TREATISE

ON THE

VENOM OF THE VIPER;

ON THE

AMERICAN POISONS;

AND ON THE

CHERRY LAUREL,

AND SOME OTHER

VEGETABLE POISONS,

TO WHICH ARE ANNEXED,

OBSERVATIONS ON THE PRIMITIVE STRUCTURE OF THE ANIMAL BODY; DIFFERENT EXPERIMENTS ON THE REPRODUCTION OF THE NERVES; AND A DESCRIPTION OF A NEW CANAL OF THE EYE.

WITH TEN DESCRIPTIVE PLATES.

TRANSLATED FROM THE ORIGINAL FRENCH OF

FELIX FONTANA,

NATURALIST TO HIS ROYAL HIGHNESS THE GRAND DUKE OF TUSCANY, AND DIRECTOR OF HIS CABINET OF NATURAL HISTORY.

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PREFACE

OF THE

FRENCH EDITOR.

THE Firſt Part of this Work was publiſhed in Italian in 1765. Monſieur Darcet, a celebrated Phyſician at Paris, thought it of ſo much importance, that he tranſlated it into French a ſhort time after. Accidental circumſtances delayed the publication of this Tranſlation. The Authour came to Paris in 1776, and gave M. Darcet ſeveral ſheets of corrections and additions, which were likewiſe tranſlated, and added to the reſt. On the following year a pamphlet appeared at Paris on the volatile alkali *(a)*, in which the nature of the venom of the viper, and the uſe of the volatile alkali againſt the bite of this animal, were treated of. Many things were advanced in this pamphlet, entirely contradictory to what our authour had

* Written by M. Sage.

written more than ten years before in Italy. He fancied he had been deceived, and sat about making fresh experiments on the same subject, determined to correct the translation I have spoken of, before it was given to the publick.

To this new examination, we owe the second third and fourth parts of this work, throughout the whole of which the most delicate experiments are conspicuous. We may say in the strictest sense, that these parts are entirely new, as well on account of the subjects of which they treat, as of the discoveries they contain.

Before François Redi, no one knew in what the venom of the viper consisted. This celebrated naturalist employed the greatest part of his researches in confuting the errours that prevailed in his time. His work on the venom of the viper is almost entirely calculated to demonstrate, that the opinions held of this venom were in general false: a humiliating reflection to man, who is incapable of attaining the truth, in any other way than by passing through errour.

We owe to Redi, what has acquired him the greatest reputation, the first discovery of the

the humour that renders the bite of the viper venomous. The experiments he has employed to demonſtrate this diſcovery, are in general very well made, although they have not appeared deciſive to M. Charas, a French chemiſt.

The latter, after having made a great many experiments on the bite of the viper, thought it concluſive that the venom of this animal conſiſts in its rage; or rather, that the ſaliva of the viper, exalted by rage, when the creature is furious and bites, becomes venomous and mortal.

Although this opinion is erroneous, it however carries with it marks of probability; ſince it is certain that a viper is more dangerous and deſtructive in proportion as it is more irritated, as will be ſeen in the courſe of this work. This is what engaged our authour to examine the hypotheſis of ſaliva exalted by rage, by deciſive experiments, and the reader may aſſure himſelf, by peruſing the firſt part of this work, that he has been not a little ſucceſsful in this enquiry.

It is certain that François Redi was miſtaken as to the part where the receptacle of the venom is ſeated, and as to the rout the venom takes, when the viper in biting communicates it to animals. He was of opinion that this humour is ſituated in the membrane that covers the canine teeth, and that it glides exteriourly along the tooth, and at the ſame time inſinuates itſelf into the animals that receive the bite. We ſee this errour repeated half a century afterwards in James's Dictionary, the author of which beſides adopts all the errours of Mead on the ſaline nature of the venom. If it was not therefore abſolutely neceſſary, it was certainly uſeful, to examine this matter afreſh, and to make it as clear as poſſible.

All the other reſearches our authour has made, properly belong to him, and we may ſay with reaſon, that he has begun where others have ended; or, with greater juſtice, that the whole of his work is new, and truly original.

It appears to me, that one of the greateſt merits of this work conſiſts, not ſo much in the rare and numberleſs diſcoveries it contains, as in the luminous method with which the very important enquiries that are introduced in it are treated. If we are aſtoniſhed at the immenſe number of errours that are every where exploded, we cannot at the ſame time help admiring the tracks, hitherto unknown to obſervers, that our authour has beaten, in examining the ſubject of poiſons.

But that which deſerves the greateſt conſideration, is the very delicate analyſis he has made of the moſt obſcure and complicated enquiries; and the ſagacity with which he has conceived the experiments, that could not avoid leading him to truth. It is to be wiſhed that he may ſerve in future as a model to thoſe philoſophers who enquire into facts, without prepoſſeſſion or prejudice. How many diſputes and opinions will then be terminated! How many truths will be diſcovered! How many errours exploded! And how will the number of books be reduced! *The art of interrogating nature by the means of experiment is very delicate. It is in vain that you combine facts, if theſe facts have no affinity with each other; if they preſent themſelves under an equivocal form; if when they are produced by different cauſes, you are incapable of aſſigning and ſeparating with a certain preciſion, the particular effects that have reſulted from each of theſe cauſes*.*

* New Experiments on the Reſiſtance of Fluids, in the Preliminary Diſcourſes of Meſſrs. D'Alembert, Condorcet, and Boſſut.

To judge of what our authour has done in this work, and of what in juſtice belongs to him, the reader ought in the firſt place to examine all the publications of Redi and Mead on the ſame ſubject. I exhort him to do ſo, and this is the greateſt eulogium I can pay to the preſent work. Compariſon, a touchſtone that can never deceive us, is all that I demand, or rather, what juſtice and impartiality require.

Let this work be compared with thoſe on the ſame ſubject that have immortalized Redi and Mead. There will be no difficulty in judging how much it ſurpaſſes theirs, whether we conſider the number of diſcoveries, or the variety and multiplicity of experiments: indeed it will ſoon be ſeen, that there is no room for a compariſon betwixt them.

I regard the having found the venom of the viper to be a gummy ſubſtance, as a real diſcovery. The exiſtence of an animal gum is both important and new.

All that we meet with on the blood, and on the nerves, as they relate to the venom of the viper, is entirely new and original. This may be called a giant's ſtride, that clears a new road to freſh diſcoveries.

But

But this is not all the merit of the present work. The treatife on the American poifon called Ticunas, and on fome other vegetable poifons; the tracts on the Cherry-laurel; and the experiments on fome other vegetable fubftances, are a new field of enquiry, in which the difcoveries and induftry of our authour are very confpicuous. However, when in confequence of fo many rich difcoveries we think that nothing is any longer hid from us, and that we have at length penetrated into the moft fecret receffes of nature, we meet with labyrinths, out of which it does not feem permitted us to extricate ourfelves. Such are the new and important confequences of the obfervations on the poifon of the Cherry-laurel; a fubject ftill obfcure, but interefting, which will give occafion to the refearches of future obfervers.

The excellent experiments our author has made, relatively to the action of poifons on the nerves, have occafioned him to enrich this work with feveral very important enquiries into the ftructure of the nerves themfelves; an obfcure matter, in which nothing was known, and in which it fcarcely feemed allowed to man to make any advances. In the hand of our authour every thing becomes clear, eaſy, and ſimple. I cannot conceive how this double order of bands, how this ſpiral, exteriour, and apparent ſtructure, in the nerves, can have eſcaped the ſight of all our anatomiſts; and I regard the certain knowledge we now have of the firſt elements of the nerves, as one of the fineſt and moſt ſtriking diſcoveries that has ever been made in animal phyſicks; a diſcovery that has eſcaped the ſight of the moſt ſkilful and moſt practiſed obſervers.

After the obſervations of Lewenhoeck, phyſiologiſts and anatomiſts were of opinion that a diſcovery of the laſt diviſions of the nerves could never be attained to; but what did not appear poſſible then, is now unqueſtionably eſtabliſhed, as any one may aſſure himſelf, by following the traces of our authour. We have reaſon to flatter ourſelves that he will ſhortly favour us with his obſervations on the nature and uſes of *the Primitive Nervous Cylinders*. This is all that remains to be known of theſe wonderful organs. He began ſome time ago to buſy himſelf in this enquiry, and what ought we

not

not to expect from so exact, and so penetrating an observer!

We are at present not only acquainted with the true structure of the nerves, but are much better informed than heretofore of that of the brain.

Our authour has likewise examined the internal structure of the retina—it therefore appears that we have scarcely any thing more to desire as to that organ.

But his observations do not terminate here. He has developed with the same success, the structure of the muscles, and of the tendons. He finds certain characteristicks to distinguish these two kinds of substances, as well from each other, as from the nerves.

The first organical elements of the nerves, the brain, the muscles, and tendons, being thus known, our authour passes on to the discovery of a new and complete system of transparent, winding, unramified, cylinders, of a much smaller size than those of the blood, but which are more extended, and in a greater number, than the arterial and venous vessels. He finds them in the whole cellular substance, a substance which penetrates, and

composes

composes, all the organs of the animal machine.

Our authour discovers what he calls winding threads, in hairs, in the nails, in the epidermis, and in the bones. He afterwards gives us a detail of observations he made on vegetables, in which it appears, that he traces a similar structure. He concludes by a series of excellent observations of fossils, on the subject of which he intersperses a few doubts, that the reader may not be led away by simple appearances; and reserves his sentiments on this subject for another work, which he purposes to publish under the title of *Microscopical Observations.——Observations Microscopiques.*

He terminates his researches on the nerves, by relating several experiments on the reproduction of them,—a very interesting matter, hitherto unknown to naturalists, on which he has thrown all possible light.——

To complete this work, I have judged it necessary to add a description of a new canal of the eye, discovered by our authour more than eighteen years ago, and which he has never published.—I have taken this description from a letter dated from London, which he he wrote at the end of the year 1779, to Mr. Adolphus Murray, a celebrated profeſſor of anatomy at Upſal; and have given the part of our authour's letter that relates to this ſubject, in his own words.

We cannot avoid being ſurpriſed at the little value our authour places on his own diſcoveries, whilſt any other anatomiſt, however celebrated, would have haſtened to publiſh them. At the end of eighteen years, he ſcarcely permits this new canal he has diſcovered, to be announced in a few lines in one of his works; at the ſame time that it has been demonſtrated at Vienna for upwards of ten years, in the common courſes of anatomy, to the profeſſors of which it was probably communicated by M. Brambilla, ſurgeon to the Emperor, and director of the military hoſpitals. Our authour ſhowed this canal to M. Brambilla, when he accompanied his Imperial Majeſty, in his travels in Italy.

Although our authour has never publiſhed this ſo long diſcovered canal, he has, however, ſhown it to a great number of his friends, and to ſeveral other perſons. — M. Murray, the profeſſor of anatomy at Upſal, in a letter he addreſſed to our authour ſome time ago, in-

forms

forms him, that the deſcription of his new canal of the eye has been inſerted in the laſt volume of the Acts of the Academy at Upſal. *In ultimo tomo* (he writes) *deſcriptio canalis a te detecti extat.*—This canal was ſhown by our authour to the Swediſh profeſſor, when this laſt, on his coming to Italy, made ſome ſtay at Florence. At his return to Sweden, he addreſſed our authour at Paris, where he then was, to procure the drawing and deſcription of the canal, which he was deſirous of publiſhing in the Acts of the Swediſh Academy, for the advancement of anatomy, and for the good of his countrymen. The drawing and deſcription were ſent from Paris, but were loſt on the road. Our authour ſent him a freſh copy of them from London, but I do not know whether they ever reached him. I have inſerted the figures and deſcription at the end of this work, with the copy of the letter that accompanied them.

Dr. Troja, a diſtinguiſhed profeſſor at Naples, and member of the royal academy of the ſame city, in a diſſertation he publiſhed in the year 1780 on the diſeaſes of the eyes, ſpeaks of this new canal of the eye, obſerving that the diſcovery of it is due to our authour, who ſhowed it him at Paris in the eye of an ox.

It only depended on our authour to give this work a more original and novel air, and even to make it appear in certain reſpects more perfect. He had merely to conceal the ſteps by which he arrived at the truths he has diſcovered, and to be ſilent as to the methods and proceſſes that conducted him to them. The enlightened reader will find, that in proportion as he advances in the peruſal of this work, and meets with unforeſeen difficulties, the experiments our authour has imagined to ſurmount them, will preſent themſelves ſo naturally, that he will almoſt imperceptibly be led to believe, that he imagined them himſelf; ſo ſimple do they appear, and ſo appoſitely are they placed. In the ſame way, the new lights that are interſperſed, and the many reſearches that are deſcribed, ſeem to grow out of the matter itſelf, and not to ſpring from the authour. Again, he might not have ſpoken of whatever remains doubtful or undecided; he might have paſſed over in ſilence, the enquiries he has not been able, even after ſo many experiments, to decide upon. His work might have been found more complete, becaufe ignorance only fuffers in proportion as it is known; but our authour has throughout preferred clearnefs and utility to vain gloriousnefs and a falfe pride.

There is a clafs of readers to which this work will certainly be unpleafing, and this clafs is neither the leaft numerous, nor that which has the feweft fectators and partizans. It is compofed of thofe pretended naturalifts who explain nature at their fcrutoire; who meditating on facts feen in a wrong point of view, and copied into books, immediately divine *the fprings of them*; and who miftake for real caufes, the ideal ones they apply to the explanation of effects which have only exifted in their own imagination: in fhort, who prefer romances to facts, and to truth.

To folks of this defcription, accuftomed either to read, or to make, romances in phyficks, the prefent work muft appear barren, tedious, and little philofophical; and I therefore cannot exhort them to perufe it;—but to thofe, on the contrary, who are fond of facts and certain obfervations, it will be infinitely fatisfactory. I do not for my own part know of any fubject, either in phyficks or in medicine, that has been difcuffed with a greater abundance and richneſs of experiments than the one before us.

When a work is founded on certain and new facts, we always gain by reading it, even though it is badly contrived and preſented, and contains falſe reaſonings. The new truths that are found in it are real acquiſitions to the philoſopher, and he may eaſily employ them as a baſis to truer ſyſtems, and to ſurer opinions, and laſtly, to the diſcovery of the true laws of nature.

But what confidence ought not an authour to inſpire us with, who, after having ſaid, *I have made more than* 6000 *experiments*; *I have had more than* 4000 *animals bit*; *I have employed upwards of* 3000 *vipers*; finds no difficulty in adding, *I may have been miſtaken, and it is almoſt impoſſible that I have not been miſtaken!* — What a difference betwixt this authour and many others! betwixt opinion and certainty! betwixt ignorance and knowledge!

This work, ſo enriched by the immenſe number of new facts, and by the length and difficulty of the reſearches it contains, could not have been executed without the protection and conſtant favours of the auguſt Mæcenas the

the authour has the happineſs to ſerve. But whilſt the bounties of a philoſophick ſovereign procure to the enlightened world ſo many experiments and diſcoveries, the uſe our authour has made of the means that preſented themſelves to him on his journies, will, without doubt, entitle him to the gratitude and admiration of men of letters; and it will never ceaſe to be a matter of ſurpriſe, that a work which has coſt ſo much labour, had its birth at Paris and at London, through which places our authour, if I may ſo expreſs myſelf, did nothing more than paſs.

CONTENTS

OF THE

FIRST VOLUME.

PART THE FIRST.

PART THE SECOND.

PART THE THIRD.

PHILOSOPHICAL RESEARCHES

INTO THE

VENOM OF THE VIPER.

PART I.

INTRODUCTION.

In which is shown how little Authours agree among themselves on the Venom of the Viper.

IT is agreed at present that there is no other guide in a search into natural truths, than a knowledge of facts; it is only on facts that the philosopher can hope either to establish a reasonable system, or to form a sound judgement of those already established. Observation is alone capable of dissipating the mists that envelop the hidden causes of the phenomena of nature. And laſtly, it is to the labours of obſervers that we owe the rapid progreſs philoſophy has made in our time. But nothing retards this progreſs more, than the little agreement found amongſt authours, even in matters of experiment; that is to ſay, in things that are expoſed to the finger and to the eye. Nothing is more common than to ſee obſervations of this kind, that are nevertheleſs made by men full of candour, frequently belied by others, or in contradiction with themſelves. What then is the cauſe and ſource of theſe errours? Is it the ſpirit of party, or is it the difficulty of nice obſervation? Be it what it will, it is not leſs true, that after having conſulted the moſt celebrated authours on any particular we wiſh to clear up, we often find ourſelves as little informed, and in as great a ſtate of uncertainty, as we were before. In ſuch caſes then, I have apprehended, that without being wanting in the reſpect due to the authority of theſe great men, I might ſolely truſt to my own eyes; and to render my inveſtigations more deciſive, I have endeavoured by a ſingular application, to diſcriminate nicely, to compare the experiments of my predeceſſors with my own, to trace and develope all the circumſtances of them, and in ſhort, to diſcover what may have occaſioned ſo great a variety in the opinions of theſe obſervers, and in their manner of ſeeing.

Such is the true motive that has induced me to give an account of the experiments which follow. Without this I ſhould willingly have paſſed them over

over in ſilence, that I might not fatigue the reader, by preſenting him with what others may have already publiſhed.

The enſuing experiments relate to the viper, and dwell much leſs on the anatomy and particular ſtructure of ſome of its parts, that on the nature of the venom of that animal. The facility with which vipers are procured at Piſa, where I made my experiments, has enabled me to multiply my reſearches, and to vary them exceedingly. It would be loſing time to have no other object in view than that of rooting out the popular prejudices on this ſubject that were ſo very prevalent in the time of Redi. We are indebted to this authour both for his having made them known, and having diſencumbered natural hiſtory of them. He himſelf knew the value of time, ſince at the concluſion of his letter to Magalotti he ſays, *che il perder tempo a chi più ſa, più ſpiace. That the more any one is inſtructed, the more he regrets loſt time.*

When I obſerved that the frequently repeated obſervations of ſo celebrated a writer as Mead, claſhed entirely with thoſe of Redi, I muſt confeſs that a glimpſe of the utility of making known the ſource of the errours of theſe two great men, and the pleaſure of diſcovering new truths, encouraged me exceedingly to this undertaking, notwithſtanding the riſk that attends the handling ſuch dangerous animals.

I have deemed it neceſſary to begin by making a few remarks on the teeth of the viper, and on ſome other of its parts, and if in doing this I repeat certain truths that other obſervers have already publiſhed, it is only to give a greater perſpicuity to my work, and the impartial reader, particularly when he ſees that theſe truths are better eſtabliſhed, and that the experiments which ſerve as a baſis to them have been varied in ſo many ways, will readily pardon me.

CHAP.

CHAPTER I.

Number, Structure, and Use, of the Teeth of the Viper (a).

A GREAT deal has already been written on the large or canine teeth of the viper; they had been examined, even with the microſcope, before the time of Redi, and were found to be hollow and tubulated to their very points; Redi made himſelf fully certain of this with the naked eye, and found that if they were bruiſed when dry, they broke into three or four pieces, from the baſis to the point, and plainly ſhowed their internal cavity; but he flatly denies that this cavity is a conduit for the yellow liquor, and that when the viper bites, this venom guſhes from the ſmall hole at the extremity of the tooth. He ſays that he has opened the mouths of vipers, and has always found that this yellow liquor, when they bite, runs along the outer

(a) *Note of the French Editor.* That the parts deſcribed in this chapter may be more readily underſtood, we have borrowed ſeveral figures of the head of the viper from Mead's works: ſee Table I, and the explanation that precedes it. We requeſt the reader to likewiſe give a glance at Table II, before he goes any further.

part of the tooth from the top to the bottom, and that it never flows from within. *I have assured myself well of this, continues Redi, by several experiments, and by the often repeated testimony of my own eyes.*

The celebrated Valisnieri adds moreover, that the canine teeth of the viper are pierced with four very small lateral holes, through which he believes that the most subtile part of the venom penetrates into the wound from within the tooth, whilst the thicker and grosser part of it runs along the outer surface. Mead and Nicholls, on the contrary, set out from the analogy betwixt the viper and the rattle-snake, in the last of which this humour is very distinctly seen to flow from the inner part of the tooth, and maintain that the venom of the viper flows likewise from the point of its canine teeth, or at least from an opening they have towards their extremity. I have several times repeated Redi's experiments, by opening the mouths of these animals when living, and acknowledge that I have never been able to assure myself whether this venomous liquor issued precisely from within the tooth, or whether it simply glided along the outer part, from the basis to the point. If I held the head of the viper with the point of the teeth downwards, I had only to press strongly against the muscles of the palate, to make this yellow liquor flow rapidly from the basis to the point of the tooth; but if I held it with the teeth turned upwards, I saw the poison collect instantly about the basis of the tooth, and entirely fill the sheath or bag that serves to enclose it. Redi maintains

tains moreover, that this ſheath is the true reſervoir in which this humour is depoſited and preſerved, and thinks it is ſecreted by a ſmall neighbouring gland ſituated below the orbit. Nicholls ſays, on the contrary, that there is a veſicle, or ſmall bag, ſeparated from the ſheath; and that this gland is deſtined to quite another purpoſe, as the ſecreting ſome lymphatick or ſalivary humour.

In this uncertainty, I conceived that the beſt ſtep would be to examine by my own proper obſervation, the ſtructure of the viper's teeth, and ſo inform myſelf well of their uſes, ſince the deſcriptions theſe authours give of them are obſcure, and the obſervations of Mead and Nicholls contradictory to thoſe of Redi.

At each ſide of the anteriour and upper part of the head, the viper has a moveable bone which forms in part the upper jaw; each of theſe two bones has two ſockets at the ſide of each other, which are only ſeparated by an immoveable, but very brittle lamen, the ſubſtance of which is ſpongy and like that of the bones themſelves: in theſe ſockets are fixed the canine teeth, which are ſometimes found to the number of four, ſeldom of three, and oftener again of two. It is obſerved that when theſe teeth are four in number, they are not all fixed in their ſockets with the ſame ſtrength and ſtability: there are at that time uſually two, or one at leaſt, moveable, and capable of being eaſily pulled out without breaking: this cannot be done with the others, which are never pulled out entire, notwithſtanding they have no roots like ours. I have ſometimes found three moveable ones. I have likewiſe ſeen ſome vipers that had only two canine teeth, both of which were however weak and moveable; but this is a very rare caſe.

At the baſis of theſe large teeth, and quite out of the ſockets, ſix or ſeven very ſmall teeth are invariably found; they ſometimes even amount to the number of eight. When they are examined attentively with a magnifying glaſs, they are found to be faſtened at their baſis by a kind of web of a fine and ſoft membranous texture. Theſe ſmall teeth diminiſh in ſize, in proportion as they are more diſtant from the ſockets of the canine teeth; thoſe which are neareſt the ſockets are likewiſe the hardeſt and beſt formed. The other ſmaller ones are more tender, more imperfect, and as it were mucous, particularly at their baſis; they ſeem in reality to owe their formation to a whitiſh and gelatinous matter.

Beſides the two kinds of teeth I have mentioned, the viper has ſtill another order of them, much more minute than the former, and reſembling ſmall hooks; they are ſtrongly fixed to the number of ten, eleven, and ſometimes fifteen, in two ſmall, pretty long, parallel, bones, which on each ſide form the upper jaw; and of eight, nine, and ſometimes twelve, in each of two bones that form the lower jaw.

The canine or large teeth, as well as the other ſmaller ones found at their baſis, are encloſed in a bag or ſheath, which covers them on all ſides, and is compoſed

posed of very strong fibres, and of a cellular web. It is however open towards the point of the tooth, and terminates there by folding together its two lamina, which at their junction are often dentated. This sheath seems to be a prolongation of the external membrane of the palate.

The canine tooth is seldom more than three lines in length, Paris measure *(a)*. Its basis is not more than half a line in diameter; its figure is that of a horn a little flattened, and somewhat bent towards its basis.

This tooth terminates in a very sharp point, towards which it insensibly loses its curvature, and becomes at length almost straight. Below the middle of the tooth, towards its point, and in the convex part, a small opening is visible to the naked eye, very narrow but exceedingly long, which ending in a small, channelled slope, that can scarcely be seen with a microscope, terminates in this way at the point. Hairs plucked from the whiskers of foxes, cats, dogs, &c. may be readily introduced into this opening, and it appears, when viewed with a microscope, to be a cleft, the length of which is almost a fourth part of that of the tooth, and the breadth the sixteenth part at most. It represents with its exteriour edge a very long or flattened ellipsis, becoming larger towards the basis of the tooth. This slope

(a) The French, or Paris inch, is somewhat larger than the English one, but not so much so as to make a sensible difference in the line, which is its twelfth part---Therefore whenever the latter is introduced in this work, it may be considered as the twelfth part of an English inch.

pene-

penetrates interiourly, and is terminated at its two ſides by two ſhort edges or lips, thick and raiſed up. Another opening is likewiſe found in the convex part of the tooth, towards the baſis, and near the place where it is fixed in the ſocket. This opening likewiſe begins by a ſhallow cavity, immediately at leaving the ſocket. It is much larger than the firſt, but not longer. In proportion as this ſlope or ſmall hollow penetrates into the tooth, it pierces it for its whole length, and forms a channel which terminates in an elliptical hole at the point. A bit of ſilk is eaſily paſſed from one opening to the other, particularly when care is taken to introduce it at the baſis, where the natural entry of this paſſage is found. The ſide of the ſecond opening reſembles a parabole, the baſis of which paſſes over the bony edges of the ſocket, and the other ſides of which end in a ſomewhat obtuſe point towards the ſummit of the tooth. The canine tooth of a viper then is hollow and tubulated for its whole length, from the baſis to the point, and has two holes in its convex part. This hollow is however not ſuch as one would ſuppoſe it to be, on viewing the third figure of Mead, and the deſcriptions of Redi.

The tooth of the viper has a double pipe or tubule almoſt for its whole length, a circumſtance hitherto entirely unknown to obſervers. Theſe two canals or tubes do not communicate with each other, and are ſeparated by a bony partition, very brittle towards the baſis, but which becomes ſomewhat ſtronger in proportion as it advances towards the point. One of theſe tubes or canals, which I call the external one, becauſe it is at the ſide of the convex part of the tooth, begins, as has been ſeen, at the baſis of the triangular opening, and goes on enlarging by degrees to the middle of the length of the tooth, whence it gradually narrows, and ends at the elliptical opening of the point. The inner canal, on the contrary, which is towards the concave part of the tooth, begins with a large opening at the baſis, from whence it advances, cloſing by degrees, and terminates at length in a blind point above the middle of the tooth. The partition likewiſe that ſeparates theſe two cavities is crooked, and its convex part is turned towards the hollow of the canal it terminates, ſo that its bony ſubſtance rather preſents an irregular, curvilinear figure, or truncated cone, than a perfect cone itſelf. The blind canal communicates with the ſocket in which the tooth is fixed, and receives veſſels and nerves, which enter by a ſmall oval hole, perceptible to the eye, and opening at the edges of the ſocket itſelf, towards the inner part of the jaw. This bone of the jaw has likewiſe a large round opening, which begins a canal placed a little below and at the ſide, and opening one way into the ſocket, and the other laterally and more below, towards the ſurface of the ſame part of the jaw.

The ſmall teeth placed at the baſis of the large ones, reſemble them very much both in their inner and outer ſtructure. Thoſe particularly that are placed the neareſt the firſt, and which are the firmeſt, are

are perfectly like them, unlefs it be that their bafis is not fo well finifhed. Like the large ones, they have all the elliptical hole towards the point, and a part of the triangular hole at the bafis. The two conduits, internal and external, are alfo feen in them.

It is not the fame with the other very fmall teeth I have mentioned, which are far the moft numerous, and in both jaws. Thefe are not channelled, and have no kind of opening, either at the point or bafis.

CHAPTER II.

Of the yellow Liquor that flows from the Tooth of the Viper.

WHEN the viper wifhes to bite, its canine teeth are raifed by a mechanifm, which Nicholls has defcribed perfectly well in the anatomical appendix he has annexed to Mead's work on poifons. But thofe of the large teeth that are not fo well faftened in their fockets, are lefs capable of raifing themfelves, in proportion as they are more moveable, and badly fixed in the jaw. Nicholls pretends, that when one or two of thefe four large teeth are moveable, the viper can only bite with one tooth on each fide. Indeed he does not found his opinion on any experiment,

riment, but feems to account for this by certain final caufes which I cannot admit, fince in phyficks thefe kind of proofs become of no weight. He remarks that there is fuch a diftance betwixt the two canine teeth of the rattlefnake, that the yellow fluid, which is carried by a conduit betwixt them, would entirely enter into the fheath, inftead of being conveyed to the animal bit by this fnake: on this account he does not hefitate to believe that the conduit of this liquor is precifely fixed oppofite the hole at the bafis of the fingle tooth on each fide, with which the viper feizes what it bites. But befides that no organs are feen to execute this function, and that the mechanifm of it cannot be inveftigated, I can take upon me to fay that I have fometimes feen all the four canine teeth of a viper equally firm and ftrongly fixed in their fockets, and have ftill more frequently found three of them, well fixed and very ftrong, in a ftate both to feize and bite. It is not to be doubted but that the viper, inftead of fimply biting with two teeth, one at each fide, muft feize equally with all thofe that are firmly fixed in the fockets, and I have even affured myfelf of this by experiment. It is not true then, as Nicholls pretends, that the conduit of this yellow liquor only adapts itfelf to one fingle tooth on each fide when the viper bites; befides, this fpace which is obferved betwixt the canine teeth of the rattlefnake, is not alike found in our vipers, the teeth of which, almoft from the bafis to the point, touch and embrace each other in fuch a way, that no fluid can pafs betwixt them, much lefs this yellow venomous liquor which is somewhat glutinous. It is moreover determined that the viper bites and seizes not only with the teeth that are fixed in their sockets, but likewise very often with those that are moveable. Of ten vipers I examined, there were three that had two moveable teeth, and two firm in their sockets; the seven others had only one moveable tooth, and two firm and well fastened. If I except one of the first three vipers, and two of the seven last, all the others to which I held a bit of tendon of beef, boiled and well stripped of its coat, seized it forcibly, and left the marks of their teeth strongly printed in it: I must however observe that their least firm teeth were not of the most moveable kind, and that when they are very loose I have found them to rise so little that it is absolutely impossible for their points to touch the body seized by the viper.

Since it is certain that this creature never bites without a risk of losing some of its teeth, Nicholls conjectures with great sagacity, after Redi, that nature has intended the small ones at the basis of the others, to replace, when there is a necessity for it, those that the viper loses from time to time. Their crooked shape renders it difficult for them to be drawn from the wound, and in the course of my researches I have sometimes observed, that not only those which are moveable, but even the firmest of them, are alike subject to accident. The thinness of the tooth, and the strength of the animal that has been bitten, contribute equally to this loss; and this opinion becomes still more probable when we consider that these small moveable teeth are ex-

actly

actly formed like the canine ones, that is to ſay, that they have likewiſe two canals, (thoſe however that are the moſt perfect) and the ſame openings at their baſis and at their point. But all theſe reſemblances were in ſhort but one reaſon more why experiment ſhould be conſulted, and the truth eſtabliſhed by nice obſervations.

I have ſometimes obſerved in one of theſockets, a very moveable tooth, the ill-formed and ſtill gelatinous baſis of which faſtens itſelf to the edges or ſides of the hollow; this tooth may even be drawn a little way out of the ſocket without detaching it entirely, by means of a tender and mucous matter that ſerves as a glue to it. On moving the jaws, I made the one next it raiſe itſelf very well, but the one of which I have ſpoken, abſolutely continued reclined on the baſis of the moveable bone of the jaw. It is clear that this tooth had been of the number of thoſe that are at the baſis of the great or canine ones.

I expreſsly drew from a large viper one of theſe laſt, which was moveable and ill fixed in its ſocket, and obſerved ſome time after that the largeſt of thoſe which are placed beneath the ſheath and beneath the ſocket, had advanced a little towards the empty ſocket. Some days after I thought I perceived it to have approached ſtill nearer. I purſued my obſervations on every ſecond day, and at length ſaw that this tooth had perfectly fixed itſelf in the ſocket, where however it was as yet very moveable, and badly faſtened. At the end of thirty days it was fixed in ſo ſolid a way as to be capable of of biting. The neceſſity one is under of frequently handling the viper to be ſatisfied of the ſtate of its teeth, and of opening its ſheath with pincers, or with the blunted point of a bit of wire, makes this experiment very dangerous. The repeated compreſſions the ſmall teeth receive, from the contraction of the muſcles of the jaw, and the action of the ſheath itſelf which conſtantly preſſes on the points of thoſe teeth that are moſt raiſed, are quite ſufficient to puſh the root of the tooth in queſtion, into the ſocket which the falling out of the old tooth has left empty.

The laſt or ſmalleſt teeth are certainly not employed in biting, but are intended to draw nearer to the throat, and to hold firmer, the animal the viper has already ſeized.

The ſingular ſtructure of the canine teeth, ſo different from that of the other teeth of the two jaws, is a powerful perſuaſive of its being from them that the yellow liquor flows; it is not however without ſome apparent reaſon that Redi, otherwiſe ſo exact, has been led into an errour.

To fully aſſure myſelf of this circumſtance, I bound the head of a viper I had juſt killed, to a table. To diſtinguiſh better and to a greater certainty, I took the precaution to remove the lower jaw: the canine tooth, in the way I had fixed the head, was turned upwards, and I obſerved the elliptical cleft with the ſtrongeſt lens of Ellis's microſcope. I gently compreſſed the palate with a ſomewhat blunted iron, when a ſlightly tranſparent yellow

low liquor, which formed itſelf into a drop, and at length glided along the outer ſurface of the tooth, inſtantly appeared at the elliptical hole of the point. I repeated this experiment ſeveral times, and always with the ſame ſucceſs. I afterwards cloſed the ſmall opening with wax, and then compreſſed the palate, but not a particle of the venom ſhowed itſelf. I however ſaw it through the tranſparent ſides of the tooth, conveying itſelf from the baſis to the point by the external canal, which it had entirely filled. I put a round bit of wax with riſing edges all about the tooth, in ſome other heads of vipers, immediately below the elliptical hole, and having ſtrongly compreſſed the palate, I inſtantly ſaw the yellow liquor guſhing forcibly, and as it were by ſtarts, from the point, and ſcattering itſelf abundantly on the piece of wax, which it entirely covered all round the tooth.

I have likewiſe been able, although with difficulty, to cloſe the hole at the baſis with wax, and I have then found it to be in vain that I compreſſed all the muſcles of the head ſucceſſively. I could never force a drop of venom from the point, nor could I even diſcover it through the ſides of the tooth. Whenever a viper is held in the hand with the teeth turned upwards, it is eaſy for an attentive eye, accuſtomed to obſervation, to ſee this drop of yellow liquor preſent itſelf at the elliptical opening, in ſuch a way that it may be more or leſs encreaſed at will. I have repeated this experiment a thouſand times, and have invariably ſeen the ſmall drop of venom exuding from the elliptical hole of the tooth. Nay, what is more, on compreffing violently, this liquor is fometimes obferved to force itfelf out fuddenly, and to fpirt to a confiderable diftance. It muft however be remarked, that when the tooth is once wetted with it, particularly when it is entirely covered with the fheath, this humour, or the drop it forms, glides fo very precipitately along the tooth, that it is fuddenly feen at the bafis without having been perceived at the point. It in this way imperceptibly fills the fheath, fo that it is difficult to perfuade ones-felf that it really iffued from the point of the tooth. This is the way that fo exact an obferver as Redi has been led into an errour. It is not proper, after his example, to employ living vipers, nor to open their mouths forcibly, fince the flowing out of the venom will then be too fudden, and fince it will then be dangerous to obferve as nearly as is neceffary to prevent being deceived.

I not only faw the yellow liquor flow from the point of the tooth that I particularly obferved, but likewife from the neighbouring tooth, when there was one, fo that it proceeds equally from all the canine teeth at a time, not excepting thofe which, without being altogether firm in their fockets, are however fufficiently fo to rife with the others. In a word, in all the heads of vipers I have obferved, I have feen this humour conftantly flow from all the canine teeth that raife themfelves fufficiently, on the compreffion of the mufcles of the palate, and on opening the mouth with a force that would be capable

ble of wounding an animal ſeized by the viper. This ſhows that Nicholls is deceived, when he pretends that the venom only flows from one tooth at a time on each ſide.

CHAPTER III.

Of the Part where the Reſervoir of this yellow Humour is ſeated.

THE yellow liquor iſſues then from the point of the vipers tooth, contrary to the ſentiments of Redi, who regarded the bag or ſheath that envelops not only the canine teeth, but likewiſe the others that are found at their baſis, as the true reſervoir of this venom. This opinion is ſtill again belied by the ſtructure itſelf of this ſheath, which has a large aperture next the cheeks, through which this liquor would inceſſantly flow with the greateſt eaſe; ſo that every time the jaws of the viper ſhould be extended, the venom would be conſtantly ſeen oozing through the extremity of the ſheath, even though the viper ſhould not bite. This is what no one has hitherto obſerved. It is beſides certain, that when this ſheath is opened with ſciſſars, neither this yellow humour, nor any kind of fluid that may have collected there, is found in its cavity.

But ſince, as has been ſeen above, this liquor flows from out the elliptical hole at the point of the tooth, it must neceſſarily be carried to the aperture at the baſis by another conduit than this ſheath, in which it is certain that no remains of venom are found. It will not be difficult, according to this hypotheſis, to diſcover the ſmall veſicle that is really deſtined to contain it.

If after having ſtripped the teeth of this bag or ſheath, a preſſure is made on the palate, this humour is obſerved to flow through a ſmall and almoſt imperceptible hole, placed at the anteriour part of the maxillary bone, within the ſheath, and at the ſide of the baſis of the canine teeth; ſo that when theſe teeth are covered by the ſheath, this ſmall orifice comes in contact with the inferiour opening of the tooth. Indeed with the help of a magnifying glaſs, a very ſmall orifice is diſcovered, ſituated in the midſt of a ſmall cleft or furrow, which anſwers to the maxillary bone. I endeavoured to introduce into this orifice a fox's hair, very fine, but nevertheleſs pretty ſtrong, and at length ſucceeded in paſſing it quite acroſs the ſheath, by a long membranous conduit, into a ſmall veſicle placed beneath the muſcles of the upper jaw, on its lateral part. It is a membranous bag of a very ſtrong and cloſe texture, which is again partly covered by tendinous fibres. Its ſhape is nearly that of an equilateral triangle. It differs from other veſicles, which are crooked or ſpheroidal, inſtead of which the baſis of this is in a manner ſtraight. This ſmall veſicle terminates next the eye in a tranſparent canal, which after having proceeded beneath the orbit for the

ſpace

ſpace of two lines, pierces the ſheath, and at length opens at the extremity of the ſockets, into the ſmall cleft of which I have ſpoken. When this conduit reaches the vicinity of the ſheath, it dilates a little, and it is here that the venom finds the greateſt obſtacle to its paſſage, which is owing to the compreſſion it meets with from the bones of the jaw.

The veſicle I have ſpoken of, and which ſerves as a reſervoir for this humour, is three or four lines in length, and two at moſt in breadth at its baſis. It never contains more than four or five drops of the venom, which is forced from it principally by the action of a ſtrong and powerful muſcle, that riſing out of the lower jaw, folds inwards a little, then makes an arch, and preceeding to the upper jaw, runs over a part of it, and faſtens itſelf there. Towards the inner angle of this conſtrictor muſcle, or rather towards the part of its curvature neareſt the upper jaw, the ſmall veſicle begins; it is covered with this muſcle for almoſt the whole of its length. Thus placed, it is as it were encloſed in a preſs; and is bound and fixed to the adjacent bony parts, by means of two tendons, and of the canal, ſo that it can neither force itſelf forward, backward, nor ſideways, and muſt neceſſarily bear the double action of this muſcle, which now compreſſes it, when the viper bites and preſſes forcibly, and now again ſuffers it to dilate, when the muſcle itſelf contracts and enlarges. That which proves this muſcle to be chiefly intended to force the venom from its reſervoir is, that it is faſtened to each jaw in ſuch a way as to be of very little uſe to the creature in cloſing its mouth, which clearly cannot be its principal purpoſe.

The hairs of a fox's whiſkers penetrate and paſs eaſily from the veſicle through the excretory duct, and go out at the orifice ſituated at the inner part of the ſheath. I have ſometimes ſucceeded in bringing them even to the elliptical hole at the point of the tooth. This is certainly the route the venom takes for the purpoſe of going out at the ſmall orifice of the ſheath, which correſponds preciſely with the height of the parabolical hole of the tooth *(a)*.

As the ſheath is very nicely fitted to the baſis of the canine tooth, the venom that goes out of the conduit at the ſmall orifice, muſt of neceſſity enter entirely into the hole of the tooth; and although it guſhes in abundance through this canal, it does not at all ſcatter itſelf in the ſheath, ſince the orifice out of which it flows is infinitely ſmaller than the parabolical hole to which the nice fitting of the ſheath makes it immediately correſpond. In a word, it paſſes entirely into it, particularly when there is only one of theſe teeth. Still more, I have obſerved on folding the ſheath back above the baſis of the teeth,

(a) It muſt appear very ſtrange that Doctor James, who has written after Mead, has aſſerted in his Diſpenſatory, that the true reſervoir of this liquor is the bag (ſheath) which covers the roots of the large teeth of the viper, and that a ſmall veſicle is found at the top of this bag, which opens at its extremity, to give a paſſage to the teeth that ſhed the venom. It appears however that this writer made many experiments on the viper, and with the intention of making them well.

and

and preffing flightly and gradually upon the conduit, that the venom is carried by a natural declivity towards the hole of the tooth, which it entirely fills before a drop of it is fpilt in the fheath. This natural declivity is fimply caufed by a fmall hollow in the jaw, which extends as far as the parabolical hole, and is fcarcely feen with a microfcope. I do not however deny but that in fome particular cafes this liquor may flow directly into the fheath, and even glide from thence to the points of the teeth, particularly when two of them are fo clofe to each other as to touch and leave nothing but a hollow betwixt them, and when the viper bites in fuch a way as to force its teeth deep into the flefh, and to ftop up the parabolical hole; it muft here fqueeze with fufficient force and long enough to comprefs the veficle, and give time to the liquor to glide betwixt the two teeth. In thefe cafes, which cannot however but be very rare, there is no doubt but that the animal may even kill without the venom having made its paffage through the ufual conduit of the tooth. I repeatedly ftopped up with pitch, fometimes the parabolical hole, at others the elliptical hole, and fometimes again both of them, and found that the yellow liquor did not then reach the bottom of the fheath but with great difficulty, and after a ftrong compreffion had been made for a long time on the conftrictor mufcle. I lay it down from hence, as an infallible conclufion, that the venom flows from the point of the tooth, and not form the bag or ſheath, whether the viper of itſelf cauſes it to flow in biting, or whether a compreſſion is purpoſely made on the veſicle I have ſpoken of.

CHAPTER IV.

The Venom of the Viper is no other than the yellow Humour that flows from the Tooth when the Viper bites.

It happens very often, in vipers particularly that have been lately killed, that this yellow humour dries, ſtops up both holes, and totally obſtructs the canal of the tooth. As it cannot then enter into the tooth ſo as to find a paſſage through it, it muſt conſequently flow from the excretory conduit into the ſheath. This obſervation is ſo much the more neceſſary, as without it 'twould be eaſy to fall into an errour, and to preſume that the venom, inſtead of being conveyed by the tooth, flows from the ſheath, and is carried from thence into the wound.

I was deſirous of aſſuring myſelf how far one may rely on the opinion of thoſe who believe that the bite of the viper is only mortal on account of the rage the creature is thrown into before it bites. I omit the mention of an infinite number of experiments I have made to be certain, with Redi, that the yellow humour which flows from the tooth of the viper is mortal when introduced immediately into the blood he medium of a wound. I ſhall only obſerve, that the various experiments of Redi and Mead agree

agree perfectly as to the truth of this circumſtance, and I cannot conceive how certain celebrated writers have continued to perſuade themſelves to the contrary, and to attribute the mortal effect of the bite of the viper to the rage of the animal, and to the power of the exalted ſtate of the ſaliva, rather than to the ſpecifick character of this humour.

I have frequently enraged vipers, and afterwards opened the mouth in ſuch a way that they could neither compreſs nor bite with it. I have then ſoaked bits of cotton well in the foam or ſaliva with which it was covered, and applied them to the wounds of animals, the bleeding of which had ceaſed. I have never ſeen any accident cauſed by this, nor have the animals appeared to be the leaſt diſordered. Neither the foam then, nor the other humours of the viper's mouth, are capable of cauſing death when introduced into the blood of an animal.

I have ſevered at one ſtroke the heads of ſeveral vipers from their bodies, at a time when far from being enraged, they were in a calm and tranquil ſtate. I have then taken the venom from the tooth itſelf, to be ſure of having it pure and unmixed. I have taken it from ſome of them immediately on ſeparating the head, and from others ſome hours after, when the head had dried in a great meaſure, and had ceaſed to move. On applying this venom carefully to the wounds of different animals, they have all without any exception been killed by it. We muſt conclude then that the humour alone which flows from the tooth, has a deadly quality,

to

to which the rage of the animal does not at all contribute. But to obviate all objection, and to prevent the being reproached with having neglected to make a viper bite after having enraged it, and having contented myfelf with introducing its foam into wounds, I took one, and made it bite feveral animals. When I conceived that there could be no longer any remains of the venom, I began to prick and torment the creature, and in a word, employed all the means I could think of to enrage it. When I faw by its hiffings, and the rapid vibrations of its tongue, that it was become furious, I prefented animals to it, which it bit with all the force it was capable of. Neither of them however died, or feemed to feel any inconvenience. This was a very natural refult, fince the liquor that flows from the tooth, which alone has the faculty of killing, had already been entirely wafted, and fince nothing more now remained than the foam and other humours that are in no way venomous, even during the moft exceffive rage of the animal. I repeated this experiment on two other vipers, with the very fame fuccefs.

I was defirous of making another experiment, which, to prevent its being dangerous, requires a great deal of precaution and addrefs on the part of the obferver, although after all it cannot be more decifive than the preceding one. This was to entirely remove the two veficles that contain the venom. After feveral fruitlefs attempts I at length fucceeded, without doing much injury to the viper, and without tearing its mouth. I made an incifion

into

into the ſkin that covers the two veſicles, and having ſeized them with pincers, cut them entirely out with a biſtoury. Thoſe who are accuſtomed to diſſect this ſpecies of animals, muſt ſee clearly that this experiment is attended with more danger than difficulty. To ſucceed in it, the neck of the viper muſt be ſeized by ſome one, or it muſt be well tied to a table, in ſuch a way that the creature cannot raiſe its head to bite. Having removed the veſicles, I had two frogs bits, ſo as to diſcharge whatever venom might remain in the teeth or in the extremity of the conduit: neither of them died. I kept this viper a long time, and at different times made it bite both large and ſmall animals, as well with warm blood as with cold. Neither of them died, nor had any other complaint than what muſt have neceſſarily been cauſed by the ſimple mechanical wound of the tooth..

CHAPTER V.

The Venom of the Viper is not a Poiſon to the Viper itſelf.

Very grave authours have likewiſe imagined, that this humour, which deſtroys other animals, is not leſs hurtful to the viper itſelf; and this is the opinion of thoſe who have written lately on the venom of animals. The examples of ſcorpions and ſpiders which kill each other with the bite or ſting, ſeemed to favour this opinion very ſtrongly. We read in the philoſophical tranſactions that rattle-ſnakes die in a very few minutes, when they bite each other. It is at preſent known that this ſnake is a ſpecies of viper, larger than ours, and therefore they have by analogy drawn the ſame concluſion as to the viper and other venomous animals.

Certain Spaniards had brought from the Eaſt Indies three ſnakes called *Cobras de capello*, and one only had ſurvived the frequent combats they had amongſt themſelves. Doctor Mead concluded that the other two died of the venom, and conſequently that the viper's venom ought to be likewiſe mortal to the viper itſelf. It ſeems to me that he ought rather to have drawn a contrary concluſion, for it is not likely but that the victorious ſnake which ſurvived, would have been ſometimes bit by the two others; and yet we ſee that it contrived to live.

'Twould have been undoubtedly much better to have made experiments, than to have founded an opinion ſo ſlightly on a mere matter of fact as Mead has done, and on a ſimple analogy drawn from caſes that are very rare; particularly as the fury with which ſcorpions and ſpiders combat and mangle each other does not prove that they die of the venom they have received. Beſides, it has been obſerved that the ſpider which leaves the combat victorious, only dies when it has loſt ſome one of its organs neceſſary to life. As to the rattle-ſnakes, the examples we have of their combats are too rare and not ſufficiently authenticated

ticated to furnifh a good analogy. This could befides be never any thing more than a fimple analogy, rendered fo much the weaker by there being certainly a very great difference betwixt this fnake and our viper, whether we regard their ftructure, or the activity of their venom.

It is not eafy to provoke vipers to bite each other, whatever care may be previoufly taken to kindle them to fury. I employed the following method to get the better of this repugnance. Having feized the neck of a viper with pincers, I kept its tail in my other hand to manage it with greater fafety. I employed an affiftant to feize a fecond one in the fame way. I held the body of one of them to the head of the other, which perceiving itfelf to be clofe grafped by the neck, hiffed, twifted itfelf, and fell with fury on every thing that came near it. The former one, which it bit feveral times, was much fmaller, and expreffed each time, by the livelinefs of its motions, the violence of what it fuffered. I found a fuperficial wound at the part where it had been bit, moiftened with blood and with the venom from the tooth. I enclofed it in a glafs vafe, where it continued tranquil for fome minutes. Two hours after I found the part where it had been bit a little fwelled; this fwelling however continued but a little time, and the viper, recovering its natural vivacity, crept along the fides of the vafe, and raifed its head with the fame ftrength as if it had not been bitten. After twelve hours I fet it free, when it appeared as ftrong ſtrong as another one I placed with it by way of comparifon. I put it again into the vaſe, and on the following day found it ſtill as lively and healthy as before. At length, thirty-ſix hours after, ſeeing no appearance of its having been envenomed, I killed it. I found ſeveral holes in the ſkin at the part where it had been bit; the muſcles themſelves of the back were very deeply pierced; and the blows of the tooth had in more than one place forced it through the body, and through the abdominal viſcera. And laſtly, the wounds were a little inflamed, but there was no appearance of ſwelling or tumour.

Two days after I tried two very large vipers, which threw themſelves furiouſly on the animals that were preſented to them. I made them bite another middle ſized viper, which received, from one of them two very deep wounds made with the teeth, from the other four. One of them even left a tooth in the wound. At every blow the viper received on its belly, all of which were directed to the ſame ſpot, it gave violent ſymptoms of pain, hiſſed, and nearly eſcaped from the hands of the perſon that held it. I put it into a glaſs vaſe, in which it remained for a few minutes in a ſtate of inſenſibility: however, on afterwards placing it on the ground, it crawled about with great agility. I could never diſcover any ſwelling at the part where it had been bit; the ſkin had notwithſtanding been lacerated, and the muſcles laid bare: there was no hemorrhage. I kept it four days in the vaſe, during

ring which time it did not appear in the leaſt diſordered. The ſecond day I held an animal to it, which it inſtantly bit, and which died two hours after. I at length killed it and found that the blows of the teeth had pierced it through and through: the wounds were ſomewhat inflamed. The ſame thing happened to five other vipers which I had bit repeatedly. I even forced a ſixth to bite itſelf at the tail. Neither of them died, nor appeared to be in the leaſt diſordered.

But to prevent any one from thinking that the hardneſs of the ſkin had kept the venom from penetrating, and to introduce it with greater certainty into the blood, I removed a conſiderable portion of the ſkin from the backs of four vipers, and had them bit by ſeven others, from which they actually received ſeveral blows of the teeth ; neither of them died, or became ill, and only one of them appeared to be heavy and languid, and had a ſwelling at its back.

Again, I irritated another viper, by pricking it on the body with a pointed iron, and afterwards made it bite a piece of jagged glaſs. The venom ſpread from the tooth over the whole mouth, which the glaſs had cut and made bleed. I kept it ſtill, and waited the event. For the three firſt days it crawled about a little. On the fourth it was more lively, but did not yet make any attempt to bite, even when provoked. On the ſeventh day I opened its mouth, and found it entirely healed, without any ſcar. On the ſame day I made it bite a ſmall animal, which died an hour after.

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I repeated this experiment on three other vipers, and employed the following method. From one of them I removed a portion of the ſkin of the neck, from another a portion of that of the back, and laid bare the muſcles of the third juſt above the tail. I wounded each of them at the part laid bare, bending the point of the lancet a little, to open the wounds the better. Into each of theſe wounds I introduced a ſmall drop of the venom, that is to ſay, as much as was neceſſary to entirely fill it. I afterwards returned theſe vipers, each into its vaſe, where they remained very tranquil, and ſeemed to have ſuffered but little. Their wounds were however inflamed, but there was no ſwelling. I kept them alive for ſeveral days.

We now ſee what opinion ſhould be entertained of the analogy betwixt the venom of the viper and that of other animals, and may judge into how great an errour thoſe have fallen who have believed that the yellow humour which flows from the tooth of the viper, and which is mortal to other creatures, is likewiſe ſo to itſelf, and that the bites of theſe dangerous animals are capable of poiſoning each other. If analogy could haye any weight in this inſtance, I ſhould be tempted to belive that, contrary to the opinion of Mead, the venom of the ſcorpion would have no ill effect on the ſcorpion itſelf, and that there is probably no venomous animal on earth, the venom of which can be hurtful to thoſe of its own ſpecies. If it be ſo, it can only be in a very few animals, and only in the ſmalleſt of them, the venom

of

of which is acrid and cauftick, fuch as bees, wafps, and hornets. It may likewife be true that the fcorpions of Afia and Africa carry a venom that is mortal to their own fpecies, fince the venom of the Italian fcorpion, when put on the tongue, is acrid and pungent. It appears to me, that the general errour which many obfervers, who are otherwife very exact, have embraced, has its fource in a deceitful experiment. It had been remarked, that when a fcorpion was furrounded by live coals, it was firft agitated, and then turned its fting towards its back, as if to wound itfelf. As it at length died, and was even burned up, from its violent agitation amongft the live coals, it was fimply believed that it died from its venom, and from its own wounds. The experiment is equivocal; it is even falfe. I have repeated it a thoufand times, and have never obferved that the fcorpion ftruck itfelf with its fting; it died roafted and burned up, and not envenomed.

It has likewife been obferved, that the frefh water polypus, in fwallowing its prey, fometimes fwallows the arms or claws with which it holds it; and likewife, that when two of thefe polypuffes difpute together, the ftronger frequently prevails, and fwallows the claws of the weaker. The polypus, however, in neither of thefe cafes dies, although its venom, as we fhall fee hereafter, is very active. The parts thus fwallowed, leave the ftomach foon after, entire and alive, without having fuffered any apparent change, and continue as before to ferve as claws to the animal.

CHAPTER VI.

The Venom of the Viper is not a Poison to every Species of Animals.

Thus far we have seen that the venom of a viper is neither a poiſon to the viper itſelf, nor to thoſe of its own ſpecies; and this ſingularity led me to ſuſpect that it might alſo be innocent to ſome other kinds of animals. Indeed why ſhould it not be ſo, as well as to the viper? If, in a word, it is not capable of decompoſing the ſolids, and altering the fluids, of any particular living machine; if it can neither diſturb the harmony of it, nor occaſion death; why ſhould there not be other living organized creatures, on which it may have as little action? We know but little of the manner in which poiſons in general act, but we know that there are many very active ſubſtances which produce the moſt terrible effects on certain parts, and which notwithſtanding have no effect on others. Stibiated tartar, for example, a ſubſtance that is introduced without danger into the eyes, is a very violent emetick when received into the ſtomach. There are perſons who are thrown into convulſions by the fragrant ſmell of the roſe. Theſe various accidents are owing without doubt to the ſtructure and organization of the animal machine.

machine. Certain ſubſtances are known to be poiſons to certain animals, whilſt far from being hurtful to ſome others, they even ſerve as an aliment to them. Such is hemlock for inſtance, which deſtroys the human ſpecies, and nouriſhes goats. It is thus that the bitter almonds we eat on account of their flavour, kill certain birds, and do no injury to others. It may likewiſe be the caſe then, that the venom of the viper may not be a poiſon to all kinds of animals, particularly if it act like narcoticks, that do not cauſe death by corroding the ſolid parts. Corroſive ſublimate is a poiſon deſtructive to all living animals, becauſe its mechanical action is in fact capable of exerciſing itſelf on all the organs of the animal machine. Narcoticks, on the contrary, ſo dangerous to men, produce no ill effect on dogs. The different ſtructure of the organs of animals may then be the occaſion, that the ſame ſubſtance may at the ſame time be a very active poiſon to certain ſpecies' of them; and altogether of an indifferent nature, or an aliment, or even an excellent remedy, to others.

It is on theſe conjectures that I engaged in the long courſe of experiments I am going to relate. I had already obſerved, that of all animals, leeches are inconteſtibly killed with the greateſt difficulty. When they are cut in pieces, each portion preſerves for ſeveral months the motions it had before it was ſeparated from the others. I conceived that an animal ſo tenacious of living, might well reſiſt the venom of the viper, without dying or even being incommoded, by it. I fixed then upon leeches, but before I had them bit, I took care on removing them from the water, to wipe them very dry with a piece of linen, fearing that the mucosity or kind of glue that covers them, and which they emit when touched, might prevent the success of my experiment. I had one of the largest kind, that are called horse leeches, bit by a very strong viper which I had previously thrown into a violent rage. It pierced its body, from which a small quantity of blood flowed, several times through the whole substance of it. I afterwards put the leech into water, and it continued to move as usual. On the day following I changed its water, and found it very lively and swimming perfectly well in the glass. It lived in this way for several days, and would certainly have lived much longer, had I not applied it to another purpose.

I took a smaller one, of that species that have different coloured stripes on the back, and that are employed in medicine. I had it bit by two vipers, which pierced its body in several places. It was bit the next day by a third, and the day following again by two others. Its skin was full of holes, from which a viscous and black matter flowed, on pressing with the fingers. Notwithstanding this it continued to live, and move about in the water. Lastly, I had many other leeches of both kinds bit in the same way, at some times in the head, at others in the body, and neither of them died of the venom.

