gummy 'albuminous molecules, which are attracted towards them. The more the proximity of the 'element

impugned some, and laments his not having extended his criticism on the most important topics in the work.—A.

M. Bonnet was indeed the author of this anonymous work; he afterwards acknowledged it; and republished it in the collection of all his works in 9 vols, 4to.—T 'element encreafes, the greater force does the at-'tractive power acquire. The organifed fluid is 'thus conducted to mucofity by degrees: it be-'comes a membrane, a cartilage, a bone, by im-'perceptible fhades, without the interpofition of 'any new part.' There is ftill the third fact, which fo well eftablifhes the primordial tranfparency of the folid parts of the germ. 'It is only 'on the fixth day that the lungs are vifible; then 'they are but ten hundredth parts of an inch in 'length. If it was not for the tranfparency, they 'would have been vifible at four of thefe hun-'dred parts. The liver is larger ftill when it be-'gins to appear: it is on account of the tranf-'parency alone that it is not vifible fooner. From 'mucous tranfparency to whitenefs there is only 'one degree, which fimple evaporation fuffices to 'produce.—White is therefore the original colour 'of the animal, and mucous tranfparency feems 'to conftitute its original ftate.' You fee what I then faid of the integuments, which are at firft fo tranfparent that the external parts which they inveft appear perfectly naked. Therefore it feems fufficiently evident, that fluidity and tranfparency conftitute the firft ftate of an animal. In different paffages, I have taken care to obferve, that this fluidity is but a fimple appearance, and only marks the extreme delicacy, or the wonderful finenefs of a texture already organized. Now, if if the ſolids of the chicken are originally ſo delicate and tranſparent, how much more ought they to be ſo in infuſion animalcula, which, when fully developed, are but infinitely minute drops of tranſparent jelly? Thus, to guard organiſed beings againſt impreſſions of the elements, no more is neceſſary than encreaſed minuteneſs and tranſparency. Thoſe animalcula of the lower claſſes, ſo extremely minute, and which, according to you, are as *ants to whales*, are perhaps themſelves as whales compared to many other animalcula. It may be ſo ſmall that our beſt microſcopes can never bring them into view. Probably we ſhall always be ignorant of the laſt terms into which organized matter can be divided. Somewhere I have ſaid, the confines of the microſcopic world alone are diſcovered.

VII. You were naturally induced, my worthy correſpondent, to make the ſame reſearches on ſeeds expoſed to heat as you had made on animalcula. I was very earneſt that you ſhould attempt ſimilar compariſons between the two kingdoms, which I have before mentioned. Your letter preſents intereſting reſults, which ſhall occupy me a few minutes.

I am not ſurpriſed that buck-wheat and maize germinated in veſſels hermetically ſealed, or that their vegetation ſoon ceaſed. The little air included would ſerve for the firſt expanſion of the germ,

germ, but would not be enough for its farther developement. So ſmall an atmoſphere muſt ſoon be ſurcharged with vapours and exhalations injurious to the young plants. Every thing that vegetates requires renewal of the circumambient air from time to time. The want of it is more ſenſibly felt by vegetables of the higher claſſes than by thoſe of the loweſt. Theſe, I ſhould conceive, would vegetate better in veſſels hermetically ſealed than legumes, and more eſpecially ligneous plants. The higher a vegetable ſtands in the ſcale of organization, and the more complicated its life, it depends on a greater number of conditions, and conditions more diverſified.

VIII. From your firſt experiments on ſeeds expoſed to the action of boiling water in veſſels hermetically ſealed, it follows, that two minutes boiling does not prevent germination; but it is otherwiſe where they have been expoſed two minutes and a half. It is very remarkable that the difference of half a minute ſhould prevent germination: and this fact tends to prove how much the duration of an experiment affects the animal oeconomy.

In this reſpect, you have not obſerved any difference between ſeeds in open veſſels and thoſe hermetically ſealed: Neither of them exhibited any ſign of germination when boiling was protracted above two minutes; which ſhould have happened after you had aſcertained that ſeeds germinate in

veſſels

vessels hermetically sealed. Here, then, the hermetic seal was a matter of indifference.

You remark, that the period of germination is proportioned to the duration of the experiment. The shorter it is, the more immediate the germination: a consequence which it was easy to foresee. As extreme heat is unfavourable to vegetation, it should naturally be retarded in the seeds longer exposed.

You also saw many more plants germinate of those in open vessels than in vessels hermetically sealed. Both were indeed exposed to the same heat; but the air in the open vessel could be renewed. The plants there should on that account have continued vegetating, while their vegetation soon ceased in the close vessels, which in fact you have seen.

IX. From all your experiments, a general and comparative result is deduced. 'It is not with 'seeds as with animalcula. The production of 'these is more immediate, in proportion as the 'heat is more intense: with the others it is pre-'cisely the reverse.' The vegetable organic system differs greatly from the animal. The laws of the one are not those of the other. The principle of the motion of the fluids is not the same in both: they are not nourished in the same manner; nor are their origin and developement subject to the same restrictions. It is true, we discover

discover many analogies between the animal and the vegetable. This is a subject which has employed me, *p.* 10. *Contemplation de la Nature.* But, amidst all these resemblances, how many dissimilarities are there? The time is not yet arrived when we shall be able to extend experiment as far as it may go. Nor have experiments and observations hitherto been sufficiently numerous and diversified. They have been made but for a day; yet what an assemblage of unexpected facts have been obtained! Still are we ignorant of the character essentially distinguishing an animal from a vegetable (1). This interesting subject

(1) One of the most modern physiologists, distinguished by his philosophical researches, has made a long comparison between plants and animals, which I shall here abbreviate.

'The difference between the solid parts of vegetables 'and the bones of animals is immediately evident. The 'wood is formed by a layer of bark changing its place. 'The bone by the expansion of a net-work, all the parts 'of which at once expand; and it borrows nothing from 'the surrounding flesh as the wood does from the bark. 'Every year the wood acquires a new ring, and the full 'diameter is preserved until decrepitude; but the bones 'become thinner. Broken bones unite; wood never does. 'The bones are nourished by the surrounding parts; the 'wood is an assemblage of lymphatics providing sap to 'the

jeƈt is diſcuſſed in the laſt chapter, part 10. of the *Contemplation*. ‘ Organiſed matter has re-
‘ ceived

‘ the plant. Marrow exiſts in the bones of an animal ‘ during all its life; the medullary ſubſtance of plants diſ- ‘ appears in a few years. Bones are connected by muſcles; ‘ plants have no moveable articulations.

‘ There is nothing in plants correſponding to the ſenſes ‘ of animals. They have neither ſenſation nor nerves; ‘ and being deſtitute of this principle of motion, as they ‘ are without muſcles, we are neceſſitated to aſcribe the ‘ particular motions ſometimes viſible to mechanical ‘ cauſes.

‘ The courſe of the vegetable fluids is very different ‘ from the animal: there is no circulation, properly ſo ‘ called, in plants. The juices imbibed by the roots aſcend ‘ and join thoſe imbibed by the leaves: the riſing fluids ‘ do not reſemble the deſcending. There are no valves in ‘ the veſſels of plants.

‘ Animals have a principle which is the origin of their ‘ motions: the heart is the ſpring of the machine, by im- ‘ pelling and preſerving the impulſe of their fluids. Plants ‘ have no organ ſimilar, or which appears to ſupply its ‘ place. We cannot be certain that all their veſſels are ‘ tubulous. Some, as the utricles, are involved in each ‘ other; and thoſe which raiſe the ſap do not reſemble ‘ thoſe that convey it back to the lower parts. Plants ‘ have ſeveral ſpiral veſſels; animals have none ſimilar: ‘ and if inſects have tracheæ, certainly they are very dif-
‘ ferent

'ceived almoſt an infinite number of modifica- 'tions, and all are as the ſhades of priſmatic co- 'lours.

'ferent from thoſe of plants, ſince in the one they are air 'veſſels, and in the other, veſſels for the juices.

'The traces of irritability only appear in certain parts 'of ſome plants, and in particular circumſtances: We 'muſt explain their motions by a peculiar mechaniſm, re- 'gulated by the encreaſe of the fibre, and eſpecially of 'certain parts, as well as by the changes of the fluids in 'their veſſels.

'Vegetables do not reſpire air like animals: If the leaves 'were lungs, moſt plants would reſpire none during win- 'ter; and if the tracheæ were air veſſels, they would not 'ſupply the place of the leaves, ſince none are in the bark. 'By the action of light on their leaves, plants decompoſe 'carbonic acid and give out oxygen gas, which is very 'different from the carbonic acid formed and diſcharged 'by animals, for the greateſt part of that formed by plants 'in cloſe veſſels is a product from their alteration.

'In animals, reſpiration is a ſource of heat: the decom- 'poſition of oxygen gas provides the blood with its calo- 'ric, and diſengages the ſuperfluous carbon, by combin- 'ing with it to form carbonic acid. But in plants, the 'conſequence is different: the carbonic acid which de- 'compoſes by light abandons its oxygen to the caloric, 'with which it forms the oxygen gas that eſcapes; thus 'depriving the plant of that ſource of heat which it might 'derive from light. Plants indeed form carbonic acid by 'the 'lours. We make points, we draw lines on the 'figure, and this we call a diviſion into claſſes 'and

'the combination of their carbon with the oxygen gas of 'the air; but the quantity is very ſmall when the plant is 'in health, ſo that the caloric diſengaged cannot be great, 'and it is diſengaged at the exterior of the plant.

'Moſt animals have a mouth for their aliments. Plants 'have as many as fibres and pores. There is no reſem-'blance between the food. The ejecta of plants are ge-'nerally gaſes, which muſt be collected and confined be-'fore becoming ſenſible. Animals feed only at ſtated 'periods; vegetables continually.

'Moſt animals poſſeſs the faculty of emitting ſounds; 'vegetables do not.

'Can the nocturnal poſition of plants be called ſleep? 'Is it repoſe or watching to have the leaves turned to one 'ſide more than to another, without any apparent relaxa-'tion? How can we ſuppoſe that the *tragopogon* watches 'three or four hours, when the flowers are open? It is 'evident, if the leaves changed their poſition their ſtalks 'would not be pendant; they are conſtantly cloſe, and 'the leaves float no more than the flowers.

'The phenomena of vegetable generation have no 'greater analogy to thoſe of animals. Hermaphrodites 'are rare in the animal kingdom; moſt plants are ſtrict-'ly hermaphrodites. Very few want viſible ſexual parts, 'but the number of animals deprived of them is very 'great.

'Animals

'and genera. We obſerve the predominant co-
'lours, while the delicate ſhades eſcape our no-
'tice.

'Animals reproduce only after attaining their perfect 'ſtate: many plants multiply that are far from it, and 'are fecundated though they want leaves. In animals, 'the generative organs remain after fecundation, but 'they fall from plants, which have none the greateſt part 'of their exiſtence: then they are repaired, and in a dif-'ferent place from their original ſituation.

'Animals, that reproduce by the concourſe of ſexes, 'propagate in no other manner, whereas plants multiply 'by ſhoots and buds, ſo that each part of a plant can fur-'niſh another complete individual. Finally, the genera-'tive organ of many becomes the fruit.

'The eggs of animals are full of a particular fluid; 'but ſeeds of a ſolid farinaceous ſubſtance.

'The number of germs is infinitely greater in the ve-'getable than in the animal kingdom; few animals re-'produce by ſhoots.

'A tree, cut down during winter, will ſometimes live 'till the following autumn, and be covered with leaves 'and branches.

'What animal may be ingrafted on another, as a peach 'tree on an almond? The graft of a polypus is different, 'for it is the ſame animal growing upon itſelf.

'Animals grow by a proportional expanſion of all their 'parts, but leaves, flowers, and fruit, conſtantly remain 'the ſame after evolution.

'tice. Plants and animals, therefore, are only 'modifications of organized matter: all partici- 'pate

'Plants loſe part of their organs during winter: ſtill 'they live and are preparing to reſume their original 'ſtate in ſpring. This cannot be compared to the ſleep 'of marmots, for in their organs, there is no apparent 'alteration.

'*Berthollet* has properly characteriſed animal and vege- 'table ſubſtances by ſhewing that the cauſtic alkali, which 'diſſolves the former, does not act on the latter; and that 'animal ſubſtances, with nitrous acid, gave out much 'azote, which, in diſtillation, produced ammoniac, by 'combining with the hydrogen of water: while vegetables 'afforded an acid by diſtillation, and an ardent ſpirit by 'fermentation, which has no reſemblance to ammoniac.

'The ſingular work of *Girod Chantram* is known to me 'only by an extract. Like him, I had obſerved the glo- 'bules of certain confervae of different ſpherical figures, 'but I found no animality. Theſe reſearches were finiſhed 'long ago. I communicated them to my friend *Spallan-* '*zani*, and requeſted him to repeat ſome of my obſerva- 'tions. He did ſo; but he could diſcover no kind of ani- 'mation in theſe plants. The chemical analyſis by *Tingry* 'of the *Conferva Bulloſa*, and my own of the green ſubſtance 'forming in veſſels of water expoſed to light, produced 'nothing more relative to animality than the analyſis of 'other plants. Even admitting the animality of theſe 'Cryptogamia, I cannot ſee how it would prove that 'of other vegetables. *Senebier Phyſiologie Vegetale, tom.* 5. p. '188.'

'pate of the ſame eſſence, but the diſtinctive at-'tribute is unknown.' The animal has derived its name from that ſoul which is conſidered the ſecret principle of its actions; and its exiſtence is judged by the analogy ſeen between the ſoul of an animal and the ſoul of man. We ſhould endeavour to diſcover the precise decree of organization when the capacity to be animated terminates; or, which is the ſame thing, at what degree a ſoul cannot be united, to compoſe that kind of identity which we denominate a mixed being. For, if in the quality of a phyſical being, the animal differs eſſentially from the vegetable, this ought to ariſe from that part of its organization which conſtitutes its phyſical animality. The nerves are the part which apparently render an animal animated. By their offices is the mind ſentient and active: they are intermediate between the ſoul and the body. By means of them, the

mind

'188.' The reputation of M. Senebier is well known, and his opinion muſt have great influence on naturaliſts. His arguments againſt the animation of plants will be found more at large in the original work. Some of them however may be liable to objection, although the reſt are well founded. Hooper's *Oeconomy of Plants*, and Darwin's *Phytologia*, contain many principles oppoſite to *M. Senebier's*. Both works merit high conſideration, as they deſcend minutely into vegetable properties. The preciſe term when animality terminates is yet uncertain, nor will general rules to find it be eaſily given.—T.

mind conceives the impreſſions of objects; by them does it act on the members; and, by the members, on every different thing. We cannot conceive that a ſoul ſhould be united to a portion of organic matter through which no impreſſion could be tranſmitted; and it can be eaſily imagined, that mind may reſide in every portion of organized matter provided with nerves or any thing analogous. Real nerves have not hitherto been diſcovered in the vegetable; but it is no reaſon to ſuppoſe them abſolutely deſtitute of either nerves or ſomething analogous. You have read what is ſaid, *Part* 10. *ch.* 30. 31. *Contemplation de la Nature*; and in *Part* 4. *de la Palingeneſie*; to which I have nothing to add (1).

X. If

(1) Theſe reflections, on the diſtinctions between the animal and vegetable, cannot be more profound or logical. By means of the difference, the author apparently means to inſinuate, that we ought not to be ſurpriſed that heat has ſuch oppoſite or various effects upon animals and vegetables. But this difference may be ſatisfactorily explained, without recurring to the diſſimilarity between the two kingdoms, which I told M. Bonnet in the following paragraph of a letter: 'When I obſerved, that *the* '*ſame reſults did not proceed from my experiments on ſeeds as* '*on animalcula; that the longer the action of heat is continued on* '*the latter, their origin is the more immediate, and the number* '*greater; and that the reverſe happened to the former in the* '*ſame*

X. If the conjectures I have already hazarded have any probability, it ought to appear more ſurpriſing, that ſeeds ſupport the heat of ſand at 185 and 212°, without loſing the germinating power, than that the germs of animalcula may

 expand

'*ſame ſituation*, I only related a fact, which may be ac-' counted for without recurring to the difference between ' animals and vegetables. The continued action of heat ' obſtructs the expanſion of vegetable ſeeds and infuſion ' animalcula. This my experiments have proved. But ' if it ſhall happen, that after a certain period the infuſions ' are full of animalcula, it is evident they do not owe ' their exiſtence to germs, which have been expoſed to ' heat, but to ſome precipitated into the infuſion after it ' has cooled. Therefore animalcula are produced by new ' germs: but ſeeds not having the ſame advantage, it is ' no wonder that thoſe which have ſuffered the action of ' heat do not germinate. Were they ſuſpended in the air, ' as animalcular germs probably are, plants would un-' doubtedly be produced as well as animalcula.'

M. Bonnet is now of my opinion. "I ſubſcribe," he replies, "to your reflections on the difference between "vegetable ſeeds and animalcula which have been expoſed "to boiling heat. You have truly good reaſon to ſay, "the higher claſs of animalcula, which are produced af-"ter boiling the infuſions, do not come from germs "which have endured ſuch violent heat, but from thoſe "that have fallen from the air after the infuſion has "cooled."

expand after enduring the heat of ebullition. As these germs seem incomparably more delicate than those included by seeds, our surprise must augment in proportion to the increasing delicacy of the organic wholes, on which these experiments are made; yet, in my opinion, this excessive delicacy of texture is the very thing that will protect them from the action of heat. The germ of a bean is large in comparison to the germ of an animalcula; thus it should suffer much more from heat; and, as more extensive portions are presented to this element, its action should be more powerful. However, we are not sufficiently acquainted with what constitutes life in the germ of an animalcula, or the embryo of a plant, to be enabled to judge correctly of the matter. By your experiments, and those of M. Duhamel, we learn that seeds do not lose the germinating faculty at 212°, and even 234°, of Fahrenheit's thermometer. Senegal is not without vegetables: there the thermometer often stands at 122°, or 133°, in the shade; which will make the direct heat of the sun 212° or 232°, according to the experiments of President Bon of the Montpelier Society (1). Thus, there are vegetables produced

(1) These experiments were fallacious, because they were made without the necessary precautions. In summer,

produced by nature there, at a degree of heat far ſuperior to that of all other climates. Your ſand, continually heated to 212°, was a Senegal in miniature, where heat was more conſtant, and where the natural viciſſitudes of day and night, which occur in the hotteſt climates, to the great relief of plants, were not admitted. We muſt agree that this heat of 212°, to which different ſeeds were expoſed, was unfavourable, ſince only one germinated, while thouſands of animalcula develope at boiling heat: a fact which excites ſome reflections on vegetation (1).

Whatever may be the ſecret mechaniſm of ve-

 getation,

mer, the difference of the heat in the ſun and the ſhade is only 2 or 3°. Note by M. Bonnet to the collection of his letters.—T.

(1) This is certainly true of the firſt reſult ſent to M. Bonnet. A ſingle bean was the only ſeed that germinated after ſuſtaining 212°. But, ſome others of many that had been expoſed to the ſame degree, alſo vegetated. I may obſerve, that the ſand in which they were heated *did not conſtantly preſerve* 212° *of heat*, as M. Bonnet ſuppoſes, probably becauſe in my narrative to him of the experiments, I had not expreſſed myſelf clearly; for I only meant, that the heat was ſucceſſively increaſed to that of boiling water, or 212°, for the ſeeds were then taken from the hot ſand. This obſervation in no meaſure leſſens the excellence of the author's reflections on vegetation.

getation, it is very certain, that its laſt effect is to extend the plant in every way, and to increaſe the ſize. This ſecret I attempted to penetrate, *Part 7. ch. 7. de la Contemplation de la Nature;* and my ſentiments, on a ſubject ſo obſcure, have been a little extended, *Palingeneſie, part* 11. 'The extenſion of the fibre infers that its 'parts can change their relative poſition; that 'they may recede from each other, but within 'limits, and theſe limits are the bounds of incre-'ment.' If we ſuppoſe that the elements of the vegetable fibre are united by a ſort of gluten, which allows them to ſlide more freely on each other, and thus to admit of their ſeparating to a certain degree, we may conceive how the heat of 212°, or 232°, ſhould tend to inſpiſſate or coagulate the glutinous matter, and to diminiſh, or even deſtroy, its motion. 'The animal or vege-'table gluten,' I continue in the *Palingeneſie*, 'is the natural connection of all the parts whe-'ther original or acquired. It merits the great-'eſt attention. Doubtleſs it is the principal ba-'ſis of the aſſimilating or nutritious ſubſtance of 'plants and animals.' Muſt it not be wonderfully fine in the lower orders of animalcula? In proportion as the plantula receives new juices, it diſcharges thoſe ſuperfluous, by ſenſible or inſenſible tranſpiration, which operates by means of the moſt minute ſecretory veſſels, whoſe ac-

tion

tion regulates the ſecretion. For this, a certain heat is required; if too powerful, the quantity evacuated is no longer proportioned to the quantity imbibed, which muſt be prepared in the viſcera. Exceſs of tranſpiration debilitates the plant, it inſpiſſates the fluids, dries and contracts the veſſels, and at length prevents circulation.

The vegetables which perſpire little ſhould undoubtedly ſuffer moſt in experiments ſuch as your's on ſeeds: ſuch, for example, are evergreens. You know the celebrated Hailes has proved that theſe trees perſpire much more than others. Their ſap ſeems more viſcous, therefore has greater tendency to thicken and coagulate by heat.

In *Article* 168, *des Corps Organiſés*, I obſerved that we are yet ignorant of the power ſecretly governing the motions of the ſap. It does not reſide in the leaves alone; the bleeding of the vine affords cogent evidence of it. Branches which had been intentionally dried, and no longer would raiſe coloured liquors as other branches, yet green, and without leaves, raiſed it perfectly: Theſe branches, I ſay, ſufficiently prove, that the courſe of the ſap depends on ſome ſecret motion of the veſſels, which is deſtroyed when they are dried. But 212° or 232° apparently are not enough to effect complete deſiccation of all vegetables; ſome expand in a certain meaſure even at this heat. Theſe curious experiments certainly merit

merit repetition, extenſion, and variation, much more than they have hitherto received; and here you have done well.

Exceſs of heat tends to deſtroy the eſſential qualities in the nutritive juices of the plant; and the alterations thence conſequent are a natural cauſe of its death. Ancient philoſophers would have ſaid, extreme heat took away the radical humidity of the plant; and although the expreſſion is not uſed by the moderns, it is good ſenſe.

XI. Your experiments on mould have excited my attention very much. When the vegetation of theſe minute plants ceaſes, in veſſels hermetically ſealed and expoſed to the action of heat, it is more than probable that the numbers, which appeared on vegetable ſubſtances that had boiled from half an hour to two hours, came from the ambient air. The ſeeds muſt be of ſuch extreme minuteneſs that we cannot wonder if they penetrate wherever air has acceſs. Theſe vegetables, ſo very ſmall, are, in the vegetable kingdom, what animalcula are in the animal. Once, my dear friend, I wrote to you how earneſt I was that ſome one ſhould perfect the botanical microſcope. How many intereſting ſingularities would it not preſent? and how incomplete is it ſtill? We are acquainted with the large and the middle ſized vegetables. The moſt able botaniſts have carefully deſcribed the chief external parts. The

The Anatomy of Plants, by Malpighi and Grew, and Duhamel's *Physique des Arbres*, have in a manner traced the history of herbs and trees. They have communicated much information on the structure and use of flowers, seeds, and fruits; and they have brought the principal viscera of the plant into view. The *Vegetable Statics* of Hales teach us the power of leaves, or their principal province in the mechanism of vegetation. *Les Recherches sur l'usage de Feuilles* have added some facts to the number already known. But the botanical microscope has not made the same progress, because it has not been so much cultivated; and because it requires eyes made on purpose. What we owe on this subject to Hook and Micheli, though precious, is very little in comparison to what we might expect from the assiduous researches of our best observers. Here are the *terræ australes* of the vegetable world, as animalcula are those of the animal. How much may the figure, the nature of life, the manner of nutrition, increment and propagation of microscopic plants, stimulate the curiosity of a naturalist who can observe and reflect! How much does the economy of this part of the vegetable kingdom, so considerable and so little known, differ from the other parts of it! How great is the hazard of error by taking analogy for our guide! Let us judge them by the real and improbable

facts

facts which the different classes of polypi have taught us, and which have created such a reform in our ideas of animality. Perhaps microscopic plants would occasion a similar reform in our ideas of vegetation. I wish a society of botanists would engage in the study of these plants alone. What do I say, '*mould*' only would demand their whole attention. Nothing is better adapted to make us forcibly feel the narrow limits of our corporal and mental abilities than applying to researches on the most minute productions of nature. All the finest faculties seem to vanish at the root of a stalk of mould.

If mould appears sooner, and more abundant, on substances that have boiled long, perhaps it is because boiling prepares them in a manner similar to what we prepare the earth by our labour. Ebullition separates them more, multiplies the surface, opens new pores, forms new channels for admission of the air and the like. Internal changes may also be occasioned in the substances favourable to the generation and expansion of mould. Finally, a certain degree of heat, which boiled substances preserve for a time, may tend to accelerate the germination of mould, and to increase its multiplication (1).

I

(1) When M. Bonnet made these excellent reflections on the botanical microscope, I had not communicated any

thing

I cannot leave mouldineſs without a reflection which I made only a moment before; that one could not make uſe of analogy here. More than once have I had an opportunity of applying the ſame reflection to the polypus. The naturaliſts, who ſtudy microſcopic plants, ſhould therefore be very reſerved in their opinions of the various ſingularities they preſent to view. If real logicians, they will not inſiſt on transferring the ideas applicable to vegetables of the higher claſſes to thoſe of the lower. Nature has not been reſtricted to work always on the ſame models. Theſe have been infinitely varied by the Divine Author. Obſervation alone can diſcover the direction which HIS WISDOM has preſcribed to Nature, relative to the different claſſes of organiſed beings. The bounds to view and review are here; and individuals muſt only be compared with the neareſt ſpecies. Who knows that all mouldineſs actually belongs to the claſs of vegetables? Who knows but there may be ſomething whoſe formation approaches nearer to chryſtallization than evolution? The ſame may be ſaid of many other analogous productions, whoſe properties have not yet been inveſtigated. It is not impoſſible that ſeveral

thing on the ſubject; becauſe only one or two reſults on the plants had been obtained. But I had occaſion afterwards to penetrate the matter deeper, as will appear by my obſervations and experiments on the origin of the plantulæ of mould.

ſeveral of the productions which botaniſts claſs among plants, may approach nearer to the mineral than the vegetable; at leaſt they may be the ſhades between the two. Among theſe plants probably are modes of multiplication and increment that have not the moſt diſtant connection with thoſe of vegetables the beſt known to us.

XII. I am delighted, my dear Malpighi, that you have confirmed M. de Sauſſure's ſingular diſcovery concerning the mode in which many infuſion animalcula propagate, and which I have publiſhed in the ſecond edition of *La Palingeneſie*, p. 426, 427. Though the figures that accompany your letters are only ſimple ſketches, they are ſufficient to ſhow that thoſe animalcula, whoſe propagation you have attended to, are of the claſs of polypi (1). This article has given M. Trembley much pleaſure, and brought to his remembrance the cluſter polypi, which he firſt diſcovered, and deſcribed ſo well in his works, and of which I have ſaid much in my three laſt treatiſes. Wriſberg has alſo ſeen theſe very minute polypi in infuſions, and given good figures of them; but he has not obſerved their mode of multiplication. When one conſiders the different figures we have of ſeminal

(1) My obſervations on the propagation of animalcula were only in their infancy at the time I communicated them to M. Bonnet.

minal vermiculi, he will be induced to believe they approach these minute polypi, if they are not actually polypi. I should have earnestly wished that a laudable scruple had not prevented you from examining the seminal vermiculi of different animals. Your descriptions would have been more exact than any hitherto given, and you would have discovered many peculiarities which escape eyes less practised and less philosophic than your's (1). I can recollect that Mr Needham bestows some reproach that you did not investigate the animalcula on which he dwells with such complaisance. The strange ideas that possessed him during his observations are not very fit to satisfy us of the truth of his observations. I would recommend a more profound and impartial enquiry.

Among the animals that occupy the lower scales of animality, none so numerous or diversified as the polypus are known. The liberal hand of Nature has dispersed them every where. The carpet, thus to speak, the bottom of pools, rivers, lakes, and seas, and they are even found in infusions. No one could have suspected this; nor would we have suspected all the accessory facts which these minute animals have exhibited in

(1) I have endeavoured, as far as in my power, to satisfy the curiosity of my illustrious friend, in the tract on the seminal vermiculi of man and other animals.

in latter times. Did not your illuſtrious countryman, Marſigli, imagine that the hiſtory of corals, corallines, and lithophytes, beſides many other productions, taken from real plants, would one day be the hiſtory of a very minute animal? This is a moſt inſtructive fact to the philoſopher: it preſents the progreſs of the human mind, in the ſecret of Nature's truths. I remarked, *page* 393, *Tom.* 1. *de la Palingeneſie*; 'One diſcovery begets another. The intellec-'tual world, as well as the phyſical world, has 'its generations, and neither are more real ge-'nerations than the others. By attention, the 'mind diſcovers pre-exiſting ideas, to uſe the ex-'preſſion, in other ideas. By reflection, the poſ-'ſibility of a fact is deduced from another fact 'actually exiſting; and by experiment, the poſ-'ſibility is realiſed. Thus when an acute perſon 'obtains a fact, he obtains the firſt link of a 'chain on which other links, alſo facts, depend.' This is the generation of ideas, which Encyclopedical Dictionariſts ought to place before us, but never do. It would require much more art to explain ſuch intellectual generations than what is employed in compoſing theſe huge compilations. A good hiſtory of the human mind would be that of the generation of its ideas of every kind; and it would be the baſis of that *Hiſtoire de l'Attention* which I formerly preſented, ed, and is mentioned, § 279, *de l'Effai de Analytique fur l'Ame*, and *Art.* 20, *de l'Analyfe Abregée, tom* 1. *Palingenefie.* 'We want a book,' I there obferved, 'and that book would be the 'moft ufeful that human genius could invent; it 'would be a *Hiftoire de l'Attention.* If well 'compofed, and maturely ftudied, it would over-'turn all logic; that is, it would be logic reftor-'ed to action.'

In my three laft writings, I have infifted much on the important leffon afforded by polypi, concerning the imaginary general rules of analogy. In *chap.* 16. *part* 8. *de la Contemplation*, I faid, 'This is not the time to form general rules, to ar-'range nature.... to raife a ftructure which fu-'ture ages, more learned and more philofophical, 'will even hefitate to project. When we fcarce-'ly knew the animal, we undertook to define it: 'now, when a little better acquainted with it, 'fhall we prefume to fuppofe we know it com-'pletely?.... How many animals ftill more extra-'ordinary than the polypus may exift, and may 'confound all our reafoning if we fhould happen 'to difcover them! We fhall then have to in-'vent a new language to defcribe our obferva-'tions. Polypi are the extremities of a new 'univerfe which one day will have its Columbus 'and Vefpucius. Is it to be conceived that the 'interior of a country has been penetrated with 'only a diſtant view of its confines? More ſub-'lime ideas of nature will be formed by conſi-'dering her as an immenſe whole, and being 'convinced that our diſcoveries are but of the 'moſt minute portion of what ſhe includes. Our 'aſtoniſhment will increaſe, but we ſhall obſerve; 'we ſhall amaſs new facts, connect them if poſ-'ſible, and attend correctly to every thing.'

It ſeemed proper to recur to theſe logical reflections, *part* 10, *de la Palingeneſie*. There I attempted to retrace and unfold my principles further on organic preformations. Four kinds were eſtabliſhed; and that my reader might not ſuppoſe that I conſidered theſe genera univerſal, I added, 'One ought not to infer from this, 'that all ſmall animals are at firſt envelop-'ed in one or more integuments, or in eggs. 'It would be deducing too general a conſequence 'from ſpecific facts. The Author of nature has 'ſpread ſuch a vaſt variety throughout, that we 'cannot define general concluſions too well. 'How many new and unforeſeen facts have de-'ſtroyed concluſions which a ſtrict logician would 'not acknowledge!—The cluſter polypus is an-'other exception much more ſingular; and is an 'additional conviction of the uncertainty, not to 'ſay the falſity, of our general concluſions. In-'fuſion animalcula will be other exceptions; and 'very probably, what was taken for eggs in them

'is

'is not ſo.—The ideas of animality, applicable
'to the higher claſſes, are too confidently tranſ-
'ferred to the lower.—How abſurd would it
'be to confine nature in the limited circle of our
'feeble conceptions! I avow then, that all I
'have ſaid on the different kinds of organic pre-
'formations, chiefly regards the ſpecies beſt
'known to us, or on which we have been able
'to make exact and connected obſervations. I
'confeſs myſelf ignorant of the laws which regu-
'late the evolutions of thoſe numerous micro-
'ſcopic beings, whoſe exiſtence, and nothing
'more, is diſcovered by our moſt powerful mag-
'nifiers, and which belong to another world,
'that may be called the world of *inviſibles.*'

Theſe paſſages are tranſcribed, my dear friend, becauſe we cannot fortify ourſelves, and our fellow naturaliſts, too well againſt the ſeductions of analogical concluſions. If it is ſufficiently evident, that we cannot refer the propagation of polypi by natural diviſion to any known kind of organic preformation, ſurely we ought to renounce the proſpect of explaining their generation by the generations exhibited by other animals. If we are unable to divine this new mode of multiplication, we ought thence to conclude that many others may exiſt in the animal kingdom, of which we can form no idea.