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I did not ſtop here, but fearing that the venom might have been enveloped and deadened by the glutinous humour of the leeches, which oozes out in greater abundance the moment the tooth of the viper has pierced the ſkin, I made deep wounds in ſeveral with a biſtoury and with ſciſſars, and poured into them large drops of the venom. I introduced into the bodies of others, bits of tow moiſtened with the venom, and paſſed quite through, and this method, which I had always found mortal when tried on other animals, was quite without effect on this occaſion, ſince neither of the leeches died. I preſerved for ſeveral months, in glaſſes filled with water, parts of leeches quite alive. Each of theſe pieces preſerved the motions there, that it poſſeſſed when united with the other part of the body. I had ſeveral of theſe bit by vipers, and made notches in others, paſſing into them bits of tow ſteeped in the venom: neither of them died. They preſerved all their motions, and did not ſeem at all incommoded. The leech then has the property of reſiſting the venom of the viper, which is quite innocent to it.

I afterwards wiſhed to try what would be the effect of the venom of the viper, on ſnails and ſlugs. I procured the largeſt of them, and of different kinds. I had ſome of them bit in ſeveral parts of the body, and by ſeveral vipers. I likewiſe made inciſions, into which I introduced the venom, taking good care before hand to wipe off the glutinous matter that covers them, that the poiſon might penetrate the readier. Out of twenty-ſeven ſlugs and ſnails on which I made theſe experiments, one ſlug only died, twenty hours after it had been bit. I could not even ſucceed in killing them with the envenomed bit of tow introduced into their bodies. The greater part of them covered themſelves with their viſcous ſlaver on being bit.

In the country about Piſa a ſnake is found, which the peaſants call aſpick, and which they repreſent as more venomous than the viper. This creature has ſome exteriour reſemblance to the viper, but has neither the canine teeth, the ſheath, nor the veſicle or reſervoir of the venom; and the experiments I have made, have convinced me that it is an animal in no way dangerous. The ſnake with two heads that was brought to Redi, and of which he gives a deſcription at the beginning of his obſervations *on living animals that are found in living animals*, was of this ſpecies. That of Redi was ſingular however in having two heads. I wiſhed in the firſt place to be certain whether the venom of the viper is mortal to this kind of ſnakes, and had one of them bit twice in the tai' y a large viper. Two days after, I had it bit by two others in the back, from which a little blood flowed, and after two days more held three other vipers to it, which gave it ſeven or eight blows on the neck with their teeth. It was rendered a little torpid by them, and was ſlower in its motions, but on examining it two days after I found it alive, and on putting it to the ground

ground it crawled along and ſeemed in perfect health.

The venom of the viper has no greater action on another larger ſnake, which in Tuſcany is particularly diſtinguiſhed by that name; this is the *adder*. I had ſeveral of them bit on the back, tail, neck, and belly; to ſome of them I have even held three vipers at a time: neither of them died. They did not even ſeem ſurprized at it, neither were they benumbed. I at length employed the envenomed tow, forcing it into wounds I purpoſely made. In ſome of them I even removed the ſkin at particular parts, to convey the venom the better to the blood. All theſe attempts were without effect. It ſeems certain then that the venom of the viper is in no way either mortal or dangerous to this ſpecies of ſnakes. It is not alone then on animals of the *worm* claſs that the venom of the viper is deſtitute of action; there are others again, the organization of which is more compoſed, and which have a heart and many viſcera, and are notwithſtanding out of the reach of its fatal influence.

I have found another ſnake called cecilia, the *orvai* of the French, which likewiſe reſiſts the bite of the viper. I have frequently made experiments on theſe ſnakes, and have had them bit by ſeveral vipers at a time, and in different parts of the body. This creature, naturally ſluggiſh, did not ſeem incommoded by the venom, even when I introduced it into its body by the means of inciſions.

These three ſnakes, the aſpick, the cecilia, and adder, are not venomous, ſo that one incurs no riſk, even when they bite ſo as to draw blood; they have no canaliculated teeth, no ſheath ſuch as that which covers the teeth of the viper, nor a reſervoir for venom; in a word, they are creatures perfectly innocent, as I have aſſured myſelf by many experiments.

I had two turtles bit by a very large viper, rendered furious, in the hind feet where the ſkin is the leaſt hard. I kept them alive for more than ten days, during which time they did not appear to ſuffer, and walked as uſual. I had another bit ſeveral times in the neck, and as a clear proof that the teeth of the viper penetrated through the rough ſkin, one of them was left in that had forced its way into the vertebræ. On the day following, I had it bit in the neck by another viper, and by a third in the fore feet. Laſtly, on the third day it was again bit by two vipers in the neck and hind feet. It not only ſurvived, but did not ſeem to have ſuffered the ſmalleſt inconvenience. One would have ſaid on the contrary, that it was become more ſenſible and active.

I had five others bit in the breaſt and belly by eight vipers, after removing the inferiour ſhell, and laying the fleſh bare. Neither of them died. They were even living four days after, which is uſually the caſe with thoſe that have been deprived of the lower ſhell only. In others I made deep wounds in the feet, and even removed the ſkin in

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ſome of them to introduce the poiſon the better. At length I forced into the wounds, large bits of tow ſoaked in the venom. Neither of them died nor had the ſmalleſt ailment.

I do not believe however that turtles are abſolutely beyond the reach of the effects of the venom. One of them died, after it had been bit by eighteen vipers. The blood oozed from the wounds theſe animals had made, in every part of its body. A third died twelve hours after it had been bit in the neck by three vipers only; and a third again in the ſpace of twenty-four hours, notwithſtanding it had been ſimply bit in the feet by two large vipers. It appears then that this venom does but rarely penetrate and diffuſe itſelf in the bodies of turtles, and that its action there is much ſlower and weaker than in the other animals with cold blood. Theſe laſt in general, die from the effects of this poiſon, at leaſt all thoſe I have had bit, not even excepting eels, which however die later, and not till the end of eighteen or twenty hours. The other kinds of fiſh are likewiſe killed by the venom of the viper. And laſtly, the ſmaller lizards ſcarcely ſurvive its bite for a few minutes.

Animals with warm blood are univerſally killed by this poiſon; I have at leaſt never met with any one that ſurvived its action. A ſmall goſs hawk died in leſs than three minutes. In a few ſeconds it began to open its beak, as if it felt a difficulty of reſpiration, and had an inclination to vomit. A few inſtants after it fell on its breaſt, and could not

again

again recover its feet. It at length died with all the ſymptoms of an extreme debility. I have generally obſerved that animals with warm blood, and the action of the heart of which is very lively, die much ſooner than the others.

There are ſeveral kinds of animals, very diſtinct from each other, to which the venom of the viper is not a poiſon; or if it be ſo, it is but very rarely, and that with the leaſt poſſible energy. There are perhaps many others we are ignorant of, that reſiſt its action. I have myſelf found ſeveral of the ſpecies of inſects and worms to which this venom is not hurtful. I ſhall perhaps ſpeak more fully of them in another work, in which I ſhall treat of the remedies againſt the bite of the viper.

All theſe particulars ought to render the philoſopher who ſtudies nature very circumſpect, unleſs he wiſhes to bewilder himſelf at every ſtep; they likewiſe ſhow us how little truſt is to be repoſed in the ſimple analogy that may be found betwixt different animals, either as it regards life, or the economy of their motions. Nature does not ſuffer herſelf to be devined. Experiment alone, in the hands of an attentive and diſcerning obſerver, can ſnatch from her her ſecrets.

CHAP.

CHAPTER VII.

The Venom of the Viper is not Acid.

IN a ſmall publication of Mead on poiſons, printed in 1739, with the falſe indication of Amſterdam and Naples, the venom of the viper is ſaid to be acid, and to change the blue colour of the turneſol to red; of the truth of which he ſays he is convinced by his own experience. To be certain of this, I received the venom of a viper I had juſt killed, on a bit of glaſs, forcing it immediately from the point of the tooth, by a gentle compreſſure of the palate. I afterwards poured this venom on a bit of blue paper, which ſoaked it up, but inſtead of becoming red, turned a little yellow, and preſerved this appearance even after it was dry. It appeared extraordinary to me that ſo learned a man as Mead ſhould have been deceived in ſo eaſy an experiment. I therefore took a greater quantity of venom, with which I rubbed ſeveral pieces of blue paper, and that nothing might be neglected, varied the experiment a hundred different ways. At times, to have the venom the purer, I took it immediately from the tooth, before it had touched the other parts of the mouth; and at others again, either forced a bit of cotton into the mouth of a living

living viper at the moment of its biting, or introduced it into that of a dead one filled with the venom. I diluted a quantity of venom in water, and wet blue paper with it. I tried to find whether the mixture of the venom with the other humours of the animal, had not deceived Mead as to the colour, and varied my experiments for that purpose infinitely, but in vain. I could never turn the paper red. It simply took the yellowish tinge found in the venom itself. Mead likewise maintains, that he has seen the mixture of this liquor with violets become somewhat red: I have tried this, but the event has not been the same. When the venom is in a greater proportion than the sirop, the mixture does indeed become a little yellow, but never becomes red. I increased, I diminished the quantity of the venom; I have taken it pure, and again have employed it mixed with the foam of the viper: I could never perceive any thing besides a slight yellow tinge, and all my experiments have only served to confirm me in the opinion that the venom of the viper neither changes red the sirop of violets, nor the dye of the turnesol (*a*).

In the same work on poisons, Mead maintains that the venom of the viper is a true acid, and that it effervesces with alkaline substances. In consequence of this I took several fluid alkalies, such

(*a*) Doctor James is likewise of opinion that the venom of the viper is acid, because, according to him, it changes the dye of the turnesol and sirop of violets red, as other acids do.

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as the ſpirit of hartſhorn, and oil of tartar *per deliquium*, with which I mixed different quantities of the venom, always pure and unmixed with the other liquors of the mouth. I never could obſerve the ſmalleſt motion nor the leaſt efferveſcence, at the moment of their union. It was in vain that I had recourſe to a microſcope, I could never obſerve the ſmalleſt air-bubble diſengage itſelf; the colour remained the ſame, and I met with nothing that gave me the ſmalleſt ſuſpicion of the exiſtence of an acid in the venom. It muſt not be thought that the rapidity of the efferveſcence prevented my ſeeing it, ſince the drop of venom was ſo ſlow in uniting itſelf to the alkalies, that it was eaſy to follow it with the microſcope, and to ſeize the preciſe moment of their perfect union.

CHAPTER VIII.

The Venom of the Viper is not Alkaline.

AS authours are to be found who pretend that the venom of the viper is alkaline, and not acid, and as it is principally on the activity and ſuddenneſs of its effects that they have founded this their hypotheſis, I thought it adviſable to conſult experiment thereupon. I took then different acid liquors,

liquors, ſuch as vinegar, ſpirit of ſalt, ſpirit of nitre, ſpirit of vitriol, and laſtly, ſeveral acid ſalts extracted from plants. I united with all theſe acids a larger or ſmaller proportion of the venom, but could perceive no other than a yellow colour, which appeared whenever the quantity of venom exceeded that of the acid. I armed myſelf with a good microſcope, and never found either effterveſcence, motion, or air bubble, to reſult from this mixture. I tried it afreſh with ſirop of violets, but it did not turn it green, as alkaline ſubſtances uſually do.

It is equally without foundation then that naturaliſts pretend that the venom of the viper is acid or alkaline; and it is ſtill with leſs reaſon that they have contrived to explain by theſe hypotheſes, the pernicious effects of this poiſon. Their irrational theories are completely belied by experiment, the only guide to thoſe who enter into the ſearch of phyſical truths. It muſt however be acknowledged, that Dr. Mead has corrected many errours as to facts, in a new edition of his work on poiſons, printed in Paris in 1751, which has reached me too late. He there retracts what he had advanced on the acid quality of the venom of the viper. He confeſſes that the experiments made with the turneſol and ſirop of violets are falſe, and that the venom neither efferveſces with acids nor alkalies. This avowal prevents me from endeavouring to account for the contradiction betwixt his experiments

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ments and mine, and from pointing out what may have occafioned his errour.

CHAPTER IX.

No Salts are difcovered in the Venom of the Viper.

THUS have I the fatisfaction of being the firft to confirm, by experiments more numerous and more diverfified than his, the truths which Mead has difcovered, and which no one that I know of has bufied himfelf about fince him. This conformity fixes in an unvariable way the certainty of my obfervations.

In the courfe of my refearches I have examined with the moft fcrupulous nicety into the exiftence of that pungent and cauftick falt, which Mead himfelf in his laft work, and all the obfervers after him, fay they have feen in the venom of the viper *(a)*.

Mead regards it as a neutral falt. He pretends that he has feen it floating in the ftill liquid venom, and defcribes it as formed of very fharp

(a) James maintains with Mead that he has feen thefe falts, although in a fmall quantity, in the diluted venom. They both fay, that the net-work it forms in drying, is entirely compofed of fmall cryftals.

points. But what was my surprize when on examining the venom with a microscope, I could never discover this collection of saline crystals which the learned Englishman believed he invariably saw! I even employed, but ineffectually, the very strong lens' made in England. I could find nothing throughout besides a yellowish and viscous humour, without any determinate shape, without distinct floating corpuscles or particles, and alike in all its mass, as an oil of any kind appears when viewed with a microscope. The venom I employed was pure and taken from the tooth alone. I varied this experiment an hundred different ways, and even had recourse to the solar microscope; I at length satisfied myself that there are in reality no salts in the venom, and that Mead must have been imposed upon by some particular circumstance.

I then recollected that I had formerly seen with a microscope, certain transparent bodies which floated on the human saliva, and which might easily have been taken for salts. Indeed any one who is not very conversant in the use of the microscope, and who is not well acquainted from habit with the shape of the different salts that are found in liquors, particularly whilst they are drying, would easily persuade himself that the small diaphanous particles which float on the saliva, are absolutely of a saline nature. They are however too light, too large, and not sufficiently transparent, to be really salts. They vary both in size and shape. The direction of these small bodies is rather

ther crooked than ſtraight; they have hollows and folds on their ſurface; and laſtly, they become ſhrivelled and obtuſe in proportion as the ſaliva dries. Thus are they to the eyes of a practiſed obſerver, nothing more than ſmall pellicles or light, plaited, membranes, and as it were the relicks of almoſt digeſted aliments. In reality, they diſappear on waſhing the mouth well, and I have obſerved on touching them with a fine and ſharp needle, that they lengthen or ſhrivel up like ſmall bits of ſkin. I have met with ſmall floating bodies ſimilar to thoſe that are found in the human ſaliva and in that of animals, with the aſſiſtance of a microſcope, in the ſalivary humour of the viper. I have likewiſe ſeen ſome of them floating in a drop of venom I had caught on a ſmall ſilver ſpatula, put into the mouth of a viper, the palate of which was ſtrongly compreſſed. I then conceived how Mead was led into this errour. He certainly took the venom from the mouth of the animal, and not in the way I did, immediately from the tooth; and regarded the ſmall bodies which proceeded alone from the ſaliva, as belonging to the venom.

It is likewiſe true that ſmall bodies or globules, ſomewhat yellow and tranſparent, are often found in the venom of the viper whilſt yet fluid. This never happens but when a ſtrong compreſſion is made on the palate or veſicle, at which time, far from being pure, the venom flows mixed with other corpuſcles ſupplied by the reſervoir.

In Mead's works we likewise find an observation, which is repeated in the Paris Edition, and which appears to establish the existence of these salts in a clear and evident way. He says, that in examining with a microscope the venom of the viper put on a bit of glass, the saline particles, in proportion as it dries, are seen to form themselves into very fine and sharp chrystals, resembling a very fine spider's web; and that these transparent chrystals or needles continue perfect for several months, so strong and firm are they, notwithstanding their smallness.

I took then a drop of the viper's venom, perfectly pure and free from any mixture with the other liquors of the mouth. I dried it on a piece of glass, and viewed it with a microscope. What was my surprise on observing, instead of the drop, a heap of different transparent bodies, of an equal surface, and dispos'ed with great symmetry and regularity! their shape was in general quadrilateral or triangular, and their points very sharp, so that they strongly resembled the net-work Mead has described. Their regularity and transparency might at first sight very easily cause them to be taken for salts, but they were too large, and arranged with too much order, not to make one mistrust this appearance. What at length fixed me in the persuasion that they were not chrystals was, that I did not see any of them in clusters, as they are found in other salts; they were all distinct, and placed at equal distances from each other. Persons who are accustomed to view the salts of other fluids, must perceive the weight of these last proofs.

proofs. I now fufpected that the venom had fplit and cracked in different places in drying, and that this had occafioned its being thus divided on the glafs, as happens to feveral fubftances, which when they dry, fplit in this way into thoufands of fragments, either pretty regularly fquared, or in a triangular form, and all at equal diftances. If thefe cracks are throughout of the fame fize, it is owing to the fame caufe, that is to fay, evaporation, acting at the fame time and with fame the force on the whole furface; it is from this that it reprefents a kind of net-work with different mefhes, exactly like the web of a fpider.

Laftly, to make myfelf ftill more certain that thefe were not falts, but rather fo many fcales and broken pieces of the dried venom, I fell upon a new experiment which I thought a decifive one. I dried a few drops of the venom, in a very pure ftate, in a fmall concave glafs; I then examined them with a microfcope, and found them as ufual, full of fmall crevices, reprefenting a fpider's web. It was however very clear that thefe chinks, towards the bottom of the glafs, were larger in proportion as the dried humour had a greater thicknefs. Thefe pretended falts were no other than the fragments of venom feparated and dried on the glafs. Thofe that were the thickeft had little or no tranfparency. They were of a yellow colour, like the venom itfelf in a fluid ftate. They are fimply caufed then by the parts of the venom retreating from each other during the evaporation; and this is even visible to the eye, without the assistance of a microscope.

But to remove all doubt and suspicion on a matter so important and so generally adopted, and on which Mead has in short founded his hypothesis of the action of the venom carried into the blood of animals, I made another experiment, which absolutely proves the nonexistence of this pretended saline net-work. I put a drop of venom on a flat and smooth glass, and followed it very attentively with the microscope during the whole time of its drying: nothing occurred however similar to what happens to salts dissolved in water. The saline particles during the progress of the evaporation collect together and approach towards the centre from the circumference, forming at first very small chrystals, which encrease in bulk, from the addition of saline particles of the same nature which unite with them. Here, on the contrary, I found nothing besides a humour which, in proportion as it dries, cracks and presents furrows, that form the quadrilateral and triangular fragments I have mentioned. These crevices, which are like the spaces betwixt the threads of a net, appear at first at the circumference, and proceed gradually towards the centre in proportion as the desiccation advances. But the quadrilateral and triangular fragments that fill the spaces betwixt the crevices, and represent meshes, do not encrease here as the saline particles do in a dissolution of salt during the progress of the evaporation. I repeated this several times with a singular pleasure. I mixed the

the venom with a few drops of very pure ſpring water, which I left to evaporate, and obſerved it patiently with a microſcope, hoping in this way to diſcover any ſalts it might contain, but I was not ſo fortunate: no better method can however be fallen upon for this purpoſe.

Two celebrated Profeſſors of the Univerſity of Piſa, Meſſieurs Perelli and Lampredi, were witneſſes to my experiments. They very gladly honoured me with their preſence, and conſtantly aſſiſted me, particularly when I made my reſearches on the ſalts of the venom of the viper. They both agree that whatever reaſon they might have previouſly had to ſuſpect their exiſtence, my experiments, joined to a little reflection, have been more than ſufficient to deſtroy the very ſhadow of a ſuſpicion.

It muſt likewiſe be remarked, that the clefts which form when a large drop of the venom is evaporated, are much larger than when the drop is ſmall, or when it is diſſolved in water, or very much ſpread on the glaſs; theſe large clefts are diſpoſed like rays that proceed to an union with each other, towards the centre of the dried venom. The ſpace betwixt theſe rays is likewiſe diſſected by other taanſverſe rays, which become cloſer in proportion as they approach the centre, and form the abovementioned figures, beſides many other very irregular ones. Theſe tranſverſe clefts are ſmaller at the circumference, are at greater or leſs diſtances from each other, and are bent into ſegments of a circle.

When the venom of the viper is viewed with a microſcope, very ſmall and tranſparent particles or ſpots are likewiſe ſometimes obſerved in it, which are the laſt to dry.

Thus have I fully ſatisfied myſelf of the nonexiſtence of thoſe ſalts which phyſicians and naturaliſts have hitherto admitted with ſo much confidence. I have ſeen the theories founded on this principle, to explain the action of the viper's venom, fall and vaniſh before experiment, which proves that no ſalt, either acid, alkaline, or neuter, exiſts in this humour.

CHAPTER X.

The Venom of the Viper has no determinate Taſte, and when put on the Tongue cauſes no Inflammation.

FROM the teſtimony of Redi, the venom of the viper was at firſt thought to be inſipid, and ſomewhat ſimilar in taſte to the oil of ſweet almonds. We however find in no part of his works that he experienced this himſelf. He ſeems on the contrary to have truſted in this reſpect to a certain Jacques, a viper-catcher, who was venturous enough to taſte this dangerous liquor. He boaſted that he could ſwallow a whole ſpoonful of it, and Redi tells us that he has been ſeen to take it ſeveral times.

Mead

Mead, on the other hand, aſſures us that he has taſted it himſelf, that he has made others taſte it, and that it is acrid and pungent; he ſays that it leaves a ſenſation of burning on the tongue for ſeveral hours, notwithſtanding it is diluted with warm water. He adds, that a pain and ſwelling of the tongue ſoon rewarded the temerity of him who taſted it pure. Theſe contradictions reduced me to the philoſophical neceſſity of taſting the venom myſelf. I did ſo, but not without repugnance; and as the celebrated Morgagni obſerves in his excellent letter on poiſons *(a)*, I ſhall adviſe no one to try it in the gaiety of his heart, leſt he ſhould happen at the time to have ſome excoriation on the tongue, which is a circumſtance not always eaſy to determine. Here however a point was to be ſettled which has divided the opinions of the moſt modern and moſt reputed authours.

I put a drop of the venom then on a bit of glaſs, and diluted it with ten or twelve drops of water; I touched the tip of my tongue very ſlightly with it, and immediately felt a ſenſation as it were of cold and inſipidity. I waited a little, in expectation of that burning ſenſation, which acid and cauſtick liquors occaſion, and at length withdrawing my tongue, paſſed it acroſs my lips, gums, and palate, that I might better come at the ſavour of the venom: notwithſtanding this I could find no taſte in it, except that of a very inſipid liquor. I then took all the venom I could expreſs from a viper, and

(a) De ſedib. et cauſis morb. Epiſt. 49.

ventured to put it in a pure ſtate on my tongue, the point of which I rubbed well with it, as the moſt ſenſible part; I likewiſe rubbed my lips with it. I found a degree of conſiſtence and viſcoſity in it, but nothing acrid, pungent, or burning; in a word, it had no *determinate* taſte. It is however not ſo inſipid as pure ſpring water. There is ſomething in it that reſembles the almoſt inſenſible ſavour of the freſh fat of animals, with a very ſlight flavour which one can ſcarcely diſtinguiſh, but which would be pretty like that of the viper's fat, if this laſt was not ſtronger and more nauſeous.

I found no greater taſte nor ſmell in it on drying it, and reducing it to powder. As I could meet with no naturaliſt bold enough to make the ſame trial, and as a ſupport to my opinion, I gave it to my ſervant, a native of Tirol, named Jacques Benvenuti, to taſte. This man, as intrepid as the one Redi ſpeaks of in ſuch terms of admiration, ſwallowed it repeatedly at different times, at ſome times pure, and at others diluted in water, varying the quantity, and never perceived it to ſwell or burn either the tongue or the mouth. He ſaid however that when he took it pure and in a large quantity, the ſenſation he felt was very different from that excited by oil of ſweet almonds, pure water, or either acid or ſharp ſubſtances: but he could not tell in what this difference conſiſted. A ſenſation ſometimes continued on the tongue for ſeveral hours, not of pain, but as he deſcribed it, ſuch as is felt on taking ſome-

ſomething aſtringent. His obſervation was juſt, for I myſelf have experienced this diſagreeable kind of ſenſation, which frequently continued for five or ſix hours, in the parts of my mouth where the poiſon had remained a long time. If it is taken in a ſmall quantity and mixed with water, it leaves no ſenſation on the tongue; and this ſpecies of diſorder in the mouth is not felt the inſtant the venom is taſted, nor immediately after, but only at the end of a certain time, and it is likewiſe neceſſary that the venom be kept a long time in the mouth. I have repeated theſe trials more than an hundred times, and have never had my tongue either ſwelled, inflamed, or painful. What is ſtill more, the venom when even applied to the eyes, cauſes neither pain nor inflammation. I have laid ſome of it ſeveral times on the tunica conjunctiva of different animals, ſuch as dormice, cats, and dogs, and neither tumour nor inflammation has ever ſupervened in this part, which is otherwiſe ſo ſenſible to the impreſſion of ſubſtances, frequently thoſe that are the moſt innocent. I have even introduced it into the noſes of theſe animals, without their ever betraying any ſign of ſuffering the ſmalleſt inconvenience from it.

It is certain then that the venom of the viper is in no way ſimilar to cauſticks, and that it is not acrid and hot like that of the bee or ſcorpion. Scarcely had I put an atom of the venom of the bee on my tongue, either pure or mixed with a little water, than it ſtung and burned with as much violence

lence as if I had applied the ſtrongeſt cauſticks that chemiſtry affords. The venom of the waſp and that of the hornet are not leſs acrid and pungent than the bee's, and the pain that each of them excites laſts a long time. I took it ſometimes from the ſting, and ſometimes from the ſmall veſicle that ſerves as a reſervoir to it, and found it in both caſes alike, and invariably productive of the ſame pain. It ſtill preſerves its ſtrength and cauſticity after having been dried, and kept for ſeveral days.

It is the ſame with the venom of the ſcorpion; the white and viſcous humour it throws out by its ſting when it darts it, cauſes a ſenſation on the tongue ſimilar to that occaſioned by the venom of the viper, but much weaker. It is on this account that the ſting of the bee is more painful than that of our ſcorpions. Probably the venom of thoſe of Africa is exceedingly cauſtick, ſince it kills animals in a very ſhort ſpace of time.

I afterwards made a trial of the viper's venom on other animals, which, although they are not, like man, gifted with ſpeech, are not backward in manifeſting by ſigns, the pleaſure or diſguſt they feel on eating any thing. I put then a drop of the viper's venom into the mouth of a dog; the creature ſwallowed it with avidity, licked its lips for a long time, as if it had met with ſomething agreeable to its taſte. I then ſteeped a bit of crum of bread in the venom, to ſuch a degree that it became quite yellow, and gave it to the ſame dog, at a time when it had already fed ſo plentifully as to refuſe

food

food. It ſmelt to it, and inſtantly devoured it, manifeſting the ſtrongeſt deſire for more: in a word, every time a drop of venom approached its lips, it licked it up with the greateſt ſatisfaction.

Every body knows that dogs, like children, are ſworn enemies to whatever is bitter and acrid, and that they are paſſionately fond of whatever is ſweet and unctuous. Hence we muſt conclude, that if the dog found the venom agreeable, it was undoubtedly owing to its ſweetneſs. Thus is it abſolutely falſe and imaginary, that it is acrid and fiery; as it *alſo is that* the tongue, on taking it, ſwells, inflames, and becomes painful.

Mead had an idea that the venom of the viper, when applied to the wounds of a living animal, cauſed a very painful ſenſation: a natural conception to thoſe who believe like him, that it abounds in ſalts, which render it hot and cauſtick. He endeavours to eſtabliſh his opinion on an experiment he made on a dog. This animal did not ſeem very ſenſible of the pain occaſioned by piercing the noſtrils with a crooked grooved needle; but when the venom entered the wound, it howled and became furious. I made the very ſame experiment on a young dog, and it appeared inſenſible to the entrance of the drop of venom into the wound. I muſt acknowledge however that I have ſeen a cat ſhake itſelf and become more agitated, at the moment the venom was forced into the lips of a wound that had been made in its noſe. But this experiment is always liable to errour, ſince the needle not only remains in the wound,

wound, but the motion of the animal is ſtill another cauſe of its being more agitated there, and of its being forced ſtill deeper, and cauſing a greater laceration of the parts. This is doubtleſs ſufficient to renew the pain, and even to wound the nerves that eſcaped the firſt introduction of the needle.

I have often poured the venom into inciſions made with a lancet, and could at no one time aſſure myſelf to a certainty, that the introduction of it was productive of pain; although it ſometimes happened that I was pretty much convinced to the contrary.

But were it well proved that the venom of the viper cauſes pain, does it follow that we can draw an indubitable concluſion that it abounds with ſalts, or that it is acrid and cauſtick? As if we had not examples of a juice which, though inſipid to the taſte, brings on violent pain when applied to a wound. I have myſelf known people, who having been bit by a viper, have notwithſtanding felt but a very ſlight pain, ſuch as might very well have been cauſed by the ſimple blow of the tooth. We have a dexterous viper-catcher at Piſa, named Bongi, who having one day been bit in the finger, did not perceive it till he ſaw the blood flow, a proof that he did not feel any pain. His father relates a ſimilar circumſtance; he likewiſe had been bit in the finger, and compares the pain it occaſioned to the bite of a fly. However they both in the concluſion became very ill of their wounds, a clear demonſtration of the venom having found its

way

way into the blood. I am well persuaded then from experience, that the venom of the viper is neither acrid nor burning *(a)*, and that it does not contain those salts which so many writers have imagined, either for the purpose of explaining its mode of action on the blood, or because they have been imperfect in their observations.

CHAPTER XI.

Properties of the Venom of the Viper.

THE deadly humour furnished by the viper, which I have neither found to be acid, alkaline, nor caustick, subsides instantly on being thrown into water, like certain heavy oils drawn from vegetables. In this situation the parts of it preserve their viscosity and natural union, and remain in that state for some time, without changing either their primitive colour, or their transparency. This poison then is heavier than water, and differs in that respect from common oils, from the fat of animals, and from that of the viper itself, all of which float on water. Oils and other liquids that are heavier than water, should be at least suspected, and are indeed often found, to be very violent poisons. Without mentioning the oils of the common and cherry laurel,

(a) The modification this expression admits of, will be shown in the sequel.

rel,

rel, the red oil of bitter almonds by diſtillation is a poiſon.

My next enquiry has been to know whether the venom of the viper is inflammable, that is to ſay, whether the phlogiſtick principle it contains is capable of taking fire. I have thrown it on burning coals. I have ſteeped a piece of paper and a bit of wood in it, and I have collected it in ſmall drops on the point of a needle: it has never taken fire, and I have not found it to be more inflammable than the other fluids of animals.

This obſervation holds good as to the venom of the bee, and thoſe of the waſp, hornet, and ſcorpion, which ſo far reſemble that of the viper. They all conſume and dry in the fire, without kindling into a flame.

If a pure and freſh drop of the viper's venom is applied to the mouth, it is found to poſſeſs a certain viſcoſity; but when it is dried in large drops on a bit of glaſs, it has the appearance of a tranſparent and yellow jelly: it then, like pitch, adheres ſo ſtrongly to the teeth, that it is with difficulty detached from them.

CHAP.

CHAPTER XII.

Peculiarities of the Venom of the Viper, and that of other Venomous Animals.

IT has been ſeen that, contrary to the opinion of Redi, the venom of the viper flows from the hole at the point of the tooth, and that it enters by the hole ſituated at its baſis. According to this diſpoſition one would be tempted to believe, that theſe teeth have been formed for the expreſs purpoſe of killing, ſo much does the ſmall hole at the point ſeem calculated to convey the venom into the blood of the animal bitten. But I do not here pretend to recur to final cauſes, and am very far from thinking that all this ſingular mechaniſm in the viper, has been expreſsly made for the deſtruction of other living creatures. The venomous liquor with which it is provided is perhaps neceſſary to its digeſtion, and I ſhall ſhow that it ſingularly diſpoſes the fleſh on which the viper feeds, to a ſpeedy putrefaction; a degree of change 'tis neceſſary for it to undergo for the purpoſe of being well digeſted. However, by an unlucky but neceſſary mechaniſm, the ſame tooth at once conveys the poiſon into the animal the viper bites, and into the aliments it feeds on. Who knows but that the depriving it of this venomous humour would expoſe it to accidents

dents ſimilar to thoſe that happen to other animals, from a defect or depravity of ſome one of their digeſtive juices?

If it were true, for example, that the human ſaliva, as has been believed, is a poiſon to certain kinds of animals, and a philoſopher in this number, reflecting and reaſoning on its nature, ſhould obſerve that this ſaliva is one of the principal juices that concur to our digeſtion, would this reaſoning animal be miſtaken? Would it not, on the other hand, have divined nature? But if, on the contrary, one of theſe ſpecies' of animals ſhould pretend that our ſaliva has been ſupplied us for the purpoſe of poiſoning them, ſince it actually does deſtroy them, would it not be fallen into a very abſurd errour? See however where thoſe incautiouſly hurry themſelves, who inceſſantly recur to final cauſes, in the examination and explanation of natural facts and events.

Finally, it is a general law of venomous animals that wound either with the tooth or ſting, to convey the venom into the wound by holes or orifices they have in thoſe parts. As to the ſcorpion, writers do not agree either on the number or ſituation of theſe orifices. Redi, by an inconceivable fatality, could never diſcover them; and as he had ſeen only a ſingle drop of venom on a plate of iron againſt which he had made a ſcorpion ſtrike its ſting repeatedly, he inferred from thence that there was a ſingle hole only at its extremity. Valiſnieri reckons as many as three. It is however very certain that thoſe of Tuſcany I have examined, never had more than two lateral openings, through which the venom flowed, and that neither a ſingle one nor three are to be found in them, as theſe two obſervers have maintained. When the ſmall veſicle which terminates the tail of the ſcorpion, and at which the ſting begins, is gently compreſſed, the two lateral apertures, and likewiſe the venom at the criſis of its flowing out of them, are ſeen with the help of a good magnifying glaſs.

But to return to the viper; its venom preſerves itſelf in the cavity of the tooth for ſeveral years, without looſing its colour or tranſparence. If this tooth is then put into warm water, the venom diſſolves very quickly, and is ſtill capable of killing animals. It beſides preſerves its activity for ſeveral months after being dried and reduced to powder, as I have many times experienced in common with Redi. It is here ſufficient that it is conveyed into the blood as uſual, by the medium of ſome wound: it muſt not however be kept too long, ſince I have frequently known it to loſe its effect at the end of ten months.

I do not heſitate to believe, that the animals the death of which is occaſioned by their touching the heads of vipers, even a long time after they are dead, are in reality ſimply poiſoned by the venom lodged in the cavity of the tooth, which being diſſolved by the blood of the wound, may have flowed out at the elliptical hole at the point of the tooth. A bit of dried venom that may happen to adhere to the

the outer ſurface of the tooth is likewiſe capable of producing this effect. I am well aſſured by all the obſervations I have made, that the head of the viper dies in leſs than twenty-four hours, and that its muſcles dry in a few days provided they are in a very dry place, or ſoon become putrid if the place is wet. The teeth of the viper are beſides very ſharp, ſo that they pierce the ſkin, however ſlightly they touch it. I have twice ſucceeded in killing animals by ſimply wounding them with a tooth which had been plucked from a viper ſeveral hours before, and which was filled with coagulated venom. If the nephew of the famous Jaques the viper catcher, as Redi informs us, wounded himſelf ſeveral times in the hand ſo as to draw blood, with viper's teeth he had juſt plucked out, without his ever experiencing any other ill effects than what would have reſulted from the prick of a pin or of a thorn, he did not however at any time make the experiment without the greateſt riſk of there being ſome remains of this mortal poiſon in the tooth. The chickens likewiſe that Redi wounded in ſeveral parts of the body, with the teeth plucked from a living viper, all incurred the ſame riſk.

I do not deny but that the venom contained in the veſicle may be likewiſe capable of killing, even on the day following that on which the head has been ſeparated from the body of the viper. To effect this it will be ſufficient that the animal has not bit before it was killed, and that the head is neither too dry nor too rotten, ſince in theſe caſes, the veſicle would either be deſtroyed, or could no longer convey the venomous humour to the tooth, by the excretory conduit already obſtructed and dried up.

From what has hitherto been ſaid, it may be conceived how certain mountebanks, according to the relation of the authour of a work on Theriaca, dedicated to Piſo, ſuffered themſelves to be bit by vipers with impunity. " There are men," ſays this authour, " who under the pretence of poſſeſſing " an antidote, have themſelves bit by vipers; they " previouſly give them a certain paſte which ſtops " up the holes in their teeth, and thus renders " their bites ineffectual, to the great aſtoniſhment " of the ſpectators, who are ignorant of the method " employed by theſe people to conceal their im- " poſture." This paſſage clearly ſhows us, that even in theſe times they had ſome knowledge of the ſtructure of the viper's teeth, and that they were of opinion, that the venom was carried by this hole into the wound. We likewiſe find in the work of Chryſogonus, entitled *De Artificioſo modo Curandi Febrium*, that this authour, who lived a long time after, was of the ſame opinion. Speaking of the viper, he ſays, " it has two teeth, the right and the " left, fixed in the lower jaw, and each of them " perforated; they are longer than the others, and " are ſhed every year when theſe animals quit their " ſkin: theſe two teeth are enveloped in two ve-

" ficles filled with venom, whence it flows into the
" teeth by the hollow canal, the inftant the viper
" bites."

This authour feems only to have added miftakes to what was known of the natural hiftory of the viper before his time. It is falfe, for example, that it fheds its teeth every year, when it changes its fkin; it is falfe that the two veficles furround the teeth; it is falfer ftill that thefe two teeth are placed in the lower jaw. This alone proves fully that he never examined the mouth of the viper.

I endeavoured myfelf to have animals bit with impunity, and for this purpofe prepared a pafte with pitch, turpentine, and yellow wax. I made two vipers bite feveral times at this compofition, and they remained for fome days without being able to do any mifchief. I found that their teeth towards the point, were indeed filled with this glutinous pafte, which ftopped up the orifice out of which the venom fhould have flowed.

I do not believe however that this method is a certain preparative againft the bite of thefe animals. We have feen that there are circumftances in which the venom may likewife pafs immediately from the excretory conduit into the fheath. The fureft way then would be to entirely remove the refervoir; and thus the mountebank would impofe on the fenfes of the vulgar with greater certainty, fince he would no longer have any thing to dread from thefe dangerous animals.

There

There are excellent naturalists who believe that the fly, which they call in Tuscany, *Assillo*, (the ox-fly) throws out a venomous and caustick juice from the end of the sting it has at the extremity of its belly. Valisnieri, who has written so well on this insect, thinks that when it pierces the hide of the larger animals with this very sharp sting, it insinuates into it a species of poison of a very corrosive nature, which irritates and as it were burns the tender filaments of the nerves of the part, so as to produce spasms, throws their blood into an effervescence, and drives them to madness (a).

Reaumur, that great and exact observer of the minutest animals believes, in opposition to the opinion of Valisnieri, that this pain is rather the effect of a simply mechanical wound, than of a venom or any other caustick matter that the ox-fly may throw out of its sting (b).

The celebrated Morgagni, after having nicely weighed these two opinions, does not precisely embrace either of them, but seems to combine them so as to form one opinion out of two. He maintains that the pain which the sting of this fly causes to animals, frequently depends on two causes at the same time; that of a considerable nerve wounded by the sting, and of an acrid and caustick venom which irritates the nerves (c).

(a) Tom. I. Page 229. Venezia,
(b) Histoire des Insect. Tom. IV.
(c) De Causis et Sedibus Morborum. Lib. II.

The opportunity I had of procuring theſe flies, inſpired me with the wiſh of examining them. The ancients were acquainted with a fly that, with its ſting, threw whole herds into fury. The Greeks called this fly *Oeſtros*. The Latins have likewiſe mentioned a fly, the ſting of which produced the ſame effect on large animals. This they named *Aſſillus*. I do not doubt but that the *Oeſtros* of the Greeks, and the *Aſſillus* of the Latins, is the ſame with the *Tabanus* of Varro and Pliny. And although the ancients have diſcovered their uſual negligence in the deſcription they have given of this fly, it is however impoſſible not to ſee that it is no other than the *Aſſillo* of the Tuſcans, and the *Taon* (ox-fly) of the French. We muſt otherwiſe determine within ourſelves, that a fly which was ſo common amongſt the Greeks and Latins, has not deſcended to us, and that its ſpecies has been long deſtroyed and extinct. I flattered myſelf that I could find with eaſe the ſmall veſicle that contains the venom of this fly, and the hollow ſting that conveys it, as they are readily found in the bee, the waſp, and the hornet: I was however deceived. The ſting, much larger than that of the bee, is notwithſtanding neither hollow nor channelled, and I could never diſcover any cavity in it, either in its outer or inner part. I did not ſucceed better in finding the reſervoir of this pretended humour; in ſearching for which the ſtrongeſt lens' were ineffectual; it was in vain that I compreſſed the extreme part of the belly and the root of the ſting, I could never perceive a fluxion of of this liquor, as it is ſeen in the bee, waſp, and hornet; and in a word, in all the animals that convey venom into the wounds made with their ſtings.

But to leave nothing undecided on this ſubject, I endeavoured ſeveral times myſelf, and engaged others in the ſame trial, to diſcover the venom by its taſte, by applying the ſting and the parts of the belly moſt adjacent to it to the mouth. I bruiſed it betwixt my teeth, and rolled it in my mouth, but could not find any thing acrid or burning in it, and did not feel the ſmalleſt pain or inconvenience. If it were however true, that this humour is ſo very acrid and cauſtick as to burn, as it were, the nervous filaments of the oxen, I certainly ought to have felt it on my tongue, ſince the venom the bee carries in its ſting, cauſes an intolerable ſmart and pain in that part.

It is falſe then that the ox-fly, at the ſame time that it pierces the hide of oxen, ſheds a poiſon. The pain it cauſes is ſimply mechanical, and ariſes from the particular ſhape of its ſting. This is compoſed of three ſmall, ſharp, and pointed hooks, of a horny ſubſtance, which when united together form a kind of pincers. The ox-fly does not uſually cauſe any great pain by its ſting, but if it accidentally wounds a large nerve or other ſenſible part of the animal, or if, which is more probable, it withdraws its ſting with fear and precipitation, and in a direction oppoſite to that in which it entered, it then happens that by tearing the ſkin and dragging the nerves forcibly with its hooks, it muſt neceſſarily cauſe that very violent and insupportable pain, which throws the herds into fury. We know how great a difference there is betwixt the slight pain caused by a sharp instrument, and that excited by a weapon that tears and lacerates the nervous parts.

I have likewise had an opportunity of examining into the nature of leeches. There are naturalists who believe them to be venomous, because the wounds they make are very painful, remain a long time open, and sometimes cause a swelling of the adjacent parts. But it is clearly proved that these small animals, so useful in medicine, are destitute of venom, and simply make a mechanical wound with the very singular weapon they have at the bottom of the mouth. This instrument is formed by three semilunar substances placed at the entrance of the oesophagus, towards the centre of which their edges would meet each other, did not this cavity separate them: they are placed perpendicularly in a direction with the length of the animal. The curved edges of these half-moons terminate in a horny substance disposed in ridges, the distance betwixt which gradually widening, they at length form a kind of very fine teeth, like those of a saw.

These worms employ the following method in sucking blood. They make a forcible application to the skin with the outer edges of their mouth. They then make a vacuum by enlarging that cavity in such a way that the semilunar instrument approaches the skin, at which time they move the three saws circularly, and by successively drawing them

them to and from each other, they make three notches in the ſkin, which unite in a ſingle point. In proportion as theſe ſaws recede from each other, the oeſophagus dilates and draws into its cavity the blood that has been pumped up.

I have tried what I advance here on myſelf. I applied a large leech to my arm, after I had cut away half its mouth, and was enabled in this way to view the whole of the mechaniſm at my leiſure.

The teeth and channellings of theſe ſaws are eaſily ſeen with a good microſcope. They may even be felt by paſſing the end of the finger over them; and by drawing the edge of a lancet acroſs them, particularly when they have been left to dry a little, may be heard to grate. In this ſtate they may be employed in ſawing the ſkin, provided they are held firm with pincers, or turned round with their edges conſtantly oppoſed to the part. I have even been able to effect this, notwithſtanding the ſoft parts of theſe ſemilunar bodies, ſuch as the muſcles, were not yet become dry. It is eaſy then to comprehend how the leech, after having contracted and ſtiffened the muſcles that form the greater part of theſe ſaws, contrives to pierce the moſt obdurate hide; and why it is that the wounds it makes are ſo very painful, and bleed for ſo long a time, ſince it only obtains this blood in conſequence of having torn with its ſaws, and made an opening in ſo ſenſible a part as the ſkin, and one ſo abundantly provided with nerves and veſſels.

I here

I here conclude the experiments, which, as I have obſerved in the beginning of this treatiſe, are the moſt certain guide to conduct us to the diſcovery and knowledge of natural truths. Facts alone are however not ſufficient to diſſipate the obſcurity that envelops them. A train of obſervations, without the help of a ſkilful hand to apply them, would be at beſt but the uſeleſs proof of a painful application. In the ſame way the moſt brilliant ſyſtems the rich and fertile imagination of a philoſoper can ſupply, do not deſerve the attention of naturaliſts, unleſs they are founded on good experiments. To come at the cauſes of the laws which regulate the courſe of the celeſtial bodies, nothing leſs was needed than the long ſeries of obſervations of the Chaldean ſhepherds, and the powerful aid of the creative genius of Newton.

CHAPTER XIII.

What cauſes the Death of Animals that have been Poiſoned by the Viper.

THE firſt object of my obſervations on the venom of the viper, was to diſcover the origin of the contradictions which, notwithſtanding they are atteſted on all ſides by learned men of the firſt rank, are found in the various experiments that have been

been made on that ſubject. I muſt confeſs, however, that in verifying and analyzing all theſe particulars, my aim has likewiſe been to find in their combination, if poſſible, a ſatisfactory explanation of the ſpeedy and deadly manner in which this poiſon acts.

I ſhall aſk then, with Redi, "in what way the "venom of the viper extinguiſhes life, and brings "on death? Whether its action depends on a latent cauſe beyond the reach of human intelli"gence? Whether, on its penetrating to the heart, "it chills and freezes up the principle of heat; or "whether, on the contrary, by multiplying this "very principle and giving it more activity, it "kindles it afreſh and conſumes it, and in this way "diſſipates and reſolves the animal ſpirits? Whe"ther it acts by deſtroying the ſenſation of this or"gan? Whether, by the means of a painful irri"tation it excites, the blood does not flow back "too precipitately to the heart, ſo as to bring on "ſuffocation? Whether it ſtops its motion, by "congealing the blood in its two ventricles, ſo that "they can no longer dilate or contract? and laſtly, "whether it coagulates, not only the blood in the "heart, but likewiſe in the whole venous ſyſtem?" "To reſolve theſe queſtions with truth," continues Redi, "is a taſk I am unequal to, and I "place them amongſt the infinite number of things "I now am, and ſhall probably always be, ignorant "of." Other authours, bolder than he, are not afraid of expoſing their ſentiments, whether badly or well founded. Before I propoſe mine, I think it ne-

neceſſary to relate the moſt reaſonable opinions that have been held by naturaliſts, as well ancient as modern, on this ſubject.

The learned Brogiani, profeſſor of anatomy at Piſa, has written a treatiſe full of erudition, on the venoms of animals. He there examines, as a ſkilful critick, the different ſyſtems and various opinions that have been eſtabliſhed on the mode of action of theſe poiſons.

It was at firſt believed that the venom, on entering into the blood, cauſed a univerſal coagulation of it, preciſely as acids do when they are introduced at an aperture made in a vein. The animals on whom this experiment is made, die in a very ſhort time, with tremblings, convulſions, and vomitings. On opening them afterwards, their blood is entirely coagulated in the veins, and as it has likewiſe been found coagulated in certain animals which, after having been attacked with the ſame ſymptoms, died of the bite of the viper, a trifling and hazardous inference has been drawn, that the venom brings on death by coagulation. But if, according to the teſtimony of Redi and the Memoirs of the Academy of Sciences of Paris, this appears not to be equally true of all the ſubjects that die of this poiſon; if it is likewiſe falſe, that they all have theſe tremblings, vomitings, and convulſions; if the blood is frequently found coagulated in this way, in every kind of dead bodies; it follows, that the queſtion yet remains undecided, and the difficulty as great as before: beſides, may there

not

not be other circumſtances capable of coagulating the blood, and exciting the tremblings, convulſions, and other accidents, without recurring to the acid of the venom of the viper? My experiments themſelves have ſhown me that this acid does not exiſt, and that no ſtreſs ſhould therefore be laid on it.

Others have believed on the contrary, that this venom kills by exciting an univerſal inflammation. But how can it be thought capable of exciting it ſo as to occaſion death in ſo ſhort a time? I will go farther, and aſſert that the fever which conſtantly attends inflammation is not always found in thoſe that die of the bite of the viper. There are even no traces of inflammation in their dead bodies, and when any ſuch are found, it is rather the effect of ſome particular circumſtance in the temperament, than of a proper and peculiar quality reſiding eſſentially in the venom of this dangerous animal.

The diſciples of Hoffman, who, at the example of their Maſter, explain every thing by the atony and ſpaſm of parts, endeavour on this occaſion to avail themſelves of a truth to ſupport their opinion. They pretend that this poiſon excites, they know not how, an univerſal ſpaſm in the machine. But again, if this ſpaſm does not exiſt in all the animals that die of this poiſon, how can it be regarded as an univerſal cauſe? It is on the contrary certain, that they invariably die, rather from an atony and univerſal reſolution, than from the rigidity and contraction of their members.

I paſs

I paſs over ſeveral other hypotheſes, which are nothing more than ſimple conjectures, and, far from being ſupported by any deciſive obſervation, are on the contrary belied by experience.

I however think it incumbent on me to relate the opinion of Mead. He ſets out on the exiſtence of cauſtick ſalts in the venom of the viper, and on this foundation grounds the whole of his theory of its effects. In the edition of his book on poiſons, printed in 1739, we find an ample detail of the different opinions of philoſophers, followed by a chain of ſyſtematick reaſonings, which, as any one may ſatisfy himſelf, are very tedious and filled with ſuppoſitions. His object is to ſhow that theſe ſalts decompoſe the globules of the blood, and deſtroy the temperament of it; and as it is difficult to comprehend how they can in this way deſtroy the whole maſs in ſo ſhort a time, he ſays that when once the venom has inſinuated itſelf into a wound, a very ſubtile and very elaſtick fluid riſes out of it, which in an inſtant extends its action to, and brings on a decompoſition of, all the parts, even the moſt diſtant ones, of this fluid. It is thus that a ſingle ſpark which touches a long train of gunpowder makes a rapid progreſs along it, and cauſes an univerſal exploſion, by the ſimultaneous diſengagement of the air encloſed by each particle.

It is without doubt unneceſſary to endeavour to combat this ſyſtem, ſince theſe pretended ſalts do not exiſt in the venom of the viper, and ſince nothing is falſer than the idea of theſe ſmall glo-

bule

bules of blood filled with claſtick air. It is beſides certain that the venom does not alter the ſhape of theſe globules, which when they are obſerved with a microſcope, are found to be exactly the ſame as before, that is to ſay, obſcure and dark coloured at their circumference, and more tranſparent, at the centre, as ſmall round bodies generally are when viewed with a microſcope. I cannot conceive how Baker, otherwiſe very exact in his obſervations, could ſay in his *Treatiſe on Microſcopes*, that the bite of venomous animals, or even an atom of their venom, corrupts the whole maſs of blood, and alters the ſolidity and ſhape of the red globules that compoſe it.

It is not on this occaſion alone that a belief has been held without any foundation, of the change of ſhape of the globules of blood. The ſmall rings that have been endeavoured to be ſubſtituted to theſe globules, are a proof that the light, the microſcope, and the obſerver who relies upon appearances, are frequently the ſource of the pretended changes that do not in reality exiſt. I ſhall ſhow in a ſmall diſtinct work (*a*), that all ſmall globular corpuſcles, viewed with a microſcope, ſeem to be ſhaped like rings, becauſe the rays of light meet the eye of the obſerver in a greater number, from the centre than from the edges.

(*a*) The work announced here was printed ſome years ago at Lucca. It is entitled, *Oſſervazioni ſopra i Globetti del Sangue.*

The

The decomposition of the globules of blood, so frequently advanced by physicians, is one of the rarest phenomena in the animal œconomy. The physicians who are mechanicians, suppose that the globules of blood are so many small round vesicles filled with a very elastick air enclosed in a fine membrane; they likewise believe that these globules (*a*) may easily crack and alter their shape, even from much slighter causes than that of the action of a caustick salt: but the fact is, that they are not vesicles, as they have persuaded themselves, and that they very rarely alter their shape.

Convulsions themselves, that are scarcely ever felt by animals with cold blood, do not prove that the venom of the viper contains caustick salts, the invisible points of which prick the nerves and irritate the muscular fibres. Narcoticks and opium bring on convulsions, but must we therefore believe that they act by like mechanical agents? Still more, convulsions are not always the effect of an irritating *stimulus*; they rather arise from the destruction of equilibrium betwixt the antagonist muscles. Weak languishing animals, that die from a loss of blood, perish in dreadful convulsions; and yet there are in this case neither points nor irritating salts. The convulsions are here likewise unjustly

(*a*) Let it not be understood that they are really globules; their true shape will be seen in a work of microscopical observations which I propose to publish soon, and in which I shall speak of whatever relates to their properties.

justly

justly attributed to the superabundance of animal spirits; it seems more reasonable to believe on the contrary, that it is to a defect of them, or to their irregular distribution in the muscles, or rather to an irregularity in the circulation of the blood, that they owe their origin.

That opium causes convulsions is owing, in my opinion, to its destroying at different times and in an irregular way, the irritability of the muscular fibres. It is besides certain that men and women of a delicate and weak frame are always the most subject to convulsions; and it is not possible to suppose in these people a superabundance of animal spirits. We know that all the muscles, even in a relaxed state, preserve notwithstanding a certain tension of their fibres, which, when they are cut, never fail to contract themselves, and to enlarge the wound. When a muscle becomes paralytick, it lengthens, and its antagonist then contracts the more; which shows that the repose of the muscles depends on the equilibrium of strength betwixt the different muscles, and betwixt their different fibres. The powers thus balanced, destroy and renew themselves at every instant, without producing any motion or sensible change. This natural tension of the muscular fibres certainly depends on an equal and exact distribution of the fluids in the whole substance of the muscles. This truth is demonstrated in a dissertation which I published in the third volume of the Acts of Sienna, which was in part reprinted some time after at Lucca, with several confiderable additions, and which was afterwards inferted in the firft volume of my animal phyficks. [*Phyfique Animale.*]

But if thefe mufcles do not receive the fame proportion of fluids, or if thefe fluids reach them, or are diftributed amongft them, with an unequal quicknefs and energy, the equilibrium of the mutual effort of the mufcles is immediately deftroyed; the ftrongeft of them contract; and hence arife the convulfions and violent agitations of the whole frame. This is the reafon why thofe who die of an hemorrhage, as well as thofe who perifh by poifon, are feized with convulfions: for it certainly is not probable that the lofs of blood and of ftrength fhould bear an equal proportion in every part, in every mufcle, and in every fibre, whilft the circulation itfelf is unequal, and the mufcular irritability is deftroyed gradually, and in a very irregular way according to time and circumftances.

But even though it might be concluded from the prefence of convulfions, that the matter which occafions them is acrid and cauftick, this would not determine it to be a falt; and becaufe falts prick, irritate, and corrode the nerves, can we fay that they alone poffefs thefe properties? too few experiments have been made to warrant the maintaining this.

The convulfions fome of thofe are feized with who have been bit by the viper, do not furnifh a certain argument to explain the nature of the kind of jaundice that fometimes attacks thofe who die of of this bite, or who ficken with the difeafe of the venom. Some authours have afcribed this jaundice to the contraction of the biliary pores at their origin in the liver, by which all fecretion of bile being interrupted, the blood becomes charged with this humour, and depofits the greater part of it in the organs of the fkin.

Others have conceived, with greater appearance of truth, that thefe convulfions, and this violent irritation of the nerves, caufe a conftriction of the biliary ducts, fo that the already feparated bile is carried into the blood, and fpreads itfelf over the whole fuperficies of the fkin. Both thefe hypothefes however are founded on a falfe principle, fince anatomy teaches us that the nerves are not irritable, and that the biliary ducts are not compofed of mufcular fibres. The firft of them is abfurd on another account, for if the bile is not primarily feparated in the liver, and afterwards returned into the blood, how can it fhow its quality and colour? I cannot conceive how very great naturalifts have brought themfelves to think that it is not neceffary for it to be feparated in the liver, to enable the blood to take a yellow tinge, and to give this colour to the fkin.

It is not fufficient that the blood contains all the ingredients of the bile, the fixed and volatile falts, the oil, and the water, to enable it to form bile. It is likewife neceffary that the organs which concur to its generation, appropriate the matter of it, and regulate the proportions; fo that the fame substances which in the proper viscus might have formed bile, can never acquire, when mixed in the blood with the other principles of that fluid, either its nature or properties. But when once it is separated, and thrown again into the mass of blood, it preserves its several qualities in such a way, that all the principles of the blood can no longer decompose them, or break their combination. It may be compared to a drop of oil, which constantly preserves its nature in the midst of another fluid, although agitated and divided *ad infinitum*; each separate particle continues to be oil as before. Thus for example, the principles of *must* (new wine) and of oil certainly exist in the vine and in the olive tree, but they only show themselves in the grape and olive.

A more apposite circumstance still, and one which ruins this hypothesis, is the example of eunuchs. The partizans of it will agree, that it is in vain for these unfortunate people to preserve in their blood, during their whole lives, the principles that constitute the semen, since it does not manifest itself by any of its effects; they resemble women, and never have the smell that characterizes the male. I will go further, and allowing that not only the principles of the bile are contained in the blood, but likewise the bile itself, it will not yet follow that it has the property of giving a yellow tinge to the skin. Animals have been known to have a scirrhous liver, or a very large abscess in that viscus, for a long time, without being jaundiced.

diced. Let us agree then, that if thoſe who are attacked with the diſeaſe of the venom become ſo, the cauſe which produces this effect muſt have intercepted the courſe of the bile after its ſeparation in the liver, without its having done any previous injury to that ſecretion. I am firmly perſuaded that it does not thus pour itſelf into the maſs of humours, but becauſe its courſe is intercepted in the ductus communis choledochus, before it diſcharges itſelf into the duodenum. The convulſions of the ſtomach and inteſtines that attack thoſe who have been bit by the viper, may very readily irritate and contract the duodenum, and ſo ſtop up this orifice. Neither muſt we be aſtoniſhed at ſeeing the ſame jaundice make its appearance in thoſe who have taken other poiſons, ſince they alſo have the ſame convulſions, with a painful drawing together at the pit of the ſtomach, bilious and convulſive vomitings, a contraction about the navel, and other complaints in the abdomen. It may likewiſe happen in certain caſes, that the bile of thoſe that have been bit, may be ſo attenuated and exalted, that it may even penetrate through the ſubſtance of the liver, and immediately make its re-entrance into the torrent of the circulation, conveying the jaundice to the whole ſurface of the body. It is thus that, in conſequence of its being exalted in certain diſeaſes, it paſſes through the thickeſt membranes, and depoſits itſelf abundantly on the colon, duodenum, meſentery, epiploon, and peritoneum, on which, as may be found by opening dead bodies, it beſtows its colour. It is well known that there are very few humours in the animal body that corrupt ſo readily as the bile; and we ſhall ſoon ſee that it is this principle of putrefaction particularly, which the venom of the viper conveys into animals.