 Therefore

Therefore it appears, that abiding by the facts beſt known, and aided by ſound philoſophy, we have reaſon to affirm, that *organiſed wholes* are not daily formed by a ſort of ſecret mechaniſm, or that they are not preſently generated. This we may admit, at leaſt it is very probable, that they have been preformed from the firſt: but we ſhould beware of preſuming that we know every mode in which the AUTHOR of nature has originally preformed the multitudes of organiſed beings peopling our planet. The propagation of the cluſter polypus, and other polypi ſimilar, is far remote from the generations moſt familiar to us; nevertheleſs, ſtrange as it appears, it has a conſtant regularity and uniformity, which is not fallacious, and muſt convince us that it is ſubjected to fixed laws, as every other mode of propagation, which new reſearches will demonſtrate more and more. If all the productions of nature are connected by a continued chain, it neceſſarily follows, that the generation of polypi is attached to thoſe of other animals by certain links which we are not yet wiſe enough to diſcover. The whole of theſe generations ſhould poſſeſs ſome common or very general character, which is as a centre to which all converge. This centre probably conceals a general preformation. If it exiſted in animals forming mechanically, they would not converge to this

common

common centre; they would be diſtinguiſhed from all the reſt by a ſpecific character, or one which would affect the eſſence of animality.

I have remarked, if man, and the animals which we eſteem the moſt perfect, propagated after the manner of polypi and puçerons; in a word, if we had never ſeen animals in copulation, could we have ſuſpected that generation was effected by the concourſe of the ſexes? Could we have imagined that, for the production of an individual, the concourſe of two individuals of the ſame ſpecies was neceſſary? But from the propagation of all the large animals by copulation, it had been precipitately concluded, that it was a general law for the propagation of every ſpecies. Becauſe it had been obſerved, that all known animals grew, after iſſuing from the womb of the mother, with the ſame precipitation, it was concluded that it muſt be ſo with the whole animal kingdom. The *ſpider-fly* has proved it falſe (1). Theſe ſtriking examples, and others which I have elſewhere cited, are well adapted to perfect the logic of the naturaliſt, and to make him reſerved in his opinion on the ways of Nature. I am ſo much ſatisfied with the principles of this logic, that it would not ſurpriſe me if, in our ſeas, there was one day diſcovered a ſpecies of whales or

(1) Contemplation de la Nature, part 9. ch. 3. 7. Corps Organiſés, Art. 323. 324.

marine monſters, propagating in a manner altogether different from that hitherto ſeen in the ſame claſs of large animals. The ſea probably contains prodigies of this kind, which would no leſs aſtoniſh naturaliſts than polypi have done. I repeat, that natural hiſtory, treated by a ſkilful philoſopher, will always prove the moſt convincing reaſoning.

All our phyſical knowledge chiefly reſts on analogy; where this is wanting, or too imperfect, we ſhould diſtruſt explications or hypotheſes founded upon it. How many different claſſes of polypi have little analogy with other animals? I cannot but applaud the prudent reſerve of my illuſtrious friend M. Trembley, who was unwilling to hazard an explanation of the polypus, though he diſcovered it, and has ſo ably deſcribed the figure, habits, regeneration, and propagation. His excellent memoirs are a real logical phenomenon of this deſcription; for how is it poſſible continually to deſcribe wonders without yielding to the temptation of accounting for them? I ſhould have to reproach myſelf with not following his example, had I not taken the greateſt precautions to prevent my readers from confounding my trivial hypotheſis with facts. I have not even hazarded an explanation of the arm polypus, excepting in thoſe things that ſeem to approximate them to vegetables. An error remarked

marked by M. Trembley in one of my explanations muſt be pointed out. It forms part of a long letter which I wrote you, 1 November 1766, on animal reproductions, which at my deſire you have communicated to the public in one of your notes on *la Contemplation de la Nature*; but you beſtow too much praiſe on my ſincere avowal of the error; for in one as fallible as me, there is little merit in publicly acknowledging he has been deceived. I ſaid ſo at the end of the Preface to *Conſiderations ſur les Corps Organiſés*. One '*I have erred' is worth more than an hundred ingenious replies.* You have alſo ſeen that I guarded againſt an attempt to explain the cluſter polypus. On theſe in particular, and on polypi in general, I have publiſhed *Philoſophical Conſiderations*, where materials are collected for logic to be employed by naturaliſts. Theſe occupy the three laſt chapters, Part 6. of the *Contemplation.* The intelligent reader, who takes the trouble to read and meditate on them, will find good preſervatives againſt precipitate judgments. 'What analogy can apply in the examina-
'tion of the bulb-polypus? Can we even define
'the bulbs, or does this name expreſs more than
'their mere appearance? How can analogy eluci-
'date the nature of theſe minute bodies, the man-
'ner in which they engender or are engendered,
'while nothing is preſented either by the animal

'or vegetable kingdom bearing the ſmalleſt re-
'lation to productions ſo different from all thoſe
'hitherto known? I ſay the ſame of the natural
'diviſion of the bell, and the ſections of the arm
'polypus. Here is an order of things entirely
'new, with particular laws, which we might diſ-
'cover if we had any method of penetrating in-
'to the mechaniſm of theſe minute beings. We
'ſhould then ſee the relations connecting them
'to the other parts of the organic ſyſtem. . . . But
'I would not baniſh analogical method from
'philoſophy; it conducts itſelf to obſervation by
'the ideas which it aſſociates on every ſubject.
'I only mean, that this method, otherwiſe of
'great general utility, cannot be applied to phi-
'loſophy with too much circumſpection. . . . Have
'we ever had a good treatiſe on analogy, though
'the want has been ſenſibly felt? We ſhall be
'indebted to ſome philoſophic naturaliſt for it.
'Analogy is connected with the doctrine of hy-
'potheſes and probabilities; in proportion as our
'knowledge extends and is perfected, all proba-
'bilities will approach to certainty. Could the
'*totality* of beings on our globe be comprehend-
'ed, analogical reaſoning would be demonſtra-
'tion.'

M. Trembley has greatly approved theſe *Phi-loſophical Conſiderations on Polypi;* and his approbation flatters me the more as it is not indiſ-

criminately

criminately beſtowed. He writes to me, 'The ſyſtem of Epigeneſis is abſurd; he would not wiſh to be obliged to explain any fact;' and adds, 'that he would rather collect a certain number, and then make ſome reflections, which would juſtify his reſerve.' I earneſtly wiſh this excellent obſerver would realize his ſentiments, and give us the reflections which various polypi have excited: but notwithſtanding the interval of many years ſince I ſolicited him to reſume the pen, I have not yet been able to obtain a ſingle page. Domeſtic occupations of much greater importance prevent him from again beginning his works on natural hiſtory: he has reſigned this department to me, and I endeavour to acquit myſelf as well as poſſible.

I have paid particular attention to that paragraph of your letter, my eſteemed correſpondent, where it is ſaid you have ſeen the propagation of infuſion animalcula by diviſion into parts. Does this mode of propagation differ from that of the other animalcula which you have obſerved dividing through the middle of the body? I have likewiſe to aſk, whether it is inſtantaneous or gradual? If the latter, it will return to the diviſion and ſubdiviſion of bell-polypi; if the former, it will be a new and moſt wonderful manner of propagation. Nevertheleſs, it is referrible to that which M. de Sauſſure deſcribes in his letter, pub-

liſhed

lished page 428 of the *Palingenesie*. 'All the 'changes are made by imperceptible degrees, and 'without the animalcule or revolving machine 'changing its place. At last the motion is ac-'celerated; and where the sphere appeared even, 'you begin to see two cross divisions, as on the 'husk of chesnuts ready to open; a little after-'wards the animal is agitated, and then divides 'into four animalcula, perfectly similar, but small-'er.'

An important remark is made in this letter, which you will not fail to notice in your new dissertation. M. de Saussure supplies Mr Needham's defective information on his discovery. 'Doubtless, in four years which have passed 'since I communicated this observation to Mr 'Needham, he must have forgot that I constant-'ly observed the parts of the divided animalcule 'become as large as the whole which they had 'composed; so that the same constancy and uni-'formity are preserved in these generations as in 'the rest of nature.' How much have I myself insisted on the constant uniformity of this new order of propagation. We may almost suppose, Mr Needham has taken that part only of the discovery which he mentions, and seemed favourable to his theory.

XIII. You terminate the abstract of your observations, my dear correspondent, with a general

ral reflection, which, if there was any neceſſity, would prove that you know when to ſuſpend your judgment on what is preſented to view in the extenſive field of Nature. 'The reſults of 'my obſervations,' you tell me, 'do not appear 'ſo deciſive, in favour of the theory of germs, as 'I at firſt conceived. That claſs of germs which 'does not periſh at boiling heat, while the ani- 'malcula produced by them die at 106° and '108°; theſe germs, I ſay, embarraſs me a little. 'Yet when the reaſons on both ſides are balanc- 'ed, it would ſeem that my experiments are 'more in favour of germs than of the imaginary 'vegetative power which, according to Need- 'ham, produces animalcula. For, according to 'the principles of this Epigeneſiſt, that power 'ſhould tend to become weaker in proportion as 'the action of heat is augmented. But we ſee the 'reverſe from the firſt reſults of my experiments 'on animalcula, and in thoſe on mouldineſs. All 'theſe reſults would rather inſinuate, that the 'productive principles of theſe organized beings 'are ſuſpended in the air (1).'

It

(1) When this was written, I had not the complete evidence of the origin of animalcula which was afterwards obtained. However, I knew the different kinds of ſeed that ſupported boiling heat without loſing the germinating faculty. To them I now add the minute ſeeds of mould which germinate after expoſure to the heat of the fire in a chafing diſh.

It is the germs, then, that resist the heat of ebullition, and their animalcula perishing at 106° and 108°, which embarrass you, and appear to weaken what you call the *theory of germs*. Perhaps you may find the conjectures on this obscure subject, which I have hazarded, Article 6. of this letter, assist you in accounting for the fact, at least in conceiving how it happens. If you have any thing more probable to suggest, I will not hesitate to give it the preference to my slender conjectures. I have not pretended to divine nature; nor have I a better opinion of diviners in natural history than of those in politics: but you will not be displeased at my communicating the various reflections which your observations have excited. Consider them only as dreams, if you think fit: yet I must flatter myself that you will judge them more philosophical than those of our friend the English Epigenesist.

You speak of the theory of germs. It is most important to understand the exact general meaning conveyed by the word *germ*. It is frequently recurred to in my two last works. I have been very sensible how much the precise meaning may affect all our reasonings on the material origin of organized bodies. The polypus first induced me to seek for a proper definition of the word germ. 'By this word is commonly understood a most 'minute organized body; so that, if it could be

'found

'found in this ſtate, we ſhould diſcover the ſame 'eſſential parts as organized bodies exhibit after 'their evolution. It is neceſſary to give the 'word a more comprehenſive ſignification, as is 'manifeſted by my principles themſelves. Thus, 'it will not only deſign a moſt minute organized 'body, but alſo every kind of original preforma-'tion from which an organized whole may pro-'ceed as from its immediate principle.' In a note, I have added, 'Remark, that I ſay immedi-'ate, to diſtinguiſh the preformed part or parts, 'in miniature, from the great whole where they 'are to grow or be developed; for that cannot 'here be viewed as the *immediate* principle of re-'production; it is only the *mediate* cauſe' (1). This is afterwards more directly applied to the polypus (2); and what is ſaid of it may extend to all analogous animals.

Before me, much has been ſaid on germs. They are diſcuſſed in all the good treatiſes on natural hiſtory or phyſiology which appeared at the end of the laſt century and in this. Yet I do not obſerve that the authors, who have recurred to the philoſophical hypotheſis of germs, have entered deeply into the ſubject, or viewed it from every ſide, as I have endeavoured to do in the *Corps*

(1) Tom. 1. page 362. firſt edition.

(2) P. 369.

Corps Organisés, and *Palingenesie*, *part* 11. 12. As these authors, valuable in other respects, have not analysed a sufficient number of facts, and facts very various; and as they have not been induced to give themselves up to the same reflections as I have, it is not surprising that they did not penetrate the theory of germs further. Therefore, when you wish to review the consequence of my principles on this beautiful part of the animal œconomy, you have only to revise part 10. of *la Palingenesie*. My latest reflections on the origin of organized beings are there. You, of all persons, are the naturalist from whom I expect most instruction in this fertile field; and your learned researches will confirm, regulate, or destroy, my trivial hypotheses. You will not listen to the language of friendship, when Nature decides against me, and I shall be the first to submit to her sentence.

The insects which like the bell, funnel, and tubulated polypus, as the animalcula of infusions, propagating by natural division and sub-division, undoubtedly follow very different laws from those which govern the propagation of arm polypi, earth worms, fresh water worms, and other animals that are multiplied by being cut in pieces (1). The solution of continuity, which art

(1) It is most singular, that the same animal can propagate its species by natural division, as by an artificial division

art or accident effects in the one, and nature herself in the other, and the method by which it is performed, we are ignorant of. Our best microscopes give us no access to the interior of these animated

division with an edged instrument. When M. Bonnet wrote to me, he was ignorant of this. Every one knows his beautiful discoveries on fresh water worms, which reproduce themselves when cut in pieces, *Traité d'Insectologie, part* 2. Muller has now observed that they multiply by natural division, which M. Bonnet informed me last year, in these words: 'Muller last year sent me a splen-'did work in quarto, illustrated with figures, on insects 'which are reproduced by section or division both natural 'and artificial. This work is unfortunately in German; 'and I could only learn his discoveries by the translation 'of some passages which a friend made *viva voce.* The 'estimable author has particularly repeated my observa-'tions, published 1744, part 2. of the *Traité d'Insectologie*. 'He confirms most of them, and adds much of his own 'enquiries. Among other things, he has seen the apodal 'worms of fresh water multiply before his eyes by natural 'division. He minutely describes this propagation, which 'is very different from that of the cluster polypus and 'infusion animalcula. Behold, how these singular gene-'rations extend more and more! I had myself observed 'the same in worms of the same kind, but from igno-'rance ascribed it to accidental causes. The fact is relat-'ed in my *Insectologie*, '*Anguilles de l'eau douce.*'—The 'cluster polypus was then unknown to me.'

animated corpuſcula: but we can eaſily conceive that the propagation of an animal, naturally dividing into two or four pieces, cannot be performed by a preformation ſimilar to that which gives exiſtence to the buds of a tree, or the ſhoots of an arm polypus. Nothing is diſcovered in the natural diviſion with the leaſt reſemblance to the known animal or vegetable generations. However, it is moſt evident that there muſt be an original preformation, which determines what precedes, accompanies, and follows the natural diviſion of the animal. Changes or alterations, more or leſs conſiderable, ſhould take place within;—a kind of contraction preparatory to the ſolution of the unity: this ſhould occaſion the diverſion of the nutritious juices towards certain particles on fibrilli, by means of which theſe fibrilli expand, and aſſume new poſitions with reſpect to each other. Thus is the enormous world conſolidated: the exterior and interior of the animal is renewed or recompoſed; and each half, or each quarter, becomes a perfect animal.

It would ſeem that this ſingular regeneration has ſome ſlight reſemblance to the firſt kind of organic preformation, deſcribed part 10 of *La Palingeneſie*: but here our glimmerings are too faint to guide us through the profound obſcurity. It is moſt probable, that an animal deſtined to multiply thus, has received a ſtructure of little

intricacy;

intricacy, or of great ſimplicity, from Nature. The parts will be diſperſed through the body, and the animal will conſiſt of ſuch only. It may be all brain, or all ſtomach, if in this caſe we can ſpeak of either. I would rather allude to nothing but my organic points, *Palingeneſie, part* 10. *p.* 363, 364; and it may be much better to be ſilent altogether concerning ſuch myſterious propagation.

XIV. It is certainly moſt proper for you, my dear philoſopher, to examine Mr Needham's *Vitality*. This idea is not one that I would baniſh to the region of chimeras: it has a philoſophic appearance which merits our attention, and belongs to that beautiful gradation of natural beings which I have attempted to gloſs over. In part 15 of La Palingeneſie, you have the *Eſſai d'Application* that I have endeavoured to make of irritability to the polypus, and other animals of the neighbouring claſſes. I had not then peruſed our Epigeneſiſt's reflections on vitality. Perhaps my own thoughts on irritability, which is the foundation of vitality itſelf, will not be uſeleſs to you in your intended examination of our friend's opinion; and I ſhall view the reſults, to which this examination leads you, with pleaſure. No new reflections on the ſubject are offered to you. In the part cited of my laſt work, is ſaid what ſeemed the moſt reaſonable or philoſophic.