But to return to the opinions of authours, as to the immediate cauſe of the death of thoſe that are attacked by the diſeaſe of the venom. The celebrated De Buffon maintains, in his great work of Natural Hiſtory, that the activity of the venom of the viper, as well as of other active poiſons, depends on thoſe microſcopical animalcules, which are diſcovered in the infuſions of vegetable and animal ſubſtances, and which he believes to be ſimple *organical particles*. I can certify that nothing like them exiſts, either in the venom of the viper, or in the other poiſons, whether of the animal, vegetable, or mineral kingdoms, particularly thoſe of the laſt. I have rendered myſelf very certain of this, by experiments made with the greateſt care, and in which I employed the ſtrongeſt lens'.

The authour of a book, entitled, *On The Reproduction of Individuals*, [*De la Reproduction des Individus*] or rather Monſieur de Buffon himſelf, aſſerts, that the venom of the viper, as well as all other active and penetrating poiſons in animals and vegetables, can be nothing elſe than theſe organical particles; and he ſays, that the ſalts Mead obſerved are preciſely the ſame particles carried to their higheſt degree of activity. He likewiſe believes

believes that the pus of wounds is filled with these moving corpuscles: but all this is without foundation. I have shown that these pretended salts are not found in the venom of the viper, any more than the particles supposed to be in motion. I have likewise examined all kinds of wounds, whether of a good quality, gangrenous, or cancerous, and have never been able to find the least vestige of these particles: I could only discover a quantity of small unequal corpuscles, more or less round, and swimming in a transparent liquor. But what appeared still stranger to me, and which is however incontestible, these microscopical animalcules are not found even in wounds of living animals, that come of themselves, whilst they are always to be traced in animal and vegetable substances, put to putrify in water, and exposed to the air.

This illustrious French naturalist has been mistaken then in all he has written on the nature and action of the venom of the viper, and of other poisons. The acids salts of Mead which have never existed in nature, and the neutral salts of the same authour, which are not real, have been metamorposed by the fertile imagination of the elegant French writer, into what are still more absurd, organical particles, endued with motion.

It is false, that the corpuscles which are seen with a microscope in continual motion, in the infusions of animal and vegetable substances, are simple organical particles, since they are real animals. It is falser still that these organical particles are seen in the venom of the viper, and in other poisons. No motion is observed in any poison whatever, and there is nothing that can give one the slightest suspicion of the existence of these particles there. It is besides impossible that the salts of Mead can be the particles of Buffon, since these salts are merely imaginary. There is no greater truth in the existence of these particles in the pus of wounds, since there is no motion in this substance. It is with regret that I see myself obliged to dwell on the errours of this writer, but his authority may easily mislead those who can only judge after others. How many are there who judge in this way! we may include in this number all those who are not capable of immediately consulting nature; who prefer hypothesis to fact, and eloquence to truth: a severe and candid posterity will, without doubt, be astonished to find, that there have been philosophers and naturalists in the eighteenth century, who, even in the most important particulars, have ventured to substitute conjecture to experiment, notwithstanding that the latter would have been made with as much ease, as it would have been decisive.

" Let indolent men," observes Senac, (a) " seek an amusement in devising the secret springs " of nature, as obscure politicians divine and re- " gulate what passes in the cabinets of princes: it " is a philosophical delirium that only hurts the

(a) Fraité du Coeur, page 29. Preface.

mind.

" mind. But where life is interefted, if we are per-
" mitted to form conjectures, it is for the purpofe
" of fubmitting them to the proof of experiment,
" which ought to decide."

In this uncertainty, feeing that the opinions of the greateft philofophers are fubject to the greateft difficulties, I thought it expedient to have recourfe to my own obfervations. Neither of their fyftems is fatisfactory, when we confider the quicknefs with which the venom of the viper kills animals. I could not comprehend how creatures with cold blood, fuch as the frog, are fo foon deftroyed by this poifon, whilft they furvive for fo long a time the lofs of the heart, inteftines, and other vifcera, and even that of the brain and head.

Doctor Mead, as we have already feen, afferted with the generality of philofophers, in the firft edition of his works, that poifons, particularly thofe of the animal kingdom, act on the blood, and are carried by this fluid into the innermoft parts. But having paid attention to the quicknefs with which the venom of the rattle-fnake caufes death, this illuftrious naturalift has changed his opinion in his laft work on the fame fubject, and has fubftituted the animal fpirits to the blood. He maintains then, that the primary action of the venom of the viper and that of other animals, is on the nervous fluid, which, being depraved by it, produces inflammation in the organs, and deftroys the animal machine; fo that the difeafe caufed by thefe venoms, communicates itfelf to the whole body in

no

no other way than by the medium of the animal ſpirits, which finally vitiate the blood, with which they unite themſelves. The falſity of this hypotheſis of Mead will be demonſtrated by and by.

Nothing is leſs known than the manner in which this poiſon acts and brings on death ; if we reflect however on the effects of opium, its mode of action may inſtruct and enlighten us a little on that of the venom of the viper. That vegetable juice begins by rendering an animal weak and torpid, and ſoon kills it by deſtroying the irritability of the muſcular fibres, as I have ſeveral times obſerved in animals with cold blood, and as the famous Haller demonſtrated a long time ago, even in thoſe that have the blood warm. The ſymptoms and accidents that follow the bite of the viper, do not differ eſſentially from thoſe I have juſt ſpoken of, and may at leaſt induce one to ſuſpect that the venom of that animal likewiſe kills by totally deſtroying the irritability of the fibres.

I recollect that being ſome years ago at Bolognia, and reflecting attentively on the action of mephitick vapours, whether natural or artificial, I could not bring myſelf to be ſatisfied with all that the different authours have written on their nature, and on the proximate cauſe of the ſudden death they cauſe to animals. Some will have it to be owing to the exceſſive elaſticity of the air, and others aſcribe it to the total loſs of this ſame elaſticity : now theſe two hypotheſes are equally belied by facts, which prove on one hand, that the changes

changes the elaſticity of the air may undergo in mephitick vapours, is never ſufficient to kill animals ſo ſuddenly; and on the other, that there are mephitick vapours in which the air abſolutely loſes part of its elaſticity. Others have conceived that this peſtilential vapour kills by irritating the nerves of the bronchiæ, and cauſes a criſpation and an univerſal conſtriction in the lungs, to ſuch a degree as to cloſe the paſſage of air, and to prevent their dilation. Laſtly, there are thoſe again who have conceived, that the vitriolick particles of the mephitick vapours act with a repulſive force againſt the particles of the animal fluids, ſo that the pulmonary veſicles, deprived of their animal ſpirits, fall into an abſolute ſtate of relaxation. It is however very certain, that even thoſe animals which live a long time without reſpiring, and without there being any circulation in the lungs, ſuch as frogs, and other animals with cold blood, and the generality of inſects, in which the circulation frequently remains for a long time intercepted, without endangering life in the leaſt; that all theſe animals and inſects, I ſay, are very ſoon killed in mephitick vapours. Beſides, the nerves are neither ſuſceptible of contraction nor of irritability, and the pulmonary veſicles are not formed of muſcular fibres. It is likewiſe certain, that there are mephitick vapours without ſulphur, ſmell, or taſte, and which do not contain either an acid or alkaline ſalt; and even though they ſhould contain any ſuch, we ſhould not comprehend the readier how they are capa-

capable of killing so suddenly, animals in which life is so tenacious, and which the knife, the fire, the very extraction of the heart, lungs, and all the other viscera, and lastly that of the brain, do not destroy without great difficulty. In consequence of these considerations, I formed a determined resolution to make artificial mephitick vapours, and to examine the effects of them on living animals. I collected the vapours of sulphur in a recipient, in which I placed a frog; it died almost instantly, after making a few leaps, and being violently agitated. I opened it, and found all its parts flaccid and relaxed. The heart still moved, but feebly and with great difficulty, and in a short time entirely lost these little remains of action. I endeavoured ineffectually to irritate, not only that, but likewise the other muscles; neither of them would contract. I forced a needle into the spinal marrow, and saw with surprize, that it no longer awakened the motion of the limbs. The colour of the blood was changed to brown, but its globules still preserved their round and spherical shape.

I placed two other frogs beneath a glass recipient, into which I had introduced the vapour of a solution of iron filings in the nitrous acid. They died instantly. I opened them, and found the blood of a brownish hue, and collected in the auricles. The heart was no longer in motion, and was insensible to stimulations. The flesh was throughout flaccid, and had likewise lost all irritability. On pricking

pricking the crural nerves, the legs remained motionlefs.

During this period, the celebrated Doctor Veratti likewife made experiments on artificial mephitick vapours. I affifted at them myfelf, in company with other profeffors, and they proved very conformable to mine. It refults clearly from all thefe circumftances, that mephitick vapours kill animals, by deftroying the irritability of the whole mufcular fyftem. This is the immediate caufe of their action, and the reafon why thefe pernicious exhalations kill animals as it were inftantaneoufly.

About the time when the firft part of the prefent work appeared in Italian, (at Lucca in 1767) I found, as has been feen above, that artificial airs kill frogs by deftroying the irritability of the heart; and the examination of the effects that mephitick vapours produce on living animals, made me conclude, that they occafion death by deftroying the irritability of the whole mufcular fyftem. But a celebrated Phyfician (Tiffot) feems not to be of this opinion in his excellent work on the nerves. He there expreffes himfelf in this manner *(a)*, " One of the greateft modern naturalifts " thinks that factitious airs abfolutely deftroy the " iritability of the heart, and that their effects are " to be explained accordingly: but there is no

(a) Traité des nerfs, &c. T. i. Seconde Partie, Article des Effets des Poifons, § 218. en note

" convey-

" conveyance by which their action can be carried " to the heart. Fixed air, which kills when respi- " red, being applied in the way of injection to the " muscular fibres of the intestines, revives their " action, awakens the principle of life, and reco- " vers sick persons at the point of death. Applied " then to the muscles themselves, it excites their " irritability, instead of destroying it."

This is not the place to speak in an express way of the effects of artificial airs on the living body. I purpose to do this in another work on respiration, which has been finished for some time, and in which I shall relate the detail of the experiments I have made on this subject, and give my sentiments on the cause of the death brought on in mephitick airs. However in the mean time I think myself under the necessity of observing, that the arguments of the learned Tissot have not hitherto been decisive; that the question remains in its original state; and that it should only be decided by having recourse to experiment, to which an authority of so great a weight as this philosopher's, is but too capable of preventing an application.

The first difficulty Tissot opposes is, that we do not know the channel by which mephitick airs deprive the heart of its irritability.

But it must be acknowledged, that the ignorance of one truth does not exclude the knowledge of another; and that we may know the effects without understanding the causes, and still less their manner of acting. All human science is of this nature,

nature. We know effects, of which we are entirely ignorant of the causes; and we know causes, of which the mode of action is absolutely concealed.

The question then is reduced to this; to determine by experiment, whether mephitick vapours destroy, or do not destroy, the irritability of the heart; and the difficulty proposed above is of no weight, whether we know or not, the way in which this is brought about, provided the experiment be certain, and the illustrious writer oppose nothing which disproves it.

I do not besides see how we can be certain that there are absolutely no channels by which the action of these vapours may reach the heart.

They destroy animals that are made to respire them. In these circumstances there is an immediate communication betwixt the lungs and these vapours. Fluid substances are continually separated from the lungs, and this viscus may receive others, if they chance to act on it. There may be a real communication then betwixt these airs and the lungs, betwixt them and the substances that are separated from that viscus. But the lungs are known to receive the blood from the heart, and to convey it thither again. I do not therefore see why the communication, or rather the action, of these airs on the heart should be impossible.

The other difficulty Tissot opposes is, that fixed air, which kills when respired, when immediately applied to the muscular fibres of the intestines, revives their action, and cures diseases; whence he deduces,

deduces, that when applied to the mufcles them-felves, it muft neceffarily excite irritability inftead of deftroying it, and confequently cannot deprive the heart of its irritability.

But, in the firft place, nothing is more common in medicine, than to find fubftances which, when applied to one part of the animal machine, act as a remedy; inftead of which they occafion difeafes, and even death, when applied to others. Several medicines, particularly in the clafs of poifons, operate precifely in this way; frefh examples of which will be given in the continuation of this work.

Electricity occafions death by depriving the heart and flefhy fibres of their irritability, as I have proved in my work on *Animal Phyficks* (a); and this fame electricity is notwithftanding one of the ftrongeft ftimulants to the mufcular fibres that are known. It reftores life by exciting irritability, in the very animals in which it had an inftant before deftroyed it. Amongft all the ftimulants that can be employed to call the animals back to life, that the electrical fhock has thrown into a ftate of in-fenfibility, a proper application of gentle fparks appears to me the moft efficacious remedy.

In the fecond place, the application of fixed air has a very different effect when introduced into the inteftines, than when it is refpired. In the firft

(a) The firft volume of this work, which I have already had occafion to quote fo often, was printed at Florence in 1775, and is entitled *Ricerche fopra la Fifica Animale*.

case

case its action is immediate; in the second, it seems to need the assistance of the blood, to convey its action to the heart. Whence it follows, that its effects may be very different in these two circumstances.

These particulars naturally led me to think, that the venom of the viper likewise kills animals by destroying their irritability. I procured fifty of the strongest and largest frogs I could meet with. I preferred these animals, because they are livelier than others; because they die with greater difficulty; because they are more irritable; and lastly, because their muscles contract even several days after they are dead.

I had each of them bit by a viper, some in the thigh, others in the legs, back, head, &c. Some of them died in less than half an hour, others in an hour, and others again in two, three, hours, or somewhat more. There were some again that were not affected, whilst others that did not die, became nevertheless swelled. There were likewise others amongst them that fell into a languishing state, their hind legs that had been bit continuing very weak, and even paralytick. In some of them I contented myself with introducing cautiously into a wound, made with a lancet at the very instant, a drop of venom. These last lived longer than those I had had bit; neither of them however escaped. I constantly took the precaution to prevent the venom I introduced into the wound, being carried out by the blood that flowed from it. Some of

 these

theſe frogs ſwelled very much, others but little, and others not at all. The wounds of almoſt all of them were inflamed more or leſs. There were ſome however that died very ſuddenly, without the ſmalleſt mark of inflammation. A ſhort time after theſe animals had either been bit, or wounded and *venomed* (*a*), the loſs of their muſcular force, as well as that of the motion of their extremities, was very evident. When they were ſet at liberty, they no longer leaped, but dragged their legs and bodies along with great difficulty, and could ſcarcely withdraw their thighs, when they were pricked with a needle, of the pain of which they ſeemed almoſt inſenſible : by degrees they became motionleſs and paralytick in every part of the body, and after continuing a very ſhort time in this ſtate, died.

I now opened the abdomen, and ſtimulated the nerves that paſs through it in their way from the vertebræ to the thighs. I employed the ſtrongeſt corroſives, but could excite no motion nor tremulus in the lower extremities. I pricked the muſcles with as little effect, and thruſt a long pin into the ſpinal marrow, without producing any motion or trembling either of the muſcles or limbs. In none of theſe parts, all of which had died at the

(*a*) I thought I might be allowed this term, to expreſs in one word, that an animal, or any part of it, had received the venom, or at leaſt that it had been applied to it. *Envenomed* would be the proper term, but cuſtom has given it a figurative and moral ſignification, which makes me afraid to apply it in its proper ſenſe. In a work of ſcience, the uſe of a new word ſhould be permitted, to avoid tedious neſs or ambiguity.

ſame

ſame time, was there the ſmalleſt veſtige of life. The nerves were no longer the inſtrument of motion. The muſcles no longer contracted, and were no longer ſenſible to ſtimuli. The heart alone in ſome few of them continued to move languidly, and its auricles were ſwelled and blackened by the blood with which they were ſurcharged. This organ did not however ſeem to have ſuffered much from the activity of the venom. It continued its motion, notwithſtanding the entire death of the other parts, and renewed its vibrations on being ſtrongly ſtimulated with needles. This motion and theſe oſcillations were however but of ſhort duration after the death of the animal.

Perſons have been ſometimes met with, who having been bit by a viper, have remained paralytick in ſome particular part of the body during life. A ſhort time ago a woman in Tuſcany, who had been bit in the little finger by a viper, became after various other complaints, paralytick throughout the whole right ſide of her body, and could never be cured. In a word, it is certain that all thoſe who have met with this accident, complain ſoon after of an univerſal weakneſs. Their muſcles refuſe their office. They become dull and heavy, have no longer the free exerciſe either of body or mind, and fall inſenſibly into a kind of lethargy: ſo true it is, that this venom induces a palſy of the muſcles, and robs them of that active property, called by the moderns, animal irritability. In the continuation of this work, I ſhall ſhow what opinion ought to be held of that fyftem, and the changes I have made in it.

Thus then it appears, that animals die of the bite of the viper, from their fibres lofing that irritability, which is the grand principle, both of voluntary and involuntary motions in the animal economy (*a*).

From thefe experiments on frogs, it feems that the venom of the polypus is very analogous to that of the viper. Scarcely has a polypus feized an earth-worm, when it perifhes, and has no longer any motion: thefe worms are known however to be very tenacious of life, and to move a long time after they are cut in pieces. Let us fay then, that the venom of the polypus (for it is one, fince it kills fuddenly, and in a very fmall dofe) attacks the animal irritability, and extinguifhes life, precifely as does that of the viper.

After having found that the venom of the viper occafions death by deftroying the irritability of the fibres, let us examine what are the changes that happen to the mufcles after they are deprived of this property. It has been conftantly obferved, that the flefh of animals lofes its motion and irritability in proportion as it has been penetrated by a putrefactive principle. We have many examples to prove that the lofs of the former invariably accompanies the firft progrefs of the latter. Mephi-

(*a*) The propofition I advance here is a very general one; the different modifications it is capable of will be fhown hereafter.

tick

tick airs, which deſtroy irritability, likewiſe haſten putrefaction, and the muſcles of the animals that are killed by them, become flaccid and livid. We likewiſe find that thoſe of the animals bit by the viper become putrid in twenty-four hours. In both caſes the very principles of the elementary fibres are attacked, and the diſunion of theſe occaſions the loſs of their moſt innate natural properties. This disjunction of parts, which the putrefaction of the muſcles invariably cauſes, muſt neceſſarily deprive the latter of their irritability and fitneſs for motion.

I am led to think that the venom of the viper produces a ſomewhat ſimilar effect, and I found my opinion principally on the analogy of the other poiſons. Indeed we find that the fleſh of animals which has been cut with a knife dipped in the juice of napel, inſtantly becomes more tender, and fitter for culinary purpoſes. Travellers inform us that in both Indies, as well as in Africa, the inhabitants uſually hunt with poiſoned arrows, and that in the ſpace of ſix minutes, or more or leſs according to the degree of the poiſon's activity, theſe arrows kill the largeſt animals, ſuch as lions, tigers, and even elephants. They likewiſe obſerve that the fleſh of theſe animals immediately ſoftens and becomes tender; an unequivocal proof that all theſe poiſons equally diſpoſe the fleſh to a ſpeedy putrefaction. I have myſelf obſerved the ſame thing, in frogs and other animals bit by the viper. Their fleſh ſoftens much ſooner than uſual, to ſuch a degree as to crumble at the leaſt touch; it ſeparates of itſelf from the bones, and corrupts and ſmells in a very ſhort time.

If after theſe obſervations it is almoſt impoſſible to deny that the venom of the viper deſtroys irritability by conveying a putrefactive principle into the fleſh of animals that have been bit, and into their fluids, we muſt agree as to the inutility of having recourſe, at the example of mechanicians, to cauſtick, ſtimulating, and inviſible ſalts, in explaining the action of this venom. Very far from favouring this action, we know that ſalts are in general better calculated to ſuſpend and ſtop it; and I cannot conceive how naturaliſts, otherwiſe very enlightened men, have imagined and wrought themſelves into a belief, that the poiſons drawn from animals, and even thoſe from vegetables, owe all their activity to certain ſalts of this nature. Beſides, we ſcarcely find the ſmalleſt trace of ſalts in the juices of ſome of theſe plants, even the moſt venomous of them. I have examined ſeveral of them with the microſcope, and have not the ſmalleſt idea of having found any ſalts in them, except in the *toxicodendron*, in which tree, as in other plants, we only find a few ſhining globules, ſwimming in a more or leſs tranſparent fluid. What I am very certain of is, that there does not exiſt in the venom of the viper the ſmalleſt veſtige of thoſe formidable ſalts, that have been ſuppoſed capable of killing animals the moment they are introduced into the blood.

It

It is the facility then with which, by the help of these pretended salts, the action of poisons is explained, that has seduced these mechanick physicians. They have conceived to themselves *spiculæ* throughout, calculated to disunite the animal fibres, and decompose the humours. But what will they reply to the example of opium? It kills by weakening, by even destroying, the irritability of the fibres. If the virulence of this vegetable juice resides essentially in its gummy and resinous part, will they likewise suppose the existence of salts there? These hypotheses have had their birth in a chymical laboratory, and are not the result of constant observations of the phenomena of nature. We must agree that these imaginary salts have been but too much abused. There are those who have not hesitated to place them every where, and who have even gone so far as to believe that they alone are capable of awakening the senses of taste and smelling, whilst nothing is less demonstrated than the presence of these salts in sapid and odoriferous substances. Besides, they do not consider that salts are capable of altering their shape without losing their natural taste; how then can they likewise change their taste whilst they preserve the same shape? It is not therefore on a certain determinate shape that their action must be made to depend, as certain naturalists will have it, who, when they set about explaining the sensations, see nothing throughout besides edges and points; in an infinity of cases this is not only supposed, but is likewise belied by experience. If there is only need of awakening the ſenſations in ſome of our organs, why is there ſo great a neceſſity for theſe ſalts? Cannot this be brought about without their aſſiſtance? Have not other particles of bodies likewiſe the properties of contact and mechanical ſtimulus? Are light and air ſalts, becauſe they ſtrike the eye and the ear? A ſubſtance of any kind that acts on a nerve, may drag and relax the medullary ſubſtance, and may either compreſs or irritate it, independent of the cauſe that afterwards conveys the impreſſion to the mind or brain. If all the external ſenſations are reduced to a change in an organ, other bodies may then operate as well as ſalts. A fluid may likewiſe relax the tender parts of a nerve laid bare, and may equally ſhrivel and dry them. There are ſpirits and oils that dry and harden the fleſh of animals, and irritate the nervous and muſcular ſyſtem, and notwithſtanding contain no ſalts. In the ſame way, poiſons may kill without ſuppoſing ſalts throughout, in the three kingdoms. May not an action of one body upon another exiſt without the aſſiſtance of wedges and points? Can any one ſay that ſalts are found every where where theſe figures are met with? or that they pre-exiſted in all the ſubſtances whence chemiſtry at length ſucceeds in extracting them? There is no more need of this, than to ſuppoſe that there are ſalts and points in camp and jail fevers, in the ſcurvy, and in a word, in all putrid diſeaſes, where the corruption of the ſolids and fluids is alike general. We muſt have recourſe to ſomething very different from ſalts to explain the deſtruc-

destructive force of these hazardous diseases which overturn and destroy the whole animal economy in so short a space of time. Their effects, and those of many other diseases analogous to them, as well as the symptoms that accompany them, are well calculated to lead one to believe that they convey a latent *virus* into the machine, which, like the venom of the viper, brings about the destruction and universal decomposition of the solids and fluids. Indeed these diseases are invariably observed to be attended with convulsions, great faintness, a prostration of strength, drowsiness, an excessive stench exhaling from the yet living body; and lastly, a speedy putrefaction, which almost immediately follows death. The very sudden failure of vital strength in the whole muscular system, is a certain indication that the disease attacks the animal irritability, and the principle of motion in the fibres. It is only in this way that, without having recourse to systems, and to free and arbitrary hypotheses, we can comprehend and explain how it is that the seeds of death are capable of spreading themselves in an instant over the whole animal economy.

I presume that it will not be possible for the future to entertain any doubt as to the true proximate cause of the death brought on so speedily by the venoms of the viper and aspick; and amongst the three species of the latter, principally of that called *nintipolonga zeilanica*. This aspick kills by occasioning a sudden drowsiness and universal weakness, followed by death, in the animal struck by it. In a word, a word, it ſeems that all the poiſons ſupplied by the animal kingdom, occaſion death by deſtroying the irritability of the muſcular fibres, and diſpoſing both ſolids and fluids to a ſudden corruption. The ſame may be ſaid of thoſe vegetable poiſons, that are no ſooner introduced into the blood, than they are ſucceeded by death.

But of all the poiſonous animals hitherto known, the polypus ſeems to poſſeſs the moſt powerful and active venom. However irritable theſe creatures may be in other caſes, and difficult to kill, it ſucceeds inſtantly in extinguiſhing the principles of motion and life in water worms. What is very ſingular, its mouth or lips have no ſooner touched this worm than it expires, ſo great are the force and energy of the poiſon it conveys into it. No wound is however found in the dead animal. The polypus is neither provided with teeth, nor any other inſtrument calculated to pierce the ſkin, as I have aſſured myſelf by obſerving it with excellent microſcopes.

Let us likewiſe be very cautious how we believe, at the example of many naturaliſts, that life conſiſts in general in the circulation of the blood and motion of the heart, and that it abſolutely ceaſes when this circulation is interrupted. The circulation is not general in animals; polypuſſes have not even a heart or other analogous viſcus, to bring about its operations. It is proved too, that ſeveral animals with cold blood live a long time without heart, and without viſcera, as is ſeen in frogs, turtles, and ſeveral

veral kinds of fish and worms, in which, although the circulation is undoubtedly then stopped, they continue notwithstanding to live and move, and are wrought on by their passions as usual, appearing to be still subject, to and sensible of, the wants of life.

I have found many animals, insects, and worms, in which there is certainly no kind of circulation in the vessels; there are others, in which it is only imperfectly carried on in some particular parts of the body, and not at all in the extremities. I purpose to give all these particulars to the publick, in a work I have been some years busy in getting ready, entitled, *Sur les Animaux Microscopiques* (*on Microscopical Animals*).

This errour has spread itself amongst philosophers, by the help of a false analogy they have supposed betwixt animals with warm blood, and those with cold; a very dangerous mode of reasoning in physicks, and belied at every step by observation and experiments. A function has been observed to be executed in a certain way in animals with warm blood, and it has been immediately concluded to be the same in all others. Thus are general laws made, and propositions on so extensive a scale advanced, merely because nature has not been sufficiently consulted. We have needed a *Tremblei* and a *Bonnet* to rid us of these general axioms, and of the idea of a necessary and common law in the generation of all animals.

I can-

I cannot forbear mentioning in this place the singularity of motion of a ſmall microſcopical animal, which Lewenhoeck has named *rotifer* (wheel-polypus). All the obſervers, even the moſt modern ones, that have ſucceeded him, have believed that this animal has real wheels (*a*); but to be certain of the contrary, it is only neceſſary to place it betwixt two pieces of glaſs, and then obſerve it with an excellent microſcope. 'Tis a ſmall gelatinous worm, commonly found in the earth or ſand collected by rain in the tops of houſes. I have likewiſe found it in other earths, as well as in waters that have been ſometime ſtagnant, and more frequently again in thoſe that have a very gentle current, and are filled with *conferva* and other aquatick plants. This worm is divided towards its head into two pretty large trunks, which appear like two wheels or ſtars, from the number of ſmall, extremely ſharp, and ſhort, branches that are attached to their circumference. They really appeared to Lewenhoeck to be wheels of a rare mechaniſm, and every one would judge the ſame, on ſeeing the creature put

(*a*) Great care ſhould be taken not to confound what we imagine, with what is pointed out to us by obſervation. Indeed there have been authours who, either guided by analogy, or puzzled to explain ſo ſingular a motion, have ventured to aſſure us that theſe wheels are not real; they have luckily ſaid the truth. It muſt however be agreed that we ought to obſerve, and not to divine, the phenomena of nature. Whoever gives himſelf up to reſearches of this kind, without the faithful guide of obſervation, runs the greateſt riſk of falling into errour.

them

them in motion. But a more exact obfervation at length convinced me that they are not wheels, but compofed of a quantity of fmall moveable arms, formed like pointed cones, and planted all round the two trunks. It lets fall thefe moveable arms or rays fucceffively, and afterwards raifes one after the other with fo much celerity, that the eye fancies they are turning round like the fpokes of a coach-wheel, or rather, like the branches of a wheeled fire-work. It never moves thefe two wheels, except when it fwims or wifhes to eat, and thefe two ftates are invariably the fhorteft of its life. In fwimming, it ftrikes the water with thefe arms or branches with great celerity, refts itfelf at different periods, and thus tranfports itfelf from one place to another. When it eats, it, on the contrary, fixes its tail in fome fubftance, and afterwards turns its two wheels, giving fuch a motion to the water, that it directs the courfe of it towards its head, fo that it prefents to its mouth all the fmall corpufcles with which it is filled. The velocity of the motion of its arms or wheels is incredible; but what is ftill more aftonifhing, is the motion of its heart. This vifcus is feen very diftinctly with a microfcope, and can never be confounded with any other part of the animal whatever. It is abfolutely immoveable when the worm does not play its wheels; but no fooner are they in motion, than the heart moves too, and its action becomes ftronger in proportion to the quicknefs with which the wheels are agitated, fo that their motions are always in an exact proportion. I do not

not take upon me to deny, but that it sometimes happens (although very rarely, and that at very long intervals) that the heart is in motion even whilst the wheels are at rest; and as the motion of the wheels is always at the disposition of the animal, so likewise is that of the heart. The heart then is a voluntary muscle, depending on the will of the animal; a circumstance which is at the present time *unique*, having never been observed in any other case. The wheel-polypus passes the greatest part of its life then without any motion of its heart, and consequently, without a circulation of blood, or of a fluid which receives motion from this muscle. This does not, however, prevent it from moving during the other intervals, when it creeps and trains itself, as worms do, amongst the bodies that surround it.

An objection may be started here, that this organ of the wheel-polypus is not the heart of the animal, but rather its stomach, since it is observed to move when the creature eats; and that it is altogether extraordinary to suppose, that the heart is a muscle submitted to the will, whilst it is not so in any other animal. It must be confessed, that this is not impossible, but it is not, on that account, very probable; and even though it should be true, it would be likewise true, that an organ such as the stomach exists with a voluntary motion, which, any more than in the other case, is not observed in any other animal.—Thus the difficulty I encounter is of no weight, since it must always be agreed, that

that a muſcular organ exiſts in this animal, which, in oppoſition to thoſe of all other animals, is ſubordinate to the will. This is preciſely what I wiſhed to prove by my obſervations, and my diſcovery, therefore, ſtill remains ſuch. It is likewiſe to be obſerved, that the rotifer puts this ſingular organ in motion, even when it does not eat; that is to ſay, at a time when it can make no uſe of it, provided it be its ſtomach. This happens every time it ſwims, or wiſhes to paſs rapidly from one place to another: it has then occaſion to move its two wheels, and this organ moves in conſequence. Hence we ſee, that the animal does not move it to eat, but that the motion of it neceſſarily takes place when it plays its two wheels, whatever may be its motive for ſo doing.

But ſince it is certain that the voluntary motions of the muſcles of animals with cold blood, do not depend more on the circulation of humours, than does the irritability of the fibres, which ſeems to be the ſource and principle of life and motion in the animal, it follows that, in animals, life conſiſts in the action of their muſcles and parts; for the moment this motion ceaſes, the animal ceaſes likewiſe to live; and its body then, as to life, differs no longer from the ſtate of any foſſil or vegetable ſubſtance whatever; and all this aſſemblage of veſſels, ſo many different organs, and this aſtoniſhing ſtructure of its parts, are no longer of any uſe to the animal, and ſhould be regarded as if no part of them any more exiſted: motion being once at an end in the machine, ſenſation and life are ſo too. The animal will return to life as ſoon as its parts regain their former motion, inſtead of which, it dies for ever, when, as happens to man, its parts not only loſe their actual motion, but likewiſe the faculty of recovering it in the ſequel. Thus the microſcopical eels that are found dry and withered in ſmutty wheat, recover motion and life as ſoon as they are wetted with a little water, and again become lifeleſs and dry, whenever they are no longer moiſtened. I have repeatedly aſſured myſelf of this with an extreme pleaſure. Thus, then, do they preſerve the power of reviving and reſuſcitating effectually, by the ſimple preſence of the water with which they are moiſtened.

The celebrated Bonguer, in his work on the ſhape of the earth, relates to us, from the teſtimony of Father Gumillo a Jeſuit, and alſo of the Indians of Peru, that a large venomous ſnake is found in thoſe countries, which being dead, and dried in the open air, or in the ſmoke of a chimney, has the property of coming again to life, on its being expoſed for ſome days to the ſun, in a ſtagnant and corrupted water.—It were to be wiſhed, that ſuch a naturaliſt and philoſopher as Monſieur Bonguer, could have verified by his own proper obſervation, a fact ſo important in itſelf, and rendered ſtill more ſo by the ſize of the animal.

I have dried the worm called *ſeta equina*, or, according to Linneus, *gordius*, ſeveral times in the open air, without leaving it there too long: it had

had loſt almoſt all its bulk and weight, and was become like a bruiſed and dry ſtraw: its ſkin had ſhrunk ſo as to leave no ſenſible cavity, and it had no longer any ſign of liſe or motion. I returned it into water, where, in leſs than half an hour, it recovered its bulk and weight, and ſoon afterwards diſcovered unequivocal and permanent ſigns of life.

The wheel-polypus I have ſpoken of above, likewiſe loſes, when dried, every kind of motion and life, and recovers both one and the other when again put into water. Laſtly, I left it, by way of experiment, in a very dry ſoil, and expoſed, during the ſummer, to the whole heat of the ſun, for the ſpace of two years and an half. I afterwards returned it again into water, where, at the end of two hours, it recovered life and motion. I put one of them on a bit of glaſs, which I expoſed, during a whole ſummer, to the noon-day ſun: it there became ſo dry, that it was like a piece of hardened glue. A few drops of water did not, however, fail to reſtore its motion and life.—I have ſince found a number of other ſmall animals, either on the tops of houſes, in earths, or in water, which, in the ſame way, alternately loſe and recover the uſe of their organs, on being dried, and afterwards returned again into water. I purpoſe ſpeaking of theſe little prodigies in a work apart, to be entitled, On the Life and apparent Death of Animals *(a)*.

But it is not the ſame as to the loſs of irritability in the muſcles of the animals poiſoned by the viper.

(a) De la Vie et de la Mort apparente des Animaux.

These remain flaccid, and their motion is lost for ever. It appears very certain, that the venom of this animal differs but little from opium as to its effects; and that its mode of action on the fibres comes very near to that of this vegetable juice. Both of them excite violent convulsions and vomitings. Each of them conveys an universal debility into the organs. They render the muscles paralytick, make the animal heavy, and finally bring on a speedy death, by destroying the irritability of the fibres. The heart alone in both cases still preserves its irritable quality for some time after the death of the other parts. It avails nothing here to animals with cold blood, that they are endued with an obstinate life, and are capable of preserving that, as well as motion, after they are cut in pieces. If either of these poisons attacks the principle of their motions, and destroys the irritability of their muscles, they die speedily, all motion is annihilated in them, and their parts will no longer give any sign of life. Their body, it is true, will preserve its organization; but an organized body that has lost its motion, is truly a body without life.

It is evident, then, that neither of the numerous hypotheses naturalists have invented, a great part of which I have taken care to relate, explain in a reasonable way the death of the animals poisoned by the viper; but that its venom kills in no other way, than by destroying the principle of motion, the only source of animal life, in the different parts. I am the more attached to the opinion that the venom of the viper acts in no other manner than by de-

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ſtroying the irritability of the muſcular fibres, from having already ſhown, in a memorial printed in the Acts of the Academy of Sienna, that the nervous fluid is by no means the true, the efficient cauſe, of muſcular motion; and even though I ſhould be of another opinion, and ſhould regard the animal ſpirits as the cauſe of irritability, and the true principle of all the motions in the animal economy, my diſcovery of the proximate cauſe of the death of animals that have been bit by the viper, would loſe no part of its importance; for whether it operates immediately on the nervous fluid, or on the muſcular fibres, it is not leſs true that this venom kills by depriving the animal of all motion, and the muſcles of the power of contracting.

Unleſs I am deceived, I have now, I think, happily terminated the controverſies that have ſo long kept people at variance, on the mode of action of the venom of the viper. I believe I have explained how it is able, in ſo ſhort a time, to kill even the animals that are the moſt obſtinate in dying. When once this poiſon is introduced into the blood, it deſtroys the irritability of the muſcular fibres, the ſource and principle of all the motions, not only during the life of the animal, but alſo after its death. I call every animal dead in which there are no longer any exteriour ſigns by which we can ſay that it lives; and, in truth, it is only according to our ſenſes, and the information they give us, that we can judge of the true death of animals; that is to ſay, of the preciſe inſtant when they ceaſe to

 exiſt,

exist, and are no longer alive. Indeed, how can we conceive a living being, without the idea of some motion in its organs? We should otherwise introduce into physicks a senseless pyrrhonism, and cast trouble and uncertainty on the most certain and most received notions and ideas. A principle of corruption penetrates into, and spreads itself in, the solids and fluids, relaxes and decomposes the muscular fibres, and causes them to lose the power of contracting. It is to this general law of putrefaction, then, and to this universal principle of dissolution and death, that the entire action of the venom of the viper on organical bodies is reduced; and to this we must confine ourselves, since in effect what is called the science of nature has its bounds, and since it is not permitted us to go beyond them. Whatever this science may be, if it is true that a putrefaction exists in nature, and that it effects the destruction of all organized bodies, it is likewise certain that we are entirely ignorant of its mechanism. Indeed when these bodies are submitted to its action, who is capable of informing us what is its mode of working on them, with what power it operates, and lastly, what changes and what revolutions it causes them to go through? The prodigious number of small movements that are exercised on parts of an infinite minuteness, are too obscure for us, and escape our senses. It is, however, enough, to see that a general principle of putrefaction and destruction, which decomposes organical bodies, and gives them up to death, reigns throughout

out nature. To ſeek an acquaintance with nature, we have nothing more to do, than to aſſemble the effects or particular accidents of bodies, and to compare them with other more general effects, that are called *principles, or laws, of nature.* This alone is what the great Newton did, when he ſubmitted all the celeſtial motions to the general law of gravitation. Of what conſequence is it beſides to an aſtronomical obſerver, to know the cauſe of the reciprocal tendency of the bodies moving in the ſky? Such a knowledge would be rather an object of curioſity to man, than a real advantage to aſtronomy.

Theſe were my ſentiments when I publiſhed this Firſt Part in Italian, thirteen years ago. I have at preſent made but very few changes and additions, becauſe the ſucceeding parts are, rigorouſly ſpeaking, merely a ſupplement, ſerving to correct what is previouſly advanced; and becauſe order would then have obliged me to give conſequences, which could not eaſily have been comprehended, till after a general idea was formed of the ſubject.

Deſtroyed irritability in the living machine was what I moſt conſtantly obſerved at that time. It is for this reaſon that I have reduced the action of the venom of the viper to this general principle, and have entirely excluded the nervous ſyſtem. I muſt confeſs, however, that the number of my experiments was then very limited, and that I had not varied them ſo much as I have ſince done. I was likewiſe ignorant of the faculty of the poiſon *Ticunas*; and alſo of the ſurprizing effects of the oil of the Cherry Laurel, for the greater part unknown to other obſervers.

I have likewiſe, in this Firſt Part, paſſed in a very curſory way over ſeveral other ſubjects, and have even given into a few ſuppoſitions, which I ſhall diſcuſs in my *Microſcopical Obſervations (a).* The principal objects of theſe obſervations will be, the figure and properties of the globules of blood; the animals that are capable of dying and returning to life, which will give me occaſion to enter into a complete hiſtory of the celebrated eels found in ſmutty wheat; and, laſtly, the cauſe of the death of animals ſurrounded by artificial and unrenewed airs.

(a) Obſervations Microſcopiques.

PART

PART II.

CHAPTER I.

On the Source of many Errours.

THE *ignorance* of a truth in phyficks may hide from us the caufe of a natural phenomenon; but *errour*, when it fupplies the place of truth, ftops the progrefs of fcience, and fubftitutes dreams and chimeras to facts and nature. It is at all times a misfortune to be ignorant of a truth; but when we are fenfible of our ignorance, we may ftill hope to come at a knowledge of it. The book that of all others would be the moft ufeful to mankind, is ftill wanted. It is one which would at once determine what we really know, and what we do not know, although we have perfuaded ourfelves that we do. Our reafonings would no longer have hypothefis and errour for their bafis, and inftead of fabricating fyftems, we fhould endeavour to prepare materials. Nature would be more confulted; we fhould reafon lefs, and know more.

There are errours and truths which concern man more effentially than others do. Thefe are, particularly, thoſe which relate to the preſervation of his ſpecies.

Man is naturally ſubject to certain diſeaſes; whilſt others again are accidental to him. Medicine employs itſelf in each claſs, and in ſeeking the remedies, renders itſelf uſeful to ſociety.

Thoſe who have diſtinguiſhed themſelves in this reſearch, cannot be ſufficiently praiſed. Poſterity will do juſtice to their labours, and immortality is ſecured to them. But, on the other hand, who does not ſee the miſchief that a remedy propoſed in very violent diſeaſes may be productive of, if, inſtead of being ſalutary, it is entirely uſeleſs, or even hurtful? To paſs ſlightly over ſo important a matter, would be expoſing mankind to the worſt of miſchiefs. The more certain we think ourſelves of the remedy, the more we deſpiſe the danger, and do not endeavour, ſo much as we ought to do, to guard againſt it. The diſeaſe attacks us, we neglect the aſſiſtance of art, and frequently become the victims of our own credulity, and of the ignorance of others.

The perſuaſion we are in that a diſcovery is made, blunts the ſtimulus that would have puſhed our reſearches a greater length, and we remain for ages in a pernicious errour, which the hope of reward, and the aſpiring to fame, would have reſcued us from. The hiſtory of human diſcoveries is filled with examples of this. We owe every thing to theſe two great ſprings of human actions, intereſt and ambition. But when a perſuaſion of knowledge is entertained, all inveſtigation is laid aſide; and far from any further diſcoveries being made, the very hopes are loſt of knowing any thing more. Such was the deſtiny of Europe when in a ſtate of barbariſm and ignorance; and ſuch are ſtill the ideas of the ſavage.

It is now more than ten years ago that I publiſhed a work *On the Venom of the Viper*, in Italian. It is this work which forms the firſt part of the preſent treatiſe. I then in a manner engaged myſelf to the publick to give a ſecond part, in which I propoſed not only to ſpeak of the remedies againſt this venom, but likewiſe to treat of ſeveral other intereſting points altogether new. I had neither time nor convenience to finiſh all the reſearches I had then in view. I was deſirous of certain and evident conſequences, and it was neceſſary to multiply experiments infinitely, and to vary them a thouſand ways. But the little ſucceſs I met with in ſeeking a ſure remedy againſt the bite of the viper, particularly occaſioned me to defer the publiſhing the Second Part for ſo long a time. Not but that I tried a great number already known, and ſeveral others which either imagination or chance threw in my way; but they all appeared to me more or leſs inefficacious, and I found none of them certain. It is natural to ſuppoſe, that, amongſt other remedies, I did not forget that moſt famous one of all, *Eau de Luce*; (which indeed is nothing more than the *fluid volatile alkali*, joined with a little oil of amber, that does not at all alter its qualities) I tried it, but the ſucceſs

cess by no means answered my expectations, so that I at length abandoned it, as I had done all the rest.

A new work has lately awakened the attention of the publick on the advantages of *the volatile alkali* against the venom of the viper *(a)*. It is announced in this work, in a tone of assurance and persuasion, to be *the true specifick* against this dangerous poison, as well as against almost all diseases, however dreadful. In reading this work, I apprehended that I had been mistaken from beginning to end. Indeed when I reflected on the experiments I had made in Italy, I knew no longer what to believe, and was at times even led to think, that the vipers of France are either less venomous and destructive than those of Italy, or that they are of a different kind; so true it is, that self-love does not allow us to acknowledge our errours till the last moment!

But what surprised me still more, was to see the errours of Redi, on the use of the bag that covers the canine teeth of the viper, which were refuted more than thirty years ago by Mead, again make their appearance in modern publications; to see likewise the errours of Mead on the acidity of the venom of the viper, which he himself had abjured; and, lastly, to find those of the same authour on the

(a) This work is entitled; *Expériences propres a faire connoitre que l'Alkali volatil fluor est le remede le plus efficace contre les asphyxies. Paris.* The Authour is Mr. Sage of the Academy of Sciences.

saline

ſaline nature of this venom, which have been refuted in Italy for more than ten years *(a)*.

If on one hand I could not perſuade myſelf that I had been deceived on ſo many points and queſtions, which I had, however, examined without prejudice, and with a wiſh to diſcriminate nicely; on the other hand, it was impoſſible for me to conceive, how certain authours could advance ſo many matters as facts, with ſo much aſſurance, and without having previouſly convinced themſelves by certain and repeated experiments. I could not comprehend any more, why the authours of theſe new publications had neglected to demonſtrate clearly the ſource of the errours into which the writers poſterior to Mead had fallen, who had flattered themſelves that they had ſhown with the ſtrongeſt evidence, both by obſervation and certain experiments, how Redi and Mead had each of them been led into errour.

As the publick are perſuaded, that in phyſicks, ſubjects are ſubmitted to experiment, and not to authority, theſe gentlemen ought to have expoſed

(a) There is however, nothing aſtoniſhing in this, when we conſider the mode that is generally adopted by our modern writers. More than two hundred authours may be named who have copied from each other on this ſubject, and have given us groſs errours for demonſtrated facts. One might reaſonably exclaim to them, "Modern parrots, the copyiſts of other parrots, ceaſe to deceive us, and for once conſult nature. Had you employed the time you have ſpent in copying each other, in making experiments, how many errours, and how much time, would you have ſpared to poſterity!"

experiments to experiments, and obſervations to obſervations, and to have developed the errours into which we are fallen. But they have not made any ſuch attempt. They have ſubſtituted their authority to experiment, and their name to obſervation. This method is altogether pernicious, and neceſſarily tends to perpetuate errours amongſt men, and to render diſputes eternal. When we know that two obſervers do not agree on a fact, or on an experiment, to which of the two muſt we truſt, provided they are both of an equal merit? We remain in an abſolute uncertainty, and can only have acquired, on their peruſal, a reaſonable pyrrhoniſm.

But is there no touch-ſtone to enable us to judge where the miſtake lies betwixt them, and of two contradicting experiments, to diſtinguiſh the true one from the falſe?

The difficulty of judging betwixt two authors, even in matters of ſimple fact, has been the occaſion of many errours and hypotheſes having laſted a long time, even after their falſehood has been demonſtrated; and many truths have been rejected, merely becauſe experimenters have not been able to repeat the experiments that proved them, in the ſame way in which they were firſt made.

For my part, I think it the duty of the lateſt obſerver, not only to repeat faithfully the anteriour experiments that contradict his, but likewiſe to give his own in ſuch a way, that they cannot leave the ſhadow of a doubt in the mind of the reader. Without this proviſo, he will loſe the aim he propoſed

posed to himself in writing—that of being believed; which he will not deserve, although he may, by accident, have said the truth.

There are three principal methods of avoiding this inconvenience, which perpetuates errours, and still keeps us in a very dangerous scepticism.

The first is, to multiply the experiments exceedingly. It is almost impossible, in repeating them so many times, that fortuitous cases do not occur to vary them, and that the final result of so many of them is not certain and constant.

The second is, to vary them in a thousand ways, changing the circumstances as the nature and species of them may require, and giving them all the precision and simplicity they are capable of. This method supposes much greater talents and genius in the observer than the first, and there are few of these, even amongst the most skilful, who can boast of having invariably put it in practice.

The third method is, not only to succeed in making experiments, decisive by their number, variety, and simplicity; but likewise to attain to a discovery of the source of the errours that others have fallen into.

It is a fault, then, in those who write the last, not to enter into a very minute detail of their experiments, and to endeavour to demonstrate their superiority and exactness, in comparison with those of their predecessors. It, however, is particularly incumbent on them to trace the origin of errours, and to show how the former observers have been deceived.

ceived. Without this, all their labour is a pure loſs, and they are by no means worthy of confidence.

From all theſe conſiderations, I have deemed it neceſſary to return to the ſubject of the preſent work, and to treat it in as particular a way as my circumſtances will allow me. The importance of the ſubject requires this of me, ſince it regards a very dangerous and mortal diſeaſe, which impreſſes with fear thoſe who have the misfortune to be attacked by it, and creates uneaſineſs in families.

Perſuaded that a perfect knowledge of the venom of the viper cannot be acquired unleſs by a ſearch into all its properties, which are, in a greater or leſs degree, unknown, I wiſhed that neither of them ſhould eſcape me, without ſubmitting it to a rigorous, and, at the ſame time, impartial, inveſtigation; and that nothing which related to the ſubject ſhould be wanted, was deſirous of examining afreſh the ſuppoſed acidity of this venom, and the ſalts of which ſome people will have it to be compoſed.

Any errour whatever that relates to this ſubject, may, in time, become dangerous in its tendency. Authours, perſuaded by a miſtake of Mead, that they were acquainted with the true nature of the venom, have been ready to fabricate ſyſtems to explain the way in which it acts, and how and by what mechaniſm it is that it produces ſo ſpeedy a death. They have afterwards invented remedies that relate to the ſuppoſed nature of this poiſon,

and

and what is ſtill more ſtrange, have found them efficacious: They have ſhouted victory, both on occaſion of the theory and the remedy, and have ſhown how the one ſerved as a guide to the attainment of the other. In a word, they have pretended that all is done, and that nothing more remains to be known of the viper's venom; maintaining, that they are acquainted with its nature, its mode of action on the animal machine, and laſtly, with the remedies capable of deſtroying its effects.—But let us leave theſe authours with their ſectaries, to applaud themſelves on knowing ſo many things, and on having divined nature. I believe, on the contrary, that we as yet know nothing about it, and that this matter is altogether new. My experiments will ſhow this, in the courſe of the preſent work.

A great part of theſe experiments required the aſſiſtance of ſeveral perſons, and I have reaſon to congratulate myſelf on this neceſſity, ſince, amongſt others, it procured me the preſence of two men of rare talents; Dr. Troja, Member of the Royal Academy of Naples, author of ſeveral excellent tracts on animal phyſicks, who happened to be at Paris at the time I made my experiments *(a)*; and M. Jean Fabroni, of Florence, a fellow traveller, and attached to the cabinet of natural hiſtory of the Grand Duke of Tuſcany, a well inſtructed, and very promiſing

(a) M. Troja viſited me almoſt every day, to obſerve my method of making experiments on various ſubjects in phyſicks.

young man *(a)*.—I name these gentlemen here with the greater pleasure, since, in thus publickly testifying to them my gratitude and esteem, I give my experiments a greater degree of authenticity.

The first question I now undertake to examine, and which has been the principal occasion of my enquiries, is, whether the *fluid volatile alkali* is a certain remedy against the bite of the viper; that is to say, whether it rescues from death an animal that would otherwise have perished by it. This first research is clearly very interesting, and deserves to be examined with all possible attention. I have multiplied my experiments on this first point, in a way that more than one of my readers will deem unnecessary. But I know of what weight the prejudice for a favourite hypothesis and the authority of a celebrated writer, are. Errour and truth seem to meet with the same difficulty and resistance from mankind; one in unrooting, the other in establishing, itself. The Newtonian system was combatted for a whole age before it was received, and it required as long a time to abandon that of Descartes. What is very certain is, that so many errours have not been spread abroad, as to the nature of the venom of the viper and its remedies, but because too few observations have been made, and experiments too little diversified.

(a) M. Fabroni was likewise present at the experiments I made in London and on my return into Tuscany, and willingly charged himself with the designs of the plates in this work.

Mead

Mead himſelf was not exempt from this fault, as I ſhall ſhow in examining the remedies he has propoſed againſt the bite of the viper. The uſe of the volatile alkali itſelf was only introduced in conſequence of a falſe theory on the nature of the venom, and was only ſupported with ſo much prejudice and obſtinacy, for want of the making of a ſufficient number of experiments. 'Tis on the ſame account that the diſputes on animal phyſicks, which would have terminated at their birth if experiments had been much more multiplied than they were, ſtill exiſt. But the art of experimenting is ſlow and painful, inſtead of which it coſts but little trouble to follow the authority of another. It is eaſier to reaſon than to make experiments; and this art, invariably long and difficult, is not within the reach of every one.

Other readers will find, that the number of my experiments, however great it may be in itſelf, is not ſufficient to decide all the queſtions I examine in this work, nor to terminate all the reſearches I make into the venom of the viper. I have nothing to oppoſe to theſe laſt, and, likewiſe, I do not take upon me to ſay, that all the conſequences I have deduced from my experiments are certain. Perhaps a number of experiments twice as great, would ſcarcely be ſufficient for this. Thoſe who are acquainted with the difficulties that are met with in experimenting on living animals, and who know how much the circumſtances betwixt one animal and another vary, (which rigorouſly ſpeaking, are never the ſame) will agree with me on this head.

Let all that has been written on the irritability and ſenſibility of the animal fibres be examined, and the ſame inconveniences, the ſame difficulties, will be diſcovered. It is true, that a very great number of experiments have been made in a few years, and that an infinite number of animals have beeen ſacrificed to philoſophy, or publick utility; but much remains yet to be known, preciſely becauſe the number of experiments is not yet ſo conſiderable as it ought to be.

I muſt likewiſe confeſs, that I have wanted both time and patience to do more. Nothing but the idea of publick utility can ſupport the horrour of ſeeing ſo many animals, ſenſible of pain like ourſelves, ſuffer under our hands; and to view them expoſed to a thouſand kinds of torments. I leave the purſuit of this career to thoſe who are more courageous than myſelf. The road is open to obſervers, and I ſhall rejoice to ſee them embrace with ardour, the ſearch of truths that are advantageous to the human ſpecies.

CHAPTER II.

Whether the Volatile Alkali is a certain Remedy against the Bite of the Viper.

I DEEMED it neceſſary to examine this firſt queſtion in the moſt circumſtantial way, and therefore multiplied the experiments extremely, and diverſified them very much. This is the only method that could lead to demonſtration, and I flatter myſelf that my readers will be freed from all doubt.

The animals I had bit by vipers were of three different kinds. I employed birds and quadrupeds with warm blood; and frogs, which have the blood cold.

Amongſt birds, I almoſt always employed ſparrows, pigeons, and fowls; amongſt quadrupeds, rabbits, guinea-pigs, cats, and dogs.

An animal may be bit by a ſingle viper, and by ſeveral. It may be bit once or more; in a ſingle part, or in ſeveral.—All theſe caſes may make great variations in the diſeaſe and effects of the venom; it was therefore neceſſary to diſtinguiſh them from each other.

Animals bit by a single Viper, and only once.

The leg is the part of the animal I constantly had bit by the viper, in all the experiments contained in this chapter. By *leg*, I mean the muscular part of the foot, that is betwixt the femur and tarsus. The facility of having animals bit in this part by the viper, made me give it the preference. There is likewise another advantage, the ease with which the remedies are in this case applied.

In the experiments of this chapter, as well as those of the following one, I employed no other remedy against the bite of the viper than the *fluid volatile alkali*, to be found in every apothecary's shop. Some that I made use of, I made myself. Its composition has been long known, and is described in all the pharmacopeias. I employed it by having it swallowed, and by applying it to the part. When I wished to treat the part bitten, I dapped it a long time with a piece of linen, well moistened with the volatile alkali, and lastly, covered it with the same linen, to keep it wet the longer. That which was swallowed, as will be seen hereafter, was diluted with a quantity of water. On many occasions, I repeated it several times, and likewise made fresh applications of it to the part. There are animals which live so short a time after they have been bit, that I thought it superfluous to make repeated applications of the volatile

latile alkali to the bite. When I ſay, ſimply, that I treated the part bit, or that I treated the animal, it muſt be underſtood, that the volatile alkali was not given internally, but only applied to the part.

I had a dozen ſparrows bit by as many vipers, a ſingle time each. I took them from the cage, one after the other, without any choice. The firſt that was bit was immediately treated; the ſecond was not; the third was treated, and the fourth not; and ſo on as to the others, each having a thread tied to its foot, with knots, to diſtinguiſh them from each other. The feathers had been previouſly cut from the legs with ſciſſars. The animal was ſcarcely bit by the viper, when it was treated; ſo that there was an interval of not more than five or ſix ſeconds betwixt the bite and the application of the *volatile alkali*.

The ſparrow firſt bit, which was treated, at the end of two minutes could no longer ſupport itſelf on its feet, and died at the end of fifteen.

The ſecond, not treated, began to reel after three minutes, and died at the end of the thirty-fifth.

The third fell on its belly after ſix minutes, and died at the end of thirty-eight.

The fourth fell after four minutes, and died at the end of twenty.

The fifth after five minutes, and died at the end of twenty-ſeven.

The ſixth after ſeven minutes, and died at the end of thirty.

The ſeventh was ſtill living at the end of three hours, and did not appear to have at all ſuffered.

The eighth fell after two minutes, and died at the end of ſeven.

The ninth fell after three minutes, and died at the end of eleven.

The tenth fell after two minutes, and died at the end of fifteen.

The eleventh fell after a minute and one-third, and died at the end of two and an half.

The twelfth fell after ſix minutes, and died at the end of thirty-two.

The ſparrow that was bit the ſeventh, as I have juſt ſaid, was ſtill living at the end of three hours. I examined its leg, and found it perfectly in its natural ſtate, without lividneſs, without ſwelling, or any apparent wound. The legs of the other ſparrows were very much changed, even immediately after they had been bit; whence it was eaſy to conjecture, that either the ſparrow above alluded to had not been bit by the viper, or that this laſt was without venom.

To diſcover which of theſe two hypotheſes was the true one, I had this ſparrow bit by the ſame viper, in the ſame leg. A little blood flowed from the wound, which I immediately treated. It fell after two minutes, and died at the end of four; a proof that the viper was provided with venom, but that the leg had not really been entered by the teeth. I had, however, no ſuſpicion of this at firſt, as the creature bit in the uſual way.

I wiſhed

I wiſhed to repeat the ſame experiment on twelve other ſparrows, with the ſame order and circumſtances. But I made the ſix that were treated likewiſe ſwallow a few drops of water, in which I had put a proportion of the volatile alkali, of about an hundredth part.

The time of the death of theſe animals is expreſſed by the following numbers, repreſenting as many minutes elapſed after the bite; 10, 7, 8, 9, 6, 7, 3, 7, 15, 18, 5, 37. The ſix firſt numbers ſhow the time the ſparrows lived, that were treated with the volatile alkali.

From the preceding experiments the following conſequences may be deduced:

I. That the vipers I employed were ſufficiently provided with venom to kill ſparrows.

II. That the venom is ſcarcely introduced into the leg of the animal, when it ſwells in a ſenſible degree, changes its colour, and becomes ſomewhat livid.

III. That it is not ſufficient to enable the venom to inſinuate itſelf, that the viper ſeizes an animal betwixt its teeth, and that it cloſes its mouth, and preſſes with it.

IV. That the fluid volatile alkali does not preſerve the lives of the ſparrows bit by the viper.

V. That the volatile alkali given internally to ſparrows, may even be hurtful to them. The ſpeedier death of thoſe that ſwallowed it, may at leaſt lead one to ſuſpect ſo.

But the number of the experiments is not yet sufficient to render the consequences I have just deduced certain; 'tis a multiplicity of them alone that can effect this.

I had twelve other sparrows, equally lively, bit in the leg as above, each by a single viper, and only once. I treated only six of them with the volatile alkali. They all died. In all of them the leg that was bit became livid, and swelled in a greater or less degree, in less than two minutes.

The six treated died in 3, 4, 6, 11, 30, 33, minutes. The six that were not treated in 4, 4, 7, 11, 18, 35.

To obtain still more certain consequences, I had twenty-four others bit. I treated twelve, and made them swallow the volatile alkali. All the twenty-four died. The following numbers show the minutes the twelve that were treated lived, 2, 3, 3, 5, 5, 5, 7, 7, 10, 15, 15, 22; and these again indicate the minutes that those survived on which no remedies were tried, 4, 6, 6, 6, 7, 7, 9, 9, 9, 10, 15, 20.

It is a truth then, established by experiments, that the fluid volatile alkali is altogether useless, whether it is simply applied to the part bit by the viper, or swallowed by the animal at the same time. We may even suspect it to be hurtful, to sparrows at least.

However evident it may appear, that the volatile alkali is not an efficacious remedy in this case to a small animal like a sparrow, it is not on that account demonstrated,

demonſtrated, that it may not be ſo to a much larger animal, and of a different ſpecies.

The venom introduced into the body of a larger animal, ſhould be conſidered as diminiſhed in quantity. Its effects ſhould certainly not be ſo violent; and indeed this is the caſe with all the poiſons that we know of. What is a remedy to a large animal, or one of a full ſize, may be a poiſon to a ſmaller animal, or to one ſtill young.

We muſt therefore again have recourſe to experiment, and ſee the effect the bite of the viper has on other animals.

Experiments on Pigeons.

I had a pigeon bit in the leg by a viper, and inſtantly treated the part. At the end of a minute it fell forward, and could no longer ſupport itſelf. In twenty ſeconds more it died.

I had another pigeon like the firſt bit in the ſame way, but did not treat it. At the end of two minutes it fell forward, and in two minutes more it died.

I had two other pigeons bit in the leg; one was treated, and the other not. The firſt fell at the end of three minutes, and died at the end of the twentieth. The other fell at the end of a ſingle minute, and died likewiſe after the twentieth.

Of two other pigeons bit in the leg, I treated only one. The one treated died at the end of forty hours, the other at the end of an hour.

I had

I had ſix other pigeons bit in the uſual way. Three were treated, and three not. Thoſe that were treated died at the end of 6, 22, 40, hours. The other three died at the end of 1, 2, 10, hours.

I had two others bit in the leg in the uſual way; one I treated, the other I did not. The treated one died at the end of eight minutes; the other at the end of two hours.

The intervals at which pigeons die that are bit by the viper are ſo different, that they ſcarcely allow a reaſonable conjecture. It ſeems, however, that two truths may already be deduced. One, that the *volatile alkali* does not preſerve from death the pigeons bit by the viper. The other, that birds larger than ſparrows live longer in the ſame circumſtances; or, if you will, that pigeons die much later than ſparrows.

But experiments muſt be multiplied, and the circumſtances attending them examined more attentively.

I do not conceive very well how it was, that of two animals of the ſame kind, bit once in the ſame part, one died at the end of two minutes, and the other at the end of 40 hours.

I likewiſe obſerved ſomething ſimilar to this in the ſparrows; and therefore determined at length to have a very large number of both kinds bit. I did not treat any of them; but, in return, I marked carefully all the circumſtances that attended the experiments. I ſhall not enter into a detail of them here, on account of the very great number of them; but

but think it ſufficient to deduce the following truths:

I. That other circumſtances alike, the larger the viper, the more violent the diſeaſe, and the more ſpeedy the death.

II. That the diſeaſe increaſes likewiſe in violence, in proportion as the viper is more enraged.

III. That it likewiſe augments in proportion to the time the viper compreſſes the animal it has bit betwixt its teeth.

IV. That the diſeaſe of the part bitten ſeems to be greater in proportion to the time the animal ſurvives.

V. That in ſome animals black and livid blood flows from the wound, as ſoon as it is made.

VI. That in others, on the contrary, it flows red, and preſerves that colour.

VII. That the animals from which the red blood flows, die later than thoſe from which it flows black and livid.

VIII. That the venom likewiſe, which preſerves its colour and its qualities, ſometimes flows out with the blood. In which caſe, the animal not only ſurvives, or is much longer in dying, but ſometimes does not appear to have had any complaint.

Theſe conſequences, the fruit of an infinite number of experiments, diverſified in every poſſible way, and in which all the circumſtances that accompanied them were rigorouſly examined, form ſo many principles, which explain how it is that of two animals

mals bit in the same part, one dies suddenly, and the other survives, or does not die till very late.

There is likewise another reason, which I have since discovered, and which may vary the effects, in animals that have been bit, very much. This is owing to the viper itself. I have sometimes, tho' very rarely, found vipers that had no venom in either of the two vesicles, and more frequently, that only had it in one.

What led me at first to suspect that the vesicles did not constantly contain venom, was observing it to be to no purpose that I had a pigeon bit repeatedly by a certain viper; and that it not only did not die, but discovered no symptom of disease, notwithstanding the canine teeth of the viper had pierced its flesh through in several places.

Having had occasion, in the course of these experiments, to cut of the heads of a great number of vipers, and to examine their venom, out of two hundred, perhaps, I found two that were entirely destitute of venom, and five that, instead of venom, had a white and opake viscous matter in the vesicles. In two of these last, I found this white matter to be perfectly innocent. But in the other three it still preserved, partly at least, its venomous quality, as I assured myself by introducing a small quantity of it into the legs of pigeons, which had been bit superficially, and which died at the end of a few minutes.

It is another established truth then, that vipers are sometimes found without any venom, and that

somewhat

ſomewhat more frequently a whitiſh humour is contained in their veſicles, which is not always venomous. But theſe caſes are invariably very rare, and only met with in examining a very great number of vipers; whence it follows that it is alſo true, that vipers have in general their veſicles filled with venom, and that this humour occaſions diſeaſes, and even death.

I obtained much more uniform conſequences, by introducing the venom into the body of the animal, inſtead of having it bit by the viper. This is the method I employed. I cut off the head of a viper with a pair of ſciſſars, and, after a quarter of an hour, opened the mouth, and with another pair of ſciſſars ſeparated the lower jaw. I then divided the upper part of the head in two with very ſtrong ſciſſars; each part being furniſhed with the canine teeth, and with the veſicle of venom. With a little courage and dexterity, which are acquired by cuſtom, it is eaſy to force the tooth of the viper, on which a compreſſion is made with the fore finger whilſt the veſicle is preſſed upon by the thumb, into the ſkin of an animal. A greater or leſs quantity of the venom may be introduced, by preſſing more or leſs on the veſicle; the wound may be made wherever one pleaſes; and, laſtly, the venom may be kept from being rejected, by letting the tooth remain a long time in the wound. A great number of experiments made in this way, prove that ſparrows die betwixt five and eight minutes, and pigeons in betwixt eight and twelve. There are very few that

die

die ſooner or later; whence it follows, that by purſuing this method, the periods of their diſeaſe are both ſhorter and more uniform.

I had a dozen pigeons bit in the uſual way, one after another, by as many vipers, and treated them all with the volatile alkali. They all died. The numbers 4, 10, 16, 52, expreſs the time in minutes in which four of theſe pigeons died; and the numbers 2, 4, 9, 15, 19, 22, 25, 36, expreſs the time, in hours, of the death of the others.

Theſe new experiments leave no doubt as to the inefficacy of the fluid volatile alkali againſt the venom of the viper.

To aſſure myſelf ſtill better of this, I had twenty-four other pigeons bit, each of them once in the leg, by a viper. I treated them all, but only twenty-two died. The time of their death is expreſſed in minutes, by the numbers 4, 4, 6, 6, 7, 8, 8, 10, 12, 14, 14, 20, 50, 50, 56; and in hours, by 1, 1, 2, 4, 7, 10, 18, 26, 30.

Two of theſe pigeons, bit in the ſame way as the others, appeared not to have ſuffered at all, walking about the chamber as before the operation. At the end of two hours, being deſirous of examining the ſtate of their legs, I could find no ſign of diſeaſe. They were neither ſwelled nor livid. I could only find in one of them a ſmall hole, and a ſmall red ſpot of blood, at the part where the tooth had penetrated. Since there was not the ſmalleſt mark of diſeaſe, it was eaſy to perceive that the venom had not introduced itſelf; or, if it had, that it had been thrown out again, so as not to occasion any complaint to the animal. After ten other hours, I had both pigeons bit once in the same leg by two vipers that had already been employed in the same way. At the end of three minutes there were signs of disease: one died at the end of an hour, the other at the end of two.

Not content with these experiments, I had twelve other pigeons bit in the usual way. I treated them immediately, and made them swallow the volatile alkali. They all twelve died, at the end of 4, 4, 7, 10, 10, 10, 15, 18, 20, minutes; and of 2, 3, 3, hours.

Whilst it is certain, on one hand, that the volatile alkali is of no effect in recovering pigeons bit by the viper; on the other hand it remains undecided whether it is in this case hurtful or not.

The periods at which these animals die are so various, that it is not possible to deduce any certain consequences from them.

Experiments on Fowls.

It is not sufficient to have demonstrated the inutility of the fluid volatile alkali administered to pigeons, to allow us to conclude that it is useless to larger animals, that are more difficult to kill. The volatile alkali may have time to act against the venom of the viper, when the disease is not so violent, and the animal slower in dying.

There are certain remedies which, although efficacious,

cacious, require a certain time to act; and, indeed, almoſt all are of this deſcription.

I had a fowl bit once in the leg by a viper, and immediately treated it; at the end of ſix hours the fowl died. I afterwards had another bit once by a viper, and did not treat it. This one died in eight hours.

I had two other fowls bit once in the leg as uſual. One was treated; the other not. The firſt died in four hours; the other in ten.

I had ſix other fowls bit as above, each once in the leg by a diſtinct viper. The three firſt were treated with the volatile alkali, and died; one in ſix hours, another in eight, and the third in nine. The three others were not treated, and died in 7, 9, 20, hours.

Although the number of the experiments hitherto made on fowls, is not yet ſufficient to allow certain conſequences to be drawn, it however appears, that the following very probable ones may already be ſtated.

I. That it is very poſſible for fowls bit once in the leg by a viper, to die.

II. That they in general die much later than pigeons; and than ſparrows, which die again with much greater facility than pigeons.

III. That birds reſiſt death in proportion to their ſize.

IV. That the volatile alkali is not only of no uſe in curing fowls bit by the viper, but that it is probably even hurtful to them.

But

But experiments muſt be multiplied much more, to ſee if the conſequences juſt deduced are well or badly founded.

I had therefore ſix fowls bit ſeparately by ſix vipers, each once in the leg. I treated them all ſix, and made a freſh application of the volatile alkali to the part bitten, every two hours. Two of the fowls died in the ſpace of four hours, one in five, two in ſix, and one in ten. A moment after, I had ſix other fowls bit by as many vipers, each once in the leg, and did not treat either of them. Two died in two hours, two in ten, and two in twelve.

Twelve other fowls were bit by as many vipers, each once in the leg. I treated ſix, and made them ſwallow the volatile alkali. The other ſix were left to themſelves. Of the ſix treated, five died; the ſixth had ſcarcely any ſymptom of complaint. Its leg neither ſwelled, nor became at all livid. There was ſimply a hole in the ſkin, which was red and a good deal inflamed. The five I have juſt mentioned died in 3, 4, 6, 7, 10, hours. The other ſix died in 6, 10, 17, 22, 36, 36, hours.

Had the experiments I have related ſo far been more numerous, the abſolute inutility of the fluid volatile alkali againſt the bite of the viper would not only have been demonſtrated, but we might even have doubted its innocence, at leaſt to this ſpecies of animals.

The treated fowl that did not die, proves nothing in favour of the volatile alkali, as will be ſeen in the continuation of this work. It is one of the

 caſes

cases I remarked above, in speaking of the pigeons and sparrows, in which the venom was not communicated to the part bit, although the canine tooth had left some opening in it, either owing to the viper not having any venom, or to the rejection of it. Nothing is found in either of these cases to favour the volatile alkali.

Having assured myself of the inutility of this remedy to the three species of birds I have submitted to the experiment, I think it time to make the same trials on quadrupeds.

Experiments on Guineapigs.

I had a large guineapig bit once in the leg by a viper, and immediately treated it. In a little time the leg swelled, and became livid. At the end of sixteen hours a wound of an inch in breadth formed itself at the part that had been bit and treated. In twenty hours the skin in this part was entirely eaten away. The wound continued open for more than twenty days, during which time the animal moved its leg with difficulty; the foot was greatly contracted, and the muscles very much diseased. The animal recovered however, but its leg still remained in a degree contracted, and it could never recover the perfect use of it.

Another guineapig, almost as large as the former, was, in the same way, bit once by a viper in the

leg,

leg, which was not treated. The animal died at the end of two hours.

I had four others, of ſcarcely a third the ſize of two preceding ones, bit in the above way. I treated each of them, and made them ſwallow the volatile alkali. They all died, one in two hours, another in three, the third in ſix, and the fourth not till the twentieth hour had elapſed.

That I might have a comparative experiment, I had four other guineapigs, entirely like the preceding ones, bit, and did not treat either of them. They all four died, one at the end of ſeven hours, another at the end of ten, the third at the end of thirty, and the fourth at the end of thirty-one.

From theſe experiments we may, I think, already draw the following inferences, which if not certain, are at leaſt very probable.

I. That the bite of the viper is capable of killing even the largeſt guineapigs.

II. That the ſmaller animals die ſooner than the larger ones of the ſame ſpecies.

III. That the volatile alkali is not a certain remedy againſt the bite of the viper.

It may be objected, that the firſt guineapig bit and treated, at length recovered, and that all which were not treated died. This is true, but proves nothing, ſince, as has been ſeen above, there are ſeveral circumſtances that may render the bite of the viper innocent; and, on the other hand, we have ſeen that the other four guineapigs died, although they were treated. Now if we conſider that the four treated died in a much ſmaller ſpace of time than the five that were not treated, we may ſuſpect that the volatile alkali was more than uſeleſs, that it was hurtful.

To remove all doubt, I had twelve other guinea-pigs bit, all alike in ſize, and ſimilar to the eight preceding ones. Six were treated, and ſix not.

The firſt I had bit was the ſame I have ſpoken of a little above, and which, far from dying of the bite, had not even the diſeaſe of the venom. Although treated, it died at the end of thirty hours. The five others that were likewiſe treated, had all of them the diſeaſe of the venom, but only three died; two in leſs than twenty hours, the other at the end of twenty-ſeven. The two that ſurvived had each of them a large wound in the leg that had been bit, and this remained open for more than ten days.

Of the ſix that were not treated two only died, in leſs than ſixteen hours. Three others had deep wounds, which remained open for ſeven days, and then healed. The ſixth had not the ſmalleſt ſymptom of diſeaſe, and I could not diſcover in its leg any mark of the viper's tooth having penetrated.

The caſes ſo far related, ſeem to leave no doubt as to the inutility of the volatile alkali, when tried likewiſe on theſe animals; and they do not remove the ſuſpicion, that it may poſſibly be even hurtful to them.

We likewiſe ſee that the ſmaller and younger guineapigs die ſooner than the larger ones.

I had a dozen very ſmall ones bit, each ſcarcely weighing five ounces. Six were treated, and ſix not. Thoſe that were treated died in 30, 40, 50, minutes, and 1, 2, 3, hours. Thoſe that were not treated in 57 minutes, and 2, 3, 4, 4, 4, hours.

I afterwards had ſix guineapigs bit, three of the largeſt of which were treated; the other three were not. Of thoſe that were treated only one died, and only one again of thoſe that were not treated. All of them, however, were very much diſeaſed, and thoſe that were treated were the laſt to recover.

Experiments on Rabbits.

It remained for me to make the ſame experiments on rabbits, in purſuance of the plan I had propoſed to myſelf.

With this view, I had a large rabbit bit by a viper once in the leg, which I immediately treated with the volatile alkali, making the animal ſwallow the ſame diluted with water. At the end of an hour I repeated the application and the internal remedy. The rabbit died at the end of three hours, with very ſlight marks of diſeaſe in its leg.

I had another, perfectly like the former, bit in the ſame way once in the leg by a viper, and at the ſame time. It had ſlight ſymptoms of the diſeaſe of the venom, and the leg became ſomewhat ſwelled. At the end of thirty hours a wound two lines in breadth, and very deep, appeared on the ſkin at the part where it had been bit. After five days more the animal was perfectly recovered.

The reſult of two experiments alone can be in no way certain, I therefore had recourſe to my uſual method.

I had a dozen rabbits of a middle ſize bit by as many vipers, each once in the leg. Six were treated, and ſix not. Only two died of thoſe that were treated, and three of thoſe that were not. Two of the four treated ones that did not die ſcarcely had any complaint. Their legs were but little ſwelled, and were not livid. The other two were very much diſeaſed, and had large wounds that were four days in healing. Of the two that died, one lived two hours, the other five. The ſix that were not treated were all of them very much diſeaſed, and had large wounds in their legs, which ſwelled violently, and became very livid. The three that died lived 14, 22, 47, hours; the others did not recover till the end of the ſeventh day.

It is a conſtant obſervation, that when the animal bit by the viper dies very ſoon, the bitten part is proportionably leſs changed, leſs ſwelled, and leſs livid. The change which takes place at the part where the poiſon has entered, I call the external diſeaſe, to diſtinguiſh it from the other, which is infinitely more violent and dangerous, and which kills the animal in a more direct way. I ſhall ſpeak more fully of this laſt in the fourth chapter of this ſecond part, in which I ſhall endeavour to account for this particular.

The

The few experiments hitherto made on rabbits may already make us ſuſpect the little efficacy of the volatile alkali, which we may be even tempted to believe hurtful. It is certain in the interim, that middle-ſized rabbits frequently reſiſt the venom of the viper.

I wiſhed to try the effects of this on much ſmaller ones, and for this purpoſe had a dozen bit in the uſual way. I treated ſix, and did not treat the others. All the twelve died; the treated at the end of 2, 3, 4, 6, 8, 9, hours; and the others at the end of 3, 5, 7, 9, 12, 13.

I repeated theſe experiments on twelve other ſmall rabbits, exactly like the foregoing ones. I treated ſix, and made them ſwallow the volatile alkali every hour. The others I did not treat. They all died; thoſe that were not treated, at the end of 1, 1, 2, 2, 5, 17, hours; the others in the ſpace of 1, 3, 3, 10, 16, 16, hours.

Theſe new experiments already ſhow very clearly the little efficacy of the volatile alkali againſt the bite of the viper, when tried on rabbits; they even lead me to ſuſpect it to be rather hurtful than otherwiſe.

We likewiſe ſee that ſmall rabbits die from the bite of the viper, whether they are treated or not; but that the larger ones frequently ſurvive its effects.

In conſequence of this, I had ſix of theſe animals, very large ones, bit each by a viper once in the leg. Three were treated, and ſwallowed the volatile alkali. Two of theſe died at the end of twenty hours; the third was very much diſeaſed, and had a large wound, which remained open for twenty-three days. Of thoſe that were not treated, one died at the end of thirty-four hours; the other two had the diſeaſe of the venom, but recovered in leſs than ten days.

I repeated this experiment in the ſame way on ſix other large rabbits. Of the three that were treated, one died; and one likewiſe died of the three that were not treated. The other two of theſe laſt recovered in ten days; and the two treated ones that ſurvived, not till the end of eighteen.

I think there can be no longer any doubt of the inefficacy of the volatile alkali to theſe animals; on the other hand, inſtead of diminiſhing it, it ſeems to ſtrengthen and reinforce the diſeaſe.

It remains to try the effects of the bite of the viper on cats and dogs. The number of my experiments on the animals of theſe two ſpecies is much ſmaller than that of thoſe on the others. The difficulty of procuring them, the danger one runs in operating on them, and, ſtill more, the inconvenience of keeping them during the long continuance of the diſeaſe, and the unpleaſantneſs of ſeeing them ſuffer, have occaſioned me to do leſs in this inſtance than the ſubject may perhaps appear to have required.

Expe-

Experiments on Cats.

I had two very ſmall kittens bit in the uſual way, each once in the leg. One was treated, the other not. The laſt died at the end of ſixteen hours. The treated one was exceedingly ill, and had a wound which remained open for five days, in its foot. It lived, however.

Three very ſmall kittens were brought to me, ſtill younger than the foregoing ones. I had them bit, as uſual, in the leg. I treated one, and made it ſwallow the volatile alkali. I did nothing to either of the other two. They all three died in leſs than ſix hours.

Theſe experiments are neither ſufficiently uniform, nor enough in number, to admit the drawing of certain conſequences from them. We ſee that, in general, the ſmaller animals of the ſame ſpecies, ſuch as cats, for inſtance, die much readier than the larger ones; and likewiſe that thoſe die which have been treated, and have ſwallowed the volatile alkali.

I had two other kittens bit, larger than thoſe I employed before. Each of them was, as uſual, bit once by a viper. One was treated, the other was not. Neither of them either died, nor was very ill. They had no wound; and both of them at the end of twenty-four hours ate very heartily. The leg, however, in each, was not yet very ſupple in its motions. I did not make the one I treated ſwallow the volatile alkali, on account of the difficulty

one

one meets with in attempting this, when these creatures are pretty large. They become extremely furious, and are very difficult to manage, at least without risk.

I had two other kittens of the same size of the preceding ones bit, and treated neither of them. They were each of them bit once in the leg. They both recovered, and had no perceptible wound. It was twenty hours indeed before they had any use of the leg that had been bit; however, they seemed perfectly recovered at the end of the third day.

Two large grown cats were, in the same way, bit in the leg. Neither of them was treated, and neither died. At the end of sixteen hours they fed a little, and could already use their legs, although not very well. At the end of thirty hours they appeared to be perfectly recovered.

Scarcely has a cat been bit in the leg by a viper, when it can no longer make any use of the part. It lies down, and continues longer in this posture in proportion to the violence of the disease. It neither eats nor drinks till the symptoms abate, and when that happens, recovers to a certainty.

Experiments on Dogs.

We are now to try the effects of the volatile alkali, which has been of no utility to the cats, on dogs that have been bit by the viper. The dog has a great affinity to man himself, and is, of all animals, the one the most susceptible of the passions.

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It is certainly much more ſo than the cat and the other animals, that have been bit in the courſe of theſe experiments. Dogs are to be met with of every ſize, even ſo large as not to differ much, in that reſpect, from an adult perſon.

The effects of the bite of the viper on dogs, may be of great uſe in judging of the bite of the viper in man himſelf.

I had two dogs of a middle ſize bit once in the leg. I treated one of them every two hours, and made it ſwallow the volatile alkali as often. Neither of them died, although the leg was ſwelled in each. The one not treated had no wound, and recovered at the end of four days; the one that was treated had a large wound, and did not recover till the cloſe of the tenth day.

I had two other much ſmaller dogs bit, and treated only one of them. They both died in leſs than three hours, with a degree of ſwelling and lividity in the part bitten.

Two large dogs were brought to me, and I conceived from their ſize, that they would recover although not treated. I had them bit in the uſual way, once in the leg. One ſcarcely had any ſenſible complaint; the other no perceptible wound. The leg of the laſt, however, ſwelled very much, and did not get well till the end of the ſixth day.

I had two other large dogs bit by a viper as uſual, each once in the leg, and did not treat them. One recovered in two days, the other in ſix.

From

From the experiments hitherto made on dogs, we may draw these conclusions:

I. That the smaller ones usually die from the bite of the viper.

II. That large ones generally recover.

III. That of the middle sized ones, some recover, and some die.

IV. That the volatile alkali seems to be neither a certain nor a useful remedy against the bite of the viper.

Experiments on Frogs.

It remained for me to try the effects of the venom of the viper on frogs. I had hitherto operated on animals with warm blood; it was likewise necessary to make some experiments on those that have the blood cold.

I had a dozen frogs bit by as many vipers, each once in the leg. I treated six of them only. Two of these died at the end of twenty hours, and the legs of the other four swelled, and were a little livid; they recovered however. Of the six not treated, three died at the end of five hours. Of the three that survived, one had a swelling and discolouration of its leg; the other two had no apparent complaint.

The consequences were as yet too vague, and too few in number, to admit any certain conclusions to be drawn from them.

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I therefore had a dozen others bit in the fame way, and treated fix of them only. To thefe I renewed the application of the volatile alkali every hour, making them fwallow it at the fame time. All the fix, one of which did not furvive the twentieth minute, died in lefs than four hours. Of the fix not treated, four died at the end of 6, 10, 12, 20, hours; the fifth had fcarcely any complaint, and the fixth recovered two days after.

I repeated this experiment on twelve other frogs, having them bit in the fame way, each once in the leg by a viper. Six were treated every hour, and fwallowed the volatile alkali as often. The other fix were left to themfelves. Five of the firft died; the fixth had fcarcely any fymptom of complaint. Of the fix not treated, three died, and the other three recovered at the end of two days.

After what has been faid, I think there can no longer be any doubt of the inutility of the fluid volatile alkali. It is very probable that, when given internally to frogs, it increafes the difeafe caufed by the venom, inftead of diminifhing it. It is at leaft certain, that the animal dies the readier under thefe circumftances.

CHAP.

CHAPTER III.

Of the Effects of the Bite of one or several Vipers, on the same Part of an Animal, or on two corresponding Parts of the same Animal.

I HAVE hitherto ſpoken of the effects of the venom on animals bit once by a viper, in the ſame part. It now remains to ſpeak of animals bit repeatedly by one or more vipers in different parts.

It is natural to conceive, that a viper which bites the ſame animal ſeveral times, muſt bring on a diſeaſe proportionably violent. After having ſeen in the firſt part of this work, that the venom of the viper is a humour ſeparated from the fluids of the animal, and ſecreted in a veſicle or gland; and that this humour is always venomous in itſelf when it is introduced by a wound into the bodies of animals, particularly of thoſe with warm blood; there can no longer be any doubt of this truth, nor of the abſolute falſehood of the hypotheſis of Monſ. Charas, who pretends that the venom of the viper is entirely occaſioned by the fury of the animal, which changes the ſaliva and other humours of its mouth, to ſuch a degree as to produce a powerful venom, ſuch as is obſerved in the foam of a mad dog.

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The vesicle is moreover constructed in such a way, that all the venom cannot flow out at once, at a single bite, however forcible it may be, and however the viper may be enraged. A description of this vesicle, with that of the gland, will be seen in the third part of this work. From the foregoing consideration it was necessary to examine the effects and diseases produced by several bites, although of a single viper. There are several examples of persons bit more than once by the same viper; and notwithstanding this case is not one of the most frequent, it occurs however from time to time.

It is not only very important to examine the effects of the repeated bites of the same viper on the same part of an animal; but likewise to observe the action of the venom on the different parts of the same animal.

We know that an animal is formed of organs and parts, differently organized. There are parts that have vessels and nerves, without having muscles; and these are in different proportions, and differently distributed: there are others again that have no nerves, and, if they have any, have only a few very fine capillary vessels. It is natural to suppose that the effects of the venom of the viper, on parts of an animal so very different from each other, must be altogether different; and that the same quantity of venom conveyed into a wound made in an animal, may produce either death, a slight disease, or none at all. In a word, it appeared to me, that nothing ought to be omitted in so important a matter.

There is likewise a case, although I think it a very rare one, in which several vipers together bite the same part, or different parts, of an animal. However rare this accident may be, it is not impossible for it to happen; and it is not an extraordinary thing to find, at certain times of the year, several vipers collected together. A man who may not have noticed this, may by treading upon them be in danger of being bit by more than one; and I knew a viper-catcher who was bit in the hand by two at the same time, and who might have been bit by more than two, since several of them were making their way at the same time out of a box.

These examples of animals bit by several vipers may, however, agree very well, making some little allowance, with the cases of the repeated bites of the same viper, whether on the same part, or on different parts, of an animal.

I said above, that I had found by experience the effects of the venom to be much more uniform, when, instead of having the animals bit by vipers, the venom is conveyed into them, by pressing with one finger the vesicle which contains it, whilst with the other the tooth of the viper is forced into the part. I have frequently employed this method during the course of my experiments, particularly in those on the sparrows and pigeons. In this way I not only succeeded in wounding the same part of the animal over again to a certainty, but even the very fibre. I could likewise assure myself, if I was desirous of it, whether the vesicle contained venom, or whether

whether the quality of the latter was suspicious or changed.

The slightest compression made on the vesicle is sufficient to bring a very small drop of venom to the point of the tooth; its transparent colour determines its activity and nature.

The first thing I thought it necessary to determine here, was to see whether the second bite of the viper is as powerful as the first, the third as the second, and so on as to the others; and how many times, one after the other, the viper can venom animals with its bite. I took a viper of a middle size, and very lively, and, without provoking it much, made it bite a pigeon once in the leg. The pigeon died at the end of twelve minutes. A moment after it had bit this one, I made it bite a second, a third, a fourth, a fifth, a sixth, and a seventh, in the same part. The second died at the end of eighteen minutes, the third of sixteen, the fourth of fifty-two, and the fifth at the end of twenty hours; the sixth had scarcely any symptoms of complaint, and the seventh continued perfectly well.

I repeated this experiment several times, and the consequences were somewhat various. I met with some vipers, particularly the largest of them, that could kill ten, and even twelve pigeons. If they are very much enraged during the first bites, the last, as I have assured myself by repeated experiments, are less dangerous.

It is an established truth then, as I have several times experienced, that the first repeated bites of a

 viper

viper are almoſt equally dangerous; and that in proportion as a viper is enraged, the diſeaſe occaſioned by its bite is more violent.

This laſt truth may tend in ſome degree, to account for the treacherous experiments of Charas on the venom of the viper. In oppoſition to Redi, as has been ſeen above, he was of opinion that this venom conſiſts ſimply in the rage of the animal, and made a great many experiments to ſupport his hypotheſis.

It was natural to conceive, that the more a viper is enraged, the greater will be the diſeaſe produced by it, and *vice verſa.* But to draw a certain inference from this, it was firſt neceſſary to be aſſured, whether the degree of the diſeaſe, or intenſity of the venom, is in proportion to the rage of the animal: a very difficult experiment, and perhaps impoſſible to make well; and which would not probably have been yet ſufficient, ſince after all this might have been an accidental circumſtance, and not the true cauſe of what was obſerved.

Charas, who was ignorant of the true reaſon of the greater intenſeneſs of the diſeaſe in the caſes in which the viper is enraged, was miſtaken in his inferences. It is not ſurprizing that the naturaliſt ſhould here take that for the cauſe, which is the effect of the circumſtances that accompany it.

There are three reaſons why the bite of an enraged viper is more dangerous than that of one which is not enraged. The firſt is, that the more a viper is enraged, the deeper it forces its teeth into the animal; the ſecond, that it keeps them there a longer time; the third, that without letting go the part it has bit, it continues to contract the muſcles that compreſs the veſicle of venom.

When one has been ſome time accuſtomed to have animals bit by vipers, it is not difficult to perceive the truth of the firſt reaſon; and it is ſometimes even obſerved, that the tooth of the viper pierces the ſkin of the larger kind of quadrupeds with great difficulty, or only imperfectly and in part. All my experiments have ſhown me, that the diſeaſe is in general more violent, in proportion as the venom has introduced itſelf deeper into the ſkin and other parts of the animal.

The ſame obſervation likewiſe demonſtrates the truth of the ſecond reaſon. We frequently ſee that when a viper is violently enraged, it does not eaſily let go its hold; one might even ſay that it finds a difficulty in withdrawing its teeth. In theſe caſes it is eaſy to perceive, that during all this time the tooth not only prevents the venom from being thrown out again with the blood that naturally flows from wounds; but likewiſe, that it facilitates its union and mixture with the fluids of the animal.

The third reaſon is ſtill of greater weight than either of the other two. It has been ſeen, that ſeveral bites of the viper are neceſſary to empty the veſicle of the venom perfectly. It has been ſeen, that the firſt bites of the viper are nearly of the ſame activity, becauſe they are ſucceeded by the flowing of nearly an equal quantity of venom. The cellular ſtructure of the veſicle does not allow it to be eaſily emptied, nor at once. When the viper keeps an animal a long time compreſſed by its teeth, and is very much enraged, it continues viſibly to contract the muſcles of its jaw. The muſcles which ſurround the veſicle alternately relax and contract without interruption, ſo that in theſe caſes we may reckon the bite of the viper, not as a ſingle one, but as ſeveral; and this may be carried to ſuch a length, that the viper, almoſt exhauſted of its venom, may not be capable of killing a ſmall animal.

It has been ſeen, that the firſt bites of the viper are all nearly of the ſame activity, and that it is only the laſt which exhibit a marked difference; and this I have accounted for.

After what has been ſaid, it is natural to conceive that the diſeaſe of the venom muſt be more dangerous, if the viper has bit the ſame animal ſeveral times. I have aſſured myſelf of the truth of this, by experiments the detail of which I ſhall not enter into here, as it would be tedious, and would not beſides anſwer any great purpoſe.

In inveſtigating this ſubject, I took care to employ animals of the ſame ſize and ſpecies, and had them bit by vipers ſimilar to each other. I more commonly availed myſelf of my uſual method, and the conſequences were ſtill more uniform. When only a few experiments are made, the conſequences may be equivocal, ſince it can ſcarcely happen

pen that the circumſtances will be perfectly the ſame. They may not only differ on account of the quantity of venom that remains in the animal's wound, which is ſubject to greater or leſs variations, but likewiſe becauſe it is very difficult to wound the ſame fibres, and the ſame veſſels. Theſe variations do not fail to occur; but in a great number of experiments, the circumſtances counterbalance each other, and ſo great a variety of conſequences preſents itſelf, that there is not the ſmalleſt danger of being miſled by them. Such has been at leaſt my opinion as to thoſe I have obtained.

A new enquiry to be made, was to know if the diſeaſe would be equal, whether a ſingle part was bit ſeveral times by a viper, or two different parts, provided the number of bites was the ſame.

This enquiry coſt me a vaſt many experiments, which I was obliged to make with the ſame circumſtances, only varying the part bit.

I not only tried it on birds, but on a great number of quadrupeds. I had them bit in the ſame part of their legs. I compared thoſe that were bit in both legs, with thoſe that were only bit in one, the total number of bites being the ſame in each animal.

Here again the conſequences were more or leſs conſtant. I was obliged to multiply my experiments till I conceived that I could advance, with great probability, the two following poſitions.

I. That an animal dies fooner when bit a certain number of times in two parts, than when the fame number of bites is confined to one.

II. That in this cafe the fingle part is fubject to a much more violent external difeafe.

By external difeafe, I mean the fwelling of the part that has been bit, the livid and black colour of the fkin and blood, and the wound that forms a fhort time after the bite. Thefe fymptoms are certainly more violent when the part has been bit feveral times; although it is a fact, as will be feen hereafter, that the animals die much later, and that fewer of them die in proportion. It is however to be noticed, that this only happens when the animals live for fome time after being bit, fince otherwife the venom has not an opportunity of effecting any notable change in the external parts; in fo much, that if the animal dies almoft immediately after being venomed, there are fcarcely any figns of local difeafe.

Before I examine the effects of the bite of the viper on the different parts of an animal, let me be permitted to relate the event of a great many experiments I made on animals of different fpecies, which I had repeatedly bit, and by feveral vipers. In all thefe cafes I employed the fluid volatile alkali, either fimply applied to the part bitten, or given internally at the fame time. Thefe new experiments demonftrate ftill more the inefficacy of the volatile alkali, and how little dependence ought to be placed upon it.

I had

I had ſix fowls bit, each of them twice by a diſtinct viper. Three were ſimply treated, three were not. The three that were treated died at the end of 3, 5, 6, hours; the other three at the end of 3, 9, 12, hours.

I had ſix other fowls bit, each by two diſtinct vipers, once in each leg. I treated them, and made them ſwallow the volatile alkali. They were all dead before the expiration of ſeven hours; one of them died in leſs than twenty-ſeven minutes.

Twelve other fowls were bit, each of them twice in the leg, and by different vipers. Six only were treated, and ſwallowed the volatile alkali. Nine died in the whole; five of thoſe that were treated, and four of the others. Two of theſe laſt lived forty-three hours; the treated five that died did not ſurvive the ſeventh.

The reſult of the laſt claſs of experiments, although it does not agree with that of the two that precede it, is nevertheleſs given with preciſion. This ſhows how much experiments of this kind may differ from each other, from circumſtances which occaſionally vary, and which cannot always be aſcertained. Thoſe that are capable of influencing the moſt are, that vipers are not always provided with the ſame quantity of venom, and that they are more or leſs vigorous in biting, and in forcing this humour from the veſicle: to theſe may be added, the effect of a milder or ſeverer ſeaſon. I began my experiments in September, and continued them with more or leſs earneſtneſs till the cloſe of the January following. I likewiſe made a few in February, March, and April, and found a ſenſible difference at theſe different times. During the ſevere froſt, the vipers were ſo weak, that it was with difficulty I could get them to bite; and their bites were in a very ſmall degree dangerous.

I cannot here paſs over an experiment I made in the month of January, and which at firſt made me ſuſpect that the volatile alkali might ſometimes be a remedy againſt the bite of the viper.

I had ſix fowls bit in the leg, each by three vipers, all of which bit three times ſucceſſively. I treated them ſeveral times, and made them as repeatedly ſwallow the volatile alkali. They all had the diſeaſe of the venom, but in a very ſlight degree, and recovered in a few days.

There remained, as chance would have it, in the ſame box, eighteen other vipers, perfectly like the eighteen employed in the preceding experiment. Perceiving at the end of fourteen hours that neither of the fowls was dead, and that they were all but ſlightly diſeaſed, I had ſix others bit in the ſame way, each by three of theſe vipers, and each viper biting three times. I treated neither of them, and only one died, at the end of ſix days. Two had ſcarcely any complaint, and the three others recovered on the third day. This experiment demonſtrates clearly, that the ſix treated fowls were not cured by the volatile alkali, but that their recovery was owing to the little vigour and activity of the vipers by which they were bit.

The

The fowl in the laſt experiment that was not treated and died, argues nothing in favour of the volatile alkali, ſince it is only one out of ſix, and ſince it did not die till the end of the ſixth day. This evidently proves, that if the venom had been in a ſomewhat ſmaller quantity the fowl would not have died. We have ſeen above, that a thouſand accidents may vary this greater or leſs quantity of venom, both in the viper that inflicts the bite, and in the animal that receives it.

On this very account, I have made it a maxim, in almoſt the whole courſe of this work, to form comparative experiments, and only to compare thoſe with each other, that were made at the ſame time and with the ſame circumſtances.

I muſt here inform my readers of what befel the vipers I employed laſt. The ſeaſon was very cold, and notwithſtanding the temperature of my chamber was twelve degrees above the freezing point, the vipers were very ſluggiſh and inactive. I conceived that I could give them a freſh vigour by additional warmth, and therefore, after keeping them in my laboratory for upwards of ſix hours, in a box pierced with holes, I at length placed the box on a ſand heat, the warmth of the ſuperficies of which was only twenty degrees. At the end of two minutes I found every one of the vipers dead. The ſame accident happened to me twice beſides, in the ſame month, and on an occaſion ſomewhat ſimilar.

Expe-

Experiments on Guineapigs bit ſeveral Times, and by ſeveral Vipers.

I had two very large guineapigs bit repeatedly in the leg by two vipers. One was treated, the other was not. They both died; the firſt at the end of two days, the ſecond of thirty-two hours.

I had four other guineapigs, preciſely of the ſame ſize, bit each in the leg by three vipers, three times by each. Two were treated, and ſwallowed the volatile alkali; the other two were left to themſelves. All four died in leſs than two days.

Again, I had four others of the ſame ſize bit in the ſame way. They were not treated. One only died, at the end of the fifth day.

Twelve very ſmall ones were bit in the ſame way. Six were treated, and ſwallowed the volatile alkali; the other ſix had nothing done to them. They all died in the ſpace of twenty minutes.

Two days after, I had twelve others bit, of the ſame ſize as the laſt, each receiving from two diſtinct vipers three bites in each leg. Six were treated, and ſix not. They all twelve died in two hours. One of the treated ones died in ſeven minutes, and two of thoſe that were not treated in fourteen.

Theſe experiments convince us at a glance of the inutility of the volatile alkali. They likewiſe ſhow, that in animals of this ſpecies the ſmaller ones

ones die fooner than the larger, and that their deaths are fpeedier and more certain, in proportion to the greater number of the viper's bites.

Experiments on Rabbits bit feveral Times, and by feveral Vipers.

I had four middle-fized rabbits bit, each four times in the leg, by two diftinct vipers. I treated two of them, which I made fwallow the volatile alkali every two hours, repeating the application as often. They all four died; the two that were treated, in eighteen hours, the other two at the end of three days. In all of them the difeafe of the venom was very violent, and their legs were very much fwelled.

I had four very large rabbits bit, each by two vipers, twice in the leg. Two were treated, and two not. The two that were treated, although they furvived, continued ill and with open wounds in their legs, for upwards of twenty days. One of the two that were not treated died on the third day; the other recovered on the tenth.

I had a dozen middle-fized rabbits bit in the leg, each by two diftinct vipers, and each viper biting three times. Six were treated, and fix not. Four of the former died, and five of the latter.

Thefe confequences not being either fufficiently uniform, or in a fufficient number, to enable me to decide

decide as to the volatile alkali. I judged it neceſſary to have recourſe to new experiments.

I had twelve rabbits, ſomewhat ſmaller than thoſe employed in the laſt experiment, bit in the ſame way. Six of theſe were treated, and ſwallowed the volatile alkali; the other ſix were left to themſelves. All of the former ones died, and five of the latter; the ſixth had ſcarcely any perceptible complaint.

I wiſhed to ſee whether there would be a ſenſible difference betwixt the effects of the venom, on animals bit a greater or leſs number of times, by a greater or leſs number of vipers. For this purpoſe, I had ſix middle-ſized rabbits bit, each once in the leg, by a diſtinct viper. I had ſix others bit in the leg each by two diſtinct vipers, each of which made two bites. I had ſix others bit in the ſame part, each by two diſtinct vipers, each of them biting four times; and ſix others again, each of which was bit by three diſtinct vipers, four times in the leg by each.

Of the ſix of the firſt claſs, three died; the other three had moderate complaints. Of thoſe of the ſecond, five died, and the other had a violent attack of the diſeaſe. All of the third claſs died in in leſs than forty-three hours; and thoſe of the fourth in leſs than twenty.

Expe-

Experiments on Dogs, bit ſeveral Times, and by ſeveral Vipers.

I had two ſmall and young dogs bit in the leg, each by two diſtinct vipers, and twice by each. One was treated, and ſwallowed the volatile alkali; the other had dothing done to it. They both died in the ſpace of thirteen hours.

I had two dogs, larger by one-half than the preceding ones, bit each by two diſtinct vipers, and twice by each. One was treated, the other not. Both recovered; the treated one in twenty-ſix days, the other in ten.

I had four very large ones bit, each by three diſtinct vipers, three times by each. Two were treated, and two not. One of thoſe that were treated died at the end of the ſixth day. The other three were exceedingly ill, and had each of them a large wound in the leg that had been bit.

Two very large dogs were brought to me in excellent order. I had each of them bit in the leg by four well-irritated vipers, each viper biting at leaſt four times. I did not treat them, on account of the difficulty of doing it effectually without the riſk of being bit. Both of them recovered in leſs than ten days. They had wounds, tumour, and lividity, in the part bit. At the end of two days they began to drink, and ate at the end of the third.

Scarcely

Scarcely have animals of any kind, and particularly dogs and cats, been bit by a viper, and are at liberty, when they lie down on the part oppoſite to that which has been bit, and in this ſtate continue very quiet till they recover. Whenever they begin to drink and to eat, 'tis an almoſt certain ſign that they will get the better of their complaints. Cats are leſs deſirous of food than dogs; I have met with ſome that did not eat till after they had been ill ſeveral days.

That the number of my experiments on dogs might be competent to the purpoſe, I procured ſix ſmall ones, of the ſame ſize, ſpecies, &c. I had them all bit in the leg, each by three vipers, and each viper at three bites. Three were treated, and three not. The three firſt died, and only two of the others; the third was exceedingly ill, had a large wound, and did not recover till the end of the fifteenth day.

Not perceiving that the volatile alkali had any good effect againſt the bite of the viper, when given to dogs, I thought it proper to purſue my experiments on other kinds of animals.

Experiments on Cats.

This animal makes a very ſtrong reſiſtance to the bite of the viper. This is not becauſe the venom is innocent to it as it is to ſome other animals, but becauſe it is very hard to kill.

I had

I had a middle-ſized cat bit in the leg by two vipers, each viper biting twice. I did not treat it. Its leg ſwelled, but not violently. It lay reclined on its belly during the whole time of its illneſs; it drank at the end of thirty-ſix hours, ate at the end of fifty-two, and on the fourth day was perfectly recovered.

I had it bit in another leg by three vipers, each viper biting twice. Here again I did not treat it. It vomited ſeveral times after the ſixth hour, and again after the thirtieth. It drank at the expiration of forty-two hours, and ate at the cloſe of the third day. On the fifth day it was recovered.

I made choice of another cat of the ſame ſize as the former one, and had it bit in the leg by four vipers, each viper biting four times. I did not treat it. It ſwelled very much, vomited ſeveral times, and did not eat till the cloſe of the ſixth day.

Two days after I had it bit by four freſh vipers in another leg. It was very much diſeaſed, and had frequent vomitings. It ate at the end of five days, and on the eighth was quite recovered.

I had another cat, larger than the former ones, and very wild, bit by ſix well-enraged vipers, ſeveral times by each. One of them could not let go its hold, and was diſengaged with ſo much difficulty, that its teeth were broken and left in the fleſh. The cat was in a violent rage, but became tranquil on being ſet free. It reclined itſelf on its belly, as the others had done, vomited from time to time, and did not eat any thing till after the fifth day. It continued ill two days more, and at length recovered.

It was quite unneceſſary to give the volatile alkali to the cats, ſince, as we ſee, when they are of a certain ſize, they do not die of the diſeaſe of the venom. Kittens are, however, known to die of it; and it is likewiſe certain, that grown cats would die too, provided they were bit by a greater number of vipers.

The bite of the viper produces a true diſeaſe in this animal, and this diſeaſe is more violent in proportion to the greater number of bites. I cannot, however, preciſely ſay, how many vipers it would require to kill a ſtrong cat of the largeſt ſize. Ten or twelve would, perhaps, be ſcarcely ſufficient.

CHAPTER

CHAPTER IV.

Of the Effects of the Bite of the Viper on different Parts of an Animal.

I HAVE hitherto ſpoken of animals bit by one viper, or by ſeveral, either once or repeatedly, but only in a ſingle part; that is to ſay, in the leg, or in two legs at moſt. We are now to ſee the effects of the bite of the viper on the other parts of an animal. It is eaſy to conceive that the conſequences will be ſomewhat different from thoſe that have already been obſerved, and that there muſt be parts in the ſame animal, more or leſs ſuſceptible of the venom; ſeveral of theſe, on having them bit, have afforded ſingular and unforeſeen appearances.

Experiments on the Skin.

The part of an animal which is firſt pierced by the canine tooth of the viper, and which feels before the others the action of the venom, is the ſkin. I have confined my experiments to the ſkin of guinea-pigs and rabbits, harmleſs animals, that are managed without riſk. I have not employed birds, as their ſkin is too delicate for theſe experiments.

Wounds made in the ſkin may be very ſlight, and altogether external; they may be more or leſs deep; and laſtly, they may pierce the ſkin through and through. I have obſerved all theſe caſes in the courſe of my experiments on the bites of the viper. I have ſometimes ſeen the viper's tooth ſtrike the ſkin ſo obliquely, that it was either not cut at all, or only ſuperficially. The firſt caſe I mentioned, happens frequently, from the viper, when it is enraged, biting at every thing that is preſented to it, in any way, and under any form whatever. The ſecond caſe is much leſs frequent; and that in which the bite is made without piercing the ſkin, ſtill rarer.

Theſe two laſt caſes may happen to man, whoſe ſkin may be more or leſs injured by the canine teeth of the viper.

This reſearch, beſides its being curious, may likewiſe be uſeful in practice, by aſſiſting to make the quality of the venom well underſtood in theſe caſes. Such an inveſtigation, well handled, may likewiſe ſerve, as will be ſeen in the ſequel, to explain the action of the venom of the viper on animals in general.

Superficial Wounds of the Skin.

I ſat out by making the following experiments. I cut the hair with ſciſſars from the ſkin of a part of the leg of a guineapig, and rubbed a portion of this, of about half an inch in length and breadth, ſeveral times, with a ſmall file. The ſkin became red, and an almoſt imperceptible quantity of blood exuded from it, which could not, however, form itſelf into entire drops. Having wiped it well, I poured on it with a large drop of venom, which, to make it flow eaſier, and to extend itſelf over the whole ſurface of the raſped ſkin, I had united with a drop of water.

The animal did not appear to ſuffer in the leaſt, and there was ſcarcely any perceptible mark of cicatrice. On the following day, obſerving it to continue ſound and vigorous, I had it bit twice in the foot by a viper. It died at the end of twenty-four minutes. This experiment I repeated twice, with nearly the ſame reſult; both guineapigs died after being bit.

I ſhaved the hair with a razor, from the external lateral part of a guineapig's leg. The ſkin was red, and a little moiſture exuded from it, which was likewiſe of a reddiſh tinge. I put two drops of venom on this part, the ſizé of which was about two thirds of an inch. The animal did not ſuffer the ſmalleſt inconvenience, and the ſkin dried without eſchar or cicatrice. On having it bit in the foot the next day, it died at the end of twenty-ſix minutes.

I removed the hair with boiling water from a portion of the back of a guineapig, and made two very ſmall, but very deep, inciſions in it, wiping away the blood that flowed from them. I applied two drops of venom, unmixed with water, to the inciſed ſkin, which was eaten away for half its thickneſs, by a wound that formed over the whole furface the venom had touched. This wound difcharged pus, and the next day was covered with an efchar. The animal was perfectly recovered in fix days, and on the feventh, on having it bit by a viper once in the foot, it died at the end of forty minutes.

I repeated this experiment, with the fame circumftances, as nearly as I could judge, on two other guineapigs. The effects were exactly the fame; wound, fkin confumed for half its fubftance, pus, efchar, and recovery. On having them afterwards bit in the foot, they both died in lefs than an hour.

I likewife wifhed to make a fimilar experiment on an animal with a fkin much thicker than that of a guineapig. I chofe a very fmall rabbit, and removed the hair with a razor, in fuch a way, that there was a fenfible difcharge of blood. I applied to this part, about half an inch in length and breadth, two drops of venom. A true wound formed, and the fkin was entirely confumed, and covered with a great deal of pus. The rabbit notwithftanding did not feem to fuffer much, and at the end of feven, days was perfectly recovered. I had it twice bit in its leg by a viper, and it died at the end of fix hours. I repeated the fame experiment on two other rabbits, with the fame fuccefs.

The following conclufions may, I think, be drawn from the above experiments:

I. That the venom of the viper, applied to the fkin of guineapigs and rabbits, flightly fcraped or punctured, is not mortal.

II. That

II. That it produces but a ſlight diſeaſe in the ſkin of guineapigs, and a ſomewhat greater one in that of rabbits.

III. That this diſeaſe is confined to the part of the ſkin touched by the venom.

I was deſirous of making a ſomewhat different experiment on the ſkin of guineapigs, and accordingly removed the hair with ſciſſars from a portion of the back of one of theſe animals, of about the breadth of half an inch. I then made an inciſion with a lancet, ſo as not to puncture it through, but only for about half its thickneſs, and applied two drops of venom. A wound, occupying the whole ſpace covered by the venom, formed, and ſuppurated very abundantly, and the ſkin was entirely conſumed, and covered with a ſcar. The animal did not appear to ſuffer otherwiſe, ate conſtantly, and recovered at the end of ten days.

This laſt experiment ſeems to indicate that when the wounds of the ſkin are deep, the effects of the venom, or its diſeaſe, are more conſiderable, although not mortal; and likewiſe, that the diſeaſe is entirely confined to the ſkin.

Wounds in the Skin, through its whole ſubſtance.

I pinched the ſkin of a ſmall rabbit's leg with my thumb and finger, and pierced it five or ſix times with a viper's tooth, from which the venom flowed. At the end of twelve hours, an encyſted tumour, filled with matter, formed in the ſkin, an inch below the wounds. The cyſt was quite excoriated and bare of hair, and a little moiſture exuded from it. The rabbit died on the fifth day.

I repeated this experiment on a rabbit of the ſame ſize, pricking the ſkin ſeveral times with a venomous tooth. At the end of ten hours, the ſame kind of tumour formed in the ſame place; on the ſecond day the ſkin fell off; on the third the tumour burſt; and the rabbit died four hours after.

I treated two other ſmall rabbits in the ſame way, and the effect was perfectly the ſame. In both of them a tumour formed, and burſt; and both died.

I had the ſkin of a guineapig's back bit repeatedly by a viper, raiſing it with pincers, to prevent the muſcles beneath from being wounded. In leſs than two hours, the part that had been bit became livid, and the animal died at the end of thirty-two hours, without an open wound. The ſkin was gangrened, and the blood black and extravaſated in the adipoſe membrane, as far as the muſcles of the abdomen and breaſt.

I repeated this experiment with the ſame circumſtances on four other guineapigs, all of which died. Neither of them had any wound, but the adipoſe membrane had a gangrenous appearance, and was filled with black extravaſated blood. The extravaſation was extended to the adipoſe membrane which covers the pectoral and abdominal muſcles, and was in ſuch a quantity as to form a bag.

Experiments

Experiments on the Adipose Membrane.

The preceding experiments not only relate to the ſkin, but likewiſe to the adipoſe membrane. Whenever the tooth pierces through the whole ſubſtance of the ſkin, the venom muſt neceſſarily communicate itſelf to this membrane; and its effects, or the diſeaſe it occaſions, will be communicated to both parts. It was therefore neceſſary to have the adipoſe membrane wounded apart, to know what related to the ſkin in the above experiments. It is not very eaſy to do this with preciſion and nicety.

I made an inciſion in the ſkin of a guineapig, near the groin, and introduced a drop of venom without its touching the ſkin. It brought on a tumour of the groin, which increaſed for two days. The third day the animal died. On opening the tumour, I found it filled with a great quantity of black, diſſolved, and extravaſated, blood.

I repeated this experiment on two more guinea-pigs, one of which died, the other did not. This laſt had ſcarcely any tumour. The one that died had a large one; with the ſame ſymptoms as in the preceding experiment. Two days after, I opened the one which ſurvived, and which appeared ſound, and in good health. I found the adipoſe membrane ſomewhat bloody, and with ſome humours extravaſated in it; but all this in a ſlight degree. There was no appearance that could induce one to conclude, that the animal would afterwards have died of the diseafe of the venom. It was vigorous, fed well, and ran about in good health, whilst the other was in the heighth of its diseafe at the end of two hours after being bit.

These experiments still leave us in a doubt whether the venom might not have been communicated to the incised edges of the skin. To obviate this, I fell upon several modes of experimenting, but invariably met with difficulty in the attempts, and something equivocal in the consequences.

After several trials, I pursued the following method:—I cut away a large portion of skin from the back of a guineapig, dried the adipose membrane well, and applied to it two drops of venom. The circular piece of skin I removed was more than an inch in diameter. I spread the venom on the membrane for about three lines in circumference, and at equal distances at all sides from the skin.

In less than six hours, the adipose membrane became black as ink, and at the end of twelve it was covered with an eschar, which continued so long as twenty-two; the animal recovered notwithstanding.

I repeated this experiment on six small rabbits, and six small guineapigs, and the consequences were somewhat different from each other.

In the first place it must be remarked, that neither of these animals died. Six of them were very much diseased, and recovered very late. Four had slight symptoms of the disease of the venom, and seemed to be perfectly recovered at the end of the second day. The others had no certain symptoms

of

of difeafe. I think it may be faid, in a general way, that the venom of the viper is not mortal, if it penetrates no farther than the adipofe membrane.

Experiments on the Mufcles.

I ftripped the exteriour mufcles of a pigeon's leg of the fkin and adipofe membrane, without producing any fenfible hemorrhage. I introduced into one of thefe mufcles a viper's tooth filled with venom. A minute after the pigeon fell forward, and died at the end of ten. The wounded mufcle was extremely livid, throughout almoft the whole of its fubftance.

I repeated this experiment on four other pigeons, all of which in lefs than two minutes fell forward. One died at the end of eleven minutes; another at the end of feventeen; the third in a quarter of an hour; and the fourth not till four hours.