I have indeed a faint conjecture on the secret cause of this irritability, but it is not sufficiently mature to lay before the public.

XV. The examination of Mr Needham's hypothesis on generation will require a severe criticism from you; but you will make it in polite, moderate, and friendly terms. You are acquainted with the character of this learned naturalist, and cherish the same esteem for him as I do myself. I have written to him on his opinions with the greatest freedom; and it should be observed in his praise, that he was never offended with it. It is true, he has not abandoned them: on the contrary, he seems to support them more and more.—His last work, which you are called upon to refute, is too good evidence. Perhaps it is reserved for you to convert him, which will be no small addition to your literary glory. Probably you will not forget to say a word of the Reggio Professor, to whose observations Mr Needham referred me with such confidence, but who, notwithstanding, was less an Epigenesist than any one in the world. *Corps Organisés, Art.* 331. *Palingenesie, T.* 1. *p.* 425, 426 (1).

In

(1) Fourteen years ago, when I was professor of philosophy in the University and College of Reggio, and beginning to make experiments on infusion animalcula, I began a correspondence with Mr Needham, who was then

making

In ſeveral parts of my *Corps Organiſés*, this author is commended, particularly *Ch.* 6. *Tom.* 2.

making the tour of Italy, and for ſome time communicated my obſervations on them. I cannot preciſely ſay what were the reſults, becauſe I have neither copies of the letters, nor the journals, where my diſcoveries were noted down. I only remember of agreeing in two facts with Mr Needham, which I communicated to him, namely, that although infuſions had boiled, they produced animalcula; and that they generally appeared when the ſubſtances began to decompoſe. Theſe two facts pleaſed Mr Needham; he thought he ſaw a confirmation of his favourite hypotheſis in them. M. Bonnet was juſt going to publiſh his *Corps Organiſés*, where there is a complete refutation of it; and enquired whether Mr Needham ſtill perſiſted in his opinion. It was natural to ſuppoſe he had abandoned ideas ſo extraordinary; but he ſaid, that far from having changed his ſentiments they were fully confirmed by a Reggian Profeſſor, as would appear from a work he was about to publiſh.

I had ſaid to Needham indeed that I did intend to publiſh a little treatiſe on animalcula; but it is far from true that I was ever an Epigeneſiſt, for there was no foundation for me being ſo: and although ſome of my reſults did correſpond with Needham's, it did not follow that I ſhould decide for Epigeneſis, eſpecially as theſe reſults could eaſily explain the oppoſite theory of germs. I had then no inclination to take any ſide. I found myſelf ne-

ceſſitated

I did it with impartiality and friendſhip. The work appeared 1762: I haſtened to ſend it to him;

ceſſitated to interrogate nature further, in hopes of diſcovering ſome deciſive fact. But my future obſervations favoured the pre-exiſtence of germs as much as they contradicted the Epigeneſis. This I have attempted to demonſtrate in the Diſſertation publiſhed ſome time ago, (Saggio di Oſſervazioni Microſcopiche.)

Needham's too great anxiety to predict the reſult of my obſervations has rendered him a falſe prophet. But I cannot be ſilent on another prediction by Bonnet, though very different from his, as it has been fulfilled. After quoting Needham's letter in the *Corps Organiſés*, he does not heſitate to affirm that the obſervations of the Reggio Profeſſor will not demonſtrate that animalcula have ſo extraordinary an origin as he would aſcribe to them. It ſhould be remarked, that I was then unacquainted with M. Bonnet, nor had I even read his *Corps Organiſés*.

The Engliſh philoſopher, though a pſeudo prophet, has given my obſervations a very gracious reception: he has approved of them; and does not conceal that they have induced him to change his opinion. I ſhall mention what he wrote to M. Bonnet and myſelf on the ſubject, to ſhew that although he was remote from real philoſophy for a time, he afterwards approached it, and did not always remain in error.

'The ſingular agreement of your obſervations with my 'remarks,' M. Bonnet writes, 'gives me the greateſt 'pleaſure, him; but he had not the ſame deſire to read, far leſs to ſtudy it. Much time elapſed; and he had

R 3 not

'pleaſure, as they have been the deciſion of Nature herſelf 'to our valuable friend. With philoſophic candour, he 'informs me, that he will adopt our ſentiments, and ex'amine the intereſting ſubject anew; adding what does 'honour to his ſincerity: *I begin to think my ideas have 'been extended too far in giving power to matter, which is un'neceſſary to explain the phenomena of the microſcopic world.*

'He alſo ſays, he is much inclined to admit my conjec'ture on the generation of animalcula, p. 217 of the *Corps 'Organiſés*. If you will take the trouble to reviſe it, you 'will ſee that I inſinuate, animalcula may multiply by di'viſion as the cluſter polypus. This appears to our friend 'a happy conjecture. He adds, *You have juſtly obſerved 'that the generation of theſe beings may be by diviſion; but it 'would require a courſe of obſervations, ſuch as I find in Sig. 'Spallanzani, to convince me of it: beſides, I ſtill have doubts 'which will probably be ſolved by reflecting maturely on the 'ſubject.*

'It is thus, Sir, that you have ſucceeded in removing 'the veil which covered the eyes of our learned corre'ſpondent. What I ſuggeſted, you have finiſhed—What 'I predicted, you have ſeen.

Needham's letter to me coincided with what he had written to M. Bonnet. 'Beſides recollecting all that I 'have at other times obſerved, comparing it with your 'obſervations, and the new diſcoveries in a courſe of ob-

'ſervations

not then perused the work. Yet he has quoted me, *p.* 219 of his *New Researches:* He there wishes to give an abstract of what is said of the formation of the chicken, after the beautiful discovery of my illustrious friend Haller. On reading the passage, it is easy to see that he has not had the account before him when he abridged it. He evidently cites it from memory: unfortunately

'servations by a young professor here (M. de Saussure), 'I am determined to limit my ideas of generation. This 'limitation will render the *pre-existence of a being, specifically 'similar*, absolutely necessary to the generation of any or'ganised being, without restricting Nature to make it vi'viparous or oviparous, to produce it by the concourse 'of two sexes, or without it to effect reproduction by shoots 'or divisions. By this means we shall comprehend all 'phenomena, and have germs or prolific parts, which 'from their subtilty may insinuate themselves through all. 'Therefore I shall abandon, in those classes of infusion 'animalcula, and in all other organised bodies however 'simple they may be, I shall abandon, I say, the vegeta'tive power of matter which I formerly believed necessary 'to explain these phenomena. Only a few difficulties re'main, which may be easily solved by the indefinite divi'sion of the microscopic beings from the universality of 'their germs or prolific parts, their extreme minuteness, 'and their instantaneous evolution, which uniformly suc'ceeds when the germs or prolific parts are in a suitable 'situation.'

tunately his memory has been very treacherous; and he has lamed my chicken. I told him so: he said it was true; and his custom, whether good or bad, was to follow the train of his ideas. I wrote to him again: 'You have not paid the 'least attention to the consequences immediately 'flowing from the facts I mention. You have 'overlooked them: but they require to be ana-'lysed. It is not in this manner that one should 'treat facts.' I am no less sensible of the obliging and truly friendly things with which the citation of which we speak is attended.

This avowed partizan of Epigenesis incessantly refers us to what he calls the *chain of his reasonings*. He seems not to suppose that it is a spider's web to catch flies. He almost uniformly draws certain conclusions from uncertain premisses. The two sophisms into which he most frequently falls, are the *petitio principii* and *imperfect enumeration*. Because the decomposition of vegetable substances produce particular filaments, whence animalcula seem to proceed, he conceives it demonstrated, that the animalcula, which he calls zoophytes, are produced by the filaments. Then, to account for this strange production, he imagines a vegetative power, which he charges with the office of organizing or animalizing. He endeavours to give us an idea of the action of this force, by a comparison with the combined action of the projectile force and gravity in fire-works. Thus does our Epigenesist undertake to penetrate the mystery of the reproduction of animated beings, by substituting occult qualities in the place of sound philosophy. He seems to treat natural history as alchemists do chemistry. He speaks of the doctrine of germs as a monstrous hypothesis: he thinks he is supported by the great Leibnitz: but no one is ignorant that this illustrious metaphysician was a most zealous favourer of the theory of germs. You have seen what I say after him, *Palingenesie*, *part* 7. how pointed it is. There is also another passage of the same profound philosopher still more applicable, in his *Considerations on the Principles of Life and Plastic Nature.* 'With Mr 'Cudworth, I think the laws of mechanism a-'lone cannot form an animal, where there is yet 'nothing organized; and I find that he justly 'opposes what some of the ancients have said on 'this subject, and even M. Descartes in his '*Man*, whose formation costs him so little, but 'he approaches as little to a real man. And I 'confirm Cudworth's sentiments, which consider 'that matter, arranged by a Divine Wisdom, must 'be essentially organized throughout; that there 'is thus a machine to infinity in the parts 'of the natural machine; and so many en-'velopes and organic bodies involved in each

'other,

'other, that an organic body entirely new, and 'without any preformation, can never be produc-'ed, and that we cannot entirely destroy an ani-'mal not yet existing.' From this circumstantiate passage, we perceive that Leibnitz not only rejects the mechanical formation of the animal, but that he admits germs or preorganization, and also the infinite involution of organic machines. Can we believe this is actual infinity? Must not every series have an ultimate term? Is the infinity of geometricians *real infinity?* However, this passage, and many others of the same author, strictly demonstrates that Mr Needham has not been aware of the real sentiments of the German Plato on the origin of organized beings.

Thus, it will be important for you to show your readers that our friend's method is unphilosophical. I do not despair, however, that your new experiments, and the logical deductions which you know how to draw from them, will triumph over his attachment to Epigenesis. It should also be expected from his love of truth.

XVI. The plan of your Dissertation pleases me much, my dear correspondent; and I offer up sincere vows for its execution. No naturalist will have equally perfected this part of natural history, fit in itself to complete the logic of the observer, and to exercise his genius.

In

In anſwering the little volume of facts which you addreſſed to me 20th of laſt December, I have made one of reflections, which have employed me ſome weeks to write. You ſend me gold, but you will receive only braſs in exchange. Should this letter ſeem worthy of appearing at the end of the work which you are preparing on animalcula, you are at full liberty to publiſh it. I have attempted to collect my lateſt reflections on a ſubject which has engaged my attention for many years. Perhaps you will make ſome notes on the letter, where I have not correctly comprehended your meaning, and always where your own opinion is different. This is the real method to render it moſt uſeful to the public; and you will afford me much pleaſure by doing ſo.

I cannot terminate this long epiſtle, my worthy correſpondent, without renewing the teſtimony of the great eſteem, and ſincere attachment, which have ever been borne towards you by the

24 *February* 1771. PALINGENISIST.

LETTER II.

My Solitude, 20 *April* 1771.

I REJOICE, my dear Redi, that you are ſatisfied with my long letter on animalcula, and that my reflections

reflections do not seem useless towards perfecting the interesting subject. With the greatest eagerness I shall read the brief commentary which you propose on the letter, when it is published at the end of your new Dissertation. This commentary will undoubtedly contain a number of facts; and many reflections on them will be necessary to enhance the value of the text, too destitute of the former, and too much loaded with the latter.

Before considering several articles in your obliging answer, I hasten to acknowledge a misconception, or error, which the learned and estimable M. de Saussure has pointed out. As he knows me well, he is aware that my regard for truth is sincere; and that I am always grateful to those who discover my errors. In the hypothesis, where I attempted to account for the phenomena of animalcula, in their germ state, supporting the heat of ebullition, and perishing at 106° and 108°, when developed; and supposed, as you have seen, that the extreme transparency of the germs protected them from the action of heat; I corroborated my supposition, by considering that the solar rays do not sensibly warm the atmosphere on high mountains, because it is too rare or diaphanous. I then quoted observations, proving organized bodies to be more and more transparent as we ascend towards their origin. Finally, I insisted much on the extreme

rarity,

rarity, and on the great ſimplicity or homogeneouſneſs of the organic texture of our animalcula, conſidered in their primitive ſtate. I tranſcribe what M. de Sauſſure wrote to me on the ſixth of this month, on a hypotheſis which I had favoured too much.

'You have more regard for truth, Sir, than to 'allow me to conceal that the indeſtructibility of 'the germs or eggs of our animalcula ſeems to 'me to depend on the nature of the mixture or 'aggregation of the parts rather than on their 'tranſparency. Without recurring to the re-'mote example of crucibles, you ſee japanning 'ſupport the heat of boiling water, and even 'a greater degree; not becauſe this heat is 'not acquired, but becauſe the intimate con-'nection and equal dilation of the parts pre-'ſerve them from deſtruction. May it not be 'poſſible that the animalcular germs, or eggs 'of which we treat, are provided with ſome 'varniſh which is ſoluble in the ſeminal fluid 'only, or in a liquid fit for the developement 'and nutrition of the included animal?

'In general, if the ſoluble and volatile parts 'of a body, inſoluble in a given fluid, are ſo con-'nected with the fixed and refractory parts, and 'theſe, ſerving as a connection, prevent them 'from diſſolving or diſſipating; and if the total 'aggregate has ſuch ductility, that the fire may

'expand

'expand it without attacking its ſubſtances, I believe I may affirm that this body, immerſed in the fluid, will reſiſt the action of the fire.

'I know well, that for eggs or germs to preſerve their fecundity, it is eſſential not only that the external integument remains entire, but that the interior retains the ſame proportions and the ſame pliancy. Therefore we muſt add to the preceding conditions, that no fluid is to be contained which heat can coagulate, and that the whole veſſels and fibres may be ſufficiently pliant to dilate without rupture, and, in contracting, to reſume their original ſituations.

'Collecting and generalizing theſe ideas, I find that all the determinations are reduced, *firſt*, To reciprocal indiſſolubility as well of the contiguous parts of the exterior and interior parts of the germ, as of the medium in which it exiſts: *Secondly*, To the fixed and refractory nature of the germ: *Thirdly*, To the proportionate capacity of contraction and dilation of its whole parts. Germs continue fertile ſo long as the heat does not exceed the conditions within which theſe determinations may ſubſiſt; and they may do ſo at heat far ſurpaſſing that of boiling water. It is eaſy to ſee how the germ may loſe them during its expanſion.

'Explanations of this kind, I acknowledge, I would prefer to thoſe deduced from tranſparen-

'cy,

'cy, however ingenious they may be. For, excepting the caſe of the ſolar rays, it has not been obſerved that tranſparent ſubſtances are more eaſily heated than opaque; that clear water is more difficult to boil than ink, allowance being made for their different denſity. It is true, there are no experiments made *ex profeſſo* on this ſubject; but, if the difference was very remarkable, it would have been obſerved as with the rays of the ſun.'

M. de Sauſſure's letter is terminated in the moſt modeſt manner, and, at the ſame time, in terms the moſt polite to the author whoſe hypotheſis he was examining. You will judge from my anſwer, my dear correſpondent, what I have thought of our judicious obſerver's remarks, who is alſo an able chemiſt, as his letter ſhows.

' *My Solitude*, 8 *April* 1771.

'YOUR crucible could not confine my little hypotheſis, my dear Becker; it has been volatilized or reduced to ſmoke; but the reſidue conſiſts of two facts, which are moſt precious to me: the one, that you love me well enough not to conceal my errors; the other, that I cannot diſtruſt my own opinions too much. Your remarks have great weight in my eſtimation. I ſhall not fail, in a ſecond letter, to correct this article of my firſt, nor ſhall the valuable

'philoſopher

'philosophy be forgot to whom I am indebted
'for the correction. I had paid too great atten-
'tion to the solar rays, which seem to have daz-
'zled me. However it is to be wished, that this
'may give an opening to direct experiments; it
'would be worth while to make them. I shall
'meditate anew on the subject, as if I had never
'attended to it before. In addition, I made other
'two conditions intervene, the extreme rareness
'of the texture, and its simplicity or homoge-
'neity: pliancy and ductility were produced by
'the first; the second occasioned a certain de-
'gree of resistance to a certain degree of heat.
'The connection of the elements of the texture
'passed unnoticed. But I repeat, that all this is
'an old spoil, of which I divest myself. My
'heart shall never be refractory to truth.'

Thus you perceive, my dear Malpighi, that I intend to reflect anew on this interesting subject. I invite you to reflection on your part; and I am convinced your meditations will not be fruitless: surely they will suggest new experiments, which must be more instructive, therefore more satisfactory, than our meditations.