I ftripped feveral mufcles of the leg of a middle-fized rabbit of the fkin and adipofe membrane, and wounded them feveral times with venomous teeth, *(a)* in fuch a way that they entirely entered the mufcles. I wounded them at the parts where there did not appear any confiderable veffels. There was fcarcely any difcharge of blood from the mufcles, which, notwithftanding, very foon became livid at

(a) Thefe are viper's teeth detached from the animal, but ftill adhering to the veficle filled with venom. I have already explained the method I purfue in experiments of this kind.

the places I had wounded. The animal not only ſurvived, but diſcovered no ſigns of ſuffering any great inconvenience; and at the end of fifteen hours there was ſcarcely any diſcoloration in the wounded muſcles. At the end of thirty hours, nothing was to be ſeen but the mechanical wound of the ſkin, where the inciſion had been made to come at each of the muſcles.

I repeated this experiment, with the ſame circumſtances, on another rabbit. The muſcle became diſcoloured, but not much; and the animal, at the end of twenty-three hours, ſeemed to be free from all complaints, except that there ſtill remained a ſolution of continuity in the ſkin.

I entirely ſtripped ſeveral muſcles of a guineapig's leg of the ſkin and adipoſe membrane, and plunged a tooth, charged with venom, betwixt the fibres in ſuch a way, that few or no veſſels were divided. The muſcle became livid, but the animal recovered.

I repeated this experiment on the bared muſcles of ſeveral other animals, ſuch as guineapigs and rabbits, and found that in theſe caſes the venom of the viper does not fail to bring on a complaint, which, although it is frequently very violent, is never mortal.

The Venom of the Viper, when simply applied to the Muscular Fibres, is entirely innocent.

I wished to know what would be the effects of the venom, when simply applied to the muscles, without cutting the fibres.

I stripped the muscles of a pigeon's leg of the skin, and contrived in such a way, that the uncovered fibres and vessels did not bleed sensibly. The experiment succeeded so well, that the muscles, stripped of the adipose membrane, appeared perfectly dry. On these muscles I laid a large drop of venom, observing that it did not communicate itself to the adjacent parts. The pigeon had no complaint, and the wound I had made healed very soon.

I got ready another pigeon in the same way, but took care that the muscles should bleed a little; one vein in particular bled considerably. I applied the venom, and the pigeon died at the end of thirty hours, with a very slight change in the parts that had been wounded.

I repeated this experiment on four other pigeons, the muscles of which did not bleed. Neither of them died, nor seemed to have any other complaint than that occasioned by the incision in the skin.

When we know how small a quantity of venom is capable of killing a pigeon, as it were, instantly, we cannot hesitate to pronounce, that the venom of the

the viper, when ſimply applied to the muſcular fibres, is entirely innocent.

The Venom of the Viper does not loſe its deadly Qualities, even after it has acted on an Animal as a Poiſon.

I was deſirous of ſeeing whether the venom of the viper, after having communicated the diſeaſe to one animal, would act as a poiſon on another. To aſſure myſelf of this, I laid the muſcles of a pigeon's leg bare, and made ſmall inciſions in them, into which I introduced about a drop of venom.

I likewiſe got ready another pigeon, making ſmall inciſions in its muſcles, as I had done in thoſe of the firſt. At the end of four minutes I put the bared muſcles of the two pigeons in contact, and kept them in that ſtate for two minutes. Neither of the pigeons died: the firſt, however, was very ill; the ſecond had ſcarcely any complaint.

I laid the muſcles of two other pigeons bare, and made ſmall inciſions in them. I wounded thoſe of one with a venomous tooth, and at the end of four minutes put them in contact with thoſe of the other, keeping them together in this way for three minutes. The firſt pigeon died at the end of three minutes more; the ſecond at the end of an hour.

I repeated this laſt experiment on two other pigeons. The one that was venomed by the tooth died

died at the end of eight minutes; the other at the end of eighteen.

Consequently the venom of the viper, as in all the cases related above, continues to be such, and does not lose its deadly qualities, when it mixes with the blood of living animals, and excites in them the usual disease.

Animals bit in the Breast.

I had a pigeon bit once by a viper in the breast. I treated it, and it died at the end of ten minutes.

I had another pigeon bit twice in the breast by a viper, and treated it. It died at the end of two hours.

I had six pigeons bit in the breast by as many vipers, each twice by a distinct viper. Three were treated, and three not. They all died; the three that were treated at the end of 10, 20, 50, minutes; the other three at the end of 17, minutes, and 2, 4, hours.

I had six others bit an equal number of times; three in the breast, and three in the leg. They all died; the three bit in the leg at the end of 10, 15, 20, minutes; the three in the breast at the end of 17, 50, minutes, and two hours.

These few experiments on pigeons would lead one to suspect, that bites in the breast are not more dangerous than those of the leg; and that it may even be the reverse. They are however not suffi-

cient

cient in number to admit of any certain consequences being drawn from them.

I had a guineapig bit twice in the breast by a viper, and immediately treated it. It died at the end of two hours.

I had another guineapig, of a much larger size, bit twice in the breast by a viper, and treated. At the part where it was bit, a large wound, which continued open for upwards of fifteen days, formed; the guineapig at length recovered.

I had a very large guineapig bit twice in the breast by a viper, and treated it immediately. It had no symptom of disease. Two days after I had it bit afresh by another viper, in the same place, and at the end of twelve hours it died.

The skin of guineapigs, particularly that part of it which covers the breast, is very tight, in consequence of which the viper finds it very difficult to seize it betwixt its teeth. I was several times deceived by this, supposing the animal bit when it was not; and was therefore obliged to repeat the experiment.

I had a small rabbit bit in the breast by a viper, and immediately treated it. At the end of thirty seconds it fell on its belly, and died in less than a minute.

I had another rabbit, of the same size, bit in the breast, and did not treat it. It had a small wound, and recovered at the end of three days.

I had four rabbits bit in the breast, each twice by a distinct viper. Two were treated, and two not. The

The two that were treated died; one at end of an hour, the other at the end of ten. Of thoſe not treated, one died in an hour, the other had a very ſmall wound in the part bit.

I had a fowl bit twice by a viper in its breaſt, towards the right wing. I treated it, and it died at the end of twenty-four hours.

I had another fowl bit twice by a viper in the ſame place, and did not treat it. It died at the end of nine hours.

I had four other fowls, like the preceding ones, bit, and obſerved the ſame circumſtances. They all four died in eighteen hours.

I had four other fowls bit, two in the breaſt, and two in the leg. The two that were bit in the breaſt died in leſs than ten hours. One of thoſe bit in the leg died at the end of twenty-ſeven hours; the other was violently diſeaſed, but recovered.

Had the number of experiments been greater, we might have deduced from them, that to fowls the bite of the viper in the breaſt is more dangerous than that in the leg. This is contrary to what was obſerved in the rabbits and guineapigs.

Animals bit in the Belly.

I had a rabbit bit twice in the belly by a viper. At the end of eighteen hours a very large tumour formed in the part bit. This tumour encreaſed for four days, and the hair fell from the ſkin, which

was

was corroded and ulcered. The animal, nowithstanding, lived twenty days.

I had another rabbit, of the ſame ſize, bit repeatedly in the belly by a viper. At the end of twelve hours a tumour formed, and the hair and epidermis came away. The tumour was moiſt and bloody, and burſt at the end of eighteen hours, when an ulcer formed, of two inches and an half in length, and more than an inch in breadth. The rabbit ſurvived, but it was more than twenty days before it recovered.

I had two others bit in the belly in the ſame way. Both of them had a tumour, which was ſucceeded by an ulcer that remained open for ſeveral days; and both recovered.

I had two other rabbits of the ſame ſize bit ſeveral times in the belly by two vipers. One died at the end of twenty-ſix hours; the other had a wound which covered the whole of the ſkin of the lower part of the belly, and continued ill twenty-ſix days.

Experiments on the Inteſtines.

I opened the belly of a rabbit, and had the *ileum*, at the diſtance of three inches from the *colon*, bit twice by a viper, binding up the part as well as I could. The rabbit died at the end of ſix hours. The inteſtine was inflamed, black, and contracted, more than ſix inches above and below the part that was bit; ſo that theſe changes had extended to the

colon.

colon. The *mesenterick vessels* were black and swelled, and the blood curdled.

I repeated this experiment on four other rabbits, each of which I had bit in the intestines in the same way by a viper. The result of these experiments was perfectly analogous to that of the former one.

Experiments on the Liver.

Having opened the belly of a rabbit, I wounded the right lobule of the liver, in the inner part, with a venomous tooth. At the end of a few seconds, the creature began to cry and to writhe itself, and died in less than two minutes. All the vessels of the liver were filled with black and clotted blood; the mesentery was in the same state; and the heart and auricles were filled with black, but fluid, blood.

I wounded the outer lobule of the liver of another rabbit in two places with a venomous tooth. The creature drew itself together, but did not cry. It died an hour after.

I introduced a venomous tooth into the outer lobule of the liver of a third rabbit, and did not withdraw it immediately. This one cried, writhed itself, and died in less than a minute and an half. The blood was coagulated both in the liver and mesentery.

I introduced a venomous tooth in the usual way into the inner lobule of the liver of two other rabbits, and kept it there for some time. These animals, as usual, cried out after a few seconds, and died in less than two minutes. The blood in the liver was black and coagulated; that in the heart and auricles was likewise black, but in a fluid state. I did the same to the outer lobule of the liver of two rabbits, but withdrew the venomous tooth immediately after having introduced it. One began to cry and writhe itself after a few seconds, and died in two minutes. The other lived nearly two hours. The blood in the liver of the first was quite coagulated; as it was in a degree in the second. In the former, the blood in the auricles and ventricles was fluid; in the latter it was coagulated.

Experiments on the Ears.

I had the ear of a middle-sized rabbit, towards its extremity, or point, bit twice by a viper. The part was a little swelled at the end of six hours; the rabbit, however, ate, and was lively. At the end of four days it was perfectly recovered.

I had two other middle-sized rabbits bit in the same way at the extremity of the ear, each twice by a distinct viper. The ears swelled a good deal, but the rabbits ate, and were lively. At the end of five days they were both recovered.

I had another rabbit bit in the right ear, towards its extremity, twice by a viper. I treated the part, in which there was a considerable swelling that did not subside till after sixteen days.

I had

I had a rabbit's ear bit twice by a viper, at a third of its length from the basis. At every hole made by the teeth in the opposite sides of the ear, a drop of blood appeared, and beside it a small drop of venom, which, although it was in contact with the blood, did not unite with it in the least. There were four holes made by the teeth at each side the ear, so that the small drops of venom were eight in number. The ear swelled a good deal, and the swelling did not subside till after twenty days.

There is no difficulty in accounting for the small drops of venom that appeared at the opposite sides of the ear. We know that the venom flows from the point of the tooth. The ear of a middle-sized rabbit is not so thick as the viper's tooth is long, which must of course pierce the ear through. When the viper withdraws the tooth, the venom has already reached the point of it; and from the elasticity of the skin of the ear, which closes the hole it went out at, is forced to shed itself at the sides of it. In finding its way to the part of the ear at which it entered, the tooth in the same way leaves the venom, which it continues to shed, at the edges of the hole at this side. I have since observed these small drops of venom on each side the ear in almost all the rabbits I have had bit in this part, and find them in general to be larger at the part the tooth went out at, than at that where it entered; particularly if the viper is prevented from withdrawing its teeth too suddenly.

I had a rabbit bit in both ears at a third of their length from the baſis. Each ear was bit three times by a diſtinct viper, and both of them ſwelled violently, for nearly eight lines towards the baſis. The rabbit was very much diſordered, and did not eat for ſeveral days, when it began to feed ſparingly. It was not perfectly recovered till the end of twenty days, and was then very much waſted.

I had two others bit repeatedly at the ſame part of the ear, by two vipers. At the end of the ſecond day, the ears were disfigured by a ſwelling, which became ſo large, that in two days more they hung down on each ſide the neck. One of the rabbits died, at the end of eight days, with its ears ulcered and ſphacelated; the other recovered, but not till the end of twenty-eight days.

I had a middle-ſized rabbit bit once in the ear by a viper. The ear bled a little, and two ſmall drops of venom appeared at the ſides of the two holes made by the teeth. I did not treat it. There was a degree of tumour and inflammation in the part, and at the end of thirty hours the rabbit was perfectly recovered.

I had another rabbit bit, of the ſame ſize as the preceding one. I treated it immediately, and made it ſwallow the volatile alkali. The ear ſwelled exceedingly, and became livid at the part where it was moſt ſwelled. The tumour continued ſix days, and in four more the animal recovered.

I had four rabbits bit in the ears by as many vipers. Two were treated, and two not. Neither of them

them died. The ears in all of them ſwelled conſiderably, and they all recovered at the end of three days.

Having aſſured myſelf that the bite of the viper in the ears of rabbits is not very dangerous, I thought of having theſe animals bit by ſeveral vipers, in different parts of the two ears. For this purpoſe I choſe a dozen middle-ſized rabbits, and had them all bit repeatedly in ſeveral parts of each ear, and each by three vipers. The parts ſwelled exceedingly, and continued in that ſtate for upwards of twelve days. Three of the rabbits had an enormous bag or tumour in the fore part of the neck, larger than the head itſelf. Theſe tumours were filled with a humour, and yielded to preſſure. At the end of two days they increaſed in ſize, and the ears became ulcerous. The rabbits recovered in ſixteen days.

Experiments on the Pericranium.

I laid bare the pericranium of a pigeon, by removing a good portion of the ſkin, and made ſeveral ſmall inciſions into it with the point of a lancet. I poured venom upon it, but in ſuch a way that it did not reach to the adjacent parts of the integuments that had been cut. The pigeon did not ſeem to be at all diſordered by it, and recovered in the ſame ſpace of time as another did, which I had prepared by way of compariſon, and to the pericranium of which I had not applied the venom.

I repeated this experiment on four other pigeons, with the fame fuccefs. Neither of them died, and in neither was the attack of the difeafe of the venom perceptible.

On the Bones and Periofteum.

I laid bare the cranium of a pigeon, ftripping off a good part of the pericranium. I made fmall wounds in the cranium with a lancet, taking care not to pierce the whole fubftance of it, and introduced a confiderable quantity of venom, preventing it as ufual from communicating to the neighbouring parts. The animal not only furvived, but did not appear to have fuffered the fmalleft inconvenience.

The confequences of three experiments on pigeons, treated in the fame way, were the fame.

Having laid bare the *tibia* of two pigeons, and freed it well of the cellular membrane, I wounded both perioftæum and bone in feveral places with the point of a needle, and poured the venom copioufly upon them. The pigeons had not the fmalleft perceptible complaint, and recovered in the fame time as two others did, that I had treated in the fame way, but without applying the venom, to ferve as a comparifon.

I repeated this experiment with the fame circumftances on two other pigeons, and the refult was exactly the fame. Neither of them died, nor had the fmalleft fymptom of the difeafe of the venom.

I laid

I laid bare the perioſtæum of the *tibia* of ſix other pigeons, and having pierced it in ſeveral places with a needle, moiſtened it with venom. Neither of the pigeons died, nor had any complaint.

Dura Mater and Brain.

I removed a portion of the cranium of a pigeon, taking care to lacerate the dura mater as little as poſſible. I wiped this membrane, which was well expoſed, as gently as I could, with dry lint, and applied to it a drop of venom. The pigeon had no ſymptom of the diſeaſe of the venom, and recovered within the ſame ſpace of time as another did, that I had prepared in the ſame way, but without applying the venom, as a compariſon.

This experiment, on two other pigeons treated as above, terminated in the ſame way.

I removed a portion of the cranium of a pigeon, and made inciſions in the dura mater all round, introducing at one of the apertures a drop of venom The pigeon recovered, without having had any ſymptom of the diſeaſe of the venom.

After having removed the dura mater of another pigeon, I made a ſlight inciſion into the brain, and introduced the venom. The animal recovered in the ſame way with the preceding one.

A third pigeon, on which I made the ſame trial, died at the end of four hours.

Marrow of the Bones.

I cut the *tibia* in two pigeons, towards the lower extremity, and introduced lengthways into the marrow two ſmall bits of wood covered with venom. Neither of the pigeons died, nor had any ſymptom of diſeaſe.

I cut the *tibia* in the ſame part, of two other pigeons, and introduced into the marrow two ſmall bits of wood, well covered with venom, keeping them there ſix minutes. Neither of the pigeons had any apparent ſymptom of the diſeaſe of the venom.

I repeated this experiment with the ſame circumſtances on four other pigeons. Each of the trials ended the ſame way, the pigeons all recovering within the ſame ſpace of time that two others did, which I employed as a compariſon, without venoming them.

The Venom applied to the Tranſparent Cornea.

I pierced the tranſparent cornea of the right eye of a large rabbit with a venomous tooth. The aqueous humour flowed out. I then, with another venomous tooth, firſt ſcratched, and afterwards pierced, the tranſparent cornea of the other eye. At the end of an hour I found the right eye filled with the aqueous humour, and perfectly ſound. At the end

end of eighteen hours, a ſmall white ſpot formed in the tranſparent cornea of the other eye, but without any inflammation about it. At the end of three days, the white ſpot in the left eye was raiſed above the cornea.

I ſcratched the cornea of another rabbit with a tooth well dried, and at length pierced it. At the end of fourteen hours a dark ſpot appeared, and two days after the cornea was raiſed up in the form of a pearl.

I poured a drop of venom into the eye of a large rabbit, which I examined every hour. At the end of eighteen hours, the *membrana nictitans* ſeemed ſomewhat redder than uſual.

I poured two drops of venom into the eye of another rabbit, and this was not ſucceeded by any inflammation.

I made the ſame experiment on the eye of a third, which continued in its natural ſtate.

I repeated it on three other rabbits, neither of the eyes of which became ſenſibly inflamed.

I moiſtened the eyes of a large rabbit ſeveral times with a conſiderable quantity of venom, and likewiſe applied ſeveral drops to its lips and tongue. At the end of three hours the *membrana nictitans* appeared a little red, but at the end of eighteen hours returned to its natural ſtate.

I put ſeveral drops of venom on the tongue of another rabbit, and ſmeared it on the lips and palate with a bruſh. There was no ſwelling in any part

part of its mouth, neither was the rabbit at all disordered.

This experiment repeated on two other rabbits was attended with the ſame conſequences. No part of the mouth was either ſwelled or inflamed.

CHAPTER V.

Experiments on the Comb, Gills, Noſe, and Neck, of Animals.

My next purſuit was that of examining the effects of the venom of the viper on the comb, gills, noſe, and neck of animals. My experiments on theſe parts have been attended with unexpected and intereſting conſequences; and therefore I have thought it proper to treat them apart, in an extenſive way.

Experiments on the Comb of Fowls.

I had the comb of a fowl bit twice by a viper. There was a conſiderable hemorrhage from the wounds made by the teeth. At the end of three hours the gills were ſwelled, and in ſix a large tumour or bladder was formed. The fowl died at the end of four days, without having either eat or drunk.

The

The tumour of the gills, which united them into one monſtrous body, was filled with a watry fleſh-coloured humour, and with an heap or web of filaments and veſſels.

I had a ſmall cock bit once in the comb by a viper, and treated it immediately. It died at the end of ten minutes.

I had another cock, of the ſame ſize, bit once in the comb by a viper, and treated it. At the end of two hours both gills had already ſwelled; at the end of twenty-two this ſwelling was very much abated; and in thirty-ſix there were only ſome little remains of ſwelling in one of them. In forty hours the cock was perfectly recovered.

I had the comb of a large cock bit three times by a viper. It was branched, pointed, and more than a third of an inch in thickneſs. It bled a little, and there were ſome ſmall drops of venom beſide the holes made by the teeth. I made a ſmall wound in the comb with the point of a lancet, and introduced a ſmall quantity of venom. The cock had no ſymptom of complaint. Two days after I had it bit twice in the comb by another viper. At the end of two hours the part appeared ſomewhat livid towards its baſis, and perhaps a little ſwelled. At the end of three hours the gills were very much enlarged, and at the expiration of twenty, were become of a monſtrous ſize, and livid for their whole extent. At the end of twenty-three hours they burſt, and the cock died very ſoon after.

There

There cannot be the ſmalleſt doubt but that the venom in the firſt caſe was thrown out by the blood, and this happens not unfrequently. It is much more difficult to account for the tumour that, notwithſtanding the cock was bit in the comb, formed in the gills. However I have frequently ſeen ſomething ſimilar happen in other animals. The bite made in the leg of a rabbit frequently cauſes a tumour, or an obſtruction of the humours in the moſt inferior parts of the ſame leg. But the experiments muſt be continued.

I had the comb of a fowl bit by two vipers, by each twice. At the end of two hours one of the gills only began to ſwell. In twenty they were both very much ſwelled, and united in ſuch a way that they formed a ſingle body. At the end of thirty-ſix they were enormouſly ſwelled and very livid. In ten days the fowl recovered. On the fourth day of the diſeaſe it reſpired with difficulty, and with a hiſſing noiſe; the glottis was inflamed and open, and the trachea arteria ſwelled.

I had the comb of another fowl bit ſeveral times by two vipers. At the end of three minutes the part next the head was livid, and appeared a little ſwelled. In an hour the livid colour and tumefaction ſeemed to be ſubſided, but on the other hand, the gills were enlarged. In three hours one of the lower eye-lids exuded blood from all its ſmall orifices. The gullet and palate were black. In twelve hours the fowl was in a dying ſtate, the gills being livid

livid and of an enormous ſize. It died at the end of thirty-three hours.

I had the comb of a fowl bit ſeveral times by a viper. One of its gills ſwelled a little. At the end of thirty-ſix hours this ſmall degree of ſwelling had diſappeared, but the fowl reſpired with difficulty, and in doing this made a great noiſe. The wind-pipe was ſwelled, and very much enflamed, even at the end of ſix days. The animal was perfectly recovered in ten.

All theſe experiments ſhow that there is an immediate communication of veſſels and humours, betwixt the comb and gills of fowls. I do not give a detail of more than ten experiments beſides that I made on fowls, ſince they terminated in the way with the caſes juſt related.

Experiments on the Gills of Fowls.

I was deſirous of knowing what would be the conſequence of having fowls bit by vipers, not in the comb, but in the gills only; that is to ſay, whether the bite would be equally dangerous, and whether the tumour would fly up to the comb without forming in the gills, or would form both in the gills and comb.

I had the gills of a fowl bit repeatedly by two vipers. At the end of two minutes they had already ſwelled, and become livid. There was a great flux of humours in the eyes, which were cloſed clofed by the enlargement of the membrana nictitans. In lefs than an hour the gills were of an enormous fize, and livid all over. The fowl died at the end of the fifth hour.

I had the gills of a fecond fowl bit twice by a viper. They fwelled in lefs than four minutes, and in two hours were extremely large and livid. The comb appeared a little dark towards its points and edges. The fowl died at the end of three days.

Thefe trials may induce one to fufpect that wounds made in the comb are lefs dangerous than thofe made with the fame circumftances in the gills.

To come at the truth of this hypothefis, I made the following experiments. I had fix fowls bit, each twice by a diftinct viper; three in the comb, and three in the gills. One only of the former died, and two of the latter.

On repeating this experiment on fix other fowls, the refult was fomewhat different. Only one of thofe bit in the comb died, and all thofe that were bit in the gills.

Thefe new experiments led me to think that my conjecture was very probable; that is to fay, that the bite of the viper in fowls is more dangerous when made in the gills, than when it was made in the comb.

The accident which fupervenes in the fowls the comb of which has been bit by vipers, is very fingular. The action of the venom, or its difeafe, is conveyed

conveyed to a remote part that has not been bit; but when the gills are bit, the venom does not fly up to the comb, nor does the diseafe communicate itself to that part, and yet the ftructure both of the gills and comb is the fame, and the veffels and nerves are common to both.

This circumftance ftruck me fo forcibly, that I thought it deferving an analyfis of fome kind, and of being fearched into by ftill further trials.

I began by having the comb of a fowl bit once, and at the end of fifteen feconds cut off both gills. The fowl not only recovered, but there was no change in the gills, nor any appearance nor fymptom whatever of the difeafe of the venom.

I had another fowl bit once in the comb, and at the end of fifteen feconds cut it entirely away. The gills did not fwell, neither had the fowl any fymptom of the difeafe of the venom.

I had the gills of a large cock bit repeatedly by a viper. In fix hours they were both very much enlarged. On the following day they were ftill more fo, and were befides livid. The cock recovered at the end of thirteen days.

I had the gills of another cock, a very large one, bit feveral times by two vipers. At the end of ten minutes I cut them off. On the following day it ate, and appeared in health, and after three days was perfectly recovered.

I repeated this experiment on the gills of fix other cocks, each of which I had bit repeatedly by two diſtinct vipers. I cut off the gills in all of them, but at different intervals; at the end of 1, 2, 4, 8, 16, 32, minutes. They all recovered, and had no other complaint, than that produced by the cutting off of the parts.

I had a large cock bit ſeveral times in the comb by two vipers, and after eight minutes cut off its gills. It died at the end of three hours.

I had another cock, a very large one, bit repeatedly in the comb by two vipers, and after four minutes cut off its gills. It died at the end of twenty-ſeven minutes. It was ſcarcely bit by the firſt viper, when it could no longer ſupport itſelf, or hold its head erect. It opened its beak, from which a glutinous humour flowed, and breathed ſhort, and with difficulty.

I repeated this experiment on ſix other cocks, each of which I had repeatedly bit in the comb by two diſtinct vipers. I cut off the gills in each of them at the end of four minutes. Three died in leſs than twenty hours; the other three were very much diſeaſed, and did not recover till the end of ten days.

Experiments on the Neck of Animals.

I had a ſmall guineapig bit twice by a viper in the back part of its neck. I treated it. It died at the end of forty minutes.

I had a middle-ſized rabbit bit twice by a viper in the upper part of the neck, and treated it. It died at the end of twenty-four hours.

I had two guineapigs bit in the neck, each twice by a diſtinct viper. One was treated, and the other not. Both of them died; the one treated at the end of an hour, the other at the end of four.

I had two ſmall rabbits bit in the neck, each repeatedly by a diſtinct viper. I treated one, and made it ſwallow the volatile alkali ſeveral times, and did nothing to the other. Both of them died; the firſt at the end of four hours, and the other at the end of twenty-two.

I had a large guineapig bit twice in the neck by a viper. In an hour the part of the neck that had been bit became ſwelled and livid. At the end of twenty-three hours a large wound appeared. At the end of the ſecond day the humours which formed the tumour, had extended to beneath the chin, and formed a large bag or bladder. In four days the tumour had ſwelled to ſuch a degree, that it almoſt covered the breaſt. The ſkin had loſt its hair and epidermis, and a ſlightly-coloured humour exuded from it. At the end of ſix days the ſwelling began to diminiſh, and the guineapig recovered at the end of fifteen.

The diſeaſe in this animal, or the matter which deſcended from the upper to the lower part of its neck, and which even reached to the breaſt where it formed a cyſt or bladder, bears a ſtrong analogy to the circumſtances that were obſerved on having

 the

the fowls bit. There is only this difference, that in fowls this effect is more frequent, and is oftener the caſe than otherwiſe; whilſt, on the contrary, it happens very rarely in quadrupeds bit in the neck, at leaſt in guineapigs. Of twenty-two animals bit in the ſame way, of which eleven were treated and eleven not, I found five in which this tumour deſcended below the neck, and formed a bladder. Of theſe five, three were treated, and two not. The number of deaths, which conſiſted of four in the whole, was equal on both ſides.

It is however certain, that having had ſome others bit, but each of them by ſeveral vipers, and ſeveral times by each, the tumour or cyſt formed in the interiour part, in a greater number of them, and that almoſt all of them died.

The conſequences were analogous, on trying the ſame experiments on rabbits. The cyſt ſometimes forms beneath the chin of theſe animals, although they have only been bit in the neck; and this happens more frequently when they have been bit by ſeveral vipers, in which caſe they die much readier.

Experiments on the Noſe of Animals.

It remained for me to examine the bite of the viper on a part that is held the moſt ſenſible, and the moſt likely to occaſion death when it receives an injury, in ſome particular animals.—This is the noſe.

nose. It appears, that the cat, an animal very obstinate in dying, perishes as readily as others, if struck in this part.

Mead reckoned it so sensible and so dangerous in dogs, that, wishing to assure himself of the efficacy of a remedy against the bite of the viper, he had a dog bit on the nose, and applied the remedy. The dog lived; and this was sufficient to give the remedy the reputation of a true specifick; so strong was the opinion, that a bite of the viper on the nose was mortal.

I shall not relate here all the experiments I made on this part, but only a small number of them, which will be sufficient to give a clear idea of the fallacy of some popular opinions.

We shall see what we ought to think of the bite of the viper on the nose, and how absolutely necessary it is to consult nature by facts and experiments. Nothing is more dangerous and more uncertain in researches of this nature, than a vague analogy, or a seducing and probable reasoning. Nature is not to be divined, and *prophets* in the science of physicks are not to be believed.

I had a small rabbit bit twice on the nose by a viper. In two minutes the part was sensibly inflamed. In three hours a tumour was formed in the neck, beneath the chin. In seven hours this tumour was become very large.—The animal recovered however.

I had another rabbit, somewhat larger than the former one, bit on the nose by a viper, and treated

 it.

it. It was bit twice, but one of the bites was made on the upper lip, at the ſide of the noſe. In the ſpace of two minutes the noſe was ſwelled, and a very large tumour formed under the chin. At the end of twenty hours this tumour burſt, and diſcharged a great deal of matter. The rabbit recovered at the end of ſix days.

I had a third rabbit, of a middle ſize, bit twice on the noſe by a viper. In a very little time the part ſwelled and inflamed. In two hours a tumour formed beneath the chin, which at the end of ſeven diſcharged blood, and was very large. In thirty-ſix hours the tumour and ſkin began to dry, and the animal recovered at the end of the ſixth day.

Six other rabbits were bit in the ſame way. Neither of them died, and the effects of the bites were pretty much the ſame as thoſe related above. The bite of the viper on the noſe of rabbits, contrary to what one would naturally have thought, ſeems to be leſs dangerous than that in other parts. The diſeaſe it produces, as to the ſeat of it, is very ſimilar to that in the comb of fowls. Here again a tumour forms, in a part where the animal has not been bit, and beneath the ſeat of the bite, in which, in moſt caſes, the venom ſcarcely occaſions a real and ſenſible complaint. The only eſſential difference is, that the tumour in rabbits is of a greater extent, reaching ſometimes to the middle of the breaſt.

We are now to ſee, whether the ſame thing happens in animals of other ſpecies'.

I had

I had a large guineapig bit on the noſe by a viper. In two hours the part was very much ſwelled; at the end of three the ſwelling was diminiſhed, but in its place a large tumour formed beneath the chin. At the end of fifteen hours, the tumour broke, and diſcharged a great deal of blood and ſerum. In thirty-ſix hours the diſcharge ceaſed, and the opening in the ſkin dried up. The animal was perfectly recovered at the end of four days. It was never very much diſordered, ſince it ate during the whole time of its illneſs.

I had another large guineapig bit twice on the noſe by a viper. The noſe and mouth ſwelled very much, but this ſwelling diminiſhed in proportion as a tumour formed under the chin. After twenty-two hours the tumour, which had broke an hour before, began to dry up, and at the end of thirty-ſix ſeemed perfectly dry. At the end of two days the animal was recovered. During the whole courſe of its complaint it ſuffered but little, and ate conſtantly.

I had a large guineapig bit on the mouth by two vipers, each biting twice. The noſe ſwelled in leſs than three minutes, and ſtill more ſo at the end of ten. Two hours after, a tumour formed beneath the chin, when the ſwelling of the noſe diminiſhed, and in a ſhort time entirely ſubſided. At the end of twenty-three hours the tumour beneath the chin was ſo large as almoſt to cover the breaſt, and in two hours more it burſt. In the ſpace of five other hours the animal recovered.

I repeated this experiment on another large guineapig, which I had bit by three vipers, each viper biting three times. The nofe and mouth both fwelled very much, but continued in that ftate only four hours. At the end of the fecond hour a large tumour appeared under the chin, which in twenty-three hours was become enormous, and reached to the breaft. This tumour broke at the end of thirty hours, but the animal was not perfectly recovered till the eighth day. The bones of the nofe were laid bare, and the furrounding fkin all confumed.

I made the fame experiment on two other guinea-pigs, but fmall ones. One died at the end of twelve hours; the other had the ufual tumour, was exceedingly ill, but did not die.

The bite of the viper on the nofe feems to produce pretty much the fame effects on guineapigs as on rabbits; and it appears that the venom is lefs dangerous in this part than in any other. The fame effects are here conftantly obferved as to the feat of the difeafe; but are they the fame in all other animals?—I fhall relate what I met with in dogs and cats, creatures that enter into the plan of my prefent refearches, and this will fhow how little analogy alone ought to be trufted, and that the fame caufe produces very different effects, on fimply changing fome circumftance, which one would not have fuppofed capable of influencing a great deal.

I had a fmall dog bit repeatedly on the nofe by two vipers. Both nofe and mouth fwelled, and the dog

dog died at the end of eight hours, without any ſymptom of diſeaſe in any other part.

I had another dog, twice as large as the preceding one, bit repeatedly on the noſe by two vipers. Its mouth ſwelled to ſuch a degree, that the lips were very much enlarged twelve hours after. It vomited ſeveral times, and continued ill for three days, when it began to drink. On the fourth it ate, and on the fifth was perfectly recovered.

I had another dog, ſtill larger than the one laſt mentioned, bit on the noſe by three vipers, each viper making three bites. In a little time its noſe, mouth, and lips, were ſwelled ſo as to become hideous. It vomited a great many times, ate and drank on the fourth day, and recovered on the fifth.

I had another dog, of the ſame ſize as the preceding one, bit on the noſe by four vipers, each biting three or four times. It had a bite at the ſide of its noſe, and another on one of its lips. It vomited frequently, neither ate nor drink till after the third day, and recovered on the fifth.

I had another large dog bit on the noſe by ſix vipers, each of which bit three or four times. Its noſe and mouth ſwelled enormouſly; it vomited a great many times, ate after the fourth day, and recovered on the ſixth.

Laſtly, I had another dog, of the ſame ſize as the three preceding ones, bit on the noſe by ſix vipers, by each three or four times. The part ſwelled violently, and the animal did not eat till after the fifth day. It vomited frequently, and recovered at the end of feven days.

Rabbits and guineapigs bit on the nofe, ufually have the difeafe beneath the chin, and not in the part bit. It is quite the contrary with dogs, in which the difeafe is entirely confined to the nofe, the part that received the bite. They therefore form an exception to the cafes related prior to theirs.

It is likewife fingular, that as the action of the venom is confined to the nofe, it does not produce incurable wounds and gangrenes in that part. We, however, find it to be quite otherwife;—bites on the nofe in dogs are very rarely attended with a wound in the part, and the animal not only makes a ftrong refiftance to the difeafe, but the latter at the fame time appears to be very flight, fince the recovery takes place in a few days.

Experiments on Cats bit on the Nofe.

We have feen above, that the cat makes the ftrongeft refiftance of any animal to the bite of the viper, although the venom conftantly produces in it a difeafe. We may therefore conjecture, that this bite on the nofe of cats will not be productive of mortal effects. But we know, on the other hand, that mechanical percuffions on the nofe are dangerous

dangerous to these animals, and that they soon die, if they fall from a height on this part.

From these considerations, I wished here again to have recourse to experiments, which can alone determine the truth.

I had a middle-sized cat bit repeatedly on the nose by a viper. Its mouth swelled for a considerable extent. It ate on the second day, and recovered on the third.

I had another, of the same size, bit repeatedly on the nose by a viper. A few minutes after, the part swelled. The cat vomited twice, ate on the second day, and was perfectly recovered on the third.

In this second cat, the disease of the venom was so very slight, that the animal appeared to suffer but little during its continuance.

I had a third cat bit repeatedly on the mouth by a viper. One of the bites fell on its upper lip, which bled a good deal, and the whole of its mouth swelled very violently; however it ate on the second day, and on the third was recovered.

I had a large cat bit repeatedly on the nose by a viper. The part bled very much, and swelled a few minutes after. At the end of twenty hours it was still more swelled, notwithstanding which, the cat appeared but little disordered. It recovered at the end of forty hours.

I had another cat, of a middle size, bit repeatedly on the nose by a viper, which likewise bit it on the mouth and lips. The mouth swelled at the end of five minutes, and at the end of five hours the

the cat vomited ſeveral times. In thirty-ſix hours it was perfectly recovered.

I had another cat, of a middle ſize, bit on the noſe, and on the mouth both below and above. After ſeven hours it vomited ſeveral times. Its noſe and mouth were but little ſwelled, and at the end of twenty hours it recovered.

Another cat, of a middle ſize, was bit by three vipers, each of which bit three times, or upwards, on the noſe, mouth, and even within the palate, from which there was an hemorrhage. Some minutes after, its mouth ſwelled, it vomited ſeveral times, but the palate did not ſwell at all. It ate at the end of three days, and at the end of the fifth was perfectly recovered.

I had another cat, ſomewhat larger than the preceding one, bit by four vipers. Each viper bit ſeveral times, on the noſe, mouth, and lips, and in the palate, inſomuch that the cat, feeling one of the bites within its mouth very ſenſibly, ſeized the viper betwixt its teeth, and almoſt ſevered its head from its body. The noſe and mouth in this cat ſwelled very much, it vomited ſeveral times, ate on the fourth day, and recovered on the ſixth.

I repeated theſe experiments on three other cats, which I had bit repeatedly in the noſe by a viper, and the effects were pretty much the ſame. We may therefore, I think, conclude, that the bite of the viper on the noſe is not very dangerous to dogs, and that it is ſtill leſs ſo to cats.

It

It is, however, very ſtrange, that in both theſe animals there is no tumour beneath the chin, and that the local diſeaſe is confined to the part bit; whilſt, on the contrary, the diſeaſe in rabbits and guineapigs is not in the part bit, but in another part of the animal beneath it.

It is clear, that this difference can only depend on the different organization and nature of theſe animals; and it is preciſely this diverſity that we are ignorant of.

I muſt here obviate a difficulty that may be made by thoſe who are not accuſtomed to ſuch experiments.

Theſe may oppoſe, that bites in the noſe probably become leſs dangerous from the animal's licking the part. This is never done by rabbits and guineapigs, notwithſtanding they are bit. I have aſſured myſelf of this particular in ſuch a way, that I have not the ſmalleſt ſuſpicion of having been deceived.

More than two-thirds of both dogs and cats that I had bit in the noſe, never licked the part, although they could eaſily have done it. I obſerved them myſelf, and had them obſerved, for whole hours. It is true, that thoſe which bled a good deal licked themſelves if they could; but it was evident, on obſerving them, that they only endeavoured with the tongue, to free themſelves from the blood which tickled them in flowing down, and that this was no ſooner effected, which happens in a moment, than they ceaſed to do ſo.—In the

expe-

experiments I made on dogs and cats, I prevented ſome of them, when they bled at the noſe, from licking the part, and ſuffered others to do it. The diſeaſe was the ſame in all; and it is, therefore, certain, that ſimply licking the noſe, whether in dog or cat, does not at all diminiſh the effects of the venom of the viper on that part.

CHAPTER VI.

Experiments on the Tendons.

SEVERAL modern phyſiologiſts have thought that the tendons are not endued with ſenſation. It is certain that it has not yet been proved clearly, that a tendon receives nerves, either from the muſcle, or from the tunica vaginalis which covers it. Neither is it more apparent that it has blood-veſſels, at leaſt in any number, and ſenſible ones. It is therefore natural to ſuſpect, that the bite of the viper on a tendon cannot be of any great conſequence, and that the venom cannot act on this part. I wiſhed neverthelefs to conſult experiment once more on this point.

In having the tendons bit by vipers, I was more than once on the point of being deceived; and if I had

had not multiplied my experiments, and varied them in ſeveral ways, as I did, I ſhould certainly have been ſo. I ſhall be circumſtantial in relating ſome of the trials I made on the tendons, to ſhow that it is eaſy for any one, even for an obſerver, to be deceived, if he only follows ſimple experiments, ſince the reſult of them may vary, although there appears to be no variety in the circumſtances with which they are made.

My experiments were made on rabbits, of which I employed the largeſt I could find, ſome of them weighing ten pounds and upwards.

Having removed the ſkin from the *tendo achillis* of a rabbit, and perfectly ſtripped it of its tunick, for a ſpace of ſix lines in length, I paſſed under it ſeveral folds of fine linen, to prevent the venom from communicating to any other part. I wounded the tendon in ſeveral places with a venomous tooth, and afterwards covered it with bits of linen in ſuch a way, that it did not ſeem poſſible for the poiſon to communicate to the neighbouring parts. The rabbit died at the end of thirty-ſix hours. The tendon was livid throughout its whole ſubſtance, but the parts about it were not ſenſibly changed.

I opened by an inciſion, the ſkin that covers the *tendines achillis* of another rabbit, and ſtripped the tunick from both. The tendons were ſmooth, ſilver-coloured, and free from veſſels. I paſſed ſeveral folds of linen beneath them, and had them bit ſeveral times by two vipers, covering them with linen, in ſuch a way, that the venom could not glide elſewhere.

where. The rabbit died at the end of thirty-eight hours. The blood in the auricles, in the ventricles, and in the large veſſels of the lungs, was black and coagulated. There were ſeveral livid ſpots in the lungs. The muſcles adjacent to the tendons were a little inflamed, and likewiſe had livid ſpots in ſeveral places.

I repeated this experiment on two other rabbits, with pretty much the ſame reſult. Both of them were dead at the end of thirty-ſeven hours.

Although it clearly reſults from the experiments I have juſt related, that rabbits die after having been bit in the *tendo achillis* by vipers, I could not however conceive, that the death of thoſe I have mentioned, was occaſioned by the introduction of the venom, and its ſubſequent diſeaſe.

It did not appear poſſible to me, that a part endued with ſo few vital principles as the tendon, which is not at all ſenſible, and which may be cut both in men and animals with impunity, could be ſuſceptible of the action of a venom that has no influence either on the mouth or ſtomach. I ſuſpected that theſe animals died from ſome other cauſe or circumſtance I could not diſcover.

In conſequence of this ſuſpicion I determined to multiply my experiments, and to diverſify them as the caſe might require.

Having removed the ſkin from the *tendo achillis* of a rabbit, and ſtripped it of its tunick above and below, ſo that it appeared ſmooth and white, I wounded it with the point of a large and ſharp needle;

dle, which pierced it through. The needle was well covered with venom, and I had put ſeveral folds of linen beneath the tendon. I wiped the tendon ſeveral times, removed the linen, and left the part expoſed. I then introduced into the aperture made in the tendon, a bit of wood well covered with venom, and having withdrawn it, poured in a drop of pure venom. At the end of twenty-four hours, the tendon ſeemed diſcoloured at the part wounded. The rabbit, however, ate conſtantly, and at the end of fifteen days was recovered.

In another rabbit, I removed a large portion of the ſkin that covers the joint of the knee, and ſtripped the ligament that binds this part of the adipoſe membrane. I wounded it obliquely with a venomous tooth, in eight places, at each of which a ſmall drop of venom appeared. I made ſmall inciſions into each puncture, with the point of a lancet, which penetrated into the ſubſtance of the ligament without piercing it through, and conveyed the venom within. The rabbit recovered in eight days, and ſeemed not to have had any internal complaint. It ate conſtantly, and continued lively and active.

Having ſtripped the *tendo achillis* of another rabbit of its tunick, and put folds of linen beneath it as uſual, I had it bit ſeveral times by two vipers. I then covered it with linen, and removed that which was beneath. The rabbit for the firſt few days ſeemed to have no complaint, but the wound in the tendon never healed perfectly. At the end of ten

days

days its belly appeared to ſwell, and, on its dying at the end of ten days more, I found it to be dropſical.

Theſe experiments ſeem to oppoſe the former ones, and to render it a matter of doubt whether the bite of the viper on a tendon produces the diſeaſe of the venom, or not. The latter caſes ſeem to indicate that it does not; but they are contradicted by the former ones. Now, as one of the principal reſearches I propoſed to myſelf to make, at ſetting out on my experiments, was to diſcover what are the parts acted on by the venom of the viper, and to obſerve its different effects on the different parts of an animal, I was determined to continue my experiments on the tendons with a degree of obſtinacy, and to ſee if I could ſucceed in clearing up this point.

Wiſhing to obſerve a greater degree of preciſion in my experiments, and ſuſpecting that the venom might perhaps communicate to the neighbouring parts in which the inciſion had been made, and that it might penetrate by degrees through the linen, how much ſoever the latter might have been folded, I conceived the idea of putting betwixt the folds, a piece of thin and pliable lead.

Having ſtripped the *tendo achillis* of a rabbit of its tunick, I paſſed beneath it a piece of linen folded eight times, in the middle of which I had put a bit of lead, ſuch as I have juſt deſcribed. I pricked the tendon in ſeveral places with two venomous teeth, and covered it in ſuch a way that it was quite enclofed, having a bit of lead both above and below. The animal died at the end of thirty-two hours. The tendon was black at the parts where it had been wounded, the mufcles near it were a little inflamed, and the blood about the heart in a diffolved ftate.

All thefe precautions, as we fee, could not prevent or retard the death of the animal. As this was, however, but one folitary cafe, I did not think it proper to ftop here.

I repeated the experiment on the *tendines achillis* of four other rabbits, well ftripped of their tunicks. I applied the linen and bits of lead as ufual, wounding the tendons with venomous teeth, that the venom might be more collected, and fpread as little as poffible on the tendon. In a word, I omitted nothing that could make thefe experiments decifive ones. The rabbits all died in lefs than forty hours. In fome of them the blood was coagulated about the heart, but not in others. The lungs were fpotted in all of them. The mufcles in the vicinity of the tendons were a little inflamed, and in two of the rabbits livid.

Thefe new trials did not clear up my doubts. If on one hand they rendered the action of the venom on the tendons probable, on the other hand I could not conceive how a part, that is neither fenfible, nervous, vafcular, nor mufcular, could either receive the difeafe of the venom, or communicate it to the animal, fo as to occafion its death. I reflected again, that I had employed large rabbits; that I had neither applied much venom, nor made ufe of many vipers; and that, on other occafions, I had found a large rabbit to die late and with difficulty, although it had been bit by feveral vipers, and died with large wounds, and with the moft affured fymptoms of the difeafe of the venom. This made me fall upon a new fpecies of experiments, from which I flattered myfelf that I fhould draw fome kind of information.

I prepared the *tendo achillis* of a rabbit as above, and paffed beneath it a piece of linen folded fixteen times, with a bit of lead in the middle. I pierced the tendon in the ufual part with a venomous tooth, and introduced a drop of venom collected at the orifice, into the fubftance of the tendon, by a longitudinal incifion three lines in length, made with the point of a penknife. The incifion did not penetrate through. I left the tendon, venomed in this way, during a fpace of fix or feven minutes, and then foaked up the venom with dry lint, and by the means of fmall pincers, wafhed the wounded part of the tendon feveral fucceffive times. In proportion as the linen became moift, I took hold of one end of it, and drew it by degrees from under the tendon. It was impoffible in this way for the water to foak through the linen, and communicate the venom to the adjacent parts. As I wafhed the tendon upwards of twenty times, it was not poffible for an atom of venom to remain within it. The rabbit died at the end of thirty-two hours; the tendon was almoft in its natural ftate, its colour being fcarcely deepened at the part where the wound was made.

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I repeated this experiment on two other rabbits, using the same precautions. They were both dead in less than thirty-seven hours.

It occurred to me, that the linen left above and beneath the tendon till the death of the animal, might perhaps bring on such a change in the neighbouring parts, as to occasion a mortal disease.

Having removed the skin from the *tendo achillis* of a rabbit, and stripped it of its tunick, I put linen beneath it as usual, and wounded it with a venomous tooth. I wiped the tendon with lint, and washed it a little, taking care that the water did not touch the adjacent parts. I then removed the linen, and applied fresh, to the part. The rabbit died at the end of thirty-six hours. The parts about the tendon were in a natural state.

I prepared the Achilles' tendons of another rabbit in the usual way, and wounded them with a venomous tooth. I left them in this state for two minutes, and then threw on them a great deal of water, repeating the ablution till I conceived that they were perfectly cleansed in every part, and that the venom was either totally washed away, or diluted so effectually, that it could not convey its action to the adjacent parts. I had found from former experiments, that when any other part of an animal was bit, or wounded by a venomous tooth, the throwing of any quantity of water on it was ineffectual, and did not prevent the animal from dying, and from having the usual disease of the venom in the part bit. The rabbit, the ſubject of this experiment, died at the end of thirty-two hours.

Another rabbit treated in the ſame way, not only recovered, but ſeemed to have no other complaint than that occaſioned by the inciſion of the ſkin, and other parts that cover the tendon.

Theſe reſpective caſes, conſidered circumſtantially, began to perſuade me, that the venom of the viper is perfectly innocent to a tendon. To be certain of this, I thought of varying my experiments ſtill more, and of making them in ſuch a way, that they ſhould at length become deciſive.

Having removed the ſkin, and laid bare the *tendo achillis* of a rabbit, I bound it very tight with a piece of packthread, at both extremities of the tendinous ſubſtance. The ligatures were made in ſuch a way, that it was not poſſible for any communication either of humours or ſenſation betwixt the tendon and the animal to take place. I put the uſual folded linen under the tendon, which I wounded in ſeveral places with a venomous tooth, betwixt the two ligatures. I covered the tendon with linen, and the rabbit died at the end of thirty-two hours.

I repeated this experiment on another rabbit, the tendons of which I tied in the way above, and had it bit betwixt the two ligatures. I waſhed the wounds with a great deal of water, which I threw on with force, and then removed the linen from beneath. This rabbit died at the end of thirty hours. Another rabbit died in twenty-ſeven hours, after having been treated pretty much in the ſame way with

with the preceding one, with only this difference, that inftead of throwing a great deal of water on the tendons, I wafhed them by degrees, applying clean and dry linen, after removing that which I employed at firft.

It feems at length pretty clear, that the venom of the viper is not the caufe of the death of the rabbits, in the cafes in queftion, and that it has no action on the tendons. A doubt ftill remained, however, which it was neceffary to clear up. I had obferved that feveral mufcular fibres had found their way into the tendinous portions that form the *tendo achillis*, and conceived that the venom of the viper might firft communicate itfelf to them, and from them to the other parts of the animal. Notwithftanding there was but little probability in this conjecture, I wifhed to inform myfelf on the fubject by experiment.

Having removed a portion of the fkin from the *tendo achillis*, and ftripped it of its tunick, I deftroyed the mufcular fibres that defcend from the crural mufcles, and implant themfelves betwixt the three portions of this tendon. I paffed feveral doubles of linen betwixt thefe tendinous portions, in fuch a way that one of them was feparated from the other two, and enclofed in the linen. I wounded this portion with a venomous tooth, and covered it fo as to prevent the venom from touching any of the adjacent parts. The rabbit died at the end of thirty-two hours, with its heart and veffels filled with black and clotted blood.

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I repeated this experiment on the tendons of another rabbit, which died at the end of thirty-two hours. The wounded portions of the tendons were dark throughout their whole ſubſtance, and thoſe which had not been wounded were ſtill much more ſo. The lungs were covered with livid ſpots, and the heart and large veſſels filled with black and clotted blood.

I made an experiment on another rabbit, in which, after deſtroying the fibres betwixt the portions of the tendon, I paſſed a folded linen under its whole ſubſtance, as I had done in the caſes related a little above, and wounded it, without ſeparating the parts, with a venomous tooth. The rabbit died at the end of thirty-three hours. The wounded tendon was become darker and redder in ſome places, and the blood in the heart, and in the veſſels that go out from it, was black, but fluid.

It appears ſtill more, that the venom of the viper is not the cauſe of the death of theſe animals, but that it depends on another cauſe, probably on the denudation of the tendon itſelf. The following experiments remove all doubts on the ſubject.

I got ready ſix very large rabbits, all alike in ſize, in two of which I laid the Achilles' tendons bare, as uſual, and wounded them with a venomous tooth, after having incloſed them well in linen. In two others I laid the tendons likewiſe bare, but pricked them with a needle in ſeveral places. In two others I ſimply laid them bare, without wounding or pricking them.

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I covered all the tendons in the fame way with linen. All the rabbits died; the two that had been venomed, died together at the end of thirty-two hours; of the two the tendons of which were pricked with a needle, one died in thirty hours, the other in thirty-two. The two in which the tendons were fimply laid bare, died, one in twenty-feven hours, the other in forty.

The inferences I have drawn from the experiments on the tendons hitherto related, are as follows:

I. That a tendon is not fufceptible of the difeafe of the venom.

II. That when a tendon is ftripped of its tunick, the animal almoft invariably dies, without any intervention of venom.

This laft inference is a very important one, and may be of fome ufe in the punctures of tendons in man. It fhows how dangerous it is to ftrip thefe parts of the *tunica vaginalis*, and how much this membrane ought to be fpared.

It remained for me to make one other experiment on a tendon, which I fhall relate here, and which may throw fome light on the nature and economy of tendinous fubftances, and of their nutrition. Having laid the *tendo achillis* of a rabbit perfectly bare, and deftroyed the mufcular fibres that enter into it, fo that there could be no longer any flefhy fibres or veffels in the tendon, I found that the rabbit ate a few hours after, and conjectured that it would

would therefore probably recover. In effect it lived, and at the end of thirty-four days recovered perfectly, the wound made in its skin healing up. I wished to see what had happened to the tendon, and whether, as one would have supposed, it had dried up from a want of vessels. All the vessels about the tendon had been cut, and it was absolutely separated from every other substance throughout, except at its two extremities. I found it covered by a substance, partly spongy or cellular and partly callous, and sprinkled with several vessels. When I got to the tendon, I found it whitish, supple, and nourished, as usual, although it did not any where appear to receive vessels.

Were repeated experiments similar to this one to be made, important consequences, and facts relative to the nutrition of certain parts, would perhaps result from them.

The multiplied and varied experiments I made on the tendons, have been of very great use to me in the pursuit of my researches. If any doubt had remained on the subject; if I had not assured myself to a certainty that the bite of the viper on this part is not attended with any consequence; if I had apprehended that the venom could communicate itself to the animal through the medium of this substance; I should have had a thousand doubts as to the parts on which the venom acts, in an animal that has been bit. No subject in nature is absolutely indifferent; and when such rare and extraordinary effects are to be examined in the animal

mal body, nothing is to be neglected—nothing is to be deemed unneceſſary.

CHAPTER VII.

On the Nature of the Venom of the Viper. Deſcription of certain Parts of the Head of the Viper, that relate to the Venom.

BEFORE I examine the properties and nature of the venom of the viper, I think it incumbent on me to ſpeak of ſome other particulars, that relate to the canine teeth of this animal, to the bag or membrane with which they are naturally covered, and to the veſicle or receptacle of the venom, which the moſt modern writers continue to confound with the bag or ſheath of the teeth. I have treated of all theſe particulars in the firſt part of this work, but think it eſſentially neceſſary to introduce ſome figures here, which will give a juſter conception of what I have ſaid in the part alluded to, and of what I ſhall ſay in the ſequel.

I have judged it expedient to devote a chapter entirely to this ſubject, and to interrupt, as it were, the chain of my experiments on the effect of this poiſon, applied to the different parts of animals; ſince it is before all neceſſary, that the reader ſhould know the nature of the venom, and not be left any longer to bewilder himſelf in the erroneous opinions, and hypotheſes deſtitute of foundation, that have been ſpread by the writers who have employ-ed ed themſelves on the occaſion, both before and after the publication of my firſt experiments. Too much cannot be ſaid for this effect; for unfortunately, when the mind is prejudiced in favour of any opinion whatever, eſtabliſhed by authority, and generally adopted, it ſeems to deny itſelf to even the evidence of fact.

In Mead's work on Poiſons, a deſcription is found of the head of the viper, the parts of which are repreſented by figures. Theſe figures of Mead, or rather of Nicholls, who is the real author of them, are ſo imperfect, that I have been obliged to ſubſtitute others I have had purpoſely made. I have found the former ones out of all truth and nature, and whoever will take the trouble to confront them with the parts from which they were drawn, will find no difficulty in agreeing with me.

Fig. 1. of plate I. of this work (ſee the concluſion of the ſecond volume) repreſents the two canine teeth of the viper of one ſide of the upper jaw, partly covered by a membrane in the form of a bag or ſheath, open, as it is ſeen, to give paſſage to the teeth. Mead pourtrays this bag as if it was fringed at its edges. It is indeed ſometimes found in this ſtate, but is oftener without fringe or indentation, and ſuch as I have repreſented it. The canine teeth are elevated and laid a little bare, as they appear when the viper is on the point of biting; when it depreſſes them, they enter entirely into the bag or ſheath. It is eaſy to ſee, that if this bag were the receptacle of the venom, the latter would naturally flow out at the opening in it, and would

paſs

pafs continually into the mouth of the viper. This errour is copied from Redi, who believed that the venom was contained in this fheath that covers the teeth, and that it was fecreted in a fmall gland feated under the eye.

Fig. 2. reprefents this bag or fheath, *s s* opened with fciffars as far as its bafis, and likewife on the bone of the upper jaw. An elliptical hole *n. e.* with rounded edges, is feen at the bafis of each of the canine teeth, and a longer and narrower hole, towards the point of each tooth, *r. a.*

At the fide of the teeth a bladder is found, *m.* refembling a fhepherd's purfe, which pierces the fheath by a long canal ending in a fmall orifice *o* betwixt the two teeth. The venom contained in the purfe or bladder paffes through this canal, and conveys itfelf to the tooth, entering at the hole fituated at its bafis, and going out by that at its point.

Fig. 3. reprefents the bladder or purfe feen with a lens. It is not formed of a fmooth even membrane, but is on the contrary full of plaits, as if it was a compages of inteftines, or of wrinkles and ridges. It is of a triangular fhape, and has a much greater width than depth. If it is cut tranfverfely, and examined with attention, it is found to be of a fpongy fubftance, and compofed of cells deeper than they are broad. Every thing concurs to the belief that it is not a fimple bladder or receptacle of venom, but rather a true gland, very voluminous and of a particular ftructure, which feparates the venom from

from the blood of the viper, and in which it is reserved for the purpofes it is deftined to by nature, undoubtedly for the animal's advantage.

The cellular ftructure of this fingular gland does not permit the viper to exprefs with facility all the venom it contains. I have found a difficulty in forcing it out by a very ftrong preffure on the gland with my fingers; and indeed we have feen, that a viper is capable of killing fix or feven pigeons, one after the other.