Since you are determined to print my long lerter at the end of the Dissertation which you are now composing, will you be good enough to add what I say concerning my hypothesis on the indestructibility of infusion animalcula? I doubt

doubt that my reveries may lead thoſe readers into error who have too great an opinion of my feeble meditations, and my trivial writings in general. From your anſwer of 24 March, I perceive you had the ſame doubt as M. de Sauſſure. 'The facts you collect,' you obſerve, 'prove, without reply, the extreme tranſparency 'of theſe germs. The heating of bodies by the 'rays of the ſun, which is always reciprocally in 'proportion to their tranſparency, perſuades us 'that the heat freely paſſes through the germs 'without altering their ſtructure. But this per'ſuaſion would become much ſtronger, if we 'could prove directly that it is the ſame with 'our fire as with the ſolar rays.' What you add immediately afterwards excites my admiration. 'It ſeems that a courſe of experiments 'on this point would be of the firſt importance, 'and your excellent conjecture might be ex'poſed to other trials: to examine whether cer'tain inſects which are very tranſparent would 'ſupport the action of heat better than thoſe 'which are very opaque. Among the animalcula 'of infuſions are ſome whoſe tranſparency is al'moſt infinitely greater than thoſe of others. Per'haps there might be foundation for ſuppoſing, 'according to your principle, that heat has leſs 'influence on the former than the latter. It is 'true, I have ſaid in my letter, that animalcula

'in

'in general perish at 106° or 108°; but as 'transparency did not then occur to me, I am 'ignorant whether any very transparent were of 'the number. I only touch on the subject;' and it cannot be too often repeated, that I view all that I wrote you before but as simple conjectures, or rather doubts (1).

(1) In the course of the letter, from which M. Bonnet has given two extracts, I had sketched an explanation similar to that of M. de Saussure, though unacquaintted at the time with the sentiments of that celebrated Genevese Professor, as appears from the date of the letter. 'If the decision of Nature should not favour your con'jecture, Why may we not explain the *indestructibility* 'of germs from the indestructibility of their component 'parts? Without recurring to asbestus or amianthus, 'there are substances known to us whose structure sup'ports the influence of heat infinitely surpassing that of 'boiling water. These are earthen vessels in which glass, 'melted by the heat of furnaces, remains many weeks, 'yet they do not suffer. Therefore a germ, composed 'of matter analogous, would easily resist the heat of 'boiling water, and be destroyed by it when beginning 'to expand; for, being replete with foreign matter, 'the primordial molecules, forming its original state, 'would be removed farther from each other; their reci'procal attraction would be diminished, consequently 'their former cohesion. Therefore the heat dissipating the

'foreign matter, would destroy the mutual relation of 'the primitive molecules, and the germ would be de-'composed though these molecules remained untouched. 'Here I only outline an hypothesis which shall be extend-'ed when I have leisure.'

It was really my intention to do so, when I wrote thus to M. Bonnet; but the intervention of other successive operations has not allowed me time. The same has happened to M. Bonnet, who promised me new reflections on the subject, and has undoubtedly been prevented by similar causes. The explanation of his illustrious nephew seems to me sufficient, especially if we admit, that not only several inorganic bodies, as asbestus or amianthus, some earths, and several other mineral substances, support the action of fire without suffering by it; but there are also particular bodies, bearing great analogy to animals, and organized like them, that enjoy the same advantages. There are certain roots which may be ignited without being consumed: an incombustible flax is made of others. Such, in the former case, is the Androsaces of Dioscorides; and, in the latter, the Indian tree Sodda. *Waller Mineralog.*

It should not be concealed from the reader, that, on putting M. Bonnet's conjecture to the test, I did not find it agree with facts; and so I informed him. 'The co-'lossal size of the animalcula in these infusions has been 'mentioned, my celebrated colleague, in my other let-'ters. As they are nearly opaque, and as others very 'minute and transparent appear along with them, if your 'hypothesis is true, it follows, that the animalcula of the

'second

'second species should support heat much longer than 'those of the first. But just the reverse has happened. 'The smallest animalcula perished at 106° ; and those of 'colossal size, only when the thermometer rose to 140° 'and 142°. We must remark that animalcula support-'ing heat so great are very rare.

'If animalcula are compared with frogs, it is at once 'evident, that these are composed of matter infinitely 'more dense and heterogeneous. Notwithstanding so dis-'proportionate a difference, however, frogs support a de-'gree of heat far greater than what is fatal to animalcu-'la.'

These facts appeared to satisfy M. Bonnet ; and he replied with that candour and indifference for his opinions, which are almost peculiar to him, and so desirable in most of the literary world.

OBSERVATIONS AND EXPERIMENTS

ON THE

SEMINAL VERMICULI OF MAN AND OTHER ANIMALS; WITH AN EXAMINATION OF THE CELEBRATED THEORY OF ORGANIC MOLECULES.

INTRODUCTION.

THE ſubject of which I am about to treat was intended for a chapter of the preceding treatiſe on Infuſion Animacula, from the great analogy between theſe beings and ſpermatic vermiculi: but the obliging ſuggeſtions of M. Bonnet have changed my determination. I had ſent him the reſult of my experiments on Animalcula, Vermiculi, and Mould; he honoured them with his approbation, and adviſed me to ſeparate the ſubjects, to treat of each in a different diſſertation. In this form he thought they would more readily fix the attention and attract the curioſity of

 readers.

readers. I have found his counſels ſalutory, and profited by them: they have enabled me to extend the matter, and enlarge my reſearches on ſpermatic vermiculi.

The reality of the exiſtence of theſe animals, and a knowledge of their peculiar nature, are as fit to engage the enquiries 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 certain animals, microſcopically examined by Leeuwenhoeck, appeared full of animalcula, which he named *vermes*, from their ſimilarity in figure and motion. But they were ſoon conſidered by ſome philoſophers as a phantom of the imagination, an illuſion of the ſenſes, or ſome imperfection in the microſcope; they ſuppoſed there was nothing real in what he had deſcribed. Others judged the Dutch naturaliſt with leſs ſeverity; they admitted there was a number of corpuſcula in the ſeminal fluid, but, denying they were animals, conceived them inorganic particles, which, from ſubtility, were raiſed and evaporated ſooner than the reſt; thus forming a fermentation and motion in the fluid that created the idea of animation.

Nearly

Nearly this opinion is adopted by the celebrated Linnæus. He thinks the vermiculi are only inert molecules ſwimming like oil in the ſeminal fluid, moving and darting in various directions, as they are affected or heated by the temperature of the ſemen.

Meſſ. Needham and de Buffon, as is well known, have publiſhed their ſentiments concerning theſe diſputed microſcopic beings; and appear to have beheld them as ſuited their reſpective theories beſt. The former thinks they originate from the vegetative power acting on the ſeminal fluid after it comes from the body of the animal, by which it is neceſſitated to vegetate, expand, put itſelf in motion, and change into beings not yet animated but *ſimply vital.* M. de Buffon, ever prejudiced for his favourite *organic molecules*, ſuppoſes he finds them in the vermiculi, and, from a long detail of experiments and obſervations, endeavours to eſtabliſh his own theory on the ruins of Leeuwenhoeck's.

Who could imagine that ſo many diſputes, and ſuch oppoſition of ſentiment, would ariſe on a matter of fact? I confeſs it has ſingularly ſurpriſed me: and I have often thought that the diverſity of opinion originated leſs from eſſential difficulties than the fault of obſervers; who had not the proper methods of examination; who were prejudiced in favour of ſome theory, and made their ſenſes the cauſe of error; or who wanted ſufficient practice in the difficult art of accurate obſervation. As I inveſtigated a ſubject analogous to the hiſtory of ſpermatic vermiculi, I wiſhed to ſtudy them: and applying with all the attention, care, and reflection in my power, to diſcover the truth if poſſible, for the greater certainty, endeavoured firſt to forget all that had been written, and act as if I had been the original author of the enquiry. In controverted facts, I have uniformly found this the ſafeſt method, to avoid confounding the opinion of the philoſopher with the reſponſes of Nature: only, after reaping a fruitful harveſt of facts, I begin to conſider what has been ſeen by others; I compare their reſults with my own, and, with reſpectful deference, allow myſelf to give an opinion. My ſincerity will be believed when it is known that I had taken no ſide of the queſtion, and that it abſolutely was indifferent to me whether my diſcoveries corroborated or refuted thoſe of others.

The ſeminal fluids examined were that of man and different quadrupeds; neither did I neglect the ſmalleſt animals. The human ſemen was uſed as recent as it could be obtained, by taking it from dead bodies while yet warm: that of animals the moment they were killed; I have frequently examined the ſeminal fluid of animals alive,

alive, and often uſed what was ejected during copulation. The importance of theſe facts, in the illuſtration of this reſearch, will be evident to the reader of the following chapters.

LETTER FROM M. BONNET.

In the Country, 16 *October* 1771.

I ACCEDE to your requeſt, my eſteemed correſpondent, and haſten to inform you that I have received the excellent letter, which is owing to your friendſhip for the Palingeneſiſt.—Excellent book I ſhould ſay; it is almoſt ſuch in ſize, and ſhall be added to thoſe of the ſame kind with which you have already enriched my library. I could not engroſs to myſelf a work almoſt every line of which has excited the deepeſt attention, but determined to reviſe it along with an obſerver worthy to underſtand and to imitate you, and who can equally well appreciate your intereſting reſearches and fortunate diſcoveries. I mean my worthy friend, the illuſtrious diſcoverer of polypi. Yeſterday we peruſed it together: I am unable to deſcribe how much your experiments delighted us; at ſome other opportunity, when you give me more leiſure, I ſhall ſpeak of them. But M. Trembley and myſelf are moſt deſirous, that your obſervations and experiments on Infuſions, Vermiculi, the Plantulæ of Mould, and the other ſubjects you have treated with ſo much

much learning and ſucceſs, ſhould be publiſhed ſeparately. They are too important not to merit and require a ſeparate impreſſion. In detached treatiſes they would make an admirable appearance, and thus more firmly fix the attention of amateurs. Send them to me whenever they come from the preſs; M. Trembley and I will cauſe them immediately be tranſlated into French, under our own inſpection. Surely you will comply with our requeſt. This new Treatiſe on 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 inform me. It has been mentioned to M. Trembley.

Your tracts on infuſions, and other ſubjects, will, in my opinion, be excellent logic for the uſe of naturaliſts; which, believe me, in my view, is not the leaſt merit of your learned reſearches.

You may well conceive we would have aſſociated M. de Sauſſure in our philoſophic amuſements, but he has been abſent fifteen days on a journey to Lyons; however, we ſhall regale him with your productions at his return.

Still a word on infuſions.—Behold the poor Epigeneſiſt reduced to an impalpable powder; and you have no leſs pulveriſed his friend De Buffon. I have read nothing on ſeminal vermi-

culi

culi with ſo much ſatisfaction, and congratulate myſelf for having induced you to ſtudy them. The obſervations are invaluable: they are both new and accurate. Would that I could reanimate the worthy Leeuwenhoeck! What pleaſure would it afford him to ſee M. de Buffon's attack ſo well repelled! I hope he will now be philoſopher enough to acknowledge that his microſcopes have not done him juſtice, and yield to your evidence.

Your mould is almoſt as new as your vermiculi.—But I forget that I am beginning to anſwer your intereſting letter in detail: if it is continued, you will not ſo ſoon know that I have received it. I therefore end, with renewing the aſſurance of my inviolable regard,

BONNET.

CHAP. I.

DESCRIPTION OF THE SEMINAL VERMICULI OF MAN, AND VARIOUS OTHER ANIMALS.

THE ſeminal fluid, taken from a dead human body, December 21, reſembled coagulated milk in colour and conſiſtence (1). Examined with a magnifier,

(1) Thermometer 48°.

magnifier, nothing could be diſcovered on account of its opacity. It began to diſſolve; and, loſing the whiteneſs, acquired the colour of ſoapy water. It was again examined with a magnifier of ſmall power; the irregular parts ſeemed to be in an indiſtinct ſlow fermentation. With the aſſiſtance of a much more powerful glaſs, I obſerved that theſe 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. The groſſer part of the fluid was evidently put in motion by them, for there was none when they were at reſt. The corpuſcula themſelves had two motions; one oſcillating from right to left and from left to right, curving the appendage from one ſide to another; the other was progreſſive, advancing by means of oſcillation. During this, one would ſuppoſe them blind: they ſtrike againſt every obſtacle; and, when amidſt a number, make agitations and contorſions, at laſt, taking that way where there is leaſt reſiſtance. They are reſtleſs and continually moving.

In twenty-three minutes the oſcillation and progreſſion had diminiſhed; and it had leſſened ſo much in an hour and a half, that very few corpuſcula preſerved any appearance of motion. The progreſſive motion generally ceaſes before the oſcillatory, ſo that the corpuſcle at laſt merely bends from right to left, and reciprocally. They continue

continue fixed to the ſame ſpot until the oſcillation inſenſibly dies away: then they remain entire in the fluid, and are better ſeen when it is diluted with water. We diſcover that each corpuſcle is not globular, but elliptic; and the appendage is not only much longer than it appeared, but the thickneſs not uniform through the whole length, and encreaſing as it approaches the body, fig. 1. B. I was unable to diſcern where it terminated, being ſo much immerſed in the fluid. When motion entirely ceaſes, the filament remains extended in a ſtraight line, or with very little deviation.

If the ſeminal fluid has been kept a day, or indeed leſs, in a watch-glaſs, it becomes tranſparent, though preſerving its original viſcoſity. A ſediment of whitiſh matter is depoſited, which, by the microſcope, reſembles a parcel of ſlender rags.

In another obſervation, January 11, the fluid diſſolving later than that of the preceding obſervation, ſome particles were put in rain water (1). The elliptic corpuſcula, ſwimming with progreſſion and oſcillation, became motionleſs when the water touched them. Other waters, as dunghill, river, ſnow, ice, and even diſtilled water, mixed with the ſeminal fluid, produced the ſame effect.

I have

(1) Thermometer 36°.

I have found nothing but ſaliva preſerve their motion; and it may be uſed indifferently, either from the mouth, or when cold; therefore I have often taken advantage of it for continuing my obſervations. Theſe ovular corpuſcula, and the phenomena exhibited, were exactly the ſame as in the former obſervation.

When the drop dries up, all, without exception, become motionleſs; and as it dries firſt at the circumference, that is, where thinner, and advances to the centre, thoſe firſt becoming motionleſs are at the edge, then thoſe of the interior, and, laſtly, thoſe in the centre. The corpuſcula do not recover on putting a drop of ſaliva, or ſeminal fluid, on that which is dried, though humidity continues long. In this obſervation, they became motionleſs ſooner. In fourteen minutes they were languid; in three quarters of an hour, there was complete repoſe. While a remnant of oſcillation continued, progreſſion had ended; and when both motions had ceaſed, moſt of the appendages remained extended in a ſtraight line.

Semen taken from a dead human body was chiefly coagulated, 18 February (1). It was inhabited by the uſual corpuſcula. In one of the preceding obſervations, it appeared to me that there were ſome larger among the reſt. I apprehended

(1) Thermometer 49°.

hended I might be deceived, and that any difference of ſize might ariſe from portions of ſemen attached to them; but I was now convinced it could not be the caſe; becauſe when the ſeminal fluid was completely diſſolved, they retained the ſame ſize, though tranſmitted through another fluid. Fig. 2. pl. 2. Motion ceaſed two hours after taking the ſemen from the body.

The ſeminal fluid of a man was like milk ready to coagulate, March 8 (1). A ſmall portion taken for examination, preſented a ſingular phenomenon. Four corpuſcules were attached, by the filaments, to a diſſolving clot. They ſeemed to make every effort to diſengage themſelves from this incumbrance, by many motions and contorſions; aſcending, deſcending, now turning to either ſide, now remaining motionleſs; the filament ſometimes deſcribing a curve, and ſometimes extending in a ſtraight line. Amidſt the ſtruggles one diſengaged itſelf, and began to ſwim like the reſt, with an oſcillatory and progreſſive motion. The other three corpuſcula, one after another, did the ſame, and the clot gradually diſſolved.

The novelty of the phenomenon made me deſirous to examine whether other ovular corpuſcula could be found in ſimilar ſituations in molecules

(2) Thermometer 51°.

molecules of ſemen incompletely diſſolved. In ſome they were free, and ſwam in the place of the clot diſſolved; others were attached by the appendage to what part of it was entire, and endeavouring by great exertions to diſengage themſelves. When altogether detached from the ſpermatic molecules, they ſwam about in the fluid. I found ſomething more. A clot was partly in filaments; many ovular corpuſcula appeared about them, which, notwithſtanding their exertions, were unable to accompliſh their liberation. In this ſemen they lived about two hours and a half.

When I took the ſeminal fluid from a dead human body, moſt part ſeemed diſſolved (1). Many corpuſcula ſurpaſſed the common ſize. Here my object was to ſearch with all poſſible care for what had been ſeen in the courſe of the preceding obſervation; and I ſaw the whole, except the phenomenon of corpuſcles, attached to clots, which could not happen as the ſemen was entirely diſſolved. Some corpuſcula continued to move for three hours.