The two figures, 4, 4, reprefent the receptacle of venom in its natural fize, feen at its anteriour, and at its pofterior part, and united with its excretory canal.

Fig. 5, fhows a tranfverfe fection of the above, feparated by many fmall partitions, *s c*, &c. and filled with the venom, which flows out drop by drop, as at *r a*, &c. It appears in this way when obferved with a lens.

Fig. 6, reprefents a canine tooth of a viper, with all its internal cavities, and its two external openings.

s s, are the elliptical hole at the point of the tooth.

c a, the opening of the hole at its bafis.

i i i, are the internal canal of the tooth, which opens at the bafis *c a*, and at the point *s s*.

There is a large opening, *e*, which forms the bafis of the tooth, and the fection of which is reprefented by *m*.

r o,

r o, of the figure at the fide, are the two openings, *i e*, of figure 6, which are difcovered by a fection of the tooth, as at *a b*.

r, reprefents the fhape of the longitudinal hole of the tooth.

o, reprefents the opening of the hole at the bafis. This fecond canal of the tooth does not communicate with the firft, and only extends as far as *r*.

Fig. 7, reprefents two canine teeth on one fide, having at their bafis feveral other teeth, more or lefs formed, *a c r*. Thefe teeth are moft frequently fix in number, and are fituated in the fheath, and covered with a very fine cellular web, which binds them, and unites them together. They are placed one over the other, thofe that are uppermoft, or neareft the canine teeth, being the largeft. The others decreafe in proportion, and the two that are neareft the canine teeth are perfectly alike in fize. The points of all them, even of the fmalleft, are pretty hard, and well formed;—they are channelled, and end by the ufual hole at the point.

When thefe teeth are feven in number, the feventh is always the fmalleft of the whole. It is fituated below all the others, and in the middle. The bafis of thefe teeth is not yet formed, and merely confifts of a flexible, tranfparent, and whitifh jelly. They are not only deficient at their bafis, but likewife want the oval hole;—the principles of it are fometimes feen in the largeft of them.

Although the matter at the bafis of thefe teeth appears a fimple jelly, even when it is viewed with the common lens, the naturalift would be very much

much miſtaken, if he ſuppoſed it to be non-organical. Stronger lens than the ordinary ones have ſhown me, that it is compoſed of a very fine webbed membrane, filled with extremely ſmall round corpuſcles. This membrane folds over itſelf, and ſeems to ſhow, even the holes, and the form that the baſis of the tooth is one day to take. I have, however, ſometimes thought I could diſtinguiſh this. Be that as it may, it is certain, that the gelatinous part of the tooth is organized, and that it exiſts in a ſtate of organization a long time before the tooth is entirely formed and in a perfect ſtate.

Of the Nature of the Venom of the Viper. It is examined as to its Acidity.

The acquiring a perfect knowledge of the nature of the viper's venom may be of the greateſt importance to animal phyſicks, and, at the ſame time, very uſeful to the human ſpecies. Vague and ſuperficial notions on this point, have given birth to hypotheſes, to theories, and laſtly, to remedies.

The volatile alkali in a great meaſure owes its reputation, to the opinion of the venom of the viper being acid.

The ancients were ignorant of what it conſiſted in, and of the part of the animal in which it reſided. François Redi was the firſt to eſtabliſh theſe points. He found it to be a humour ſimilar to the oil of ſweet almonds, which the viper conveys with its

tooth

tooth into the wound it makes in biting. But he was miſtaken in almoſt all he ſaid beſides on the ſubject of this venom. He believed that it reſided in the bag, or plaited membrane, that covers the canine teeth. He could never diſcover that it entered into the tooth itſelf, and flowed out of it; and thought that the ſmall gland ſeated under the eye of the viper, ſerved to ſecrete this humour, into the nature of which I do not find that he ever made any reſearch.

Before the time of Redi, there were none but very vague and confuſed ideas on the venom of the viper. We owe to this celebrated Italian naturaliſt the firſt advances into a ſubject, which he found in its infant ſtate, filled with hypotheſes and vulgar errours. Theſe errours were proper to the time he lived in, and it required a genius like his to combat them, and to open a new road to truth. It ſeems as if we only throw off our ignorance to plunge ourſelves into errour, and that it is at this criſis that the man of genius gives us ſome glimmerings of light. We ſet out at ignorance, which leads us to errour, and from errour we at length arrive at truth. This is the uſual progreſs of human intelligence, and through theſe gradations the moſt enlightened nations have paſſed.

Mead is the firſt who in any way examined the nature and qualities of the venom of the viper; but from a fatality to which even the moſt diligent obſerver is oftentimes ſubject in his endeavours to make the earlieſt opening to truth, Mead found that this venom was acid, and that it changed the dye of the turnesol red, and even gave a reddish tinge to the sirop of violets.

A few years after, Mead himself, in a second edition of his work on poisons, retracted all he had advanced on the acidity of the venom of the viper, and confessed, as a candid and ingenuous man, that it neither changes the dye of the turnesol, nor the sirop of violets red, and that it is neither acid nor alakaline. Doctor James, who assures us that he repeated the experiments of Mead, has latterly found this venom to be acid; he however does not speak of the posterior experiments of the above cited authour; neither does he inform us how, supposing him right at first, he was deceived on the second occasion. This manner of publishing ones ideas, or ones experiments, necessarily tends to perpetuate doubts and hypotheses, since, after all, the authority of one man is of as much weight as that of another, and since we cannot guess which of the two is in the wrong. Another writer, still more modern than Doctor James, has received it as a truth, that the venom of the viper is acid; supporting his opinion on the bare authority of Mead, without telling us, that this authour has since denied its acidity.

It was natural to conceive, that experiment itself had demonstrated to these writers, that Mead was mistaken the second time, and that he was right in his first trials, when he found the venom acid; and this consideration obliged me to examine the matter

matter afresh. I hope that no doubt will any longer remain, and flatter myself that I have discovered the errour into which Mead fell when he first examined this venom; an errour against which Doctor James was not able to guard.

I have sometimes, but rarely, found, that the venom of the viper gives the dye of the turnesol a light-red colour. This circumstance, instead of inducing me to believe the venom acid, excited me rather to examine once more into the cause of it, which might be accidental. I observed that in these cases it was not very pure, and on examining it with a microscope, discovered globules of blood floating in it. I then examined the mouth of the viper, and found the two bags or sheaths which cover the teeth slightly inflamed. It is not uncommon to meet with vipers that are naturally in this state, and it is still more frequent to find these bags reddened after the vipers have bit. We likewise frequently see the venom stained with blood, if its receptacle is too strongly compressed. All these cases may happen, and in all these cases the dye of the turnesol may become red, without supposing an acidity in the venom. It is, therefore, not unlikely that Doctor James has been deceived in the same way with Mead. It is certain, that in the few cases in which I have found the dye of the turnesol reddened, the venom was not pure, but was mixed with blood.

Aware of all these accidents, I took the utmost precaution in collecting the venom. I generally cut off the head of the animal at a blow. Some hours after, when the muſcles had loſt their motion, I opened the mouth carefully, and contrived that the points of the canine teeth ſhould be free from their ſheaths. I then made a gentle preſſure on the receptacle of the venom, and received the latter in a glaſs, as it flowed out at the point of the tooth. In this way it is uſually ſo pure, that it appears, when viewed with a microſcope, like an oil, more or leſs yellow. No extraneous matter is obſerved in it; and when I accidentally thought I perceived corpuſcles floating in it, I did not employ it in the experiments of which I am about to give a detail.

When the venom was drawn in this way from the tooth, I never could perceive that it changed the dye of the turneſol red, however often I made the experiment, and I repeated it very many times. In moſt caſes, I began with uniting a drop of venom with thirty drops of the dye or tincture, and not finding the colour of the latter to be changed, I added another drop of the venom, proceeding in this way to a tenth, or one-third the quantity of the tincture, which never either reddened or changed its colour, but only appeared not quite ſo clear as before. I repeated this experiment too often to apprehend that I was miſtaken.—I not only tried the venom with the tincture, or dye, of the turneſol, but likewiſe made the ſame experiments on the blue juice of radiſhes, a liquor very ſenſible to the action of acids, even of the weakeſt of them. It continued blue as before, without my being able to obſerve

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the flighteft change in it. I likewife had paper well tinged with the juice of radifhes, and let fall large drops of venom upon it;—the venom foon dried, and, except a yellowifh tinge it gave where it fell, I could perceive no change in the colour of the paper.

On feveral other occafions I diluted the venom with water, but could find no greater change in the paper on which I dropped it, than when I tried it pure.

I cannot deny but that I fometimes obferved a weak reddifh tinge on the blue paper, when I made the experiment in the following manner:—I covered a large ball of cotton with the paper, and on forcing the viper to make a ftrong bite at it, perceived this very pale tinge of red, at the parts the animal had pierced with its teeth. I did not, indeed, multiply my experiments fufficiently to be able to fay with certainty whence this tinge proceeded in thefe circumftances. We may fufpect that it was owing to a fmall quantity of blood from the mouth, blending itfelf with the venom; it is, however, very certain, that that taken from the vefícle, neither changes the die of the turnefol, nor the juice of radifhes, red.

But even though we fhould agree that the venom of the viper may be capable of giving a red tinge to thefe liquors, does it follow of neceffity, that the volatile alkali is a certain remedy againft this venom, and that the latter occafions death precifely becaufe it is acid?

The rock that men uſually ſplit upon, a rock that the moſt circumſpect philoſophers have not always been able to avoid, is that it is ſufficient for them to find a circumſtance which accompanies the effect, to be too readily perſuaded that it is the cauſe of it.

The innate deſire we have of knowing every thing, makes us ſtrive to explain every thing. If we ſee an effect produced after the application of any given ſubſtance, we immediately endeavour to ſee if there may not be ſomething in that ſubſtance that may ſerve in ſome way to explain the effect; giving ourſelves very little trouble to examine whether the cauſe we have diſcovered is proportioned or not to the effect produced. This errour ſeems to have been committed by two men of the firſt talents, Mead and Juſſieu. Mead, when he publiſhed the firſt edition of his work on poiſons, perſuaded of the acidity of the venom of the viper, judged that it muſt neceſſarily kill animals, becauſe it coagulates the blood as acids do.—Juſſieu, perſuaded likewiſe of the acidity of the venom, from the authority of Mead, immediately found a ſpecifick againſt it in the volatile alkali *(a)*.

(*a*) Juſſieu was not the firſt after Mead to recommend the uſe of the volatile alkali againſt the bite of the viper, but as he made a brilliant cure, it is to him that this remedy owes its greateſt reputation.

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The venom of the viper, as well as many other ſubſtances, is formed of ſeveral principles we are ſtill ignorant of. All the qualities we find in bodies do not conſtitute their real nature;—ſome of theſe qualities are accidental, others are not ſo. The acidity, even though it ſhould be conſtantly obſerved in the venom of the viper, may, nevertheleſs, be nothing more in it than an accidental quality; and the venom, in ceaſing to be acid, may not ceaſe to be a poiſon. Chemiſtry furniſhes us with a thouſand ſimilar examples. It is, therefore, improper to deduce the cauſe of the death from the acidity, and to deduce from the ſame acidity the uſe of the volatile alkali, as a remedy; for even ſuppoſing the venom to be conſtantly acid, and that this acidity cannot be ſeparated from it, does this enable us to ſay, that it kills becauſe it is acid, and that the volatile alkali is its ſpecifical remedy, becauſe it is capable of ſaturating it? The venom of the viper may likewiſe have ſeveral other qualities that we are unacquainted with, and may occaſion death by each of them ſeparately, or by all of them together. Why then are we to ſuppoſe, that it derives its noxious qualities from its acidity? There are arguments that demonſtrate the contrary.

Water abſorbs about its own bulk of fixed air, and conſequently a cubick inch of water can contain but very little more, if it does contain more, than a cubick inch of this air. It is not yet proved that a cubick inch of fixed air weighs an entire grain. A cubick inch of water weighs about 373 grains, and consequently the fixed air contained in a cubick inch of water, cannot be more in weight that its 373 part. Now a cubick inch of water impregnated with fixed air, is capable of giving a red tinge to 60 cubical inches of the tincture, or dye, of the turnesol, that is to say, to 22380 grains. Whence we see that the $\frac{1}{22380}$ part of a grain of fixed air is capable of bestowing a sensible tinge of red on a grain of the dye of the turnesol. Now granting this hypothesis, there cannot be at most in a grain of venom more than the $\frac{1}{22380}$ part of acid matter, and since the thousandth part only of a grain in weight of the venom is capable of killing a sparrow, as will appear by and by, we must suppose, that the $\frac{1}{22380000}$ part of a grain of acid can kill an animal simply as an acid principle.

Who does not now see, that even though it should be granted that the venom of the viper gives a red tinge to the dye of the turnesol, it would not, on that account, follow, that it would kill because acid? Its acidity would be so inconsiderable, that it would produce no sensible change in the animal body. And where is that violent acid, or any other principles of bodies, which is active to such a degree, that in diminishing its quantity it does not at length become innocent?

Let any one suppose, if he will, that the acidity of the venom of the viper is as great as that of the *glacial* oil of vitriol (oil of vitriol concentrated to the consistence of *ice*) itself. If the mortal effects of the former depended on its acidity, the glacial vitri-

vitriolick acid, thrown on a wound, although in a very ſmall quantity, would occaſion the death of animals. Glacial oil of vitriol applied to a wound, may indeed render the ſtate of it worſe, and may corrode the fleſh, but will not kill the animal on which it is tried. Very little of it can be introduced into the circulation of animals, and the little that is introduced is then weakened by the blood with which it mixes. It is true, that, as well as the venom, it may kill, if injected in a ſmall quantity; but this only happens becauſe it is not yet mixed with the humours, and weakened by them. Both the venom of the viper and the oil of vitriol may be abſorbed by the veſſels, and notwithſtanding the former is abſorbed in a very ſmall quantity, and very much diluted by the blood, it will kill an animal which will not be killed by the oil of vitriol. The venom of the viper does not therefore occaſion a very ſudden death from its acidity, but from other principles as yet unknown to us.

Mead, who changed his opinion as to the acidity of the venom of the viper, never wavered however in his ſentiments in regard to its ſuppoſed ſalts. He has always remained in the perſuaſion of having obſerved them floating in the yet fluid venom, ſoon after having taken it from the animal; and not only believes in the exiſtence of theſe floating ſalts in the venom, but aſſerts that the venom itſelf changes to a ſimple ſaline network, of a very beautiful ſtructure, which he compares to a ſpider's web. He ſpeaks of the ſolidity and firmneſs of theſe ſalts, which he defcribes minutely, and even gives a feparate drawing of them. He adds, that he has difcovered here and there in thefe falts, fmall circularly-formed knots, which are extremely folid, and never lofe the fhape they have at firft taken.

This fubject, which appeared to me extremely interefting, I examined very extenfively, in my work publifhed in Italy, which forms the firft part of this publication. I even flattered myfelf, at that time, that I had not only demonftrated the errour of Mead in an inconteftible way, but had likewife difcovered the fource of it. *To refute an errour in phyficks in a decifive* manner, nothing can be more effectual than the recurring to its origin. But even this does not feem fatisfactory to certain authours, who perfevere in maintaining, after the authority of Mead, that the venom of the viper is a mafs of falts; notwithftanding it is more than twelve years fince Mead was refuted on this point. I demonftrated at that time, that this venom is an homogeneous fluid, which, when taken pure from the tooth, is never found mixed with falts floating in it, nor with other heterogeneous particles; and that thefe floating corpufcles, when they are to be found, are merely accidental, and are by no means falts. The fmall knots feen by Mead, are nothing more than fmall bubbles of air interfperfed in the venom. Thefe fmall air-bubbles are never feen when the venom is taken immediately from the veficle, and may be made to appear at pleafure, by taking it blended

blended with the faliva of the animal, from the mouth of the viper *(a)*.

The faline net-work, which Mead fays he obferved, and which has been defcribed by many authours after him, is no other than the fragments of the dried venom, which, when taken from the tooth, and put on a bit of glafs, very foon dries, and whilft it is drying cracks in different parts, prefenting pieces and fragments very different from real falts. The Count de la Garaie made *falts* of the fame kind, by thoroughly drying his extracts on earthen plates, the glazing of which gave the hardened fragments a kind of fhining faline appearance.

If a drop of the venom of the viper, put on a bit of glafs, is examined with a microfcope, the fubftance of it will be feen to crack gradually at the circumference, where it dries fooneft. The fiffures in

(a) To have demonftrated the falfehood of any opinion whatever, is not a fufficient caufe for its being laid afide, if it is generally adopted by authours. Nothing lefs is needed for this effect than the renewal of the entire generation, to the end that it may flatter itfelf, that it cannot be reproached for rejecting an errour it has not committed. It required half a century to eftablifh the circulation of the blood, and the attraction of Newton, amongft philofophers. Man, always filled with a fecret pride, thinks that he is humbled if he difcovers himfelf liable to err; and the vulgar, never to be trufted in their decifions, are of the fame opinion. We have unfortunately too many examples of this kind, not to perceive that the love of truth is by no means the firft fpring of human actions.

this part are ſmaller and more crooked than elſewhere; but, by continuing to obſerve the venom, larger, broader, and deeper ones, which advance towards the centre of the drop where they end and meet, are ſeen at every part of the circumference. Theſe crooked lines are obſerved very diſtinctly with a microſcope, running to the centre, and lengthening in ſuch a way that one might miſtake them at firſt ſight for ſmall ſnakes, writhing themſelves from the circumference of the venom to the centre. After all the fiſſures are formed in this way, they enlarge ſtill more in proportion as the venom becomes drier, and occupies a leſs ſpace on the glaſs.

I do not know any microſcopical obſervation more certain and more evident than this, and in regard to which one may aſſure ones-ſelf with better grounds, that circumſtances are thus, and not otherwiſe. But that not the ſmalleſt doubt may remain, even in thoſe who may not have an opportunity to repeat my experiments, I have thought it incumbent on me to to repreſent, by ſeveral figures, a drop of venom in the act of deſiccation. It will be ſufficient to give a glance at theſe figures to be ſatisfied of the truth.

Fig. 1. of plate II. repreſents a drop of venom at the moment of its beginning to dry on a bit of glaſs. The fiſſures that are the moſt curved, at the circumference of the drop, are already entirely formed, the venom beginning to dry at the circumference. The others are ſeen becoming ſtraighter, length-

lengthening, and approaching to the centre, where the venom dries the floweft. When it is perfectly dry, the firft figure changes to the fecond, (fee figure 2.) in which the fiffures appear carried on to the centre, after having taken different curvatures. The fiffures in the centre are broader, becaufe the venom, which is there in a greater quantity, fepa-rates more on that account in drying.

Fig. 3. reprefents feveral fragments of the dried venom, in which the fiffures are defcribed by fpiral lines. Thefe fpires, as at *a*, are formed particu-larly, when the venom is dried in a confiderable quantity, and when it is pretty thick on the glafs of a watch. The fragments, which in this cafe are pretty large, open in the middle, and the open-ing, as I have juft faid, is of a fpiral form. The letter *e* reprefents a cleft that feparates the frag-ments from each other.

In Fig. 4. a drop of venom is reprefented, taken from the mouth of the viper, and dried. The fmall balls, or knots, of Mead are feen in it, as at *o*. Thefe fmall balls are real bubbles of air, which are made to difappear with the point of a needle, as all air-bubbles are that are produced in fluids. Letter *m* reprefents a cleft that feparates the frag-ments, as above.

It is an errour then, founded on ill-contrived experiments, that there are falts floating in the venom of the viper; and the regarding the frag-ments of this venom, when it is dried, as falts, is another errour. It is equal and homogeneous through-

throughout, and nothing of this can consequently be obſerved in it.

Mead, who regarded the venom of the viper as a maſs of ſalts, likewiſe believed it to be cauſtick and acrid when put on the tongue. He quotes himſelf and ſeveral of his friends, as having taſted it. He likewiſe obſerves, that when the viper bites, and when the venom begins to find its way into the wound, the animal cries out, writhes itſelf, and and exhibits other manifeſt ſigns of pain. Without pretending to decide at all on this queſtion, which I have likewiſe examined in the firſt part of this work, I ſhall obſerve here, that the experiment on dogs, which howl when they are bit, is not a certain and evident proof of the cauſtick nature of the venom. Perhaps when it is united in theſe caſes with the fluids of the animal, it is decompoſed, and acquires qualities it did not poſſeſs a moment before. It is true, that this howling which is mentioned, is ſometimes obſerved, but not always, and may be occaſioned by its frequently happening that a nerve is pricked by the teeth of the viper, in which caſes the venom may cauſe the ſame pain as any other body, or ſimple fluid, applied to the nerve itſelf.

If Mead taſted the venom, and found it cauſtick, I have taſted it likewiſe, and have made others taſte it, and we have neither found it cauſtick nor acrid. According to my ſentiments, it has no kind of taſte when put on the tongue, and is neither perceived to ſting or heat the part. It is

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true that a ſenſation is felt ſoon after, which may have made thoſe who believed it compoſed of ſalts, and who waited for ſome extraordinary change, ſuſpect that it was cauſtick and hot. The ſenſation it leaves when taken by the mouth, is that of a torpor or ſtupefaction in the part it touches. The tongue particularly ſeems numbed; it even appears to be grown larger, and its motions are ſlower and more difficult. This is certainly extraordinary, but appears very different from the effects occaſioned by cauſtick and acrid ſubſtances, when put on this part.—Laſtly, Mr. Troja wiſhed to taſte it himſelf, and aſſured me that he found it neither hot nor cauſtick, but that this ſenſation of torpor and ſtupefaction was the conſequence of it in the mouth. I can likewiſe take upon me to ſay, that I put five or ſix drops at a time into the mouth of ſmall animals, ſuch as rabbits, guineapigs, &c. without even having been able to obſerve any ſwelling or redneſs. Theſe experiments, when made on man, cannot be obſerved without a degree of repugnance, ſince, after all, a ſmall excoriation in the mouth, or on the tongue, may cauſe them to be too dearly paid for by the obſerver. I conceived that I could aſſure myſelf as to this particular in another way, and on a part even more ſenſible than the tongue itſelf; that is to ſay, on the eyes of different animals.

I put ſometimes one, and ſometimes ſeveral drops of venom on the eyes of a cat, and kept its eyelids open by force. I let it fall into the eyes of

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ſeveral rabbits without their perceiving it, and did the ſame thing to dogs. It was ſeen running over the tranſparent cornea and opake cornea, and getting within the eyelids. I could not perceive in any of theſe caſes that it acted as a cauſtick or acrid ſubſtance.

If Mead was miſtaken when he believed the venom of the viper to be compoſed of ſalts, he was not miſtaken, however, when he aſſerted that it was neither acid nor alkaline, ſince, in effect, it neither effervesces with alkalies or acids.

It is needleſs, after the experiments recited in the firſt part of this work, to enter here into a detail of thoſe I was induced to repeat upon this occaſion, and which can no longer leave any doubt in the minds of thoſe who are ſkilled in obſerving. It is an eſtabliſhed truth that the venom of the viper does not effervesce with any of the mineral or vegetable acids, nor with any kind of alkali we at preſent know of. I have repeated theſe experiments too often to have any doubt of having been miſled by them.

But it is not ſufficient to have ſatisfied ourſelves that the venom of the viper is neither acid nor alkaline; that it is not compoſed of ſalts; and that it is not corroſive to the palate; to inſtruct us in what it really is. I do not know with what other ſubſtance that is better known, it may be made to agree. It is principally to this point that the efforts of obſervers ſhould be directed, ſince it is certain that we are not thoroughly acquainted with

with the true nature of any ſubſtance, although we are more or leſs acquainted with the properties of certain ſubſtances.

When the venom of the viper is yet liquid, it unites in a greater or leſs degree with acids. But we muſt likewiſe examine it when dry.

I put ſeveral drops of very pure venom into the concave part of the glaſs of a watch; as it dried, it became yellow, and full of cracks. I poured oil of vitriol on it, but no viſible ſolution followed. I raiſed from the bottom of the glaſs, with a capillary tube, ſeveral fragments of the venom, which floated in the oil of vitriol without diſſolving. At length, after ſome time, they ſeemed to begin to divide a little, and though they were indeed reduced to a kind of liquid paſte, ſtill preſerved their natural colour. There did not appear to be a true and perfect diſſolution of them, at leaſt during the time I obſerved them.

The marine acid, when poured on the dried venom, acts pretty much in the ſame way as the oil of vitriol. The fragments of venom do not appear, in a ſtrict ſenſe, to be diſſolved by this acid, although they are ſoftened by it.

The nitrous acid ſeems to have no greater power to diſſolve the dried fragments of venom, although it at length ſoftens them. Notwithſtanding the venom is rendered flexible by this acid, it ſtill preſerves a certain conſiſtence or tenacity which keeps it together, and it becomes yellower. If examined

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in this ſtate, it appears to be compoſed of an infi. nite number of very ſmall ſpherical corpuſcles.

Thus then it appears that the ſtrongeſt acids have but a very ſlow and weak action on the dried venom of the viper, and that the diſſolution they at length *occaſion is but* a very imperfect one.

Vegetable acids, however concentrated they may be, do not diſſolve this venom better than do the mineral ones; and alkaline ſubſtances have no greater tendency to this effect.

I was likewiſe deſirous of knowing whether eſſential oils would diſſolve it, and on trying them did not find them to poſſeſs that property.

The hepar ſulphuris makes no greater impreſſion on it.

Theſe experiments, which I varied ſeveral ways, made me ſuſpect by degrees, that the venom of the viper might be either a gummy or a lymphatick ſubſtance, ſeparated from the blood of the animal. I had obſerved a long time before, that the dried venom appeared to be tenacious, like one of the *ſtrongeſt gums, when broke* betwixt the teeth. Freſh experiments were neceſſary, however, to be certain that it poſſeſſed the nature of a gum.

Chemiſts know that gums neither diſſolve in ſpirit of wine, nor in oil; but that they diſſolve very readily in water. This kind of examination might without doubt be ſatisfactory, but it was firſt neceſſary to prove that it was not of the ſame nature as animal lymph, or the white of an egg. We know that theſe ſubſtances coagulate in warm water,

ter, inſtead of diſſolving, as gums do. I got ready for this trial a great quantity of venom, which I kept in a ſmall capſular glaſs till it became perfectly dry. On this venom I threw at once about half an ounce of boiling water, by which it was inſtantly and effectually diſſolved, inſtead of being coagulated. On repeating this experiment ſeveral times, the conſequence was invariably the ſame. The water, after having been thrown into the glaſs, ſtill preſerved upwards of fifty degrees of heat.

Having thus, by direct experiments, excluded the hypotheſis of a lymphatick animal matter, I proceeded to the experiment of the ſpirit of wine.

I had a good quantity of venom dried as uſual in a ſmall glaſs, and poured on it half an ounce of highly rectified ſpirit of wine. I left it in an undiſturbed ſtate for upwards of two hours, when I found the venom undiſſolved at the bottom of the glaſs. I broke it into ſeveral ſmall bits with the ſharp point of a ſmall glaſs tube, and ſhook the whole together for ſome time. There was, however, no diſſolution, the ſmall pieces of venom continuing whole, hard, and of their uſual colour. This experiment will always ſucceed in the ſame way, if the ſpirit of wine is good; but if it ſhould contain too much phlegm, the venom may be partly diſſolved by it. Even this laſt circumſtance proves that the venom of the viper is a gummy ſubſtance, ſince gums are very readily diſſolved in water, which likewiſe diſſolves the dried venom, as I have aſſured myſelf an infinite number of times.

If the venom is perfectly pure, the water does not lose any part of its transparence. Distilled water is the best calculated for these experiments.

I have frequently held the dried venom to the fire, and have increased the heat by degrees, but it has never melted. If it is thrown on a live coal, it swells and puffs up, but does not begin to take fire till it has assumed the appearance of a coal.

Another experiment now remained to be made, to render this matter decisive.

All chemists know that gums dissolved in water are precipitated by spirit of wine; and that in this trial, the water in which they are dissolved becomes very white.

I put equal proportions of water into two small glasses, and added to one of them a quantity of the venom of the viper, and to the other an equal quantity of gum arabick. The solution of gum arabick, which was made by heat, being reduced to the temperature of the liquor in the other glass, I poured several drops of spirit of wine into each of the glasses. The number of drops thrown into each was pretty much the same, when I began to perceive a whitish cloudiness, which disappeared a moment after in both solutions, at every drop of spirit of wine poured into them. On continuing to throw an equal quantity of spirit of wine into each glass, I saw the white cloud, instead of disappearing, extend itself over the fluids, which became whiter and more opake, at every addition of the spirit. On ceasing to throw it in, I perceived that the white matter be-

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gan to precipitate; and on adding a few drops of the ſpirit afreſh, found that there was no longer any ſeparation in either of the liquors. At the end of twenty-four hours the precipitation was complete, and there was at the bottom of each glaſs, pretty nearly the ſame quantity of an equally white, ſoft, and paſte-like, ſubſtance.

The venom of the viper, when diſſolved in water and precipitated by ſpirit of wine into the form of a white powder or meal, cracks in different parts when dried afreſh, and its fiſſures are of the uſual reticular form.

When a clear and tranſparent oil of vitriol is mixed with the venom, precipitated by ſpirit of wine, and dried in a glaſs, it becomes at the end of a certain time, of a dark vinous colour. The ſame changes are obſerved in the ſolution of gum arabick in water, precipitated by ſpirit of wine. This gum, in drying, likewiſe adheres to the glaſs and cracks, and if a few drops of oil of vitriol are thrown on it, they become in the ſame ſpace, of a dark vinous colour. The analogy betwixt the venom and the gum cannot be more perfect. They alike diſſolve in water; they are precipitated in the ſame way by ſpirit of wine; the precipitated powder or meal is of the ſame colour; both of them crack in drying; oil of vitriol does not ſoften them till after ſome time; and changes its colour in the ſame way with each of theſe ſubſtances.

I made another experiment on the venom of the viper, which though it does not prove any thing eſſential as to the internal nature of this venom, is ſtill a further proof that it has a great analogy to the gums.

I put ſix grains of very pure dried venom into a ſmall matraſs, and added to it fifty drops of nitrous acid, to throw off its airs. There came off from it, by the aſſiſtance of heat, as much air, or perhaps ſomewhat more, as the matraſs could contain. This was common air, a little changed in its qualities. I continued the fire, and a clouded air came off, which on examination I found to be compoſed, one third of fixed, and two thirds of phlogiſtick air.

Gum arabick, in the ſame circumſtances, likewiſe afforded fixed and phlogiſtick air, and the conſequences of both experiments were ſo perfectly ſimilar, that they might have been confounded together. It is true that gum arabick likewiſe affords nitrous air, but this only happens when it is in a conſiderable quantity. If the quantity is very ſmall, the little nitrous air it furniſhes, decompoſes itſelf, and unites with the common air in the matraſs.

It ſeems then to be demonſtrated, that the venom is in reality a gum; we at leaſt ſee, that it has all the properties and principal characteriſticks of ſuch.

This venom is found in an animal, is elaborated in its organs, and formed of its humours. It therefore ought to be conſidered as a true animal gum, particularly as the viper feeds on animals. Although we are unacquainted with any other animal gum, I do not think that the venom on that account ſhould

ſhould be denied to be ſuch, ſince it has all the properties of a gum. It ſhould therefore for the future, be inſerted in the catalogue of gums, and this diſcovery may perhaps induce naturaliſts to examine, whether a gummy ſubſtance may not likewiſe be found in ſome other animal.

Allowing the venom of the viper to be a real gum, this will not lead us to conceive what it is that conſtitutes it a venom, ſince it is a known truth that gums are not ſo, and that they may be employed with impunity. It would be ſuperfluous to relate the experiments I made on this ſubject, out of pure curioſity. I aſſured myſelf in a thouſand ways, that gum arabick is entirely innocent when applied to wounds. But ſuch is the condition of man, and ſuch is the nature of what we call ſcience. We at length arrive at certain bounds, to carry us beyond which all our efforts are to no purpoſe. The idea that the venom of the viper is a gum of ſome kind, does not ſerve in the leaſt to explain to us, how this gum brings on a violent diſeaſe in an inſtant, and how it is that, in ſo ſmall a quantity, it deſtroys life in ſo ſhort a time. Whatever the principle that renders it venomous may be, the proportion of it is ſo ſmall, that it does not at all change in it the uſual properties of a gum; and the ſmalleſt veſtige of this principle cannot be traced, whether the ſtrongeſt microſcopes are employed, or the venom obſerved in any other way. The moſt active ſubſtances are rendered ſuch by quantities of matter that cannot be traced. The point of a needle that has touched a variolous puſtule, preſerves its activity for years, and brings about violent changes in the bodies of ſeveral perſons ſucceſſively pricked with it.

How far are we ſtill from penetrating the depths of this myſtery! Through how many difficult and unknown ways muſt we not paſs, in getting ſome inſight into a matter ſo obſcure and difficult as this! Happy at length, if all the pains that are taken, if all the efforts that are made, do not prove totally ineffectual.

This diſcovery, which enriches natural hiſtory with a new gum, ought not to be neglected by naturaliſts. It may in time lead to a better knowledge of the nature of the venom of the viper, and of the complicated effects it produces. It may perhaps be one day uſeful to us in enabling us to comprehend; why animals with cold blood are ſo long in dying of the bite of the viper; why there are ſome that are not killed by it; and why the venom, in whatever way it is introduced into its body, is altogether innocent to the viper itſelf. If the animals with cold blood that die late; if the others that do not die; if the viper to which the venom is not at all hurtful; had humours or parts of ſuch a nature, that they could be but little, or ſlowly, or not at all changed by this animal gum: we might then in ſome way explain a ſubject which is as yet very obſcure, and which does not ſeem capable of being cleared up, till after we have acquired a thorough knowledge of the venom itſelf, and of the moſt la-

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tent principles and qualities of the animal bodies on which it acts.

On Bees, Drones, and Wasps.

In the first part of this work, I related a few experiments on the venom of the scorpion, and on the humour which flows from bees when they wound with their sting. I have had occasion since to make some other observations, not only on bees, but likewise on wasps, hornets, and drones. I do not know that any naturalist has examined in a proper manner, the liquor with which these animals are provided, that wound with a sting. Indeed Mead says that he found the humour of bees, to be composed of very small saline needles, or points. He assures us that he examined it with a microscope, and found it filled with these pointed salts. I do not know whether this observation made by Mead, has been confirmed or not by other naturalists, but can for my own part take upon me to say, that I never have been able to find any thing saline in this humour, whatever attention I paid in investigating it, and notwithstanding I employed the strongest lens for that purpose. I am persuaded that Mead has been mistaken in this particular, as he was in observing the venom of the viper. He assuredly saw particles floating in this humour before it it was dry, and immediately persuaded himself that they could be no other than floating points.

We may easily conceive that Mead only examined this humour, in an impure state, and mixed with corpuscles that were foreign to it; and that this was sufficient to induce him to believe it composed of salts. He was deceived on this occasion, as he was in his opinion of the venom of the viper, in which there is nothing to be met with of all that he fancied he saw; and he seems in both cases to have erred exactly in the same way. The humour of bees, after the manner of the venom of the viper, cracks in drying, and presents the usual sharp and regular fragments. This was sufficient to persuade Mead that it was a true salt.

I can venture to say, that when the observation is well made, nothing can lead to such an opinion. But if, in expressing the liquor from the bee's sting, the greatest care is not taken to prevent the breaking and mixing any thing with it, it may easily be charged with other irregular bodies; and when it is put on the port-object, some small degree of motion may likewise be observed in these bodies, which may float in a greater or less quantity. But this accidental motion, which is not proper to these substances, soon ceases altogether, when the humour is left undisturbed. By degrees it dries, and, in drying, breaks, cracks, and forms angles and points.

When the venom of the viper and the humour of bees, are dried and observed with a microscope, no sensible difference can be observed betwixt them.

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I have only taken notice, that the humour of bees, exposed to the open air on a bit of glass, is much longer in drying than the venom of the viper, and that the cracks or fissures in the former, are likewise formed much later than those in the latter, supposing the degree of desiccation in the two fluids alike.

These two humours not only agree in the appearances their parts present in drying, but likewise in other qualities. If a bit of the dried humour of bees is strongly compressed betwixt the teeth, it, as it were, glues them fast together; and exactly the same thing happens on trying the venom of the viper, and all hardened gummy substances. The dried humour of bees likewise dissolves in simple water, and resists the action of spirit of wine, as the venom of the viper and gums in general do; so that I am almost inclined to believe that, as the venom of the viper is most assuredly a gummy substance, this humour is so too. Indeed the quantity one is able to collect of it is so very small, that one can scarcely attempt to make any certain experiments on this substance; the consequences, however, of those I have made have been sufficiently uniform to lead me to think that I cannot easily have been mistaken in what I have conjectured.

I have met with the same success in examining the humour of wasps and drones, and of the other flying insects in general, that wound with a sting, and are provided with a humour. In all these, the humour is bitter and acrid, and has all the appearance of being of a gummy nature. When left to dry on a bit of glafs, it cracks throughout like the venom of the viper, and when chewed, is tenacious, glutinous, and elaftick.

But it muft not therefore be thought to be the fame as the venom of the viper, and that it has all the other qualities of this poifon. The venom of the viper neither has any fenfible tafte when taken into the mouth, nor is fufficiently acid to give a red tinge to the tincture of turnefol, or juice of radifhes. The humour of bees, and of the other analogous infects, the moment it is applied to a piece of paper that has been previoufly ftained with the juice of radifhes, gives it a flight red tinge, which afterwards changes to a pale yellow, fo that one would conjecture that this humour deftroys the blue colour of the paper. This experiment, which has been repeated feveral times, and always attended with the fame fuccefs, proves that this humour is united with an acid, and not with an alkaline principle; we fee, however, at the fame time, that the quantity of acid it contains is very fmall, and abfolutely incapable, as an acid principle, of occafioning the fmalleft fenfation on the tongue, or in the part pricked by the fting of the animal.

A quantity of water impregnated with an equal bulk of fixed air, gives a red tinge to paper ftained with the juice of radifhes. This tinge, which is pretty ftrong, continues a confiderable time. A fmall quantity of water impregnated with fixed air, fcarcely contains a fufficient degree of acid to be fen-

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ſible to the taſte, and is likewiſe entirely innocent when applied to wounds.

We muſt therefore regard the hypotheſis of thoſe naturaliſts, who have advanced that this humour occaſions a ſwelling in the parts into which it is introduced, and that the volatile alkali, as ſaturating the acid principle, is a remedy againſt it, as falſe and erroneous.

Experiment ſeems to indicate that this humour acts by the medium of a bitter and cauſtick principle, which is neither acid nor alkaline. If it is put on the tongue, it has a hot bitter taſte, as I obſerved before, and not that of an acid or alkaline ſuſtance.

There are many ſubſtances which, without being either acid or alkaline, are hot and acrid to the palate, and are productive of violent and diſagreeable ſenſations. Cantharides, and ſeveral aromatick plants, are of this claſs. In the preſent caſe it appears certain, that neither the pain, (which is frequently inſupportable, and greater than that which would be cauſed by oil of vitriol itſelf) nor the ſwelling nor inflammation of the parts, can be brought on by an acid principle introduced into the ſkin of the animals that have been ſtung; and therefore the theory laid down by certain authors to explain the effects of this humour, muſt be regarded as abſolutely falſe, and the conſequences they have deduced from it as no truer than the theory itſelf. A pretended concentrated acid, a *naked* acid, an uncombined acid, and a phoſphorick acid that produces ſuch wonderful effects, are hypotheſes that are

are not capable of resisting the investigation of reason and experiment, and are unworthy the enlightened age we live in. It is no longer the season to imagine nature; we must consult her. If chemistry has increased the number of our intelligences, the abuse of chemistry has frequently retarded our progress in the sciences. It has frequently led us into errour, and has substituted hypotheses for facts and experiments.

Although bees, and the other insects that are analogous to them as far as relates to the humour they throw out at their sting, are not capable of killing, I think notwithstanding, that if they are not considered as venomous animals in the most received sense, they should at least be considered as animals that secrete in their bodies a small quantity of a matter, which is not destructive simply because it is in too small a quantity. The most active poisons and venoms, such as arsenick, corrosive sublimate, and the venom of the viper, when taken or applied in a very small quantity, not only do not occasion death, but do not even produce a sensible derangement, very far from their effects equalling those that are produced by a large hornet, when it wounds with its sting. These quantities, however, although very small, are capable of killing the smaller species' of animals, whilst more considerable ones are not sufficient to kill those of the larger species'. Hence we see, that the difference entirely consists in the quantity of the venom, and in the different degrees of strength in the animal that receives it, and not in the nature of the venom, which is always the ſame. The venom, for ſo I ſhall call it, of bees, is very active, conſidering the ſmallneſs of its quantity, and we may eaſily judge from the pain and inflammation it excites in an inſtant, that if the doſe of it were increaſed, it would produce the moſt violent derangements, and perhaps even a very ſpeedy death. Nay, I am almoſt inclined to think, that a grain in weight would kill a pigeon in a few ſeconds. The difference that is found betwixt the ſting occaſioned by a bee, and that of a hornet, notwithſtanding the difference in the reſpective quantities of their venom is but very inconſiderable, is very great. The ſame thing may be obſerved of the common ſcorpions of Italy, and thoſe of other countries, as well as of the bite of ſpiders. The larger produce in general the greateſt derangement, and thoſe of Africa, or of Aſia, even occaſion death: all of them, down to the ſmalleſt, poſſeſs a greater or leſs degree of activity.

There are other animals, particularly inſects, which when they bite or ſting, bring on a very violent pain and inflammation, ſo that they may reaſonably be ſuſpected of introducing a cauſtick and venomous humour into the wound. In this number we may reckon ants, which inſinuate into the ſmall wound they make in biting, a very ſharp and poignant humour, which they force from a veſicle ſeated in the hinder part of their body. I ſhall not make a digreſſion here to ſpeak particularly of this humour, becauſe I have treated of it in a very ample

ple way, in a paper of which the object was an examination of the *acids of animals*, &c. and particularly of *the nature of that of ants*, printed in the journal of the Abbé Rosier. I there demonstrated that the humour of ants is a true acid, and that it is in reality the acid of fixed concentrated air, deprived of its elasticity, and rendered liquid.

PART

PART III.

CHAPTER I.

Action of the Venom of the Viper on Parts of an Animal that have been previously bit.

THE subject of this part is the most interesting one that the matter it treats of can present to a philosophick observer.

All the questions that are here discussed become of consequence, since they tend to throw great lights on the nature of venom. The animal economy itself is by their means better explained, and many hypotheses that have been imagined, fall before experiment. It is the touchstone that makes us soon distinguish all that does not belong to nature, all that is the effect of art, of prejudices, and of the imagination; in a word, of man.

Experiment alone may conduct us through the unknown paths of nature, and may lead us to new and unexpected truths. But at the very time that man, profiting by this torch, is making bold strides towards the truth, and soars as if he meant to govern nature herself, she stops him every moment,

ment, and by only discovering herself to him in part, seems afraid of being recollected; she thus continually reminds him of his weakness, and shows him that his hopes are either vain, or confined within very narrow limits.

Man, who assigns to comets the course they are to keep, and who fixes the time that is employed by the light in its progress from the sun to our hemisphere, is not, with all this knowledge, acquainted with the air that surrounds him, or with the fire that warms him. Such is our condition, and such is the state of human science.

The first question that presents itself, after what has hitherto been related, is to know whether the venom of the viper is a poison to all the animals with warm blood. It will be seen in a little time, that this large body of animals has not been separated without design from the other, which comprehends those that have the blood cold. When I say that a substance is venomous to an animal, I mean to express, that it produces in it very violent to all of them. We have ſeen that it has proved ſo to all the ſeven ſpecies' that have hitherto been examined; and I very well recollect that I could not find any animal in Italy, with warm blood, to which the venom of the viper did not prove a real poiſon. I tried it on all the birds I could meet with, and on all the quadrupeds I could procure, provided they were of a moderate ſize; as to the horſe, the camel, and the ox, ſetting aſide their bulk, I could not procure them eaſily for this purpoſe.

We may therefore, I think, conclude with a great deal of reaſon, that the venom of the viper is a poiſon to all the animals with warm blood; that is to ſay, that neither of them is beyond the reach of the effects it uſually produces, when it is introduced into the body in a ſufficient quantity.

The ſecond enquiry, which ſprings immediately from the firſt, is to know whether the venom of the viper is a poiſon to all the animals with cold blood.

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To reply properly to the queftion I have juft propofed, it is certain that all the animals with warm blood exifting on the habitable globe, fhould be bit by vipers. The fubject is not fufficiently interefting to deferve fo long and difficult a labour. However, if the analogy betwixt the different animals with warm blood may be allowed, I am not afraid to advance, that the venom of the viper is a poifon

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The ſecond enquiry, which ſprings immediately from the firſt, is to know whether the venom of the viper is a poiſon to all the animals with cold blood.

It has already been ſeen in a former part of this work, that even the frog, a cold animal, and one very hard to kill, dies in a few hours, if it is bit by the viper. This, however, is not ſufficient to admit a certain concluſion, that all the other animals with cold blood would die in the ſame way. We frequently incur the riſk of being deceived by this method of employing analogies on too narrow and limited a ſcale.

A ſingle ſpecies of animals is not ſufficient to furniſh an analogical argument of any weight.

Had five or ſix hundred kinds of animals with cold blood been examined, and had certain ſymptoms of poiſon been obſerved in all of them after they had been bit, the analogy, in this caſe, would have formed an argument of probability, and we might have been enabled to draw concluſions on this ſubject, not only in regard to animals with warm blood, but likewiſe as to thoſe that have the blood cold.

We can ſcarcely do otherwiſe than ſuſpect that the venom of the viper is innocent to the viper itſelf. This animal, in all the diſeaſes or wounds of its mouth, would otherwiſe run a very great riſk of killing itſelf with its own venom. It is not very unuſual to find vipers with the bag or ſheath of their teeth inflamed and bloody. Small red ſpots are frequently obſerved in the mouth of this animal when it bites, and it is beſides eaſy to conceive, that if it ſhould be bit in the mouth by any other animal, its own venom would prove deſtructive to it, if its particular nature did not guard it againſt ſuch an accident.

The venom of the viper is conſtantly ſecreted and laid up, in the ſpongy gland. This gland has its canal continually open, through which the ſuperfluous venom that cannot be contained in the gland, is forced to ſhed itſelf into the viper's mouth.

However, it is eaſy to have recourſe to experiment. In the firſt part of this work a detail may be found of a great number of trials I made on this ſubject, and from which it reſults, that the venom of of the viper is not a poison to vipers, but that, on the other hand, it is altogether innocent to them. I was desirous of repeating several of these experiments over again, and out of the great number, which brevity obliges me to omit, I think it sufficient to relate a single one.

After having enraged a viper very much, I forced it to bite itself several times in the part towards its tail; it, however suffered nothing from this, although it had certainly forced its teeth well into the part. I repeated this experiment on three other vipers, with the same success. It is therefore very certain, that the venom, or bite, of the viper, is entirely innocent to this animal when it bites itself, and it likewise is when one viper bites another.

But this very singular exception is not confined to the viper. There are other animals to which this venom is innocent, and others again in which, although they were small, one or two vipers are scarcely capable of producing any sensible change. I have mentioned some of these *cold animals (a)* in the first part of this treatise, but to come at the number of them, the experiments should be extended to other species' I could not at that time procure, and on which I thought it superfluous to make this trial.

If it is altogether extraordinary, that the same matter is entirely innocent to several species' of

(a) Animals with cold blood.

animals, and that it is mortal to an infinity of others, it is much more ſurpriſing, and at the ſame time more difficult to conceive, how, and by what principles, it happens, that an inſipid gum, as far as we can perceive, excites the moſt violent diſorders in ſo many very large animals, and that it does not bring about the ſmalleſt change in others that are incomparably ſmaller and weaker.

The known diſtinction of animals with cold, and animals with warm blood, which is only founded on a greater or leſs degree of heat, and on ſome other trivial difference in the circulation of humours, is of no uſe in the preſent caſe, ſince there are certain animals with cold blood that die of the venom, and others again that are not at all acted on by it.

If a compariſon is formed betwixt two cold animals, one that dies of the diſeaſe of the venom, and the other that ſurvives its action, they will be found to poſſeſs the ſame organs, the ſame circulation, an equal tenaciouſneſs of life, and, in a word, to the eyes of the obſerver, they will both of them appear perfectly alike.

What is it, then, that cauſes this matter which flows from the viper's tooth to be a poiſon to one, and not to the other? We are not only entirely ignorant of this, but it appears that we are likely always to remain ſo. To obtain ſuch a knowledge, it would be neceſſary to be acquainted with the moſt hidden nature of this extraordinary animal gum. It would be neceſſary to penetrate into the moſt internal and latent ſubſtance of the ſolids and fluids

fluids of animals with cold blood, to know the mechanism of their organization, and to comprehend perfectly the principle of life. We might then reply to all that could be asked on this head. But how is it possible to acquire so extensive an information, whilst the activity and penetrability of our organs are so limited and confined?

But if we are not permitted to know what this very active principle of the venom of the viper is, which when it is introduced into a living animal causes its death; we are allowed, however, to enquire into the quantity of this venom, that is necessary to kill an animal of a certain size. This enquiry, very curious in itself, cannot but be of some use in the practice of medicine, particularly in cautioning us against thinking the danger greater than it really is, when any one of our own species has the misfortune to be bit by this animal.

To be able to speak with some degree of precision, in this research, it was proper to begin by determining very small quantities of venom, and by introducing them without loss into the substance of the body of a living animal. It was likewise expedient to operate on very small animals, that would die soon and to a certainty, to the end that the consequences might be less equivocal. It is true, that by an endless multiplication of experiments, the same consequences might at length be obtained from large animals; but a longer time, and greater conveniences would be required, and one ought besides to be perſuaded of the importance of the undertaking.

In the following experiments, I made choice of ſparrows and young pigeons, knowing them by experience to be readily killed by the venom.

To determine ſmall known quantities of venom, I began by taking four grains in weight of the venom of the viper, and mixing with it eight grains of diſtilled water. I then, with a ſmall bruſh, ſpread it equally over a ſquare inch of thin paper. This may be done with a ſufficient degree of eaſe and preciſion to exclude any conſiderable errour, and indeed I found that the halves and quarters of the ſquare inch of paper were of the ſame weight when dried.

I cut this paper in two, and again divided one of the halves, continuing in this way till I had made ſix diviſions, reckoning the firſt. I then did the ſame with the other half, that I might have two pieces of the ſame ſize, and of each ſize, inſtead of one.

I ſtripped the muſcles of the leg, in ten ſparrows, of the ſkin, and bound upon them the ten bits of paper I have mentioned. The conſequences, beginning with the larger bits of paper, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, were as follows. Of the two ſparrows to which the papers marked $\frac{1}{4}$ were applied, one died at the end of fifteen minutes, the other not till the end of thirty-five. One of thoſe with the papers marked $\frac{1}{8}$ died at the end of an hour, the other ſurvived. One of thoſe with the papers marked $\frac{1}{16}$ died

died at the end of two hours, the other recovered. One of the two with the papers marked $\frac{1}{32}$ died at the end of two hours, the other at the end of five. And of the two with thofe marked $\frac{1}{64}$, one died at the end of three hours, the other at the end of feven minutes.

On repeating this experiment, the confequences were ftill more irregular. I therefore abandoned this method, as altogether infufficient and delufory. This is probably owing to the paper, which, when put in contact with the humours of the animal, may not allow itfelf to be either equally or entirely deprived of the venom that adheres to it. This obliged me to have recourfe to another method, which is perhaps lefs exact in determining the precife quantity of venom, but which has afforded me confequences as conftant and as uniform as can be expected in fo difficult an undertaking.

This is the method I employed :—I took a given quantity of venom, for example three grains, and fpread it over a bit of glafs, in fuch a way that it occupied a determinate fpace of a circular form.

The venom in the centre was not more than a quarter of a line in depth.

I procured a fmall capillary glafs tube, which terminated in a fmall fcoop of about half a line in diameter. I plunged this fmall fcoop vertically into the centre of the venom, and drew it out in the fame direction.

To determine the quantity of venom that adhered to the fmall fcoop, and to know at the fame time,

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whether this quantity would be conſtantly the ſame, I put the three grains of venom, laid on the bit of glaſs, into a very nice ballance, and plunged the ſmall glaſs ſcoop ten times ſucceſſively into the liquor, taking care to wipe the ſcoop well every time. After the ten plunges, I found the equilibrium to be loſt, and that about the $\frac{1}{100}$ part of a grain of venom was deficient. I now plunged the ſmall glaſs ſcoop ten other times ſucceſſively into the venom, and the ballance having again loſt its equilibrium, found that the venom was diminiſhed about the $\frac{2}{100}$ part of a grain. With a little practice, one can make this experiment in leſs than two minutes, and in that ſpace the three grains of venom are not ſenſibly diminiſhed in weight by the natural evaporation, as I have aſſured myſelf by trial. I cannot take upon myſelf to ſay, that all the quantities are here rigorouſly the ſame; nay I agree that, in repeating this experiment ſeveral times, a ſenſible difference, which I have indeed met with myſelf, cannot fail to occur: but all theſe differences taken together can make but a very trifling variation in the quantity of venom that adheres each time to the ſmall ſcoop. On the whole, I can lay it down as an eſtabliſhed rule, that the ſmall glaſs ſcoop plunged perpendicularly into the venom, in the way I have deſcribed, carries away with it about the $\frac{1}{1000}$ part of a grain of the venom of the viper.

I laid bare a portion of the right leg of a ſparrow, and made a ſmall longitudinal inciſion into the muſcles with a lancet. Into this inciſion I introduced,

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at the very inſtant, the ſmall ſcoop armed with venom, and kept it in this ſituation for thirty ſeconds. The ſparrow died at the end of two hours, with a lividneſs of its leg.

I repeated this experiment on ſix other ſparrows, exactly obſerving the ſame circumſtances. They all died, one after the other, at the end of the following times, expreſſed in hours, 2, 2, 3, 4, 5, 7.

I again repeated it in the ſame way on twelve other ſparrows, and the conſequences were ſtill more irregular than thoſe of the preceding experiment. One of the ſparrows died at the end of four minutes, another at the end of three days, and another at the end of five. The ſymptoms of the diſeaſe were, notwithſtanding, indubitable in all the three. The nine others all died at the end of the times expreſſed by the following numbers, which denote ſo many hours; that is to ſay, 2, 3, 3, 5, 6, 9, 10, 12.

The firſt conſequences ſhow, that the quantity of venom I have mentioned is ſufficient to kill an animal of the ſize of a ſparrow, but that it produces in theſe animals very unequal effects, and a diſeaſe of greater or leſs violence. An animal that dies at the end of three minutes, and another perfectly ſimilar to the firſt, that does not die till the end of five days, prove that the diſeaſe in each of them has been very diſtinct. But ſuppoſing the quantities of venom which were introduced to have been equal, and that the inciſions were ſo too, a little more, or a little leſs, blood, oozing from the inciſed veſſels, might have cauſed all this difference, ſince it might have

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occasioned a greater or less quantity of venom to enter into the circulation of the humours, or, to express it still better, into the animal itself.

I wished to see whether I could bring on a more speedy death by doubling the quantity of venom; and being at a loss for a certain method of collecting this double quantity together, I made two incisions instead of one, and introduced the scoop I have mentioned into each. Twelve sparrows on which I made this experiment, all died, but at very different intervals. One died at the end of three minutes, another at the end of twenty-seven, and a third at the end of forty; the others at the end of the hours expressed by the following numbers, 1, 1, 2, 2, 2, 3, 3, 5, 6.

The experiments I made on sparrows, and the method of introducing a given quantity of venom into the incision in their muscles, have occasioned me to make a very interesting observation. I usually kept the small scoop in the incision for about twenty seconds, and observed that the lips of the wound became livid at the end of a certain time. I regarded this symptom as a sure mark of the communication of the disease, and it will be seen by and by that I was not mistaken in this conjecture.

I was desirous of seeing what effect would result from a certain quantity of venom, applied to an animal larger than a sparrow.

I stripped a part of a pigeon's leg of the skin, in such a way that the muscles beneath were entirely bare. Having made the usual incision, I introduced

duced the small scoop, which had beed first plunged into the venom, and kept it there till I saw the lips of the incised part become livid; this happened, as in the sparrows, in the space of about twenty seconds. At the end of half an hour the leg became slightly livid, but neither seemed swelled nor hard. The pigeon neither died, nor suffered in any sensible degree.

I repeated this experiment on six other pigeons, exactly observing the same circumstances. One of them had not the smallest symptom of disease, neither did the incision become livid, notwithstanding I kept the small scoop in it for upwards of a minute. Four others had symptoms of the disease of the venom, and two of these did not recover till after forty hours had elapsed. The sixth, like the first, was not at all diseased; the incision, however, I made in its leg, bled at the time I introduced the venom.

I repeated this experiment, with the same circumstances, on six other pigeons. One of them died at the end of six hours. Three had all the symptoms of the disease of the venom, and did not recover till the third day. Two others had not any symptom of the disease. I think it proper to observe here, that the incisions in the leg, in these two last, bled very sensibly when I introduced the venom. This shows that the blood which flows from the vessels, may prevent the venom from entering them, or from remaining in them after it has entered, I repeated this experiment over again on twelve pigeons, one of which died at the end of ten hours. Two others were exceedingly ill. The other nine had no ſenſible complaint.

Theſe new experiments ſhow, that the quantity of venom which uſually kills a ſparrow is not fatal to a pigeon. We ſee, however, at the ſame time, that a caſe may occur, in which ſuch a quantity of venom is introduced as is capable of killing a pigeon, notwithſtanding that the quantity employed in the experiment would ſcarcely have been ſufficient, generally ſpeaking, to kill a ſparrow.

I was deſirous of trying on pigeons, as I had done on ſparrows, what would be the effect of two inciſions, and a double quantity of venom.

Having laid bare the muſcles of a pigeon's leg, as uſual, I made two ſmall inciſions into them, introducing into each, the ſmall ſcoop armed with venom, in the accuſtomed way. The livid ſpot appeared at each inciſion, and almoſt the whole of the leg became livid, and remained in that ſtate for two days, at the end of which time the animal was perfectly recovered.

I repeated this experiment on twelve pigeons, and the conſequences were ſomewhat various. Two of the pigeons died at the end of three days. The others had all a lividneſs of their legs, and all recovered notwithſtanding.