In the examination of this ſeminal fluid (2), which was at firſt a little thick, I accidentally diſcovered a method of obſerving the corpuſcula and

(1) March 27. The Thermometer 54°.

(2) April 15. The Thermometer 60°.

and their appendages with much greater convenience and precision. As the thickness of a clot on a talc slider prevented me from seeing it distinctly, I swept it with a hair-pencil; but the talc was not so clean that some little portion of spermatic matter did not remain which dried in a moment. I cannot say what induced me to examine the talc again with the microscope, but I there found what could never have been supposed. Scores of corpuscula were visible, and the whole motionless because they were dry; they were very distinct and free from any mixture with the spermatic matter. The appendage of some was curved; of others straight, and nearly of the same length in all, that is, about six times the length of the body; not much pointed at the extremity, and thicker as it approached the body; and it was evidently perceptible, that the corpuscule or body made one whole with the appendage, which, in the thickest part, was thrice as small as the body, and even more. Each corpuscle was somewhat like the red globules of blood, but smaller. Both the appendage and the corpuscle seemed to be composed of an homogeneous substance.

After this fortunate event, I frequently wiped the talc bearing drops of semen, and constantly saw the corpuscle with equal precision. They remained dry on the talc several days without any change of figure.

I wiſhed to proceed rigorouſly with my reſearches, as it appeared that the reſults hitherto obtained were inſufficient to beſtow the character of real animals on the corpuſcula. We have not had that aſſemblage of characteriſtic marks to decide their animality. Doubtleſs however we may name them ſelf-moving corpuſcula, or poſſeſſing a ſpontaneous motion; for the teſtimony of our ſenſes will not permit us to believe this double motion of oſcillation and progreſſion the effect of any external cauſe. The ſequel of the Tract will make us more intimately acquainted with their nature.

Theſe obſervations likewiſe demonſtrate another fact, that the duration of motion, after the corpuſcula come from the body of the animal, depends in a certain degree on the temperature of the atmoſphere. At 36°, all motion was gone in three quarters of an hour; at 48°, 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 60°, only after three hours and three quarters.

Thus I remarked that motion continued longer as the temperature of the atmoſphere increaſed; and examining human ſemen, in the warmer months, to learn whether the phenomena already witneſſed might be then obſerved, I had the ſatisfaction of ſeeing them again. The duration of motion conſtantly increaſed as the heat

was

was greater; fo that when the thermometer rofe to 81°, in the middle of fummer, our corpufcula moved feven and three quarters, even eight hours. While this heat continued, the experiments were varied.—A portion of feminal fluid, taken from a man, was expofed to the air of an apartment where the thermometer ftood at 82°; and another portion put in a cave, where the heat was 66°; a third in an ice-houfe, where the thermometer ftood at 42°. Here the corpufcula moved an hour; in the cave, four hours; and in the apartment, eight.

Each of the fluids hitherto examined was full of moving corpufcula. The fmalleft drop included an innumerable multitude.

After the feminal fluid of man, that of the horfe was examined. No method of obferving it could be more proper, as it was always obtained at the moment of copulation. I ufed the femen of different horfes.—The firft was without clots, very fluid, and of a light cinder colour (1). The corpufcula were not fo numerous as thofe of human femen: there was no difference that I could difcover, except that thofe of the horfe were a little larger. The appendage is more vifible, probably from being thicker; it is diftinctly and completely feen, though immerfed in the feminal lymph, fig. 3. Their ofcillatory motion is not

 fo

(1) 11 March, the thermometer, from a cold north wind, only at 43°.

ſo great as that of the human corpuſcula, which may be the reaſon why 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 the whole die in the ſame time. A few have continued moving more than an hour; the greater part died in three quarters; and ſometimes, but ſeldom, they did not live above half an hour. When motion ceaſes, they remain entire, with the appendage extended in a ſtraight line, or a little curved.

The ſemen of the horſe is very glutinous and filamentous (1). There we ſee the corpuſcula attached by the body, and particularly by the filament, to various irregular ſubſtances mixed with the fluid; and, unable to diſengage themſelves, the ſubſtances are ſenſibly agitated by their motion. Thus it is that in ſeveral places 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 prolonged, it is not uncommon to ſee the bodies divide and form two diſtinct corpuſcula. I am well aſſured it is not an optical illuſion, regarding the corpuſcula of a different ſize, but a poſitive fact. Some were a third larger than the reſt; which diverſity was

(1) 22 March, the thermometer 57°.

was remarked in a former obſervation, 11 March. I particularly attended to the diminution of their motion, in proportion to the time that the fluid had been expoſed to the air. Scarcely has it come from the animal when the corpuſcula are ſeen in great agitation, darting through it with vaſt rapidity, and oſcillating to both ſides. This activity inſenſibly decreaſes; 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 become ſucceſſively 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 vibration diſappears, and the appendage remains extended in a ſtraight line, after the manner of theſe beings.

Many aquatic animals of the apodal claſs tranſport themſelves by contorſions of their members vibrating and oſcillating from ſide to ſide: and indeed one can poſitively affirm, that the anterior part of the body is puſhed forward, and moves progreſſively by the contorſions and oſcillations of the poſterior part. I paid the ſtricteſt attention to diſcover whether the anterior part of the corpuſcle was puſhed forward by oſcillations of the appendage. When in very rapid motion, the quickneſs of the mutual vibrations of the body and appendage render it impoſſible to be diſtinctly obſerved; but, beginning to relax, we eaſily ſee their mode of advancing, and that it is the ſame with that of the aquatic animals juſt mentioned. When the appendage ceaſes to oscillate, progreſſion alſo ſtops, but begins again when the oſcillations recommence. I made this important obſervation, not only on the corpuſcula in the ſeminal fluid of the horſe, but on thoſe of the human ſemen, and of all the animals I ſhall afterwards name. The motion here did not continue above an hour and a half.

The corpuſcula of the two portions already ſpoken of were very numerous; in a third portion they were rare, but perfectly ſimilar both in figure and properties. Their motion continued eight hours (1).

I examined the ſemen of other ſix horſes. The corpuſcula, except in being more or leſs numerous, were exactly like the preceding, therefore it is needleſs to deſcribe them. When the ſeminal fluid is mixed with water, or even with ſaliva, all inſtantly become motionleſs.

The ſeminal fluid of the bull contained moving corpuſcula in numbers ſurpaſſing thoſe in the human ſemen (2). The appendage is longer than that of the human ſeminal corpuſcula, and the body alſo ſeems a little larger, fig. 4. Pl. 2. The

(1) May 2, the thermometer 64°.
(2) March 30, the thermometer 57°.

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. Here likewiſe is progreſſion 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 others. The ſmall quantity of the ſemen of the bull which I had on this occaſion prevented me from extending my reſearches farther.

At another time, having abundance, I was more able to ſatisfy my curioſity (1). Beſides the phenomena before deſcribed, I remarked, 1. That the corpuſcula both ſwam horizontally, and roſe and ſunk in the ſemen, as fiſhes do in water; 2. When the fluid dried, their motion was irrecoverably gone; 3. In times of equal heat, they moved quicker than thoſe of man or the horſe; 4. The mixture of every kind of water, even of ſaliva, was fatal to them. Similar phenomena were obſerved in the ſeminal fluid of other three bulls. I had the ſemen from all theſe at the moment of copulation.

The teſticles of a dog, alive and in perfect health, being opened, the epididymis was full of ſemen, a little viſcous, very thick, and of a dark aſh colour (2). The thickneſs prevented me

(1) May 30, the thermometer 68°.

(2) February 14, the thermometer at 48°.

from feeing the corpufcula accurately. Only a confufed agitation of the fubftance was perceived, which ceafed on mixing the femen with water: the corpufcula now become motionlefs were diftinctly feen. I then fufpected they had been the caufe of the agitation, which mixture with water had deftroyed: my fufpicions were confirmed on mixing faliva with the femen, for the tumultous motion continued.—I faw it was produced by prodigious numbers of corpufcula. The reader will not be furprifed if nothing is faid of their figure, fize, and motions, as it would be repeating all that I have faid of the human corpufcula; for thefe of the dog moft exactly refemble thofe in the feminal fluid of man. In three quarters of an hour, they became motionlefs.

The femen of a dog, which I procured at the moment of copulation, was a little vifcous, and like turbid water (1). The moving corpufcula were not fo much immerfed as in that taken from the epididymis: and it was unneceffary to mix it with any other fluid to fee them. Every part was fufficiently vifible, and their motion very rapid, but the velocity infenfibly diminifhed; and two hours after the fluid came from the animal's body, all motion was at an end: the corpufcula for

(1) April 27, The thermometer 61°.

for the moſt part remained with their appendages extended in a ſtraight line.

Theſe experiments were repeated on other five dogs, and the reſults found perfectly alike.

If between the human and canine corpuſcula no difference has been diſcovered, neither has any been ſeen in the rabbit, except that the body and appendage are a little leſs. Though I obtained the fluid in ſummer by ſpontaneous emiſſion, their motion did not continue half an hour: and, repeating the obſervation in winter, they ceaſed to live in ten minutes.

Nine hours after a ram was killed, I opened the teſticles, and expreſſed the fluid into a watch glaſs (1). All the corpuſcula were motionleſs: they were larger, therefore more eaſily ſeen than thoſe of the dog or man.

In the ſeminal fluid of a living ram, all the corpuſcula were in motion (2). The oval part or body of each corpuſcle ſometimes immerſed itſelf in the fluid and eſcaped the eye, and ſometimes came to the ſurface. Their properties reſembled thoſe of others, if we add a certain gentle vibration and a little more activity. When the corpuſcle contracted itſelf, the appendage was leſs curved. The duration of motion was much

(1) May 10, The thermometer 66°.

(2) June 18, The thermometer 66°.

much lefs than in many others: though the heat of the atmofphere raifed the thermometer to 66°, all ceafed to move in half an hour.

The epididymis of another living ram was examined (3). The quantity of feminal fluid was fo great as to fill two-thirds of a watch-glafs. Viewed with the naked eye, it feemed in continual agitation, notwithftanding the glafs was fituated on an immoveable plane. A drop was examined with a magnifier of fmall power: the whole feemed in motion: and the microfcope proved that this was produced by the agitation of the corpufcula alone. They hardly moved an hour.

After examining the feminal fluid of fo many warm blooded animals, I determined to examine the femen of fome whofe blood is cold. I began with fifhes; and for this purpofe the experiment was delayed until they fpawned. The milt of a living carp being taken, and the fluid expreffed into a veffel, it appeared tenacious, thickifh, and of a dull white. Many moving corpufcula were feen, but I could not obtain diftinct vifion until the denfity was diminifhed by water. Here was a new fcene: the corpufcula were no longer compofed of two parts, a body and an appendage, but a united whle ref e mbling minute fpheres, and apparently folid,

(1) July 15, The thermometer 77°.

folid, Pl. 2. fig. 5. Thefe fpherules, of a darkifh colour to the eye, fwam through the liquid in every direction, advanced, retreated, mutually avoided each other, immerfed themfelves deep in the fluid, and ceafed to move in a moment. In a word, they had many motions and properties peculiar to infufion animalcula. Their number was infinite: their courfe continued a quarter of an hour, then they ftopped and moved no more. I repeated the experiment five times, expreffing the fluid of the milt anew, and the confequences were the fame. If any frefh liquid was mixed with the femen, I was fure of putting the fpherical corpufcula in motion, or rather of increafing their motion; but, if the liquid was ardent or corrofive, inftead of being increafed, it was deftroyed.

Having at that time water newts, I cut the tefticles of a male in pieces, and expreffed the fluid which was thick and glutinous. Applied to the microfcope, the appearance changed: there was an immenfe number of long flender corpufcula. Some were extended in a ftraight line, others curved, fome folitary, others entangled like a fkein of thread. I examined thofe that were fingle as the moft eafily diftinguifhed. Each corpufcle was compofed of a body and a very long appendage, fig. 6. They moved with difficulty, the greater part of the body being immerfed in the vifcous viſcous ſubſtance. Being diluted with common water, they all began to traverſe the liquid. As it was at perfect reſt, and no external cauſe appearing to act upon the corpuſcula, I was inclined to think this motion ſpontaneous and peculiar to them. My opinion was afterwards confirmed by diſcovering the efficient cauſe of motion. With ſtedfaſt attention, two rows of minute points on the ſides of the appendage were obſerved moving like moſt minute oars, fig. 7. and then it was that the ſituation of the corpuſcle changed; but when their motion ceaſed, it alſo ceaſed to move.

When the mixture of ſemen and water dried up, the motion of the corpuſcula was irrecoverably loſt, though again wet with freſh fluid.

Similar experiments were repeated on ſemen taken from the teſticles of other newts, and with the ſame reſults; but, on diluting it, the corpuſcula often collected in numbers, placed themſelves parallel to each other, and bent 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, they began to revolve round a common center like a reel, and continued for ſome time.

I found corpuſcula not only in the teſticles of newts, but alſo in the vaſa deferentia. Theſe veſſels reſemble two very white little pipes, running along rhe the middle of the ſpine. One end is fixed near the head of the animal; the aperture of the other is evidently in the cloaca, through which the excrement paſſes. The veſſels are always full of ſemen, particularly while the males fecundate the eggs of the females. The ſemen is very white, like milk, and the number of corpuſcula it contains is ſo great that the fluid part is ſmall compared with the maſs they form. They are perfectly ſimilar to thoſe in the ſeminal fluid of the teſticles; with this difference, however, that neither water nor any other liquid is required to increaſe their motion. In the ſemen, they naturally move with the quickneſs of the corpuſcula in fluid from the teſticles diluted with water. The corpuſcula of the vaſa deferentia retain motion much longer than thoſe of the teſticles; yet it is far from equal to that of the human corpuſcula, thoſe of the horſe and other animals. I have always found them in male newts in every ſeaſon of the year.

By expreſſing the ſeminal fluid from the teſticles of frogs, it is viſibly full of corpuſcula. They are infinitely ſhorter than thoſe of the newts. They have progreſſion, and in advancing make gentle vibrations. They are of a long elliptical ſhape, and very ſoon ceaſe to move, fig. 8. Plate 2.

CHAP.

CHAP. II.

THE PRECEDING OBSERVATIONS COMPARED WITH THOSE OF LEEUWENHOECK.—CONFUTATION OF SOME OPINIONS CONCERNING THE NATURE OF SEMINAL VERMICULI.

AFTER this courſe of obſervations on the ſeminal fluid of man and different animals, I determined to read and conſider what Leeuwenhoeck and Buffon had written, two authors who, more than all others, had made this ſubject their particular ſtudy. Several years had elapſed ſince I read their diſcoveries on 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 prepoſſeſſion for the obſervations of others.

I ſhall begin with Leeuwenhoeck; and that the reader may have before him the real ſentiments of this naturaliſt, and that he may compare

pare his obſervations with mine, it is neceſſary to tranſcribe ſome of the chief paſſages where he ſpeaks of ſpermatic vermiculi. M. de Buffon has, before me, employed part of the paſſages to compare his own obſervations with thoſe of this eminent obſerver,—and here I have the pleaſure of following his example.

The excellent Dutch microſcopiſt ſent a communication of his diſcoveries on the human ſemen to Lord Brounker, Preſident of the London Royal Society, 1677. "Poſtquam excellentiſſimus dominus Profeſſor Cranen me viſitatione ſua ſaepius honorabat, literis rogavit domino Ham. Cognato ſuo, quaſdam obſervationum mearum videndas darem. Hic dominus Ham me ſecundo inviſens ſecum in lagenula vitrea ſemen viri gonorrhœa laborantis, ſponte diſtillatum attulit, dicens ſe poſt pauciſſimas temporis minutias (cum materia illa jam in tantum eſſet reſoluta, ut in fiſtulae vitreæ immitti poſſet) animalcula viva in eo obſervaſſe, quæ caudata, et ultrá 24 horas non viventia judicabat. Idem referebat ſe animalcula obſervaſſe mortua poſt ſumptam ab aegroto terebinthinam. Materiam praedicatam fiſtulae vitreae immiſſam, praeſente Domino Ham obſervavi, quaſdam in ea creaturas viventes; at poſt decurſum 2, aut 3 horarum eandem ſolus materiam obſervans, mortuas vidi.

Eamdem

Eamdem materiam (ſemen virile) non ægroti alicujus, non diuturna conſervatione corruptam, vel poſt aliquot momenta fluidiorem factam, ſed ſani viri ſtatim poſt ejectionem ne interlabentibus quidem ſex arteriæ pulſibus, ſæpiuſcule obſervavi, tantamque in ea viventium animalculorum multitudinem vidi, ut interdum plura quam 1000 in magnitudine arenæ ſeſe moverent; non in toto ſemine, ſed in materia fluida craſſiori adhaerente ingentem illam animalculorum multitudinem obſervavi; in craſſiori vero ſeminis materia quaſi ſine motu jacebant, quod inde provenire mihi imaginabar, quod materia illa craſſa ex tam variis inhæreat partibus, ut animalcula in ea ſeſe movere nequirent; minora globulis ſanguini ruborem adferentibus hæc animalcula erant, ut judicem millena millia arenam grandiorem magnitudine non aequatura(1). Corpora eorum rotunda, anteriora obtuſa, poſteriora ferme in aculeum deſinentia habebant; cauda tenui, longitudine corporis quinquies, ſexieſve excedente, et pellucida, craſſitiem vero ad 25 partem corporis habente praedita erant, adeo ut ea quoad figuram cum cydaminis minoribus longam caudam habentibus optime comparare queam: motu caudae ſerpentino, aut ut anguillae in aqua natantis progrediebantur; in materia vero aliquantulum craſſiori caudam octies, decieſque quidem evibrabant antequam latitudinem capilli

(1) Leeuwenhoeck's calculations of the minuteneſs of animalcula are generally received: but in my opinion they are very liable to objection.—T.

capilli procederent. Interdum mihi imaginabar me internoſcere poſſe adhuc varias in corpore horum animalculorum partes, quia vero continuo eas videre nequibam, de iis tacebo (1)".

These obſervations were accompanied by others, written by Leeuwenhoeck to the Secretary of the Royal Society, 1678. He compoſed them becauſe ſome perſon had ſuggeſted to him to examine the ſeminal fluid of animals. "Si quando canes coeunt," Leeuwenhoeck anſwers the Secretary, "marem a fæmina ſtatim ſeponas, materia quædam tenuis, et aquoſa (lympha ſcilicet ſpermatica) e pene ſolet paulatim extillare; hanc materiam numeroſiſſimis animalculis repletam aliquoties vidi, eorum magnitudine, quæ in ſemine virili conſpiciuntur, quibus particulæ globulares aliquot quinquagies majores permiſcebantur.

"A cuniculorum coitu lymphæ ſpermaticæ guttulam unam, et alteram e fæmella extillantem examini ſubjeci, ubi animalia praedictorum ſimilia, ſed longe pauciora comparuere."

In the ſame year, 1678, Leeuwenhoeck alſo communicated to the Royal Society the animalcula he had found in the ſemen of the dog.---- "Seminis canini tantillum microſcopio applicatum iterum contemplatus ſum, in eoque antea deſcripta animalia numeroſiſſima conſpexi. Aqua

 pluvialis

(1) Philoſophical Tranſactions, No. 141, p. 1041.

pluvialis pari quantitate adjecta, iisdem confestim mortem accersit. Ejusdem seminis canini portiuncula in vitreo tubulo unciæ partem duodecimalem crasso servata, sex, et triginta horarum spatio, contenta animalia vita destituta pleraque, reliqua moribunda videbantur."

He confirmed his discoveries during the following years, and made additions to them. In a letter to Mr Wren, 1683, he thus expresses himself respecting the seminal vermiculi of frogs: "Hic animalculorum numerus erat tantus, ut credere subiret ad quodvis fæmellæ ovulum a masculo emitti forte 10000 talium animalculorum, quæ in semine ejus continentur." And in the year 1699, writing to the Royal Society on his theory of seminal vermiculi, some of which he believed male and some female, he proceeds: "Si porro his addamus, quod me antehac in observationibus meis animadvertere censui inter animalcula ex semine virili quædam apparuisse, quæ aliquantulum ex se mutuo differre videbantur, unde concludere non verebar, alterum genus mares, alterum vero fæmellas repraesentare, atque si cogitemus idem in omnibus seminibus masculinis locum habere, nullus video, &c." There is another passage coinciding with this: "Sed jam ubi etiam in seminibus masculinis animalium, avium, piscium, imo etiam insectorum reperi animalcula, multo certius esse statuo, quam antea,

hominem

hominem non ex ovo, ſed ex animalculo in ſemine virili oriri; ac praeſertim cum reminiſcor, me in ſemine maſculino hominis, et etiam canis vidiſſe duorum generum animalcula. Hoc videns mihi imaginabar, alterum genus eſſe maſculinum, alterum fæmininum."

In 1701, Leeuwenhoeck wrote to the Royal Society in theſe words: "Die Julii 27, circa horam nonam veſpertinam accepi teſticulos juvenis arietis. Cum vero lanius hiſce teſticulis primam detraxiſſet cutim, ſeu membranam, ego viciſſim quoque eos altera privavi membrana, ut hac ratione vaſa ſeminifera nuda viſui expoſita jacerent. Primo ergo aperui vaſa ſeminalia in teſticuli parte exteriori ſita, iiſque exemi ſemen maſculinum (quod nudo obſervatum oculo album repræſentabat colorem) illudque microſcopio appoſui, atque hoc pacto oculo admovi, quando mihi animalcula ſeminalia tam ſtupendo apparuere numero, ut vix fidem apud quemquam, niſi teſtem oculatum, inventurus ſit. Haec animalcula nubium in morem integris agminibus inter ſe vagabantur, natitabantque, quorum multa eodem tendere natatu videbantur, ut mox aliquot millena ſeſe ab uno agmine ſeparantia alteri ſeſe agmini adjungebant, illudque ſequi videbantur."

A little lower he adds, "Haec vero animalcula nuper a me obſervata caudas habent juxta corpus craſſiores, atque ſenſim fiunt tenuiores,

 adeo

adeo ut earum extremitates ubi materia, cui animalcula insunt, atque innatant, paulo densior est, visum plane effugiant; atque sic horum animalculorum caudae fabrica plane convenit cum omnium piscium caudis."

Respecting the nature and properties of spermatic vermiculi, these are the words of Leeuwenhoeck: "Quotiescumque animalcula in semine masculo animalium fuerim contemplatus, attamen illa se unquam ad quietem contulisse, me nunquam vidisse mihi dicendum est, si modo sat fluidae superesset materiæ, in qua sese commode movere poterant; at eadem in continuo manent motu, et tempore, quo ipsis moriendum, appropinquante, motus magis, magisque deficit, usque dum nullus prorsus motus in illis agnoscendus sit (1)."

From these quotations, it may be easily seen, that Leeuwenhoeck and myself have remarked the same facts in the animals of human semen. This observer calling them animalcula or spermatic vermiculi. We both agree, 1. on assigning the same figure to the corpuscula in the seminal fluid of man, the ram, the dog, and rabbit. In my description of them, I have said they seemed composed of two parts, a body and an appendage. Leeuwenhoeck also acknowledges the existence of these parts,

(1) Leeuwenhoeck Opera, vol. 1.

parts. 2. That we agree concerning the ſize of the body, the length, the figure, and proportions of the appendage, will farther appear from the deſigns he has given of the ſpermatic vermiculi of man and other animals. 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 alſo allowed them the ſame properties, and ſaid their motion in ſwimming was ſerpentine like that of eels, and uninterrupted unleſs towards the termination of life: that the progreſs of the vermiculi in the groſſer parts of the fluid met with great oppoſition. But I have obſerved all this in the preceding chapter. We have both remarked one ſingularity, that rain water deprived the canine vermiculi of motion. I have likewiſe found the ſame effect on thoſe of the ram and man even from other kinds of water, as dunghill, ice, ſnow, and river water. I have conſtantly remarked, that when motion ceaſes, the appendage never encircles the body, but always remains extended in a ſtraight line, or in one very little curved; a fact obſerved by Leeuwenhoeck, as appears from his engravings of the 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.

Therefore 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 preciſely the beings which Leeuwenhoeck terms worms or ſpermatic animals. The laſt appellation ſhall alſo be uſed, not only to ſpeak in the language of this naturaliſt, but becauſe I eſteem the facts related ſufficient authority to beſtow the name of animals on them. Spontaneous motion and contorſions of the body, by means of which they move from one place to another, are characteriſtics ſufficiently deciſive of animality. Of this we ſhall have more concluſive evidence in the ſequel.

What has been ſaid is enough to ſhow how erroneous the opinion of Sir Charles Linnæus muſt be, when he maintains that the vermiculi are only particles of inert matter ſuſpended in the fluid, and put in motion by heat. As the celebrity of the Swediſh naturaliſt might induce us to ſuppoſe that he does not advance this without foundation, the reaſons which made him determine to adopt his ſentiments ſhould be mentioned; and that none of their force may be loſt, I ſhall give them, ſuch as they have been written by the author himſelf in a Latin Theſis, while he was Preſident 1759, entitled Generatio Ambigena. "Vermiculos ſeminales Leeuwenhoeckii vivos eſſe vermes, in omni genitura prolifica maris praeſentes, ad noſtra tempora firmiter ſatis credi-

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dit orbis eruditus. N. D. Præſes Lugduni Batavorum 1737, commoratus curioſorum quorundam amicorum, et commilitonum utebatur conſortio, quales erant J. Fr. Gronovius Florae Virginicæ auctor, hodie Conſul Leidenſis; D. V. Swieten, hodie liber Baro, et Archiater Imperatoris; Iſaac Lawſon, piæ memoriæ Scotus, Med. exercitus Angliae. D. Lieberkuhn, p. m. Berolinenſis; D. Kramer, auctor libri artis Docimaſticæ; Joh. Bartſch, p. m. Regiomonte Boruſſus, med. Surinamenſis; et D. Abrah. Ens, Pomerano-Petropolitanus. His igitur quodam die congregatis, oſtendebat D. Lieberkuhn præſtantiſſima ſua microſcopia, quem rogabat N. D. Præſes, ut horum ope vermiculos in cane obſervandos praeberet, quod ſtatim impetravit; contemplabatur illos adcurate, atque inſectorum naturæ gnarus, ſtatim vermiculos hoſce Leeuwenhoeckianos non eſſe corpora organis praedita, et animata, atque adeo neque inſecta, neque vermes, ſed particulas motas, quarum motus a calore dependeret liquoris, rotundo ore exclamat. Præſentes omnes attenti hos intuebantur, et oculis ſuis alii credere, alii vix quidem videbantur. Concluſionem hujus rei in diſſertatione de Sponſaliis Plantarum, anno 1746, page 24, edidit. N. D. Præſes his quidem verbis. ‘ Vermiculi iſti Leeuwenhoeckiani minime ſunt animalcula proprio, et voluntario motu gaudentia, ſed corpuſcula inertia, quæ calidæ genituræ innatant, non ſecus ac particulae oleoſæ, quod ſelecta Lieberkunii microſcopia nobis manifeſte oſtenderunt.' Hoc poſtea etiam vidit, et confirmavit ſummus Phyſiologus illuſtris D. V. Hallerus, ut adeo auctoritas vermium ſeminalium jam prorſus fere in deſuetudinem venerit."

This ſingular opinion of Linnaeus was unknown to me until communicated in a letter from M. Bonnet, who tranſcribed his words. He, undoubtedly from politeneſs, or perhaps with a deſign to encourage me to proſecute my reſearches on ſpermatic vermiculi, added the following paragraph: 'You perceive there is here cited 'the eminent teſtimony of a Gronovius, a Van 'Swieten, a Lieberkuhn, &c. and even that of 'Haller. At ſome future period, I ſhall write to 'him about it; nevertheleſs, all theſe authorities 'could not weigh with me, nor, in my view, ba'lance your opinion, becauſe I know you to be 'a much better judge of ſuch things than the il'luſtrious authors Linnaeus names in his theſis. 'You have paid much more attention to the 'vermiculi in queſtion, and long ſtudied the 'animalcula analogous to them; you have thus 'obtained a kind of touchſtone, which experi'ment more and more improves, and which can 'never prove deceitful.'

Before

Previous to deep consideration of what was advanced by Linnaeus, I could not dissemble my extreme surprise to see Haller cited as one of those who denied the existence of spermatic vermiculi, whereas he had always been one of their most strenuous supporters. His annotations on 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 tract, I shall have occasion to use the authority of so great a physiologist.

We see how easily the opinion of the celebrated Upsal botanist is established; scarcely has he viewed Leeuwenhoeck's animalcula when he decidedly pronounces they are not animals. I leave it to the judgment of the learned and impartial reader, Whether a hasty glance of the vermiculi, and of only one species, is sufficient for positively deciding their nature, and deciding it better than Leeuwenhoeck, 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 that naturalists have bestowed in ascertaining the nature of certain organised bodies doubted whether to belong to the class of animals or plants. Yet these bodies were not, like seminal vermiculi, microscopic animals; their size admitted of them being completely manipulated, and easily seen with the eye. Linnæus should

ſhould have been better convinced, he who, with reiterated examinations and infinite patience, has characteriſed ſuch a number of bodies in the three kingdoms of Nature. If his laborious and uſeful occupations had left him ſufficient leiſure to penetrate the world of *inviſibles*, where, as the illuſtrious Muller obſerves, one may well be a ſtranger without a crime (1); and had he applied to this ſubject with that diligence and acuteneſs which he has diſplayed in his diſcoveries in the viſible world, we cannot doubt that he would have omitted in his theſis the quotations we have juſt made; for, with an attentive view of the ſeminal vermiculi, he would eaſily have ſeen that they do not ſwim in the liquid like oleaginous particles; but at ſome depth, I have had eminent proofs of this. Penetrating the firſt ſtratum of the dog's ſemen with the microſcope, I preſſed it downwards ſo as to reach the lower parts; in each an equal number of vermiculi were viſible. The ſame method was adopted with

(1) *In Mundo Inviſibili, de quo Diſſertationem dedit, hoſpes.* In fact, as far as is known, this very learned naturaliſt never employed himſelf in contemplating inviſible animals, and his doubt whether infuſion animalcula were living beings, or rather *oleaginous* or *ſaline* particles, (a propoſition diſgraceful to our days,) forcibly convinces one of it.

with a confiderable quantity in a watch-glafs. Wherever the microfcope penetrated, the motion of vermiculi was feen. A third experiment has demonftrated their prefence at a great depth. A thin-fided chryftal tube, half a line in calibre, and five inches in length, was filled with femen. I held it perpendicular; and, in this pofition, applied a magnifier to the fides, the great tranfparency of which permitted me to fee the included femen clearly, when the tube was interpofed between the fun and my eye. Wherever the lens was applied, whether to the top, bottom, or middle of the tube, I always faw vermiculi. The experiment was repeated with a tube of much greater capacity; it was a third of an inch in diameter within, and at leaft four inches long, but the opacity of the fluid prevented me from feeing the vermiculi it contained. However, the bottom, being infecurely ftopped, allowed fome drops to exude; all were equally full of vermiculi as the liquid in the higher part of the tube. The confequences were the fame on repeating the experiment with the femen of the dog and other animals.

In the fecond place, had the Swedifh naturalift beftowed on feminal vermiculi the attention they deferve, he muft have perceived they are not inert corpufcules, but poffeffing a fpontaneous motion well characterifed; that it is regulat-

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ed by the mode in which they advance; that they ſwim in the ſpermatic lymph, contorting and vibrating their parts like many other aquatic animals; he would not have aſcribed their motion to the heat of the ſemen, ſince, when this is gone, and the heat of the atmoſphere only remains, which happens a little after the ſemen comes from the animal, the vermiculi do not ceaſe to move; but their motion continues for a limited time; ſometimes ſeveral days when they are included in ſmall tubes.

In ſhort, the ſpermatic vermiculi of frogs, fiſhes, and newts, completely refute the opinion of Linnaeus. Their ſeminal fluid being deſtitute of every ſenſible principle of internal heat, as it conſtitutes part of cold blooded animals, heat cannot here be the cauſe of the motion of vermiculi, on the contrary, they ſhould abſolutely be motionleſs.

All this evinces how much two modern naturaliſts, Valmont de Bomare, and Erneſt Aſch, are deceived in thinking ſeminal vermiculi do not exiſt in nature, 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 obſervation. A ſimilar objection had been ſtarted, after Leeuwenhoeck's diſcoveries, who was content with remitting its authors to their ſtudies. 'Dominos 'illos

'illos nondum eo uſque profeciſſe, ut res recte 'obſervare valeant.' I muſt be pardoned if I make the ſame anſwer to theſe new opponents; for we muſt ſay their obſervations have been very unfortunate, their viſion very bad, their microſcopes good for nothing, or their inaccuracy and inexperience very great in the art of obſervation. It is true M. de Bomare aſſerts that his eyes are very good, and his microſcopes excellent, and does not heſitate to ſay, 'We have repeated all the experiments of animalculiſts on ſemen; and although our eyes are 'very good, and our microſcopes excellent, we 'have been able to diſcover nothing (1).' I was almoſt prompted to exclaim, May heaven preſerve us from ſuch good eyes and ſuch excellent microſcopes; for, with them, we ſhould be in great hazard of regarding as illuſions, the moſt beautiful microſcopical diſcoveries which philoſophers have hitherto made; and we ſhould have to dread that we and our poſterity would be forced to relinquiſh the proſpect of making new ones! With this acuteneſs of viſion, and this perfection of microſcopes, we ſhould have to reviſit the ignorance of our anceſtors in the world of inviſibles. But I would rather believe M. de Bomare's

(1) Bomare Dictionnaire. Art. Semence. T. 10. p. 420. Edit. d'Yverdon.