On repeating this experiment on twelve other pigeons, four of them died; one at the end of ſix hours; another at the end of twenty; and the two others

others not till the fifth day. All the others had the diſeaſe of the venom, but recovered.

Inſtead of making two inciſions only, I tried the experiment on twelve other pigeons, by making four inciſions at the ſide of each other. Nine of theſe pigeons died; one in ten minutes, two in an hour, two in two hours, and three in five hours. The other three had the diſeaſe of the venom, and their legs became livid, ſwelled, and hard.

What is the Quantity of Venom required to kill an Animal?

We may, I think, from the above experiments, determine with ſome probability, the quantity of venom it requires to kill an animal. This queſtion already begins to become important to us on our own accounts, ſince we may at length be enabled to flatter ourſelves, that the bite of the viper is not ſo dangerous as we have been hitherto taught to imagine it to be.

We have juſt ſeen that the $\frac{1}{1000}$ part of a grain of venom, introduced immediately into the muſcle by an inciſion, may be a ſufficient quantity to kill a ſparrow, although this animal does not always die in conſequence of the introduction of ſuch a quantity; and that it requires about four times as much to kill a pigeon. We may even ſuppoſe it to require about ſix times as much to kill the laſt animal to a certainty.

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The ſparrows on which I made my experiments weighed ſomewhat leſs than an ounce each, and the pigeons ſomewhat more than ſix ounces each. Now let us ſuppoſe that ſparrows weigh exactly an ounce, and pigeons exactly ſix. The quantity of venom it will require to kill a large animal, an ox for inſtance, ſuppoſing it to weigh 750lb. will be about twelve grains; and it will require nearly two grains and a half to kill a man, ſuppoſing him to weigh the fifth part of what an ox weighs, that is to ſay 150 lb.

It is true that this calculation takes for granted ſome new hypotheſes more or leſs probable, but of which neither is unlikely. A ſufficient number of experiments are wanted, to render them either abſolute truths, or ſuſceptible of ſome reſtrictions.

The firſt hypotheſis ſuppoſed here is, that the venom of the viper acts on an animal in proportion to its quantity. There is nothing unreaſonable in believing this to be the caſe, ſince if a very ſmall portion of venom is capable of deranging the economy of an animal *to a certain point*, a greater doſe of it ought to produce a greater derangement, a more violent diſeaſe. Beſides we have ſeen, that animals bit ſeveral times by one viper, or by ſeveral, die ſooner than thoſe that are only bit once by a ſingle viper; and we know that a viper which bites ſeveral times, introduces freſh venom into the part at each bite.

The ſecond hypotheſis is, that the diſorder produced in the animal economy by the venom of the viper,

viper, is leſs in proportion, or rather that the power of the animal to reſiſt the action of the venom, is greater in proportion, as the animal is larger. This is generally ſo, although there may be exceptions to this law, that may prevent its being rigorouſly the caſe.

The third hypotheſis is, that from the effects produced in an animal of one ſpecies, we may argue as to the effects produced in an animal of another ſpecies; that is to ſay, from birds to quadrupeds. This argument is drawn from a ſimple analogy; but this analogy is at the ſame time formed betwixt animals with warm blood, and it may therefore be deemed of ſome weight.

Now granting that a viper of a middle ſize has in its veſicles two grains in weight of venom, it will require the venom of ſix vipers to kill an ox, and nearly of two to kill a man.

But if we reflect that a viper which bites, does not leave itſelf without venom; that at each bite, at leaſt for the firſt three or four, it may bring about the death of an animal with almoſt the ſame facility; it will not appear altogether unlikely, that it may perhaps require twenty vipers, each biting only once, to kill an ox, and five or ſix, with the ſame reſtriction, to kill a man.

CHAPTER II.

Of the Time it requires for the Effects of the Venom of the Viper to become ſenſible.

A QUANTITY of the venom of the viper which ſcarcely weighs the $\frac{1}{500}$ part of a grain, produces, on being introduced into the body of a ſmall animal, ſo violent a diſeaſe, that death follows in a few minutes. It is therefore very clear, that it muſt poſſeſs a great degree of activity, and that its effects muſt be both ſudden and powerful. I have advanced in ſeveral parts of this work, that the venom of the viper renders the parts that have been bit, in animals, and that almoſt in an inſtant, incapable of exerciſing their uſual functions. I am at leaſt certain, that I have obſerved this effect in ſeveral that I have had bit. It has been ſeen that the venomed part becomes livid after it has received the bite, but that this does not happen till within a certain ſpace. The wounded parts ſoon become ſwelled and painful, and the adipoſe membrane is ſhortly after filled with a black and diſſolved humour; whilſt the blood that remains in the veſſels is black and coagulated.

One would naturally ſuppoſe, that the action of this venom on the organs of an animal is momentaneous, and that it is not different from that which takes place when two ſubſtances of different natures are blended together, and of which chemiſtry furniſhes us a thouſand examples.

Deſirous of purſuing this idea, and flattered with the hope of diſcovering ſome effect, or ſome particular, that might be ſerviceable to my preſent reſearches, I formed a new plan of experiments.

My firſt trials had for their principal aim, the obſerving of the changes the venom of the viper would produce, when introduced into a part cut from an animal, but ſtill warm and palpitating.

Experiments on the Limbs of an Animal, recently ſeparated from the Body.

At the very inſtant the part was cut off, I had it bit by a viper, ſo that when the experiment ſucceeded well, as it frequently did, there could ſcarcely paſs a ſecond betwixt the amputation, and the bite.

I made choice of young pigeons for this experiment, becauſe I had obſerved in theſe animals, that the venom of the viper very ſpeedily produces a livid ſpot, in the part of the muſcles through which it has introduced itſelf.

To make this experiment, a perſon holds the animal in one hand, and in the other a pair of open ſciſſars, betwixt which is the leg of the pigeon to be cut off. Another perſon holds this leg in one hand,

and in the other the head of a viper with the teeth laid bare, and forces these teeth deeply into the muscles of the leg, the instant it is separated from the body. The head of the viper has been separated from the body some minutes before, and, to make the experiment more commodiously, has been deprived of the lower jaw. This head is still alive, *and the smallest compression that can be made*, is sufficient to make it of itself draw its teeth from out of their bag or sheath, and force them into the parts that are made to approach it.

It is certain that there never passed, in any one of the dozen experiments I first made, more than three seconds betwixt the amputation and the bite; several of these experiments were made in a single second, or precisely at the very instant of cutting off the limb.

In some of the legs that were cut off, the venom was seen surrounding the holes made by the teeth; in others it was seen flowing out of the holes; and in others no venom was to be perceived exteriourly. On examining the muscles bit in this way by the viper, I could discover no sign of a communicated disease, neither could I observe any supervening lividity about the holes. The blood continued in a fluid state in the veins and arteries.

These legs, which were yet warm and palpitating, and which bled, being kept for minutes, and even for hours, afforded me nothing further that was worthy of observation.

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I repeated this experiment on the bared, and almost pale and tranſparent, muſcles of twelve frogs. The event was exactly the ſame; there was not the ſmalleſt apparent ſymptom of communicated diſeaſe.

I repeated theſe experiments afreſh, as well on pigeons as frogs, having the amputated legs bit by freſh vipers, previouſly well irritated. The reſult was the ſame in all.

I got ready the legs of pigeons and frogs, and as ſoon as they were cut off, wounded them with teeth taken from the head of a dried viper. The ſymptoms that reſulted from theſe ſimple mechanical wounds, were not ſenſibly different from thoſe of the wounds into which the venom had been introduced, although made within the ſame time.

It ſeems then to be an eſtabliſhed truth, that the venom of the viper produces no ſenſible change in parts ſeparated from an animal, notwithſtanding they are yet in a ſtate of palpitation. This truth appears to me of the higheſt importance in eſtabliſhing the theory of the venom, and deſerving of the utmoſt attention.

In the firſt place, it is certain, as I have particularly aſſured myſelf, that there ſtill ſubſiſts in the amputated leg, for upwards of twenty ſeconds, the degree of heat it had before it was cut off. A perfect irritability is ſtill retained in the muſcles, which continue to move, even for whole minutes.

The arterial and venous fluids ſtill remain in the part, at leaſt in a great meaſure, and they ſtill keep in motion there during ſome time.

Thoſe who have examined the circulation of the blood in cold adimals, know that this fluid ſtill continues to circulate for a long time, in the parts of theſe animals that have been cut off.

Notwithſtanding this, the venom ſeems to be entirely *inactive and innocent, in all the caſes I have* related above, although every thing ſubſiſts in the part bitten; that is to ſay, humours, arteries, veins, nerves, irritability, and motion.

This circumſtance appeared to me ſo new, and at the ſame time ſo paradoxical, that I was deſirous of trying a new kind of experiments, in which the amputated part of the animal ſhould approach ſtill nearer to its natural ſtate, at the moment of being bit by the viper.

I divided the muſcles, nerves, and blood-veſſels, of a pigeon's leg with a ſharp knife, and left the bone untouched. The inciſion was made at the beginning of the *tibia*, immediately below the *femur*. At the ſame inſtant, I had the fleſh beneath the inciſion bit by a viper.

But in ſpite of all this precaution, I could not perceive, either that the muſcles were rendered livid, or that the diſeaſe of the venom had been communicated to them.

I repeated this experiment on eleven other pigeons, and, although I kept them alive a great while

while, could never obſerve any ſymptom which indicated in the ſmalleſt degree, that they would have died in conſequence of the operation.

We may therefore regard this circumſtance, however paradoxical it may appear, as beyond all kind of doubt. On obſerving it, I began to flatter myſelf that ſome truth in phyſicks, relating to the mechaniſm of the venom of the viper, might be drawn from it; and that we might likewiſe gather from it ſome principle that would be uſeful to the comprehending of animal motions. In the firſt place it is certain, that the venom, as far as can be obſerved, does not act by a ſimple mechanical motion, or by a ſimple mixture of fluids; ſince, if that were the caſe, as the muſcles were provided with both the accuſtomed humours and motions, it ought to have produced its ordinary effects in the inſtances related above. Neither does its action ſeem to depend on an effect in chemiſtry, ſuch as is brought about, for example, by the contact of an acid with an alkali; and preciſely for this reaſon, that no effect is produced, although the venom is in contact with the humours of the leg of the animal.

Experiments to determine the Time the Venom of the Viper requires to produce its Effects, after it is introduced into a Wound.

To have excluded an hypothesis of any kind on the mode of action of the venom of the viper, may without doubt be a step towards the truth; this is not, however, sufficient to instruct us how, and on what part of an animal, it acts. My curiosity was therefore rather excited than satisfied, and I immediately began to consider how I ought to pursue my experiments.

I reflected that, if the venom of the viper produces no effect on a detached part of an animal, however near it may be to its natural state, it is certain that it produces very violent, and very sudden effects, on parts that have not been yet separated.

The first enquiry that naturally presented itself, was to know whether this venom produces its usual effects, or rather, whether it communicates its disease to the part bit, at the instant, or not till the end of a certain time.

With this view, I enraged a large viper, and made it bite the leg of a pigeon twice, the second bite instantly succeeding the first. I immediately cut off the leg, and examined it with attention. It was very easy to distinguish in it the holes made by the teeth; but although I kept it a great while, I could never discover any mark of disease or lividness.

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I had six other pigeons bit in the same way, each repeatedly by a single viper, and almost immediately after cut off the leg that had been bit. There was very little difference in the time of my doing this, to all the six. As there appeared no symptom of disease in the part, it follows as an incontestible truth, that the venom of the viper does not act instantly on the part that has received the bite, but that it requires a certain time for this purpose; since it is well known, that the parts wounded by this animal, ultimately become livid and swelled.

The space of time it requires to act, was to be determined by experiment.

For this purpose I had a dozen pigeons bit, each once by a distinct viper. I measured with a watch the seconds that passed betwixt the bite of the viper, and the succeeding amputation, and managed in such a way, that the intervals of time increased in a ratio of ten seconds; so that the legs were cut off at the end of 10, 20, 30, 40, 50, 60, 70, 80, 90, 100, 110, 120, seconds after they had been bit. I had previously stripped the skin from the muscles, without cutting or lacerating them; and wiped away the blood that flowed from them after the incision, with a wet sponge. In the leg of ten seconds, I could perceive no change, nor any livid spot; but in that of twenty there were symptoms of disease. I conceived, at least, that I saw an incipient lividness about the holes made by the teeth of the viper. In all the others, the disease of the venom was ſo decidedly apparent, that not the ſmalleſt doubt could remain on the occaſion.

I repeated this experiment on twelve other pigeons; but inſtead of making the intervals of time betwixt the cutting off of the legs in an increaſed ratio of ten ſeconds, I made them in an increaſed ratio of ſeven.

The leg cut off after ſeven ſeconds had no appearance of diſeaſe. That of fourteen was in the ſame ſound ſtate; but all the others, beginning at that of twenty-one, had marks of lividneſs about them. The livid ſpots were in general greater in proportion to the delay that was obſerved in the amputation. This rule was, however, not without ſome exceptions, occaſioned by a great variety in the circumſtances, which, as any one may readily conceive, are never exactly the ſame.

To obtain a more preciſe information of the time in which the diſeaſe is communicated, I had twelve other pigeons bit, and in cutting off their legs obſerved a ratio of from five to ſix ſeconds, beginning with five.

There was ſome doubt in that of twenty ſeconds, but the diſeaſe was certain in that of twenty-five. Thoſe of five, ten, and fifteen, were without any marks of diſeaſe, or the ſmalleſt livid appearance.

A certain concluſion may, I think, be drawn from theſe repeated experiments, that the action of the venom of the viper on the part bitten is not inſtantaneous, but that it requires a certain time for its effects to become ſenſible in that part.

The ſpace of time that elapſes before the venom gives manifeſt tokens of the diſeaſe it produces, is from fifteen to twenty ſeconds, or thereabouts.

We muſt naturally conceive that this time varies in different animals, and that the diſeaſe diſcovers itſelf ſooner in ſome, and later in others. The peculiar conſtitution of the animal, and its ſize, ought to make a ſenſible variation, and to modify in a greater or leſs degree, the action of the venom of the viper.

But it is ſufficient for us to know, that this venom does not operate inſtantaneouſly, and to be in ſome meaſure acquainted with the time it requires in acting on certain ſpecies of animals. Theſe *data* open the way to further reſearches.

Is it by the ſimple local Diſeaſe, or by a Diſorder excited in ſome of the moſt eſſential Principles of Life, that the Death of the Animals bit by the Viper is occaſioned?

The firſt enquiry that preſents itſelf, and which is very important, is to know whether the venom of the viper produces a diſeaſe, independant of that which diſcovers itſelf in the part of the animal that has been bit; that is to ſay, whether it deranges the animal economy in ſuch a way, after a bite has been received in any particular part, that the animal may die in conſequence of ſuch a derangement alone.

I have ſeen animals, even pretty large ones, ſuch as dogs, fall proſtrate on being bit by a viper, without

out being able to ſtir for ſome time, and with a ſcarcely ſenſible reſpiration. I have ſeen others void their urine and excrements at the very inſtant, as if their ſphincters had become paralytick at the moment of their being bit. It is not a rare caſe to obſerve men fall into a ſwoon almoſt immediately after they have received a bite from a viper. But the agitation of certain animals, and the fear of others, may contribute a good deal to the producing of theſe effects; and ſince it is invariably the caſe that there is ſtill a communication of organs, and a continuation of humours, betwixt the animal and the part that has been bit, we may miſtake for a communication of diſeaſe, what is no more than a ſimple correſpondence betwixt the part bit, and the other parts of the animal. After all, it muſt be left to experiment to decide on this, as well as on every other point.

I had a pigeon's leg bit repeatedly by a viper, and cut off the part ſoon after at one blow, at the articulation of the femur with the tibia.

The leg, when cut off, had all the ſymptoms of diſeaſe; the holes made by the teeth of the viper were livid, and the uſual ſmall ſpots were diſtinguiſhed. The pigeon died at the end of four minutes.

I had remarked, in making the experiments related above, that the amputation of the leg is not mortal to pigeons; at leaſt, I found ſeveral that were deprived of that part, ſtill living at the end of ſeveral hours.

To

To prevent the following experiments from being in the least equivocal, I cut off in the first place the legs of six pigeons, a leg from each, to serve by way of comparison to the others.

I had twelve pigeons bit successively, some once, others several times. Betwixt the bite and amputation, there did not elapse in any one of them, less than one minute, and more than two. All the pigeons died, and the times of their death are expressed by the following numbers, denoting so many minutes, 2, 2, 3, 4, 4, 4, 7, 7, 10, 12, 12, 14.

Of the six pigeons mentioned above, the legs of which I had cut off without having them bit, neither died, nor did either of them appear to have suffered in any sensible degree. I let them live eight days, during which time they fed as usual, and they then served me for other purposes.

These first experiments show, and that in an unquestionable way, that a mortal disease is communicated to the animal in a very little time; and that it dies, independently of the local disease, by an interiour derangement, which the venom has already communicated to its whole system.

This new discovery was of too much importance not to require still further experiments.

I had twenty-four pigeons bit by as many vipers, and at the end of a minute, or with very little variation, if any, from that time, cut from each of them the leg that had been bit. They all died, at the times expressed by the following numbers, denoting

ing ſo many minutes, 3, 3, 3, 4, 4, 5, 5, 7, 7, 7, 7, 9, 9, 10, 10, 10, 10, 10, 12, 12, 13, 13, 14, 20.

It is certain, as I have ſince aſſured myſelf by freſh experiments, that the amputation of the leg is not only not mortal to pigeons, but that it does not ſeem to be productive in them of any kind of complaint. It is equally certain, as we ſee by the experiments related above, that the pigeons bit in the leg by the viper die, notwithſtanding the part is removed, provided the amputation is delayed till the end of a certain time. It is therefore a demonſtrated truth, that the venom of the viper excites in an animal that has been bit, a diſeaſe independent of the part bitten; and that the animal dies of this ſecond diſeaſe, and not of the local diſeaſe of the leg; ſince the latter ſubſiſts no longer when the part is cut off, which does not however prevent the death of the animal. This at leaſt has unqueſtionably been the caſe in the pigeons on which the above experiments have been made. But what is ſtill more extraordinary, is that theſe animals die much ſooner when the venomed leg has been removed, than when it has not. We have already ſeen, that in pigeons the ſimple amputation of the leg is of no conſequence, and it is therefore very ſurprizing that the local diſeaſe, which becomes extremely violent, being removed, this circumſtance, inſtead of retarding the death of the animal, rather accelerates it. This would lead one to ſupect that the part bitten ſerves to divert the vitiated humours in the animal, and that it is, if I may ſo expreſs myſelf, a diſeaſe excited by the animal

animal itself, or rather by that principle which exists in a living animal, and which, agreeably to the opinions of Hippocrates and Sydenham, seems to preside over its life, and to be the moderator of it.

Is the internal Derangement which the Venom of the Viper causes in Animals that are bit, produced at the Instant of the Bite, or some Time after?

What is now of the greatest consequence to be known, is whether the disease of the venom is communicated to the animal instantly, or not, on the introduction of the venom itself.

We have already seen what the local malady is, and what are the symptoms of it; the time has likewise been determined that the venom requires to produce any sensible effect on the part bitten. The internal disease is that which becomes universal in the animal, and which even occasion its death, independently of the external and local disease just mentioned.

To determine whether the internal disease is instantaneous, or not, I made the following experiments.

I had a dozen pigeons bit in the leg by as many vipers, and cut off the part immediately after, in each of them, at a single blow. There was not more than three or four seconds betwixt the bite and

and the amputation. Neither of the pigeons died, nor had any ſymptom of diſeaſe.

I repeated this experiment on twelve other pigeons, which were likewiſe bit and mutilated within the ſpace of three or four ſeconds. Neither of them died, nor had the leaſt apparent illneſs.

It is therefore certain that the venom of the viper does not produce the internal diſeaſe inſtantaneouſly, but that it requires a certain time to communicate itſelf to the animal. We are now to enquire what that time is. Is it the ſame as that which it requires to produce the external diſeaſe?—If this is the caſe, by what common principle do theſe two effects go hand in hand together? And why may not the external diſeaſe be anteriour to the internal one? The venom begins by touching the local part, and previouſly mixes with the humours of that part.

But let us proceed to experiment.——I had a dozen pigeons bit, each once in the leg by a diſtinct viper, and cut the leg from each, with an interval of five ſeconds betwixt the reſpective amputations. The firſt leg was taken off at the end of five ſeconds. The others at the times expreſſed in ſeconds by the following numbers, 10, 15, 20, 25, 30, 35, 40, 45, 50, 55, 60.

That of ſixty ſeconds died at the end of ſeven minutes; that of fifty-five at the end of ſix; that of fifty at the end of ſeven; that of forty-five at the end of ſix; that of forty at the end of twenty; that of thirty-five at the end of an hour; that of thirty at the end of three hours; and that of of twenty-five at the end of ten hours. Those of twenty, fifteen, ten, and five seconds, neither died, nor seemed to suffer in any sensible degree.

However irregular the time of death in these animals may appear, we, notwithstanding, remark, in one particular sense, a degree of regularity. Neither of the pigeons died on which the amputation had been made before twenty-five seconds; and neither of those recovered, the legs of which had been cut off, on or after twenty-five seconds.

We likewise observe, that in general the pigeons on which the amputation was made the latest, were those that the soonest fell victims to the disease of the venom.

I was desirous of repeating this experiment of twelve other pigeons, observing the same intervals of time. The consequences were it is true somewhat different; but there was still a great regularity betwixt the time of the amputations, and that of the deaths.

The pigeons on which the operation was made at the end of 5, 10, 15, seconds, recovered. That of twenty died at the end of seven minutes, and that of twenty-five survived. Those of thirty, thirty-five, forty, forty-five, fifty, fifty-five, and sixty, all died; and the times of their death, beginning with that of sixty, and going back, are 5, 10, 7, 7, 6, 40, minutes, and eight hours.

Here again we observe, that neither of the pigeons the leg of which was amputated before twenty seconds, died; and that only one of those

lived

lived in which the operation was performed at twenty seconds, or afterwards. They in general died the sooner, in propoition to the delay observed in the cutting off of the leg.

The pigeon that died, notwithstanding it was mutilated as soon as twenty seconds, made me suspect (as in the former cases, neither of those that had been submitted to the operation at this period died) that the size of the viper, and still more the circumstance of its having been irritated, might, partly, however, have occasioned this difference.

To be certain of this, I had two pigeons, perfectly alike in size, bit, one of them by a large well-enraged viper, the other by a small one, that was not irritated. I cut off a leg from each pigeon at the end of twenty seconds. The first died at the end of five minutes; the second had not the smallest symptom of disease.

This experiment convinced me, that the time in which the internal disease is communicated, may be greater or less, according to the different circumstances, that the vipers, and the pigeons or other animals, may be in at the time, and according to the manner of biting.

To assure myself still more fully of this circumstance, I had two other pigeons bit, one by a very large viper, the other by a very small one. The first was enraged, and hissed at the time of biting. The other was made to bite, without being provoked in the least. The amputation of the leg was made in both of them at the end of fifteen seconds.

ſeconds. The firſt pigeon died at the end of nine minutes; the other had not the ſmalleſt complaint.

It follows from all that has been obſerved, that it requires a certain time for the venom of the viper to be communicated to an animal, and that this time is ſomewhere betwixt fifteen and twenty ſeconds.

It has been ſeen above, that it requires pretty much the ſame time for the external diſeaſe to be communicated to the part bitten; and hence it appears, that theſe two diſeaſes accompany each other, and that the venom produces both within the ſame ſpace of time.

This agreement of diſeaſes and effects, which has thus far appeared ſo very regular and conſtant, fully deſerved to be confirmed by a continuation of experiments, ſtill more preciſe and ſimple than the preceding ones.

Of the Symptoms which characterize the Diſeaſe.

The difficulty conſiſts in determining the death or diſeaſe of the animal by the ſymptoms that appear in the part bitten; and *vice verſa*, in determining the ſymptoms of the part bitten by the death of the animal. On one hand, theſe ſymptoms, as has been remarked before, are neither equivocal nor difficult to obſerve; and on the other, the death of the animal, in consequence of the introduction of the venom, is a truth established by experiment.

It would be long and tiresome to enter into a detail here, of the distinct consequences of the experiments, more than eighty in number, that I made with this view. It will be sufficient for me to say in general, that neither of the animals on which they were made (except one indeed, the case of which was doubtful) died without manifest symptoms of the disease of the venom in the part bitten; and that (except in five instances only) I observed in *all* the others, *that* when the animal recovered, there was no symptom of the local disease of the venom. The few exceptions that occurred, which might have depended on a thousand accidental causes, do not render the law that these two diseases observe, nor the constancy with which they are at the same precise point of time excited in the animal, less certain.

This agreement, so constantly observed, made me suspect still more the existence of a certain principle in the animal machine, which presides and watches over life.

Scarcely has an animal encountered any thing that troubles and deranges the functions of its life, than a new force seems at the same time to be excited, and to be, as it were, awakened, which endeavours strenuously to keep the cause of death from the organs that are the most essential to life, and to carry the morbifick matter to the part that is the most disposed to receive it, whether on account

count of wounds that have previoufly been made in it, or of humours that are extravafated by the rupture and laceration of veffels.

The venom of the viper occupies but a very fmall fpace in the leg of an animal, and may, if one wifhes it, be driven into fo narrow a compafs, as fcarcely to occupy the hundredth part of a line in fuperficies, without any phyfical or fenfible folidity.

Now granting the fuppofition that this fmall quantity of venom is entirely abforbed, and carried into the torrent of the circulation, it ought to be equally diftributed in the mafs of humours of the animal, to the fize of which, or to its veffels, the diftribution of it ought to be proportioned.

But it is quite the contrary; the humours and the blood are carried tumultuoufly, and in hafte, to the part that has been bit, and the blood not only collects about the fimple mechanical wound made by the tooth, but fpreads to a great diftance, and changing its colour, pours in torrents into the adipofe membrane, whilft another part of this fluid penetrates in a diffolved ftate through the coats of the veffels.

It therefore appears, that all the efforts made by an animal which has been bit by a viper, are directed to the difcharging of the blood and humours that are affected by the obnoxious principle the latter conveys by its venom, and to the throwing of them, as much as it can, on the part that has been bit. If it fucceeds in this way, in fupporting the highly neceſſary functions in the vital parts, it ſurmounts the very ſudden and dangerous internal diſeaſe that would otherwiſe have been deſtructive to it.

As to the external diſeaſe, the circumſtances are altogether different. It becomes ſimilar to many other diſeaſes cauſed by an obſtruction of humours in the veſſels, of fluids extravaſated in the adipoſe membrane, and of blood which threatens gangrene and ſphacelus. If the animal is very ſtrong, however great the local diſeaſe may be, it at length recovers; and I have obſerved monſtrous ſwellings, enormous extravaſations, and an entire lividneſs and gangrenouſneſs of the parts, and, notwithſtanding all this, the animal has got about again. This is frequently obſerved in the larger ſpecies' of animals, ſuch as reſiſt for ſeveral days the action of the venom.

I wounded the crural muſcles of three pigeons with venomous teeth, and, almoſt at the very inſtant, cut off the leg in each of them. The muſcles of the firſt pigeon's leg had no apparent ſymptom of diſeaſe. Thoſe of that of the ſecond had a ſmall red ſpot, which penerrated through the fibres without changing its colour. In thoſe of the third pigeon's there was a ſmall red ſpot, ſimilar to the former one, which penetrated to the tibia itſelf, where it appeared ſomewhat darker than uſual.

I wounded the crural muſcles of two other pigeons, with teeth which had been dried a long time, and which I had previouſly well waſhed, and

a moment

a moment after cut off the leg in each of them that had been thus punctured. In one of these legs there was no symptom of disease or wound; in the other there were two red spots, which penetrated into the muscles, insensibly losing their redness.

I wounded the crural muscles of three other pigeons with venomous teeth, and bound and cut off the legs at the very instant. In one of these legs there was an appearance of black and extravasated blood. The symptoms of disease in the other two were perfectly visible and certain; that is to say, a livid colour, and black and extravasated blood for the whole depth of the muscle.

I wounded the crural muscles of two other pigeons with dried teeth, and at the same time bound and cut off the legs. The blood was extravasated in both, and was become of a dark colour.

Experiments to determine whether at the Moment of Amputation, a subtile Principle of some Kind does not escape from the Blood.

The little constancy I met with in these experiments, and the suspicion that a volatile fluid of some kind might have escaped from the blood on the moment of its being discharged from the vessels, and exposed to the open air, induced me to engage in some other trials, which I conducted in the following manner:—I held the pigeons in such a way, that although their legs were perfectly dry, their thighs were entirely plunged in water. The amputation was made beneath the water, in the thigh, ſo that the inciſed part could have no communication with the air; and the muſcles were wounded under water with venomous teeth. This being done, I kept the foot under water for three or four minutes, drew it out again, and examined it.

I repeated this experiment on the ſame number of pigeons, and ſimply wounded their muſcles with dried teeth. There were marks of the ſimple mechanical wounds, not only in the venomed muſcles of the pigeons in the former experiment, but likewiſe in thoſe that had not been venomed of this one. As I found no difference betwixt them, I cannot take upon me, with any degree of probability, to eſtabliſh a fact of any importance on theſe appearances.

I examined ſeveral times into the parts adjacent to the one that had been bit, either in animals which were already recovered, or in thoſe in which there were no longer any certain ſymptoms of diſeaſe, and of which the parts had almoſt regained their uſual motions. I obſerved with ſurpriſe in ſeveral of theſe animals that had been bit in the leg, that there was ſtill a great extravaſation of humours in the adipoſe membrane, at a very great diſtance from the part that had been bit; and likewiſe that all the abdominal muſcles were ſtill red and inflamed. Every thing in ſhort concurred to perſuade me of the exiſtence of that principle, which

which has been either suspected or admitted by others; and to convince me that the local disease is not a mechanical effect of the introduction of the venom into the part, but is rather the means this vital principle employs to drive towards the exteriour parts the morbifick matter that circulates in the humours, and to relieve from it the organs that are the most essential to the preservation of the animal. I shall point out at the conclusion of this work, the purpose that may be drawn, and the utility that may be derived, from this distinction of the two diseases that the viper occasions in an animal by its bite. The want of attention to these two states of the animal, so different from each other, has thrown the greatest perplexity on this subject, and has enveloped it in errour and obscurity. That which belonged to the one has been ascribed to the other, and thus has every thing been confounded.

CHAPTER III.

On the Action of the Venom of the Viper upon the Blood of Animals.

IF the matter of the preceding chapter has been of ſome importance, which cannot be denied; if it has preſented new and altogether unexpected phenomena; if it has been a guide to us in eſtabliſhing principles and vital powers in the living machine; the ſubject of the following chapters will certainly not be leſs important, whether we regard the novelty of their contents, or the uſe and applications that may be made of them, in obtaining a knowledge of the venoms that are analogous to that of the viper, and in explaining the animal mechaniſm, as well in a ſtate of diſeaſe as in perfect health.

Mead, to determine whether the venom of the viper had any degree of action on the blood of an animal that had been bit, mixed five or ſix drops of it with half an ounce of blood, in the colour and conſiſtence of which he could obſerve no change, as the conſequence. There was in ſhort no difference betwixt this blood and another quantity drawn at the ſame time, which he had put into a veſſel ſimilar to that which contained the firſt, by way of comparing the two together. This experiment

ment I repeated, and received the blood which flowed from the divided veſſels of an animal, immediately into a concave glaſs, which I had previouſly warmed, and into which I had put five grains in weight of the venom of the viper. The paſſage of the blood from the veſſels to the glaſs was ſo quick, that it is not poſſible to have it out of the veſſels, in a condition approaching nearer to its natural one. On the moment of the union of the venom with the blood, I obſerved the latter, the quantity of which was about an ounce, or ſomewhat more, with a very ſtrong lens. I could never perceive any kind of motion in it, neither could I diſtinguiſh in it any diſſolution, nor the ſmalleſt appearance of coagulum; in a word, it was entirely in its natural ſtate. Its globules were of their uſual ſhape, and its colour was equally preſerved. This particular ought not to ſurprize us, after the experiments that have been made on the legs of pigeons bit by the viper at the very juncture of their being cut off; and likewiſe after thoſe in which they were cut off ſometime after they had been bit. The blood in theſe caſes certainly approaches much nearer to its natural ſtate than when it is drawn from the veſſels. There is here both the natural heat and ordinary motion of the humours, and in ſhort the life of the organs themſelves.

Nothing appears more natural than the deducing from theſe particulars, that the venom of the viper has no action on the blood of the animal that receives the bite. This is indeed the inference that Mead

Mead has drawn from the above recited experiments on the blood of animals taken warm from the veſſels.

However perſuaſive this experiment on the blood might have been, and however reſpectable the authority of Mead, I determined to try a new kind of experiments, partly analogous to thoſe related above, but more direct and more ſimple. Theſe experiments conſiſt in introducing the venom of the viper in an immediate way, without touching any of the parts that are previouſly cut, into the blood. They are indeed ſomewhat difficult, but are ſtill poſſible, and are made by injecting the venom of the viper, by means of a ſmall glaſs ſyringe, into a vein that has been opened with a lancet. I foreſee an objection that will be made, that experiments of this kind are altogether uſeleſs after thoſe that have been related, to which they are beſides perfectly analogous; and that ſeeing there has been no change obſerved in the venomed blood, it ought, from a parity of reaſoning, to be concluded, that there will be no greater change in it, in theſe experiments. Such is the riſk of being miſtaken, that thoſe incur who love rather to reaſon than to experiment; and this is the mode of arguing of thoſe philoſophers, who, perſuaded that they are arrived at the fountain-head of natural ſciences, flatter themſelves that they know every thing, and are capable of explaining every thing.

Injection

Injection of Venom into the Blood Veſſels; and its Effects.

The experiments I am about to relate, were made on large rabbits. The jugular vein was the veſſel on which I operated.

When a great portion of hair has been removed from the inferiour part of the ſide of a rabbit's neck, and a large inciſion made in the ſkin, the jugular vein is diſcovered dividing itſelf into two ſmaller branches. I ſtrip in the experiment, the two branches and a part of the trunk of the jugular vein, of about ten or twelve lines at leaſt in length, of the adipoſe membrane, and the other neighbouring parts. I tie one of theſe branches with a thread, at the diſtance of ten lines from the trunk, and tie another thread to the ſame branch, about ſeven lines below the firſt, ſo that this ſecond thread is only three lines from the trunk. This laſt thread has a knot, ready to be drawn tight at a proper time. But before I go any further, I think it neceſſary to explain the manner of making uſe of the ſmall ſyringe, intended to convey the venom into the veſſels.

This is a ſmall common glaſs ſyringe, terminating in a capillary tube of ten lines in length, and crooked. I put into this ſyringe the venom that I mean to introduce into the vein. I uſually cut off two vipers' heads, and receive all the venom from

from their veficles in a fmall cryftal fpoon. I add to this venom the fame quantity of water, and when the liquors are well blended together, draw them up by fuction into the fyringe. There ufually enters into the fyringe at the fame time, a fmall air-bubble, which is eafily difperfed, by pufhing the pifton forward a little towards the tube. The fmall quantity of the liquor that flows with the air out of the point of the tube, is received in the fmall fpoon, and is fucked up again into the fyringe, by once more withdrawing the pifton a little.

The fyringe being thus freed of the external air, I withdraw the pifton in an almoft infenfible degree. The venom retreats a little, and leaves the point of the capillary tube, which remains full of air, for the length of four lines. The quantity of this air is very trifling, on account of the fmallnefs of the diameter of the tube in that part. I now wipe the crooked part or extremity of the fyringe with a piece of very fine moiftened linen, and introduce a very fine and dry linen thread, for the length of two lines, to cleanfe the venom, and likewife the fmall fpace in the capillary tube, that is occupied by the air.

The fyringe being thus in readinefs, I raife a little, by the uppermoft thread, the branch of the jugular vein to which the two threads are faftened, betwixt which I open it with a lancet, and introduce the capillary extremity of the fmall fyringe at the orifice, continuing this till it has entered four or five lines into the principal trunk. I now draw the end ends of the threads together, the lower one of which binds the coats of the veſſel very ſtrongly to the capillary tube of the ſyringe. Things being in this ſtate, I puſh forward the piſton of the ſyringe by degrees, and force out of it all the venom, which paſſes entirely to the trunk of the jugular vein, to be carried an inſtant after to the heart.

This experiment requires two perſons at leaſt, and ſucceeds ſtill better when there are three. If the ſyringe has been previouſly got in readineſs, it does not continue at the moſt for more than two minutes altogether; and when the parts of the animal are known, and it has been made a few times, is not ſubject to any inconvenience.

Before the ſyringe is drawn out of the veſſel, I have been accuſtomed to withdraw the piſton a little, that a ſmall quantity of blood may enter the capillary tube, and that none of the venom may remain at the orifice of it. At the moment of my drawing out the ſyringe, I again tighten the lower thread, ſo that the veſſel remains perfectly cloſed. I raiſe with pincers the portion of the jugular vein betwixt the threads.

It was not without reaſon that I made choice of a veſſel which branches out into two others; neither was it at hazard that I introduced the capillary part of the ſyringe into the principal trunk itſelf.

I wiſhed that the venom ſhould be carried immediately to the heart; and I could not think of a better expedient than that of procuring a very large lateral veſſel, where the blood continuing to run in a full ſtream towards the heart, muſt neceſſarily carry with it the venom that it meets with in the trunk.

Theſe experiments are too important not to be related with ſome degree of detail. They at leaſt require me to deſcribe the principal circumſtances by which they were accompanied. I ſhall give them here in the order in which they were made.

I injected into the outer jugular vein of a large rabbit that weighed ſeven pounds, the venom of two viper's heads, got ready in the manner deſcribed above, and with a nice obſervance of all the precautions I have juſt laid down. The venom ſcarcely began to enter the vein, when the animal gave ſeveral horrible cries, diſengaged itſelf, writhed itſelf about, and died a moment after.

The novelty of this ſtrange and unexpected event, prevented me from calculating the exact time the animal lived after the injection of the venom; neither could I aſcertain the time I employed in propelling the whole of the venom from the ſyringe. It is certain, however, that the animal did not live more than two minutes, and that the injection was made within the ſpace of eight or ten ſeconds.

As I was deſirous of ſeeing whether this experiment was a certain one, or whether the animal died in conſequence of ſome circumſtance I was ignorant of, I examined the ſtate of the viſcera in the dead animal, and likewiſe that of the blood in its veſſels. I was likewiſe induced to vary ſome of the circumſtances in making the ſucceeding ones.

I got

I got ready another rabbit in the above manner, and began by injecting a quantity of water into a branch of the jugular vein, equal to that of the mixture of venom and water in the preceding experiment. The rabbit did not suffer in the least. I kept it in this state during five or six minutes, and perceiving that it did not become at all disordered, sat about injecting into the same vein, the quantity of venom mentioned above.

The animal, however, neither cried out nor was agitated. At the end of a few minutes I perceived that it had sickened, and it died at the end of twelve hours. All the parts I had stripped of the skin, to lay bare the jugular vein, were violently inflamed, and very livid. The adipose membrane was filled with black extravasated blood. All the pectoral muscles at the side on which I had injected the venom, and a part of the abdominal ones, were already become livid. The very intestines were inflamed. The inner part of the thorax was inflamed likewise, and was bloody; and the heart had formed adhesions. The blood, both in the large vessels and heart was coagulated and black; and the lungs were marked here and there with somewhat livid spots.

This second experiment convinced me of the very great importance of thoroughly examining the state of an animal after its death. It is principally by this state, that we ought to judge of the action of the venom on the blood.

But how came it about that the first rabbit died as it were instantly, and the second not till the end of of twelve hours? To what is this difference to b ascribed?

I inſtantly proceeded to a third experiment, hoping to draw ſome further information from it.

I got ready a rabbit, and injected the venom o two vipers as before, into the branch of the jugula vein. The rabbit did not ſeem to ſuffer in the leaſ from this operation, and recovered of the externa diſeaſe in a few days, as readily as if it had only undergone the preparation neceſſary to the injection of the venom. An hour after this injection had been made, I found it eating, as if in perfect health.

This third experiment completed my perplexity, and I began to miſtruſt altogether. In the firſt place I ſaw an animal die, as it were at the moment of injection, and diſtinguiſhed a real diſeaſe in that which lived twelve hours. It was therefore certain that the venom, when united with the blood, was capable of producing ſuch a derangement in the animal machine, as to excite in the animal a very violent diſeaſe, terminating in death. All this was real; but how could theſe two caſes be reconciled with the third?

Some doubts occurred to me as to the method I had purſued in making theſe experiments, which had not been altogether conducted with the exactneſs and preciſion I deſcribed a little time ago. I did not make the ſecond ligature in the vein; I did not examine whether the capillary tube reached into the principal trunk; and I did not withdraw the piſton of the ſyringe, before I drew the capillary tube

tube of the latter out of the veſſel. The neglect of theſe precautions made me look upon the three experiments I have juſt related as ſuſpicious, and I ſat about experimenting afreſh, with a greater degree of attention and preciſion than before.

For this purpoſe I got ready a large rabbit, healthy, and in good plight. I made the two ligatures in the external branch of the jugular vein, and introduced the capillary tube into the common trunk, tightening the thread on the tube, and injecting the whole of the liquor at once. I took care to withdraw the piſton a little, before I drew the ſyringe out of the vein, and to tighten the thread once more. In a word, I did not neglect any one of the precautions I had previouſly determined to take. The conſequences were as follows.

The whole of the venom had ſcarcely paſſed from the ſyringe into the jugular vein, when the rabbit gave ſeveral horrid ſhricks, and was ſeized with very violent convulſions. It died in leſs than a minute and an half. There were not more than ſeven ſeconds ſpent in the injection.

The blood in all the large veſſels was black and coagulated. It was likewiſe ſo in the heart and auricles. The coronary veins were ſwelled and livid; and an extravaſated black blood, in large ſpots, was ſeen about them in the muſcular ſubſtance of the heart. The pericardium was entirely filled with a liquor, ſo as to be diſtended like a bladder; and this liquor was tranſparent, with a ſlight red tinge.

The lungs were full of the usual spots, through which, when they were touched in the slightest degree, the air rushed out of the water that covered this viscus. The intestines, stomach, and mesentery, were covered with small, livid, and red spots.

This experiment succeeded too well to leave me in any kind of doubt as to the nature of its consequences. The animal died almost instantly, and cried out the moment the venom had entered the vessel.

The two viscera that are the principal organs of life, were instantly attacked by a violent and mortal disease. The blood was immediately coagulated in the large vessels, in the lungs, and in the heart. In a word, every thing concurred to the sudden stoppage of the circulation, and to the death of the animal.

The extravasation of the blood of the coronary veins is surprizing, and the livid spots of the lungs, and dilacerations of this viscus still more so. But what suprized me most of all, was the blood collecting in such an abundance, in so many vessels, and in so many cavities. In this disease an extreme dissolution of a part of this humour, exuding every where through the vessels, takes place; and, at the same time, a coagulation of another part, which fixes and condenses in a few moments.

Every advance I made, in this new career of experiments, presented me either with something paradoxical, or with a novel and unexpected circumstance. I passed on to the fifth experiment, which I made

I made exactly as I had done the fourth. Although the result of it was somewhat different from that of the last, it agreed very well as to the nature of the disease, and as to the opinion that ought to be held of the introduction of the venom of the viper into the blood. On the injection being made, the rabbit did not cry out, neither did it seem to suffer in any sensible degree. At the end of an hour, however, it appeared to be sick, refused its food, and died at the end of twenty-four.

On opening its body, I did not find the abdominal viscera to be much inflamed; but to atone for this, the usual livid spots, and the air gushing freely out of them, were seen on the lungs. All the muscles of the breast were considerably inflamed, and the whole of the adipose membrane, from the neck to the lower part of the belly, was filled with black, extravasated, and fluid blood. The blood in the heart, in the lungs, and in the large venous vessels, was coagulated; but much less so than in the cases related above in which the rabbit died almost instantaneously.

I immediately proceeded to the sixth experiment, to see whether there would be any degree of uniformity betwixt the injection of the venom, and the death of the animal. I neglected to remark, in relating the preceding experiments, that I had found sometimes a greater, sometimes a less, quantity of venom in the viper's heads, and that in some of them I had even observed a white and somewhat glutinous matter flow from the tooth.

 I had

I had likewiſe found the palate of ſome of the vipers I employed inflamed to a certain degree, and the two ſheaths or bags of the teeth likewiſe inflamed and red.

But I could not ſay poſitively, whether theſe circumſtances could have been capable of influencing the effects of the venom on the animal. I therefore reſolved to take it in future from no other heads of vipers, but ſuch as were perfectly ſound, and the beſt ſupplied; and to procure it in a greater quantity.

I got ready a large and ſtrong rabbit in the uſual way, and introduced into the ſyringe the venom of two very large vipers, the heads of which were in a ſound ſtate.

The venom was not yet completely injected, when the rabbit began to ſhriek, and died in leſs than two minutes in very violent convulſions. Having opened the breaſt, I found the auricles and ventricles filled with grumous blood. That of the large venous veſſels was in the ſame ſtate. There was a great deal of lymph in the pericardium, in which there was likewiſe extravaſated and concreted blood. All the inteſtines were in a very inflammatory ſtate, as were alſo the ſtomach and meſentery. The arteries were in general empty. The lungs were but little ſpotted; but in inflating them beneath the water, the air was ſeen ruſhing out in ſeveral parts, and the ſmall ſpots were then apparent. The blood in the lungs was likewiſe concreted.

I got

I got ready another rabbit, and injected in the usual way into the jugular vein, the customary quantity of venom.

It scarcely began to enter the vein, when the rabbit cried out, and in less than two minutes died, with the most terrible shrieks and convulsions.

I opened it, and found the lungs spotted as usual, and the blood coagulated in the two ventricles. It was much more so in the right ventricle than in the left, as I had also found it in all the preceding cases. It was likewise in the same state in the auricles and veins. The pericardium was filled with water mixed with blood. The coronary veins had, for their whole circumference, two large, longitudinal, and livid spots. The blood in the lungs was black and grumous, and the air gushed out as usual. The intestines were inflamed, as were also all the abdominal muscles; and there was a great deal of extravasated and dissolved blood in the adipose membrane.

These two last cases are very uniform, and agree too well with the preceding ones, to admit of any doubt as to the immediate action of the venom of the viper on the blood.

Further Experiments on the Jugular Vein of Rabbits.

Notwithstanding the uncertainty and obstacles that are met with in experimenting on the blood-vessels, I was desirous of making some further trials on them, conducted with all poſſible care and attention, as the very great importance of the ſubject ſeemed to require. I choſe for this purpoſe, two of the largeſt rabbits I could procure, each of them weighing ten pounds. I took the venom from two ſound vipers, which I had previouſly examined with great attention for that purpoſe. I had not yet finiſhed the injection in either of the two rabbits, when they gave ſeveral loud ſhrieks, and died in the moſt violent convulſions in leſs than two minutes. Having opened the thorax of each rabbit, I found the lungs ſpotted as uſual, and the blood veſſels and auricles filled with black and grumous blood. The pericardium, as in the former caſes, contained a humour, and the inteſtines and muſcles were, as uſual, inflamed.

The immediate action of the venom of the viper on the circulation of animals with warm blood, is therefore both indubitable and conſtant. It is a fact, however, that would not have gained any degree of credit, had it not been for theſe laſt experiments, ſince it ſeemed in ſome meaſure to be contradicted by the former ones, which, although they were leſs direct and leſs ſimple, were nevertheleſs made on the blood. This ſhows us how cautious we ought to be in the inferences we draw from experiments; and at the ſame time proves to us, that we know little or nothing, at leaſt with any certainty, and without incurring the riſk of being deceived, beyond that which is demonſtrated to us by experiment alone.

But

But in what way are we now to reconcile the immediate action of the venom of the viper on the blood, when it is injected by the veins; and the inactivity of this same venom, not only on the parts of an animal recently cut off, but likewise on those that have remained in an entire state, and united with the animal, during a period of 15 or 20 seconds, after it has been introduced?

I must acknowledge that this is a very great difficulty, and that it would be no easy task to explain it in a satisfactory way. It appears that there can be nothing deficient in the parts that are still connected with the animal when they are bit by the viper. It even seems probable that these cases have an advantage over those in which the trial is made on the blood, since both the muscular fibres and nerves are wounded by the viper's teeth, instead of which the venom, when injected into the vessels, certainly touches neither one nor the other of these parts. What is then the cause that retards the disease of the venom for several seconds in the part of an animal that has been bit; and how it is that the venom does not produce any disease in the parts that are either cut off and bit immediately after, or are cut off immediately after they are bit.

It is probable that there may be an unknown principle in the blood circulating in the vessels, which ceases to exist the moment this fluid is drawn out of them, and which is likewise no longer to be found in the parts that are recently separated from the body of an animal. This principle, granting the ſuppoſition, therefore, poſſeſſes ſuch very active and ſubtile qualities, that it is diſſipated at the very moment the part is ſeparated from the animal.

We have ſeen that the venom ſcarcely comes in contact with the blood in a veſſel, when the moſt violent derangements are produced. The animal ſuffers extremely, and the blood is condenſed in an inſtant. If this ſame venom is mixed with the blood as it flows warm from a veſſel that has been opened; or if it is introduced into any part of a muſcle that has been ſeparated an inſtant before; it produces no effect, and no appearance of diſeaſe is obſerved, nor any condenſation of humours. Here, however, every thing is the ſame, unleſs it be that in the caſe in which the venom is introduced into the veſſel, there is a blood circulating with the reſt of the humours, and always covered by the coats of the veſſels; inſtead of which, the blood that is drawn from a vein is out of the torrent of the circulation; and that of the parts which have been recently cut off has already been in contact with the air, and the veſſels which contain it are open.—However it may be, ſince the effects are very different from each other, the circumſtances muſt be ſo likewiſe; and the only concluſion we can draw, as to the humour contained in a veſſel, and the humour drawn out of a veſſel, is, that there exiſts in the firſt caſe, ſomething that is not to be met with in the ſecond.

Agreeably

Agreeably to this hypothefis, this new principle which exifts or refides in the blood, in the veffels of a living animal, does not produce the fame effects every where equally, and in the fame time. The venom has no fooner united itfelf with the blood in the jugular vein, than the animal is attacked by a very violent difeafe, and the blood is coagulated a very few inftants after. Inftead of which, in the parts that are more diftant from the heart, where the veffels are fmaller, it requires a certain time for the difeafe to difcover itfelf, and for any fenfible change to take place in the part into which the venom has been introduced.

It thererefore appears, that this principle obferves certain laws in governing the animal economy, and that it is itfelf fubject to certain regulations.

In the cafes in which the difeafe is the moft remote from the heart, and the leaft dangerous, the blood coagulates by degrees, is driven back to the parts bitten, and affords time and opportunity for the efforts of nature to overcome the difeafe, and to preferve the circulation in the organs the moft neceffary to life.

But at length what is this new principle, and what are the organs that fecrete it, and convey it to the veins?

In this very difficult enquiry, it appeared to me that experiment could alone furnifh me with fome light, and conduct me to fome new truth. But where are the experiments to begin?

CHAPTER IV.

Experiments on the Nerves.

IN the long courſe of my experiments on the venom of the viper, and in collecting together the circumſtances and ideas that preſented themſelves, I never loſt ſight of the principle of ſenſation in an animal, which appeared to me to be acted on by the venom of the viper. I have in conſequence of this, judged it neceſſary to examine the nerves in which it reſides, or which are the organ and inſtrument of it.

Mead ſays, in the introduction to his work on Poiſons, that having better conſidered the nature and quality of the ſymptoms of the bite of the viper in animals, he is certain that this diſeaſe is altogether nervous, and that it is communicated by the medium of the nerves, and not of the veſſels. In conſequence of this theory, he has recourſe to the animal ſpirits, againſt which he believes that the immediate action of the venom of the viper is exerciſed. Indeed if we examine the ſymptoms that this venom produces in animals, we are eaſily led to believe that a diſeaſe of ſuch a nature belongs to the claſs of diſeaſes which the phyſicians ſtile

 nervous.

nervous. In the courfe of my experiments, I have feen a pretty large dog fall down motionlefs, the moment after it had been bit by two vipers. I at firft thought it dead, but at length perceived fome little remains of refpiration, which was, however, fo flight and feeble, that it could fcarcely be diftinguifhed. The dog continued in this lethargick ftate for more than half an hour. I have feen feveral others thrown by the venom into very violent convulfions. Vomiting, anxiety, and rage, occur very frequently; the motion of the heart is irregular and convulfive, and the arterial fyftem hard and contracted. In fhort, they die in the midft of the moft unequivocal fymptoms of fpafms and contractions, and, in a word, with the affections that are by the faculty termed *nervous*.

Another idea occurred to me, that perhaps an active principle, a fubtile fluid, is fecreted by the nerves themfelves, which, when it mixes with the blood, contrives in fome way to animate it, to render it vital, and to maintain its fluidity. In this cafe, the action of the venom of the viper may perhaps have been directed againft this principle itfelf; and thus we may explain, why it is that the blood, when drawn from the veffels, and in the open air, is no longer fufceptible to the action of this venom.

Experiments on the Nerves, Spinal Marrow, and Brain, of Frogs.

I opened the belly of a frog, and laid bare the crural nerves. I poured a ſmall quantity of venom on theſe nerves, taking care that it did not ſpread to the ſurrounding parts. At the end of two hours I pricked the nerves with the point of a needle, and the muſcles of the foot contracted. At the end of four hours, however, no part of the animal was ſenſible to ſtimulation. A frog intended for a compariſon, lived twelve hours, notwithſtanding I had opened the abdomen, lacerated the inteſtines, and punctured the lungs.

I repeated this experiment twice, and the event was each time pretty much the ſame. I began, however, on a little conſideration, to think the method I had adopted fallacious. It is almoſt impoſſible to prevent the venom, when applied to the nerves, from communicating to the adjacent parts. In this caſe, the diſeaſe, or death, of the frog, may be the effect of the communication of venom to the other parts of the animal, and not the conſequence of its contact with the nerve itſelf.

I changed my mode of experimenting, but ſtill employed the ſame animals.

I cut off the heads of two frogs, alike in ſize, and touched the ſpinal marrow of one of them, but not of the other, repeatedly with the venom. At the end

end of three hours the venomed frog appeared to be dead, whilſt the other continued to live, and to leap about.

I introduced a pin into the ſpinal marrow of the frog to which the venom had been applied. Its fore-legs remained motionleſs, but there was a ſlight tremulus in the feet. The heart and auricles likewiſe had a ſmall degree of motion. In an hour more every part was at reſt. The ſecond frog leaped about the chamber at the end of twenty-four hours.

I cut off the head of another frog, and introduced a drop of venom into the ſpinal marrow. At the end of an hour the frog ſcarcely gave any ſigns of life. On opening the breaſt, the heart and auricles ſeemed ſtill to preſerve ſome degree of motion, which was however perceived with difficulty. A pin introduced into the ſpinal marrow, occaſioned an almoſt imperceptible motion of the fore-legs and feet. The heart, however, on being ſtimulated, performed its oſcillations for a long time.

I cut off the head of a frog, and removed a ſmall portion of the ſpinal marrow. I introduced by the great opening of the vertebræ, a drop of venom. At the end of two hours the frog was to appearance dead. The heart ſcarcely preſerved ſome little remains of motion, which were not encreaſed by ſtimulation. A pin introduced into the ſpinal marrow was barely capable of exciting a feeble motion in ſome of the muſcles.

I cut off the head of another frog, and having removed a ſmall portion of the ſpinal marrow, introduced a drop of venom into the great foramen. At the end of three hours the frog appeared to be dead. Having opened the thorax, I remarked that the heart was ſtill irritable; a pin, however, that I introduced into the ſpinal marrow ſcarcely occaſioned a ſenſible contraction of the feet.

I repeated this experiment on two other frogs, and the reſult was pretty much the ſame as in the above experiments. The death of the frogs ſucceeded the operation and introduction of the venom in a ſpace of betwixt two and three hours. The heart was ſomewhat irritable, but the muſcles were little ſo, or not at all, notwithſtanding my ſtimulating the ſpinal marrow with a needle.

I now thought it proper to make a little variation in my experiments.

I removed a portion of the cranium of a frog, and applied a ſmall quantity of venom to the brain. At the end of four hours the frog was dead, and the heart inſenſible to every ſtimulation. On pricking the ſpinal marrow with a needle, not the ſmalleſt motion was reſtored in it.

I opened the cranium of another frog, and applied a drop of venom to the brain. The frog ſurvived this operation two hours, at the end of which time the heart had ſtill retained a ſlight degree of motion; it was ſhrivelled, black, and contracted. On ſtimulating the ſpinal marrow, there was an almoſt inſenſible contraction of the muſcles.

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I repeated this experiment on the brain of four other frogs, and the consequences were very analogous to those of the two preceding ones. However having removed the cranium of the two frogs without applying the venom to the brain, by way of a comparative experiment, they both died in the space of ten hours.

The consequence of these experiments appearing neither sufficiently clear nor uniform, I had once more recourse to the cutting off of the heads, thinking that by dint of multiplying my experiments in that way, I might assure myself of the action of the venom on the nerves.

I cut off the heads of two frogs, and applied venom to the spinal marrow of one of them, but did not venom that of the other. At the end of three hours the venomed frog was to appearance dead; the other was still living, and had a free motion in all its parts. I introduced a pin, which had been dipped in venom, into the vertebral opening of the first frog, and it excited a very feeble motion of the feet, but had no such effect on the fore-legs. I scarcely stimulated the spinal marrow of the other frog with a needle, when the frog leaped about briskly. At the end of the fourth hour there was not the smallest perceptible degree of motion in the venomed frog, and neither the heart nor auricles were any longer sensible to stimuli. The other frog was still leaping about at the end of thirty hours.

I cut off the head of another frog, and introduced the venom into the ſpinal marrow. At the end of two hours the frog was to appearance dead. Having opened the thorax, the heart was motionleſs, and even inſenſible to ſtimulations. The ſpinal marrow, when likewiſe ſtimulated, ſcarcely excited any degree of motion in the feet.

I repeated this experiment, obſerving the ſame circumſtances, on another frog. At the end of three hours I found it dead. The heart and muſcles were perfectly motionleſs. On treating another frog in the ſame way, the conſequences were altogether ſimilar.

I cut off the head of another frog, and applied venom to the ſpinal marrow. At the end of five hours the frog ſtill retained ſome feeble ſigns of life. On opening the thorax, I found the heart motionleſs; it however renewed its oſcillations on being touched.