Bomare's ſight is truly very acute, and that his microſcopes are the moſt perfect. What reaſon can we then aſſign for the unſucceſs of his attempts to ſee any thing in ſemen with all this aſſiſtance? The concluſion is ſimple. When I preſume to affirm his experience is not infinite in the obſervation of ſuch objects, 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 that juſtice. But no one knows that he has been, or that he may be, a microſcopical obſerver. To make microſcopical obſervations with accuracy, many natural and acquired qualities are requiſite, and many more are neceſſary to guard againſt being deceived in the ſubtile reſearches after beings of infinite minuteneſs.

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 made obſerve the ſemen of man and different animals. Some, though all had diſtinguiſhed merit in other purſuits, were certain they ſaw nothing, notwithſtanding they looked long at a time through the microſcope, and even returned often to obſervation; and the number of theſe was the greateſt. Others, after

very

very long and painful examination, feemed to perceive an indiftinct obfcure fermentation in the fluids. A few could fee the body or corpufculum of the vermiculi, but were unable to diftinguifh the appendage: and very few could perceive their form and all their motions. Thefe laft, indeed, were well accuftomed to ftudy microfcopical objects, and might reafonably be called profeffional obfervers. Let thofe who deny the exiftence of fpermatic vermiculi endeavour to enter this clafs, and practife on minute objects, and I affure them, if they repeat their experiments on the feminal fluid, they will fee vermiculi as I and Leeuwenhoeck, with many other naturalifts, have feen long before. Then, if they chufe to communicate their obfervations, they will enjoy one advantage: if their previous publications excite the compaffion of the learned, their future works may perhaps merit an eulogium.

CHAP.

CHAP. III.

A BRIEF ACCOUNT OF M. DE BUFFON'S OBSERVATIONS ON SEMINAL VERMICULI.—CRITICAL REFLECTIONS ON THESE OBSERVATIONS.

WE defcend from the obfervations of Leeuwenhoeck to thofe of Buffon, which, although very numerous, comprehenfive, and fpecific, we fhall but abridge. With the compound microfcope he obferved the fluid from the feminal veffels of a dead human body (1). It was full of filaments, moving about and branching into many parts. The filaments fwelling burft, and many ovular corpufcula efcaped, which ftill remained attached to the filaments by a thread; then they ofcillated like a pendulum, and during the ofcillations the thread extended. The corpufcula, at length detached, traverfed the moft fluid part of the femen along with their filament, the extreme length of which impeded their motion, and they feemed to him to endeavour to free themfelves of it. Having diluted the femen with rain water, the microfcopic view was better defined. It clearly appeared that each ovular corpufculum had

(1) Hiftoire Naturelle, tom. 3. edit. 12.

a double motion, oſcillation, and progreſſion. In two or three hours the ſeminal matter acquired greater fluidity: the filaments diſappeared; the number of corpuſcula increaſed; the threads contracted; oſcillation relaxed; and progreſſive motion increaſed. In five or ſix hours, the corpuſcula, having loſt the threads, reſembled animals more than ever; not only becauſe their quickneſs in ſwimming increaſed, but becauſe their courſe was directed to every quarter. The ſize and figure of ſeveral ſeemed to change. In twelve hours, the activity of the corpuſcula was great, and ſome revolved upon their axis; others changed from an ovular to a globular figure, under the obſerver's eye. Several divided aſunder, ſo that one formed two. After one day, the number diminiſhed; and on the third, none were to be ſeen.

In other ſemen, which appeared entirely filamentous, the ovular corpuſcula did not proceed from the filaments, but theſe dividing were metamorphoſed into corpuſcula. They were embarraſſed by a thread: the longer it was, the more it impeded their motion; but it gradually ſhortened, and was at laſt completely deſtroyed.---- Their figure then reſembled that of infuſion animalcula: they ſwam with a progreſſive motion, though the thread at firſt confined them to ſimple oſcillation.

Buffon having examined a new drop of femen, ten or twelve hours after coming from the animal, he faw the whole corpufcula proceed in crowds from the fame fide where there was a net of filaments which continually produced them. The fize of the net gradually diminifhed, and at length appeared lefs by one half.

In his firft obfervation, the author thought the corpufcula gradually became fmaller, at the fame time he was not certain; however, another obfervation convinced him of it.

He then obferved the feminal fluid of a dog. It was clear, and without filaments. The ovular corpufcula almoft refembled thofe in the human femen completely, only he found them more active and lefs numerous. On the fourth day, very few had threads.

In another portion of the feminal fluid of the fame dog, befide this, he faw corpufcula proceeding from a mucilaginous fubftance in the femen, that feemed internally animated by an inflating motion, which induced him to think the mucous fwelled in fome parts forming little tumours. All thefe corpufcles were without a thread. The figure of fome changed: they extended, contracted, and were inflated; and, amidft thefe wonderful operations, divided afunder, each giving exiftence to two corpufcula, whofe figure and properties

properties were ſimilar to thoſe of the generating corpuſcles.

The French naturaliſt extended his experiments to rabbits. From the ſeminal veſſels of one, he took the fluid, and, mixing it with 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, as their ſize diminiſhed, their number and activity increaſed; their figure alſo varied, becoming ovular, ſpherical, and elliptical.

He repeated the experiment on the ſemen of another rabbit ejected at the moment of copulation. Ovular corpuſcula were found, ſome with a filament, others without it: the former greatly reſembled thoſe of the dog and of man, except in being leſs, more entire, and the thread ſhorter. He could not be certain whether he ſaw real filaments, or only faint furrows formed in the liquid by the courſe of the corpuſcula.

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

M. De Buffon alſo examined the ſeminal fluid of ſome fiſhes: of the carp, the pike, and barbel, procuring it while the animals were alive. There, he found many corpuſcula in motion, of a very dark colour, almoſt black, and extremely ſmall.

Such is an abbreviation of the obſervations made on the ſemen of animals by the illuſtrious author of *Natural Hiſtory;* whence he draws a general concluſion, that the corpuſcula examined and deſcribed by Leeuwenhoeck a century ago cannot, with the Dutch obſerver, by any means be termed ſpermatic vermiculi, becauſe they poſſeſs no characteriſtics of animality. The labour experienced in drawing along their tails, in diveſting themſelves of them, in changing their figure ſo often to form anew, before the obſerver's eyes, their diviſion into parts, and diminution of ſize, ſeem to him peculiarities incompatible with animality. On the other hand, not being able to ſay they were bodies perfectly inert, becauſe he had really ſeen ſigns of animation, he inclines to conſtitute them into a particular claſs under the name of *organic molecules*, which are particles diſſeminated throughout matter, original, incorruptible, animated, and always active. Nor does he heſitate to confide the formation of the winged univerſe to theſe molecules.

Here I enter not into a diſcuſſion on organic molecules, but occupied ſolely with the facts which M. de Buffon relates, I ſincerely regret that the eſſential difference between his account, and what I have myſelf ſeen, has made a deep impreſſion on my mind. It is not that I wiſh to flatter myſelf

ſelf my obſervations are of more value than his, from exactneſs of execution or aſſiduity in continuance. If it may be allowed, I ſhall even ſay they may be preferred from being more numerous. At the ſame time, M. de Buffon's perfect conviction of the truth of his obſervations: his great confidence that the readers who repeat them will find them ſcrupulouſly exact: the natural and decided manner in which he oppoſes them to thoſe of Leeuwenhoeck; and the errors he reproaches him with: all made me judge it poſſible that the Dutch microſcopiſt and myſelf might be deceived. And this was aided by a conſideration which, though foreign to the ſubject, is plauſible; I mean the great reputation and celebrity the French naturaliſt deſervedly enjoys. I long heſitated whether to proſecute my obſervations, and ſubject them to as rigorous an examination as might be poſſible, or whether it might be more proper to abandon them leſt they ſhould 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 ſeminal vermiculi of various animals. I replied, it had already been partially done, but my labours had been ſuſpended on 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 correspondent, that I am not much surprised to find you in opposition to M. de Buffon with regard to spermatic vermiculi; nor do I forget, what he has in some measure told us, and which I have repeated after him, that his theory preceded his observations. Now you know as well as I do, that a favoured theory is a mirror which changes the appearance of objects.

'Fear not that M. de Buffon's authority will in the least invalidate the truth of your discoveries on seminal vermiculi. You have proved yourself an excellent observer, and acquired the right of being believed. You have cherished no theory, but are satisfied with interrogating nature, and giving the public a faithful account of her responses. Philosophers will always listen to you; and they will esteem your observations so much the more certain that you prove yourself to possess the art of observation.'

These obliging invitations encouraged me to draw my observations from the obscurity in which I had left them, and to continue increasing them as much as my humble talents would admit. Without interruption, this was my chief employment for the greater part of three years. But the various facts gradually discovered, and of which an abbreviated narrative shall be given, little correspond

respond with thoſe 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 him to think as he does.

One of the principal phenomena which the French author conſiders as the chief baſis of his hypotheſis, is the formation of the ſpermatic vermiculi, being derived from the mucilaginous parts of the ſemen and its filaments, which are tranſmuted into theſe animated beings under the obſerver's eye, as he has found in the ſemen of man and the dog. Mr Needham readily embraces this opinion: he ſuppoſes the vermiculi do not exiſt in the ſemen while in the body of the animal, but are formed ſome minutes after coming from it, and when beginning to decompoſe and change by the influence of the air (1).

In my experiments on ſemen, related in the firſt chapter, this formation is not mentioned, becauſe I had not ſeen the ſmalleſt indication of it. Whether the vermiculi were obſerved at firſt, or not till the ſemen ſettled, I never perceived the groſs or filamentous parts give exiſtence to them. 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,

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(1) New Microſcopical Diſcoveries,

as it has ſince been in my other experiments. I have therefore directed the utmoſt obſervation to what happened in the ſolid or filamentous part of the ſemen as it diſſolved, but I never could ſee the preſent production of vermiculi, and have even inconteſtible evidence of the reverſe. In the mean time, the origin of the French naturaliſt's error has been diſcovered, which will be demonſtrated by ſome facts that I muſt be permitted to detail.

From human ſemen, as yet but partially diſſolved, I took two little clots forming a net of filaments, and began to conſider them with the utmoſt attention. The vermiculi included in the filaments occaſioned a motion; the filaments diſſolved before me, and the two clots in a few ſeconds became two drops of ſemen. It was with ſingular ſurpriſe I ſaw the few vermiculi in theſe drops compared with the numbers obſerved in others much ſmaller, and not formed as they were by diſſolution of coagulated ſemen, but of a portion found fluid in the ſeminal veſſels.

The experiment was repeated on a clot leſs diſſolved than the former, and ſtill fewer vermiculi found. Then it began to occur that they perhaps did not inhabit the groſſer parts of ſemen, but the more fluid. The ſuſpicion was ſtrengthened on ſeeing all the corpuſcula periſh when the fluid evaporated. But that the fact might

might be verified or confuted, 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 are commonly immerſed in the fluid parts, even while in the ſeminal veſſels. Theſe trivial obſtacles were at length overcome. With the extremity of a pair of ſmall pincers, I took a portion of human ſemen, ſimilar to coagulated milk, from the ſeminal veſſels: as it was moiſt, I drew it along a dry piece of glaſs that it might depoſit its humidity, and then put it into a watch-glaſs, attending its diſſolution in order to apply the microſcope. It was not without vermiculi. Compared with another portion of the fluid part, taken from the veſſels, there was no proportion; the number in the ſolid was ſo much ſmaller than in the fluid parts.

Theſe reſults did not ſatisfy me: the few vermiculi in the ſolid ſemen might be owing to ſome little portion of fluid remaining along with it; and, inſtead of finding few, I could have wiſhed to find none, or almoſt none. Having taken another clot of human ſemen from the veſſels, I endeavoured, as far as poſſible, to diſengage it from the fluid that might remain in and about it. Here we ſhould obſerve, *en paſſant*, that this operation of drawing along the ſolid part, to take away the fluid, ſhould be performed with

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great adroitneſs and celerity, otherwiſe the ſolid part gets time to diſſolve during expoſure to the air; and by drawing it long or ſlowly over the glaſs, inſtead of drying, it always becomes more humid. I ſucceeded in freeing a clot of ſemen from all ſenſible moiſture. I put it in a watch-glaſs; and, when diſſolved, examined it with the microſcope. The truth is, that here were found no vermiculi, nor were any diſcovered in other clots treated 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. Repetition of ſo important an obſervation was not delayed. In my journal of experiments, I find this has been done fourteen times; in thirteen, the diſſolved clots exhibited no vermiculi, and only once a ſmall number in the coagulated clot.

The ſeminal fluid of the rabbit is always partially coagulated; therefore, whenever taken from the veſſels, it afforded the means of repeating my experiments. But I have never diſcovered vermiculi when the ſpermatic lymph could be completely taken away. Theſe united facts convince me that the natural habitation of the vermiculi is in the fluid part of ſemen. The ſame facts aſcertain the degree of credibility we ſhould beſtow on what M. de Buffon ſays of their formation. It is evident, that far from being generated by the

the ſolid or filamentous part of the ſemen diſſolving, they even do not attach themſelves to it, as has been demonſtrated. When they are found there, it is on account of the fluid part, which is their natural abode, mixing with the ſolid. This is doubtleſs the cauſe of M. de Buffon's error. He ſaw the thick and filamentous parts in motion, and, amidſt the agitation, vermiculi proceeded from it: he even obſerved the number increaſe, in proportion as the groſs and ſolid parts decreaſed; and remarked that the number was greateſt when the filaments had entirely diſappeared. Allowing himſelf to be deceived by theſe appearances, it was eaſy to believe the decompoſition of the filaments was the productive cauſe. But, in truth, the vermiculi pre-exiſted in the filaments; they were concealed and enveloped in the parts immerſed in the ſeminal fluid, and only when diſengaged did 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, carried it to the fire, as the ice 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 before diſſolution of the ſemen. He ſhould firſt have examined the fluid part, which, on on opening the feminal veffels, we find mixed with the folid. There he would have found abundance of vermiculi, though at that time they could not be produced by the folid femen diffolving, fince this folid part only begins to diffolve after it proceeds from the animal, and experiences the influence of the air.

What has already been faid, and what I fhall continue to fay, proves the falfity of Needham's opinion, who affures us the vermiculi are produced fome minutes after the femen comes from the animal, that is, after it begins to be altered and decompofed by the air. Refpecting the human femen, it is neceffary to confider whether the portion is folid or fluid. If the former, when completely deprived of the fpermatic fluid, no vermiculi are feen, although it remains during fome time expofed to the air, and though it changes and is decompofed. If the latter, vermiculi appear in it before the time required for this alteration. It has often happened, that the time confumed in taking the fluid matter from the veffels, ftill warm, did not exceed a fecond, yet I found the fame number of vermiculi as afterwards, even when the fluid had been long enough expofed to the air to effect its decompofition.

My obfervations, on the femen of other animals, further convinces me of the falfity of fuch

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an opinion. I prepared a ram, alive and vigorous, ſo that a friend cut the epididymis, which is uſually full of ſemen, and while he cut it, one might take a drop of ſemen, and immediately preſent it to the microſcope where my eye was kept ready fixed. It may well be ſeen that the time in performing the operation could not be ſhorter. I ſaw numbers of vermiculi, and all very vivacious. This experiment was repeated on the ſemen of a newt, and I ſaw the ſame. In ſpring, when the vaſa deferentia are full of ſemen, it was unneceſſary to open the animal to obtain it for obſervation; on gently preſſing the belly, it eſcaped by the anus where the two veſſels terminate.

This animal has afforded me proof ſtill more deciſive: I let the males ſuffer hunger ſo long that they became much emaciated; the vaſa deferentia then contained a very ſmall quantity of ſemen, and from the tranſparency of the tunics of the veſſels, now rendered very thin, it might eaſily be obſerved with a magnifier. Opening the abdomen, and applying the microſcope to the veſſels, without affecting or deranging their ſituation, we ſaw vermiculi ſwimming in the fluid, and they were viſible to thoſe leaſt ſkilled in the art of obſervation, becauſe, as it has been remarked in the firſt chapter, they were infinitely longer than others.

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

END OF THE FIRST VOLUME.

Eng.d by Kirkwood