The conſequences of all theſe experiments together may reaſonably induce us to ſuſpect, that the venom of the viper acts on the nerves, and that when it is applied to theſe parts in frogs, it produces a mortal diſeaſe. But this mode of experimenting is not entirely irreproachable. The ſpinal marrow and brain are too ſmall to enable us to be certain that the venom does not communicate to the adjacent parts. No precaution whatever can in my opinion prevent this. When the venom is applied, it is too near to the veſſels and other parts; and how indeed can it be kept from the blood-veſſels of both brain and ſpinal marrow?

This

This enquiry is too important to be confined to the limits of ſimple probability, I ſtill flattered myſelf, that the purſuit of it would tend very much to the obtaining of a true knowledge of the venom of the viper and its qualities, and of the animal economy itſelf.

With this view I formed a plan of experiments, to be made on the nerves of the largeſt rabbits I could procure. This animal is hard to kill, and as it is gentle in its nature, may be managed agreeably to one's wiſh; it is likewiſe not ſo ſmall, but that its nerves may ſerve for the moſt deciſive experiments.

Experiments on the ſciatick Nerve of Rabbits.

I made choice of the ſciatick nerve as the ſubject of my principal experiments. I removed the hair with ſciſſars from the ſkin that covers the great gluteus muſcle, and made an inciſion, beginning on the great trochanter, and deſcending in a direction with the thigh. I detached the anteriour part of the gluteus muſcle from the os innominatum and trochanter, and gradually raiſed the muſcle with my fingers, freeing it from the adipoſe membrane. A little cuſtom in theſe experiments, enables one to make them in leſs than two minutes, and in ſuch a way, that after removing the ſmall quantity of blood which flows from the integuments, it is not ſucceeded by any freſh hemorrhage that is capable

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of retarding or difturbing the operation. Now, holding the great gluteus mufcle with one of my hands, I paffed, with the affiftance of fmall pincers, a piece of fine linen in feveral folds under the fciatick nerve, upon which, being in this ftate, I began my experiments.

Having got ready one of the fciatick nerves of a large rabbit, in the way I have juft defcribed, I wounded it in feveral places with a venomous tooth. The rabbit fhook itfelf a little at the time of my doing this. At the end of twenty hours it ate, and feemed in full vigour. It died however at the end of feven days, with a large wound in the part that had been cut. This experiment was not made fo well as it fhould have been; more than half the gluteus mufcle was cut, and there was a great hemorrhage.

I laid one of the fciatick nerves of another rabbit perfectly bare, paffing beneath it feveral folds of linen. I then wounded it, in upwards of twenty places, with the venomous teeth of two vipers. The rabbit fcarcely gave any figns of feeling pain, and at the end of ten hours, ate and appeared lively. It was in this ftate at the end of twenty-four hours, but died at the end of forty-eight. The nerve was marked here and there with dark red fpots; the parts about it were violently inflamed; and the blood in the auricles and heart black and coagulated.

In wounding the part with the venomous teeth, I took the greateft care imaginable to prevent the venom from communicating to the adjacent parts;

and conſtantly covered the nerve after I had wounded it.

Having got ready one of the ſciatick nerves of another rabbit, I paſſed under it the uſual folded linen, and wounded the nerve in ſeveral places with the venomous teeth of two vipers. I covered the nerve well with linen, and ſtitched up the ſkin. I had obſerved this latter precaution in the preceding experiments.

The nerve was prepared for the introduction of the venom in leſs than two minutes, and the hemorrhage from the integuments was very ſlight indeed. Notwithſtanding this the rabbit died at the end of eighteen hours. The nerve was to appearance in its natural ſtate. The blood in the heart and auricles was black and grumous. The muſcles in the vicinity of the nerve were a little inflamed, and there was a degree of lividneſs on their ſuperficies.

Theſe experiments, although few in number, and not diſtinguiſhed by any great uniformity, began however to make me ſuſpect, that the bite of the viper is leſs dangerous to the nerve, than to many other parts of an animal. The rabbits lived much longer than one would naturally have conceived, and notwithſtanding it appears that they all died ſooner or later, I conjectured that here, as in the caſes of the tendons, the venom might have communicated to the neighbouring parts, and that the animal might probably have died rather from this, than from any other cauſe.

As a ſtill further precaution in purſuing theſe experiments, I had recourſe to the piece of lead I had before made uſe of, putting it betwixt the folds of the linen. In this way the nerve was very well ſecured from the other parts, and it did not ſeem poſſible for the venom to ſpread beyond it.

I wounded a ſciatick nerve of a rabbit in ſeveral places, after having got it ready in this way, with the venomous teeth of two vipers; covering it with linen, and binding it up ſecurely afterwards. During the time employed in forcing the teeth into the nerve, the rabbit cried out ſeveral times, and was ſeized with violent convulſions. It died at the end of twenty hours. All the muſcles about the nerve were livid and ſphacelated for their whole ſubſtance; and the ſphacelus extended for the whole length of the leg. The lungs were ſpotted; and the nerve itſelf was likewiſe covered with red and livid ſpots. The blood in the auricles and great venous veſſels was black and coagulated.

The circumſtances that accompany this experiment are ſufficient to induce one to believe, that the venom of the viper has in effect a ſtrong action on the nerves. The ſphacelus of ſo many muſcles, even of thoſe that were diſtant from the wounded part, made a great impreſſion on me. I, however, did not on this account terminate my experiments.

Having laid one of the ſciatick nerves of another rabbit perfectly bare, I wrapped it carefully in the linen, in which I had not, however, encloſed the bit of lead. I now wounded it in ſeveral places with the

the teeth of two vipers, and covered it with linen as uſual. The rabbit died at the end of thirty-two hours. The nerve was but little redder than it naturally is, and was not ſpotted. The blood in the auricles and large veſſels was but ſlightly coagulated. When I opened the rabbit, I found it ſtill to poſſeſs a degree of warmth.

This experiment is very different from the preceding one, and ſhows how little confidence we ought to place in experiments themſelves, however nicely they may have been made, unleſs they are in a great number, and agree with each other.

I laid bare a ſciatick nerve of another rabbit, and wrapped it carefully in the folded linen, in which I had previouſly encloſed the bit of lead. I wounded it in ſeveral places with the venomous teeth of two vipers, and afterwards covered it ſecurely. The rabbit died at the end of thirty-two hours. Several parts of the nerve were red, and there were livid ſpots in it. The muſcles adjacent to it were in their natural ſtate; but the lungs were livid and ſpotted. The heart, auricles, and principal veſſels, were filled with black grumous blood.

I repeated in four other rabbits, the application of venom to one of the ſciatick nerves, but with ſome little change in the circumſtances. I apprehended, that probably the linen which encloſed the nerve on all ſides, and remained on the wound, might occaſion the death of the animal, and the derangements we have obſerved. It therefore became neceſſary to ſeparate theſe two circumſtances,

 and

and to remove the linen after wounding the nerve with the venomous teeth. Before the linen was removed, I wiped the venom carefully from the ſurface of the nerve with ſmall bruſhes, which I repeatedly changed. After this, I dipped bits of linen in water, and holding them with pincers, employed them in waſhing the whole extent of the nerve. The linen I had paſſed under the nerve, folded upwards of ten times, prevented the water from communicating to the adjacent parts. I now removed this linen, and threw on the nerve a conſiderable quantity of water, which waſhed at once, nerve, muſcles, &c. ſo that it was not poſſible for any particle of venom, however ſmall, to continue lodged in the parts that ſurrounded the nerve.

Theſe four rabbits all died in leſs than thirty-ſeven hours. In three of them no ſenſible change was to be obſerved in the parts adjacent to the venomed nerve. The muſcles, except that they were a little redder than uſual, were in their natural ſtate.

I confeſs that on one hand it did not appear poſſible to me, that the venom could, notwithſtanding all the precautions I had taken, have been communicated to the ſurrounding parts; and on the other hand, I could not find any ſymptom of diſeaſe, any effect of the venom, in the muſcles adjacent to the venomed nerve. The death of the animal was the moſt conſtant reſult in theſe experiments; and this did not however take place till very late, and was not attended with either ſpaſms or convulſions. If the bite of the viper is really venómous to the nerves of animals, it is certain that it acts on these parts with less force and activity than on many others.

As this enquiry appeared to me a very important one, I thought it proper to persevere, making some little change in my experiments.

Experiments on the Sciatick Nerve, divided in its upper Part.

I laid bare the sciatick nerve of a rabbit on one side, in the usual way, and with a pair of scissars divided it in its upper part, as near to the vertebræ as possible. The cleared part of the sciatick nerve, towards the extremity, was about an inch and an half in length. I wrapped it in linen, which was as usual in several folds, wounded it in several places with venomous teeth, and covered it securely, to prevent the venom from communicating to the surrounding parts. The rabbit died at the end of thirty-six hours.

I opened it whilst it was yet warm. The blood in the heart and auricles was black, but not grumous. The muscles adjacent to the nerve were somewhat inflamed.

I laid bare one of the sciatick nerves of another rabbit, and divided it in the manner described above. I wrapped it in linen, wounded it with venomous teeth, and covered it. The rabbit died at the end of eighteen hours. The nerve in some parts was dark and livid; the adjacent mufcles were in a very flight degree inflamed; and the blood in the heart ftill fluid.

The principal aim of this method of experimenting, was to fee what effect the venom of the viper would produce, when applied immediately to a nerve, leading it is true to an organized part, and one endued with fenfation, but which had no longer any immediate communication with the life of the animal. The action of the venom in the two above cafes could not in any poffible way be communicated from the nerve to the animal, and could not awaken in it any immediate pain or fenfation. This however did not prevent the nerve from communicating the difeafe of the venom to the inferiour part in which it terminated. It muft be obferved, that in this part the humours continue in motion as before; that the mufcles are in their entire natural ftate; that the fibres ftill preferve their irritability; and that the part retains its fenfibility, in confequence of the other nerves that terminate there. With all this, no difeafe is obferved in it. There is no tumour, no gangrene nor fphacelus, and no extravafation of black and grumous blood.

Thinking, however, that two experiments alone, would not be fufficient to render this particular, which is of fo much importance, certain, I was defirous of repeating them in the fame way.

I deftined fix rabbits to this purpofe, and in each of them laid bare and divided one of the fciatick nerves, wounding it as ufual with the venomous teeth,

teeth, and covering it carefully with linen. These rabbits all died, two in eighteen hours, the other four before the expiration of thirty-six. The adjacent muscles were in their natural state, and the nerves, in a greater or less degree, dark and spotted.

It is therefore certain, that the venom of the viper is not communicated by the nerve to the parts into which that nerve enters and ramifies; notwithstanding it is true that the animal dies on which this experiment is made.

Experiments on the Sciatick Nerve, divided in its inferiour Part.

But if the disease of the venom is not communicated to the parts beneath that in which the nerve has been divided, it may nevertheless be communicated to the upper parts, with which the nerve still preserves its former union and correspondence entire. The animal continues to be sensible to the smallest violence offered to the nerve, which consequently never ceases to be an organ and instrument of sensation, and in which that principle, whatever it may be, still exists, that produces sensation in the machine.

Having laid bare the sciatick nerve in the usual way, instead of dividing it in the upper part towards the vertebræ, I divided it in the inferiour part towards the feet. The cleared part of the nerve was, as usual, about an inch and an half in length. I

wrapped

wrapped it in linen as in the preceding experiments, and wounded it with the venomous teeth, taking care to cover the whole carefully, to prevent the communication of the venom to the adjacent parts.

The following are the experiments I made in this way:

The ſciatick nerve of a rabbit being laid bare, I cut it in the inferiour part towards the feet, and wrapped it in linen which had been folded ſeven times. I now wounded it repeatedly with the venomous teeth of two vipers, and, in the midſt of this operation, the rabbit exhibited ſtrong marks of pain. It died at the end of twenty hours. The nerve was ſpotted and livid, as were likewiſe the lungs. The blood in the heart was black and grumous; but the muſcles about the nerve ſcarcely ſeemed to have undergone the ſmalleſt change.

This experiment ſeems to confirm us ſtill more in the opinion, that the venom is not communicated to the muſcles adjacent to the nerve, and that there is no local diſeaſe in theſe parts.

I laid bare the ſciatick nerve of another rabbit, cut it in its inferiour part, and wounded it as uſual with the venomous teeth of two vipers. The rabbit cried out and writhed itſelf during the act of wounding the nerve, and died at the end of ſixteen hours. The nerve was livid and inflamed in ſeveral parts. The lungs were covered with large black ſpots. The heart, auricles, and large venous veſſels, contained black grumous blood. All the adipoſe membrane covering the muſcles of the abdomen, was inflamed,

inflamed, and inner part of the ſkin was ſo likewiſe. The ſkin, adipoſe membrane, and muſcles towards the breaſt, were all gangrened. The muſcles adjacent to the nerve were livid for the depth of a line.

This experiment is very different from the preceding one, and may induce one ſtrongly to ſuſpect, that the venom of the viper is alſo venomous to the nerves, and that in theſe caſes, the diſeaſe of the venom is communicated to all the parts of the animal, above that where the nerve has been cut. In ſuch an uncertainty there is no way of coming at the truth, than that of purſuing the experiments. It is almoſt impoſſible not to obtain, in a long continuance of them, ſome agreement and conſtancy in the effects.

I divided one of the ſciatick nerves of a rabbit in the uſual way, and having wrapped it in linen, wounded it with the venomous teeth of two vipers. The rabbit ſhricked violently at the moment of its being wounded, and died at the end of thirty-ſeven hours. The nerve was full of black and livid ſpots, and the parts adjacent ſomewhat inflamed. The heart was very hard, and very much ſhrivelled. I did not open the rabbit till upwards of an hour after its death. The venæ cavæ, however, ſtill oſcillated with force. Their motion began at the part where they open into the auricle, and they continued to move for upwards of five hours longer, notwithſtanding the cavity of the thorax was expoſed to the open air.

Having

Having divided the ſciatick nerve on one ſide, of another rabbit, and wrapped it carefully in linen, I wounded it in ſeveral places with the venomous teeth of two vipers. The rabbit died at the end of ſixteen hours. The nerve had ſeveral black ſpots on its ſurface, and the adjacent muſcles were livid throughout their whole ſubſtance. The blood in the heart, auricles, and large venous veſſels, was fluid, and a little darker than uſual.

I repeated this experiment with the ſame circumſtances on ſix other rabbits, and the conſequences were perfectly analogous to thoſe I have related above. The rabbits all died ſooner or later, but neither of them in leſs than ſixteen hours, nor after thirty-ſeven. In ſome of them the muſcles circumjacent to the nerve were inflamed and livid throughout their whole ſubſtance; and in others, on the contrary, they were ſimply a little redder than uſual. The blood in the heart, in ſome of the caſes, was fluid, and in the others coagulated. The muſcles, adipoſe membrane, and ſkin, of the breaſt, were inflamed in only one inſtance. The only thing conſtant in theſe experiments was the death of the animal.

We may, in my opinion, deduce in general, from the experiments I have thus far related on the nerves, that the changes obſerved in the muſcles adjacent to the ſciatick nerve, or thoſe in other parts of the animal, are entirely accidental; ſince they ſometimes exiſt, and ſometimes do not.

Experiments

Experiments on the Sciatick Nerve, on which a Ligature was made.

A new ſpecies of experiments remained to be made on the nerves, which might probably decide the queſtion. I reflected that the nerve could only communicate the diſeaſe of the venom to the animal, in conſequence of there being a free communication betwixt the nerve and the animal itſelf, and thought of putting an entire ſtop to this communication, without even cutting the nerve. We know that a thread which makes a ſmall preſſure on a nerve, entirely prevents this communication; that the muſcle no longer obeys the will of the animal; and that the nerve is no longer the inſtrument or organ, either of motion or ſenſation.

In conſequence of this hypotheſis, I laid bare the ſciatick nerve on one ſide, of a rabbit, and tied it ſtrongly in two parts with a thread. There was a portion of nerve betwixt the two ligatures of more than ten lines. I covered it with a piece of linen in ſeveral folds, and wounded it repeatedly with the venomous teeth of two vipers, taking care to cover all the parts about it effectually, to prevent a communication of the venom. The rabbit died at the end of ſixteen hours. The part of the nerve betwixt the ligatures was white; the muſcles adjacent to the nerve were but very little redder than uſual; the heart, auricles, and great venous veſſels, were

were filled with a fluid blood, ſcarcely darker than it is in its natural ſtate.

I laid bare the ſciatick nerve on one ſide, of another rabbit, and tied it in the way I have juſt deſcribed. I then wounded it betwixt the two ligatures with venomous teeth, and covered it with linen. The rabbit died at the end of eighteen hours. The nerve was in its natural ſtate. The adjacent muſcles were red and livid, for the depth of four lines and more.

Having laid bare one of the ſciatick nerves of another rabbit, I wounded it as above. The rabbit died at the end of ſeventeen hours. The nerve was in its natural ſtate, and the muſcles about it ſcarcely inflamed.

Theſe three experiments ſhow, that the greater or leſs degree of inflammation and lividity in the muſcles adjacent to the ſciatick nerve, is not owing to the venom; and that even the death of the animal may ariſe from ſome other cauſe. It is very certain, that in the caſes in which the nerve is tied, we do not ſee any livid ſpots on this part; and, conſequently, that they are occaſioned by the free communication of the nerve with the animal.

I repeated this experiment, with the ſame circumſtances, on four other rabbits, all of which died in leſs than nineteen hours. The nerve in each of them was white, and in its natural ſtate. The adjacent muſcles in two of them were ſcarcely inflamed; in the other two they were livid for a certain

tain depth. In one of these two last, a part of the pectoral muscles was inflamed.

I confess, that in combining all these experiments together, I find nothing that can give me the smallest suspicion, of the nerve being a means of communicating the venom of the viper to an animal, and of exciting in it the disease this venom occasions. It is true, that there are livid spots on the venomed nerve, which are not observed when it is tied; but may not these be purely mechanical, and the effect of the wounds made by the teeth? And even though they should be occasioned by the venom itself, does it on that account follow, that the venom acts on the nerve, as a venom and not otherwise? Is it therefore demonstrated, that the nerve ought to communicate it to the other parts of the animal?

We are now acquainted with the consequences that ensue from the application of the venom to the sciatick nerve; when this nerve is entire; when it is cut, as well above as below; and lastly, when there are two ligatures made in it. It remains to compare all the effects already known, with those that will be observed, in inflicting on the nerve simple mechanical wounds. After what we have seen, these comparative experiments can leave no future doubt.

As the experiments thus far related on the sciatick nerve, were made in three different ways, so I shall divide the comparative experiments into three corresponding classes.

Expe-

Experiments on the Sciatick Nerve, in which mechanical Wounds were made.

Having laid bare the ſciatick nerve on one ſide, of a rabbit, and wrapped it, as uſual, in linen, to the end that all the circumſtances might agree with thoſe of the preceding experiments, I wounded it in ſeveral places with a viper's tooth, that had been dried for upwards of a month, and had been carefully waſhed in water, to remove all ſuſpicion of its concealing any venom. The rabbit appeared to ſuffer violently when the tooth pierced the nerve. It died at the end of twenty-four hours. The nerve was in ſeveral parts red and livid; the muſcles adjacent to it were inflamed and dark, and theſe appearances extended to the lower part of the leg. The abdominal muſcles and integuments were likewiſe inflamed. The right ventricle contained grumous blood.

I laid bare one of the ſciatick nerves of another rabbit, and having wrapped it in linen as uſual, I pierced it in ſeveral places with the point of a fine needle. The animal ſhrieked terribly, and died at the end of thirty-ſix hours. There were ſeveral dark ſpots in the nerve, and the parts adjacent to it were ſomewhat inflamed. The blood in the heart was black and coagulated.

Having laid bare one of the ſciatick nerves of another rabbit, and wrapped it in linen, I pricked it it ſeveral times with a needle. The rabbit exhibited marks of pain, and died at the end of twenty-ſeven hours. The muſcles about the nerve were ſomewhat livid and inflamed, and the nerve itſelf covered all over with red and black ſpots. The blood in the heart was black and coagulated.

Several important truths are demonſtrated by theſe experiments.

I. That the livid and red ſpots of the nerve are the effect of ſimple mechanical wounds.

II. That the death of the rabbits is owing to the ſimple wound of the nerve, and not to the venom.

III. That the venom of the viper, communicated to the nerves, neither occaſions in any degree the diſeaſe of the venom, nor haſtens the death of the animal.

IV. And laſtly, that the venom of the viper is altogether innocent to the nerves, having no greater action on them than pure water, or the ſimple ſolution of gum arabick in diſtilled water. I have aſſured myſelf by other experiments, that it is not at all offenſive to theſe organs.

The experiments I have juſt related were not yet ſufficient to ſatisfy and convince me perfectly. I knew by experience how eaſy it is to be miſled by facts, when they are but few in number, and was therefore deſirous of repeating the ſame proceſs over again on four other rabbits. The event was perfectly ſimilar to that of the three caſes related above. The rabbits all died; the ſciatick nerve, in each of them, was more or lefs covered with livid and red fpots; the adjacent mufcles were, in a greater or lefs degree, inflamed and livid; and the blood in the heart was in general black and coagulated.

Experiments on the Sciatick Nerve.

Having laid bare the fciatick nerve on one fide, of a rabbit, I tied it in two places with a thread, and pricked it feveral times with a needle betwixt the two ligatures. The rabbit died at the end of thirty-three hours. The lungs had feveral dark fpots; the nerve was white, and in its natural ftate; the blood in the heart was dark, but fluid. The rabbit, when I opened it, was ftill warm.

Having laid bare the fciatick nerve on one fide, of a fecond rabbit, and tied it in two places, I pricked it betwixt the two ligatures with a needle. The rabbit died at the end of eighteen hours. The nerve was white, and in its natural ftate; the blood in the heart black and coagulated; and the mufcles that furrounded the nerve red and livid.

I repeated this experiment on two other rabbits, tying the nerve, and pricking it with a needle, as ufual. Both rabbits died; one at the end of thirty hours, the other at the end of thirty-five. The nerves were in a natural ftate, but the mufcles were inflamed, and in one of the rabbits, livid for a confiderable ſiderable depth. The blood in the heart was black and grumous.

Experiments on the Sciatick Nerve divided above and below.

Having laid bare the ſciatick nerve of a rabbit, I divided it in its inferiour part, and wrapped it in linen, in the way I had done in all the caſes related above. I pricked it ſeveral times with a needle. The rabbit gave repeated ſhrieks, and died at the end of thirty-ſeven hours. The nerve was covered with black and livid ſpots; the adjacent parts were ſomewhat inflamed, and the heart ſhrunk, and very hard. The venæ cavæ continued to move for five hours after I had opened the thorax, their motion beginning at where they ariſe from the auricles.

I divided the ſciatick nerve of another rabbit, and having wrapped it in linen, pricked it ſeveral times with the point of a needle. The rabbit died at the end of fifty-four hours. There were black ſpots in ſeveral parts of the nerve; the muſcles about it were ſcarcely inflamed; the blood in the heart was in a fluid ſtate.

I made the ſame experiment on another rabbit, the ſciatick nerve of which, when divided, I pricked ſeveral times with a needle. The rabbit died at the end of thirty hours. The nerve was in ſeveral places red and livid; the muſcles were livid and inflamed; and the blood in the heart black and grumous.

I was desirous of repeating the same experiment, with precisely the same circumstances, on four other rabbits, all of which died in less than forty hours, and one of them before the expiration of eighteen. The muscles in all of them were in a greater or less degree inflamed, and the nerve more or less red and livid. The blood in the heart, in some of them only, was black and coagulated.

Seeing that all these experiments correspond in a certain degree, both with each other, and with the relative ones of the venomed nerves, I did not think it necessary to make a great number of trials on the sciatick nerve cut in the upper part. I therefore made only two, and these agreed in their consequences with those of a similar kind in which I employed the venom.

I do not conceive that any doubt can remain after these experiments, as to the entire innocence of the venom of the viper, applied to the sciatick nerve, and as to the impossibility of the bite of this animal producing the disease of the venom, when confined to a nerve alone.

This new truth in animal physicks is of the greatest importance in understanding the nature of the venom of the viper, and its action on the animal body. I must acknowledge, that I had need of all the experiments on the nerves thus far related, and which are in so great a number, and varied so many different ways, to be fully and clearly persuaded of this circumſtance. Every thing concurred to a belief of the contrary. The rapidity of the diſeaſe, the ſuddenneſs of the death, the momentaneous loſs of ſtrength, the very violent convulſions, the very acute pain, and, in a word, every ſymptom that characterizes the diſeaſes of the nerves, ſeemed to exiſt in the animal when the nerve was bit. It is however certain, that the venom of the viper does not communicate itſelf to the other parts by the medium of the nerves, and that the ſubſtance of them, which cauſes the ſenſation of the animal, and on which life itſelf ſeems to depend, is not changed by the action of this venom. The experiments are direct, they are in a great number, and varied exceedingly; the fact is certain, and the errour was on our ſide, the offspring of prejudice and opinion, and not of nature and experience. On the other hand, we have ſeen that the venom of the viper, introduced into the blood, without touching any veſſel or any ſolid part, kills animals inſtantly, bringing on very acute pains, and very violent convulſions. I have ſeen the relaxed ſphincters give paſſage both to the urine and fœces.

Here an occaſion preſents itſelf of examining the principles and grounds on which this doctrine of theoretical and practical medicine, that aſcribes diſeaſes to the nerves, and ſubmits ſo many motions and functions to a nervous principle, is ſupported. The field is ſo very extenſive, that, although this diſcuſſion might be very uſeful to the practice of medicine, I cannot allow myſelf a moment's ſtay in it. It will be sufficient for the present to draw this general conclusion—that the usual symptoms of nervous diseases are equivocal and deceptious; that they may exist without there being any disease of the nerves; and that a simple change in the blood may be capable of producing all this derangement, and that instantly.

Had the celebrated English physician, Mead, known that a small quantity of the venom of the viper introduced into the blood, kills a large and strong animal almost instantly; and that this venom is entirely innocent to the nerves; he certainly would not have had recourse to the animal spirits, and to the nerves, to explain the action of the venom on animals that have been bit. But he was entirely ignorant of these two important truths, which were likewise unknown to all the other physicians of his time.

Mead employed the same principle, that is to say, the nerves and animal spirits, to explain the nature and effects of the other poisons. The nerves are affected every where; the animal spirits are deranged and in motion every where; and tumult and nervous agitations are observed every where. He will have this principle applied, not only to the effects of the venom of the viper and the other poisons, but likewise to several other very violent diseases, and amongst others, the plague. This theory is absolutely false as to the venom of the viper, which it seemed to favour the most. I do not believe it any truer as to several other poisons, par-

particularly thofe of the animal kingdom; and after the experiments I have made, I do not find it demonftrated as to the plague, and other difeafes.

When we examine the reafons that have determined naturalifts and phyficians to recur to the nerves in explaining thefe difeafes, (whether natural or the effect of poifon) we find them to be founded on two principles—the rapidity of the difeafe itfelf; and the convulfions and very fudden proftration of ftrength in the animal.

The firft of thefe two reafons is of no weight, fince I have fhown, that a very fmall quantity of the venom of the viper introduced into the blood, kills an animal in a few inftants; and the fecond is neither evident nor certain, fince experiment itfelf has demonftrated to us, that a little of this venom, in the fame circumftances, produces very violent convulfions, and caufes a proftration of ftrength in the animal in a few moments, notwithftanding it only touches the fluid parts of the blood. I do not befides think it difficult to explain the convulfions, without recurring either to the animal fpirits, or to the nervous fyftem. In the firft part of this work, I have mentioned the circumftance of convulfions arifing fimply from a want of equilibrium in the parts, occafioned by the unequal diftribution of blood in the organs, and by the unequal lofs of irritability in the mufcles. I did not know at that time, either that the nerve was not obnoxious to the venom of the viper, or that this venom was mortal when fimply introduced into the blood. This fubject appears to me of fufficient importance to deferve a work apart. In that work the other poifons might be examined, as I have done the venom of the viper; their effects might be analized; and all the circumftances attending them might be noted. What lights might not fuch an undertaking throw on animal phyficks, on the theory of poifons, and on medicine itfelf! The road is open, and nothing is needed but a patient and induftrious obferver. I can venture to promife him, in the courfe of a few years, the moft brilliant, and perhaps too, the moft ufeful, difcoveries.

But let us return to our experiments.

Although I had affured myfelf of the innocence of the venom of the viper, when applied immediately to the nerves, I had a conftant fufpicion however, that the nerves might at leaft be neceffary to the action of this venom on the blood. They might perhaps depofit fome unknown principle, fome fubtile fluid, in the veffels of the animal, and this principle, or fluid, might be effential to life, and likewife to the found ftate of the blood. This was a new mode of confidering the action of the venom on the living body, differing effentially from all the hypothefes that phyficians had hitherto invented; and it remained to know whether the venom of the viper would produce a difeafe of greater or lefs violence, on the nervous communication betwixt the part bitten and the animal being ftopped.

Bite

Bite of the Viper on Frogs that had been deprived of the Head.

To obtain ſome information on theſe reſearches, I made the following experiments.

I cut off the head of a frog, the leg of which I had bit, twice by a viper. This was followed by no ſymptom of diſeaſe in the part.

I cut off the head of another frog, and, after ſtripping off the ſkin, had one of its legs bit repeatedly by two vipers. The frog has no ſymptom of diſeaſe.

I cut off the head of a third frog, which I had bit repeatedly in the leg, the ſkin ſtill remaining on. There appeared ſome ſlight ſymptoms of diſeaſe in the leg. At the end of two hours I introduced a pin into the ſpinal marrow, and there was ſome ſmall degree of motion in the muſcles.

I repeated this experiment on four other frogs that had been deprived of the head. Three of them had no ſign of diſeaſe; in the fourth there were ſome little indications of it.

Theſe experiments appearing to me neither ſufficiently clear nor uniform, I was deſirous of repeating them on twenty-four other frogs, in which I cut off the head, as uſual. Twelve were bit in the leg repeatedly by ſeveral vipers, and the twelve others had punctures made in the leg, either with fine needles, or with vipers' teeth dried, and free from

from venom. The confequences were very vague Of the twelve that were bit, three only had the difeafe of the venom; and of the twelve that were not bit, but pricked with needles, or wounded with venomous teeth, one had fymptoms of inflammation and lividity in the leg, that might have been confounded with the fymptoms that accompany the difeafe of the venom.

We may, I think, fay in general terms, that a frog deprived of its head, contracts the difeafe of the venom with greater difficulty on that account; and that in thefe circumftances the part bitten is lefs changed by the venom than otherwife. Thefe experiments, however, did not throw a fufficient light on the queftions I propofed to myfelf to clear up, and I therefore determined to fall on a new mode of experimenting.

Experiments on Frogs, in which the Spinal Marrow was divided.

I divided the fpinal marrow of a frog, at two lines above the part where the nerves that are fent to the legs and feet, rife out of the vertebræ. I then had one of the legs of the animal bit repeatedly by two vipers. There was no appearance of a communication of difeafe.

I repeated this experiment on four other frogs, and the event was the fame; I could not obferve any fign of difeafe in the legs that been bit.

After

After having ſeparated the head from four other frogs, I entirely deſtroyed the ſpinal marrow with a bit of wood. I then had the legs of the animals bit, but could obſerve no ſubſequent ſymptom of diſeaſe.

On repeating the experiment of the divided ſpinal marrow on ſix other frogs, I found in four of them not the ſmalleſt appearance of diſeaſe. There was ſome doubt as to the fifth, but the ſixth appeared to have a real attack of the diſeaſe of the venom.

This laſt caſe made me ſuſpect the experiments I have related above on the nerves of frogs, and I therefore thought it expedient to proceed to ſome experiment that would be more luminous and leſs equivocal. I procured large rabbits for this purpoſe.

Bite of the Viper on the Parts, the Nerves of which had been divided.

I divided the ſciatick and crural nerves of the right leg of a rabbit. I ſtitched up the inciſed ſkin, and had the leg bit by three vipers, each viper biting thrice. The rabbit returned to its food a little time after it had been bit, and recovered. At the end of twenty days I employed it in another experiment. I muſt remark, that a degree of motion ſtill continued in the leg, and that I had my my doubts as to the crural nerve having been effectually divided.

I cut the ſciatick and crural nerves of the right leg of another rabbit, aſſuring myſelf that they were completely divided. Having ſtitched up the ſkin, I had the leg bit by three vipers, each of which bit thrice. The rabbit died at the end of eighteen hours; the muſcles of the leg that had been bit were black, livid, ſwelled, and ſphacelated; the abdominal muſcles were inflamed; as was alſo the whole of the internal part of the ſkin.

Theſe two experiments could not have been more contradictory to each other; it is however certain, that in the ſecond caſe there was a true diſeaſe of the venom. The firſt caſe proves nothing more, than that an animal may, on ſome particular occaſion, be bit repeatedly, even by ſeveral vipers, and not have an attack of the diſeaſe. Caſes analogous to this have been related in the preceding parts of this work.

I cut the ſciatick and crural nerves of the leg of another rabbit; the nerves were effectually divided, and the leg had loſt all motion. I then had the part bit by three vipers, repeatedly by each. The rabbit died at the end of ſixteen hours. The muſcles of the leg were livid and gangrened throughout their whole ſubſtance.

I repeated this experiment with the ſame circumſtances on two other rabbits, one of which died at the end of twenty hours, the other at the end of twenty-four. They had both the moſt determined ed ſymptoms of the diſeaſe of the venom, in the leg that had been bit.

Theſe experiments are poſitive and uniform, and prove indubitably, that it is of no conſequence as to the diſeaſe of the venom, whether the nerves of the parts that are bit, are, or are not, cut; or whether they communicate, or have no longer any communication, with the animal.

But in theſe experiments, there ſtill ſubſiſts a nervous communication betwixt the part bitten and the animal. This communication is formed by the ſkin of the animal that covers the part where the venom is received. We muſt therefore put a ſtop likewiſe to this communication, by removing the ſkin.

After having divided the ſciatick and crural nerves on one ſide, of a rabbit, and ſtitched the inciſed ſkin, I made a circular inciſion in the leg, at four fingers diſtance above the part where I had propoſed to have it bit by vipers. As ſoon as the inciſion was made, I ſtitched it all round. I then had the leg bit repeatedly by three vipers, the teeth penetrating into the ſkin. At the end of two hours there was no ſymptom of diſeaſe; at the end of ſix the part that had been bit was viſibly inflamed; at the end of ten, there was an oozing of blood from the part of the ſkin that had been punctured by the teeth of the viper; at the end of twenty-two, the blood flowed in ſtill greater abundance; at the end of twenty-four, the part was very much ſwelled, but no lividneſs was to be diſcerned; at the end of

thirty

thirty the ſkin opened, and a wound formed. The rabbit after living eight days, was employed on another occaſion.

In this laſt experiment, there can be no doubt but that the diſeaſe of the venom was communicated to the part bitten, although it was not very violent.

I now thought of making a comparative experiment.

For this purpoſe I had a rabbit bit repeatedly in the leg by three vipers, without cutting either the nerves or the ſkin. At the end of eight hours the leg was ſwelled, but not livid. At the end of twenty-two, a cyſt or bladder, filled with a dark-coloured humour, formed betwixt the legs, near the part that had been bit. The rabbit died at the end of forty hours. The ſkin was broken, and corroded at the part bitten; the muſcles of the leg were livid and gangrened; the heart, auricles, and large veſſels, were filled with black and grumous blood; and there were concretions of blood even in the aorta, which is uſually found empty.

I repeated the preceding experiment on three other rabbits, and, after dividing the nerves, making the circular inciſion in the ſkin, and ſtitching it, had each of them bit repeatedly by three vipers. They all had ſymptoms of the diſeaſe of the venom in the part bitten.

I had another ſuſpicion, that there might ſtill ſubſiſt, after all, ſome nervous communication betwixt the animal and the leg, after the ſciatick nerve was divided. I conjectured that the viper might

might have ſtruck its teeth on ſome fibre of the great gluteus muſcle, which deſcends very low in the leg. This was a ſufficient inducement to me to make the following experiments.

I divided the ſciatick and crural nerves on one ſide, of a rabbit, and made a circular inciſion in the ſkin, which I afterwards ſtitched. I had the leg bit by three vipers, by each repeatedly, but in ſo low a part as to avoid the above-mentioned muſcle. At the end of two hours the part that had been bit began to inflame; at the end of twenty-two the ſkin was broken, but not puffed up; at the end of forty-two the animal was apparently recovered; and at the end of eight days, was employed in other purpoſes.

I divided the ſciatick and crural nerves on one ſide, of another rabbit, and made an inciſion, which I afterwards ſtitched, for the whole circumference of the leg. I had the leg bit in its inferiour part by three vipers, each viper biting thrice. At the end of eight hours, the ſkin burſt, and diſcharged a humour; at the end of twenty-two this broken ſkin was ſwelled and livid; and at the end of ſixty, the rabbit was in a dying ſtate. I opened it, and found all the muſcles of the leg gangrened, and almoſt the whole of the adipoſe membrane that covers the abdominal muſcles full of extravaſated blood. The blood in the heart was in a diſſolved ſtate.

I divided the ſciatick and crural nerves of another rabbit, and made an inciſion in the ſkin, which I ſtitched, all round the leg. I had the inferiour part of the leg bit repeatedly by three vipers. At th end of two hours the part that had been bit was become swelled; at the end of eight it was considerably so; at the end of twenty-two, the skin was broken, but without swelling; at the end of forty two hours, there was simply a wound at the part bitten. The rabbit lived ten days, and was employed in other experiments.

These experiments show, that the venom of the viper produces its usual effects on the parts bitten, notwithstanding all nervous communication has been stopped, betwixt these and the other parts of the animal. But it is not yet decided, whether, supposing some active principle which mixes with the blood to be separated from the nerve, this principle does, or does not, cease to exist the instant the nerve is divided; particularly as the nerves still remain in the part bitten, although they are no longer the instruments of sensation, and voluntary motion. This reflection made me fall upon the following experiments.

I divided the sciatick and crural nerves on one side, of a rabbit, and likewise made the circular incision in the skin, which I stitched. I left the part in this state during sixteen hours, and then had it bit below the incision by three vipers, each of which bit repeatedly. The rabbit died at the end of twenty-two hours. All the muscles of the leg were livid, gangrened, and putrid; the pericardium was filled with a transparent fluid; the right ventricle and right auricle were filled with black grumous blood;

blood; and the blood in the large vessels was in the same state.

Effects of the Bite of the Viper on Rabbits, the spinal Marrow of which was divided.

I shall conclude my experiments on the parts deprived of their nerves, and bit by the viper, by relating three experiments made on rabbits, the spinal marrow of which had been entirely cut through. I divided it beneath the loins, and in so effectual a way on all sides, that there could be no suspicion of any communication of nerves, betwixt the legs and the other parts of the animal.

The spinal marrow of a rabbit having been divided in the way I have just mentioned, and a circular incision round the leg made in the skin, which I had stitched, I had the inferiour part of the leg bit by three vipers, by each repeatedly. At the end of an hour, a small tumour formed in the part bitten; at the end of two this part was very much swelled, and very livid; and the rabbit died at the end of seven. The part that had been bit was gangrened all over, and the gangrene penetrated into the whole substance of the muscles that had been wounded by the teeth. The blood in the heart was black and grumous.

I divided the spinal marrow of another rabbit, and by the help of scissars detached a great portion of the ſkin that covers the crural muſcles. When the latter were in this manner laid bare, I had them bit repeatedly by three vipers. A few minutes after, there were ſymptoms of the diſeaſe of the venom in the part, and the rabbit died at the end of ſeven hours. The muſcles that had been bit were livid and inflamed; the blood all about them was extravaſated into the cellular membrane; there were livid ſpots in the lüngs; and the heart was filled with blood, which was almoſt in an entire ſtate of diſſolution.

I repeated this experiment on another rabbit, with the ſame circumſtances, and the reſult was likewiſe the ſame. The rabbit died at the end of ſix hours, with the muſcles of its leg affected by the diſeaſe of the venom.

Thus are we aſſured, that the nerves which are ſent to the parts bitten, in no way contribute to the diſeaſe of the venom of the viper, and that this venom is altogether innocent to the nerves; important truths we were before ignorant of. What is ſtill hidden from our ſight, is the occaſion of the blood, united with the venom, coagulating in an inſtant when it is incloſed in the veſſels of the animal, and not coagulating in the open air.

Effects

Effects of the Venom on the Parts of an Animal, the Circulation of which was interrupted.

I hoped to draw fome information from the expetiments which follow. They confifted in examining the effects of the bite of the viper, on the parts of animals in which the arteries and veins had been previoufly tied by ligature. This was altogether a novel enquiry, and it was not at all amifs to know the effects that would be produced in fimilar cafes.

I made a ligature in the belly, on the aorta defcendens and vena cava of a rabbit. Having ftitched the fkin, I had the leg of the rabbit bit by three vipers, by each repeatedly. The animal died at the end of nine hours. The leg was gangrened in the part where the bites had been received, but in no other.

I divided and removed in the belly of a rabbit, the arteries and veins that go to the right leg, and likewife removed a large portion of the fkin of the leg, which I had bit at the part where the mufcles were bare, by three vipers, each of which bit thrice. At the end of an hou· there were certain fymptoms of local difeafe. At the end of two hours the leg was livid at the part bitten, but not elfewhere. The heart was filled with black and grumous blood.

I made a ligature, as in the firft experiment, on the arteries and veins within the belly of two rabbits. Each of them was bit repeatedly by three vipers. In one of them the ſkin was in an entire ſtate; in the other it was cut circularly, as in an amputation, and ſtitched. They were both dead at the expiration of twenty hours. There were ſymptoms of diſeaſe in the parts bitten. However, the diſeaſe was ſlight, and neither deep nor extenſive. The blood in the heart was black and coagulated.

I divided in the belly, the arteries and veins of another rabbit, but neglected to have it bit by vipers. It died at the end of ſixteen hours. The lungs were livid; and the heart, auricles, and great veſſels, filled with black and grumous blood. This experiment is a ſtill further demonſtration to us, that the grumous blood in the heart and neighbouring veſſels is an equivocal ſign, when it is taken alone, and without being accompanied by others.

I repeated this experiment of making a ligature in the belly, on the veins and arteries, on three other rabbits, having each of them bit in the leg by three vipers. They all three died in leſs than ſeventeen hours. The muſcles that had been bit were attacked by the diſeaſe of the venom, but not the adjacent ones. This local diſeaſe was but of little conſequence.

We may deduce with certainty from theſe experiments, that the venom of the viper produces its uſual effects, even when the parts bitten no longer participate of the circulation of the blood in the animal machine. In theſe caſes we ſee in general, that the diſeaſe is leſs extenſive and leſs violent, than when the blood circulates in the part; and this

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particular agrees very well with the experiments in which the venom was injected into the jugular vein.

Effects of the Venom on Parts, the Vessels of which were cut.

I was desirous of seeing what would happen to a rabbit, the crural arteries and veins of which were tied, and cut beneath the ligature, several hours prior to the leg being bit. In these cases the blood has not only ceased to circulate in the leg, but has been a long time stagnant: it may already be changed in a great measure, may have sustained a considerable loss in its quantity, and may be deprived of a subtile principle of some kind or other. The rabbit that I got ready in this way, remained in this state during upwards of eight hours. At the end of that time, I had it bit in the leg by three vipers, each of which bit repeatedly, the skin having been previously removed from the part. The rabbit died three hours after. The muscle at the part where the vipers had bit, was somewhat darker than in the adjacent parts; but this was scarcely sensible.

I cut the crural artery and vein of a rabbit, in the same way beneath the ligature, and waited ten hours before I had it bit. At the end of twenty hours it was very lively, and I had it again bit repeatedly by three vipers, the leg being previously bared of the skin, in the part bitten. It died six hours after. The muscles that had been bit were livid throughout their whole ſubſtance, but the diſeaſe was confined to the part on which I operated.

I repeated this experiment on two other rabbits, having them bit each in a leg, without removing the ſkin, eight hours after the ligature had been made, and the crural artery and vein cut. I took the precaution to make repeated compreſſions on the leg, that the arterial and venous blood might flow out at the opening in the veſſels. Both rabbits died in leſs than eleven hours. The fleſh where the teeth had entered appeared of a deeper colour than uſual, and this diſcoloration penetrated to the depth the teeth had extended to. The other parts were in a natural ſtate.

I got ready two other rabbits in the ſame way, as a comparative experiment; they were therefore not bit by vipers. They were both dead at the end of ſeventy-two hours.

It now remained to examine the effects of the venom of the viper, after having tied the arterial and venous veſſels ſeparately.

For this purpoſe I made a ligature on the vena cava in the belly of a rabbit, and afterwards made the circular inciſion in the ſkin of the leg, and ſtitched it. I then had the leg bit repeatedly by three vipers. At the end of twenty-four hours, there were ſymptoms of the diſeaſe of the venom in the part bitten. I killed the rabbit in this ſtate, and found that the diſeaſe was circumſcribed to the inciſion made in the ſkin. The muſcles were livid, and the

the adipose membrane filled with dark extravasated blood.

I tied in the belly, the vena cava of another rabbit, and had its leg bit repeatedly by three vipers. At the end of two hours there was an extension of the skin at the part that had been bit, but scarcely any sensible swelling; at the end of four hours, a moisture exuded from it; at the end of ten hours the swelling had encreased; and the rabbit died at the end of fifteen. The part bitten was livid and gangrened throughout its whole substance; the disease was, however, entirely confined to the leg.

The consequences of experiments on two other rabbits treated in the same way, were pretty similar.

I tied the aorta in the belly of a rabbit, and had its leg, covered by the skin, bit repeatedly by three vipers. At the end of six hours, the symptoms of disease were perceptible, and the rabbit died at the end of fifteen. The leg was swelled and livid, and the discoloration penetrated some depth into the muscles. The blood was black in the part that had been bit, and was coagulated in the large vessels.

This experiment repeated on two other rabbits was attended with pretty much the same success.

I shall conclude this chapter, by relating in a few words, two experiments made on rabbits, in the belly of which I had divided all the lymphatick vessels I could find, as far as the ductus thoracicus. An hour after this operation, I had both rabbits bit in the legs, covered by the skin, repeatedly by three vipers. At the end of six hours the leg in each, exhibited the most certain marks of the disease of the venom. It was livid and swelled, and a good deal of humour oozed from it. Both rabbits died at the end of eighteen hours, and the muscles of the leg in each, were livid throughout their whole substance.

Expecting nothing from the continuation of these experiments, and seeing that the stoppage of the circulation of the lymph and chyle has no influence on the usual effects of the venom of the viper, I did not think it necessary to make any further progress in this subject.

CHAPTER V.

Effects of the Venom of the Viper on Blood exposed to the open Air.

ALTHOUGH the experiments hitherto related afford us very important information, we are still in the dark as to the circumstance of the blood coagulating when united in the vessels with the venom of the viper, and not coagulating when blended with the latter in the open air. I have at least conceived that I constantly distinguished a very sensible difference in this fluid, when I had the leg of an animal

animal bit after its being ſeparated from the animal itſelf, and when I had it bit, either ſtill adhering in an entire ſtate to the animal, or faſtened to it mechanically.

In this uncertainty, I judged it expedient to make an analyſis, followed by the experiment of Mead, that relates to the effects of the venom of the viper on the blood drawn from an animal; and as Mead made his experiment on a ſmall quantity of venom and a large quantity of blood, I determined that my trials ſhould be on much ſmaller quantities of the latter, that the effects might be more ſenſible.

I let fall into a ſmall conical glaſs, three drops of the venom of the viper, and twenty drops of the blood that flowed from the neck of a fowl, into which I had made an inciſion. I reclined the glaſs, and ſhook it circularly for ten ſeconds, that the venom and blood might mix well together.

At the ſame time I let fall into a ſimilar glaſs, twenty drops of the blood of the ſame fowl, in the ſame ſtate as the laſt. I ſhook the glaſs as I had done on the preceding occaſion, that, the venom excepted, all circumſtances might be alike. At the end of two minutes, the blood unmixed with the venom coagulated, and was of a fine vermillion colour. On the contrary, the blood united to the venom was black and fluid, notwithſtanding it was a little viſcous and compact.

I repeated this experiment, and the event was the ſame. The venomed blood did not coagulate, and, as before, was of a dark colour; whilſt, on the contrary,

trary, the blood that was not venomed coagulated very ſoon, and ſtill preſerved its bright red colour.

I repeated this experiment on the blood of a guineapig, one of the legs of which I had cut off. The venomed blood at the end of twenty-four hours was ſtill black and diſſolved, whilſt the other coagulated in leſs than two minutes, and continued to preſerve its red colour. The venomed blood did not harden till it dried by degrees and ſplit into ſcales, and preſerved its black colour till the laſt; inſtead of which the blood in a pure ſtate, ſtill continued red, even after it had dried and ſplit.

The black colour of the blood that was mixed with the venom, agrees very well with the moſt uſual effects of the bite of the viper on animals, and with the effects of the venom introduced into the jugular vein of rabbits. But the other circumſtance in theſe latter experiments is altogether ſingular and unexpected. Inſtead of the venom coagulating the blood, as one would ſuppoſe it ought to do, it even keeps it from that coagulation which is natural to it in the open air, and preſerves it in a conſtant ſtate of diſſolution. Here then the venom not only does not produce its ordinary effect on the blood, namely, that of coagulating it, but produces an altogether contrary effect, namely, that of keeping it in a diſſolved ſtate, and ſo preventing that coagulation which would otherwiſe take place.

This ſingular effect of the venom on blood expoſed to the air, ſeemed to promiſe ſome important diſcovery, relative to the action of it that ſucceeds

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the bite of the viper in animals. I reflected, that this bite is entirely innocent to the viper itſelf, and likewiſe to many other animals with cold blood, and that it is not mortal to certain animals, ſuch as frogs, in which it does not produce the diſeaſe of the venom till very late, and with difficulty. In conſequence of this, I perſuaded myſelf that the effects of the venom on the blood of vipers and frogs muſt be very different from thoſe it produces on the blood of warm animals, and that from this difference, that of the diſeaſe and death of theſe animals muſt preciſely depend. Theſe were my reaſonings, and I drew no ſmall expectations from them.

I accordingly put into a ſmall conical glaſs, three drops of venom, and thirty drops of the blood that flowed from the neck of a viper, after I had cut off its head. I ſhook the glaſs, as uſual. The blood, which became ſomewhat darker, did not coagulate. At the end of two hours there was a ſeparation of ſerum, which floated on the top, the red part of the blood being beneath. This was dark, and viſcid like glue, but was not coagulated.

I had at the ſame time got ready a comparative experiment. I had put into a glaſs of the ſame kind, thirty drops of the blood of the ſame viper, but without having introduced the venom. I ſhook the glaſs, as uſual. The blood did not coagulate, and became covered with a great deal of ſerum, through which the ſanguineous fibres, which were very red, were diſtinguiſhed. At the end of two hours

hours the ferum was in a much larger quantity than in the preceding experiment; at the end of twenty-four the red fibres were in their ufual ftate; but notwithftanding this, the blood was thinner than that which had been venomed. At the end of thirty-five it was ftill fluid, with a great deal of ferum fwimming on the furface; at the end of fifty it was become thicker and more tenacious; and at the end of fixty was dry and red.

I mixed in a glafs, three drops of venom with fifty of the frefh blood of a viper, and received in another glafs fifty drops of the fame blood, without making any addition to it. I fhook both glaffes a little, and equally. The blood unmixed with venom, remained till the laft redder than the other, and with a greater proportion of ferum. At the end of thirty hours the venomed blood coagulated, but the other did not.

We fee by thefe experiments, that the colour of the blood of vipers which has been united with the venom of the animal, agrees very well with that of the blood of warm animals blended with the venom in the fame way, notwithftanding there is a great difference in all the other circumftances. Thefe experiments, however, are as yet too little varied to admit any certain conclufions to be drawn from them.

I put three drops of venom into a glafs, and added thirty drops of the blood of a frog, the head of which I had juft cut off. I likewife put thirty drops of this blood into a glafs, without adding any venom.

venom. I ſhook the two glaſſes, as uſual, and at the end of thirty minutes examined the blood in each. I found the venomed blood black, but not coagulated. The blood unmixed with venom had leſs ſerum than the other, was redder and more fibrous, and was likewiſe in a fluid ſtate. At the end of three hours the blood was black and diſſolved, but viſcous, and without any apparent ſerum. The other blood had a great deal of ſerum on its ſurface. At the bottom it was red and coagulated, but the coagulum was moveable, fibrous, and viſcous.

Not ſatisfied with this laſt experiment, which I repeated twice with pretty much the ſame ſucceſs, I determined to make at the ſame time, experiments on the blood of vipers, on that of frogs, and on that of guineapigs, and to follow minutely all the changes that I ſhould obſerve.

I took ſix conical glaſſes, ſimilar to thoſe I had employed before, and put into each of the three firſt, four drops of venom, and thirty drops of blood. In the firſt glaſs was the blood of a viper, in the ſecond that of a frog, and in the third that of a guineapig. In each of the other three glaſſes I ſimply put fifty drops of blood, taken from one of the ſame animals, and in the ſame order. I ſhook the ſix glaſſes a little, and equally, and then left them in an undiſturbed ſtate for ſome time. At the end of a few minutes the blood in the three venomed glaſſes was black, and much more diſcoloured than that in the three others, and was already coagulated; the blood of the viper unmixed with venom, was however much less so than the others, and was perhaps rather viscous than effectually coagulated. The blood of the viper is besides of a darker red than either that of the frog or guineapig. At the end of some time, I observed that the venomed blood of the viper, and that of the frog, had a good deal of serum on their surface, but that there was none on that of the venomed blood of the guineapig. There was likewise no appearance of serum on the surface of the blood in the three glasses, in which it was unmixed with venom. At the end of eight hours the blood of the frog contained as much serum as the venomed blood, and was in the same dissolved state as the last, but redder. The pure blood of the viper never had any serum on the top, and continued coagulated as usual; but the venomed blood of the viper was darker than the other, dissolved, and extremely viscous. At the end of three days the venomed blood of the viper still preserved the large quantity of serum it had at the beginning; it was, however, black and viscous. The blood of the viper, unmixed with venom, had a little serum, was red, fibrous, and almost wholly coagulated. The venomed blood of the frog was entirely dissolved, of a greenish hue, and contained a little serum; but that which had not been venomed, had a great deal of serum on its surface, was coagulated, and redder than the other. The venomed blood

blood of the guineapig was black, viſcous, and without ſerum.

At the end of eight hours I examined the red globules of the blood in the three venomed glaſſes, and found them but very little changed in ſhape, and ſcarcely different from the globules of the pure blood in the three other glaſſes. At the end of eight days, however, I found that the globules of the venomed blood of the viper were conſiderably altered in ſhape; ſeveral of them were broken, and they were in general much more changed than thoſe of the viper's blood unmixed with venom. The globules of the venomed blood of the frog were almoſt all diſſolved; thoſe that were not were disfigured, and very much broken. The venomed blood of the guineapig had, on the contrary, its globules enlarged; they had in ſome meaſure changed their ſhape, and were in a greater or leſs degree diſſolved; they did not differ a great deal from thoſe of the pure blood of the ſame animal.

Theſe laſt obſervations on the globules of blood can be of no uſe in explaining the immediate effects of the venom of the viper on its being introduced into the veins; and the changes we have ſeen in theſe globules are not obſerved till a long time after the action of the venom on the animal. If the animal is ſmall, it is dead a long time before any of theſe changes are obſerved.

I repeated the experiment on the blood of the viper, frog, and guineapig, twice, and the conſequences, although they were not in every reſpect perfectly ſimilar, were very uniform. I have therefore deemed it unneceſſary to enter into a detail of them.

We ſee in a general way, that the venom of the viper changes the blood both of warm and cold animals black; that of the animals on which it acts as a venom, and that of thoſe on which it has no ſuch action. This very uniformity of change of colour ſhows, however, that the venom of the viper does not kill animals, in conſequence of the principle that changes the blood with which it is united, black. It would otherwiſe be a venom to the viper itſelf, which it is not.

But it is not the ſame as to the coagulation of the blood. The venom acts little, or not at all, on the blood of the viper, and the trifling variations we have obſerved in this reſpect are not at all to be attended to. It is otherwiſe as to the blood of the frog, and ſtill more ſo as to that of the guineapig. Scarcely is the latter in the glaſs, when it coagulates; inſtead of which if it is blended with a few drops of venom, it coagulates no more, and remains black, viſcous, and without ſerum. This effect of the venom is the more ſingular, as one would ſuppoſe that it ought to produce a very contrary one. Does the venom, when united with the blood, becauſe it is a venom, or from ſome other principle, deprive it of the power of coagulating?

It has been ſeen that the venom of the viper produces a ſenſible change on the blood drawn from the veſſels of an animal. In theſe caſes the blood

blood becomes black, and remains fluid, instead of coagulating, as it constantly does, when it is not mixed with the venom. On the contrary, this venom, when it is introduced into the circulation of an animal, suddenly impedes it, by causing a coagulation of the blood. The effects of the venom on the blood of animals are certain, but we do not on that account know, on what they depend, nor by what mechanism all these changes are wrought. Does the venom of the viper act on the blood simply as a venom? that is to say, by the very principle that renders it mortal?—We have seen that this venom is a true gummy substance, and that it has all the properties which characterize gums. We have likewise seen, that gums are entirely innocent to animals; and I have observed, that when they are injected in a very small quantity into the blood, the death of the animal is by no means subsequent. But why may not the black colour of the venomed blood, and the fluidity it preserves out of the vessels, depend on the gummy principle of the venom? We know that gums abound with phlogiston, and that phlogiston gives a black tinge to the blood. It is true, that as a gummy substance, it seems that it ought rather to coagulate the blood, than to keep it in a state of dissolution; but experiment alone can reply to all these doubts.

Comparative Experiments made with Gum Arabick.

I diſſolved a few grains of gum arabick in a ſmall quantity of warm, diſtilled, water. The mucilage was tranſparent, and almoſt fluid. I put three drops of this mucilage into a glaſs, and added ſixty drops of the blood of a pigeon, at the juncture of its flowing from the divided veſſels.

At the ſame time I put three drops of the venom of the viper into another glaſs, and added ſixty drops of the ſame blood, perfectly in the ſame ſtate.

I ſhook both glaſſes for a minute, that their contents might be well blended together. At the end of two minutes the blood united with the gum and coagulated, its colour continuing red as in its natural ſtate, and no ſeparation of ſerum followed, although I kept it two days in the glaſs. The blood in the other glaſs ſuddenly became black, and continued fluid as uſual.

We ſee by this experiment, that gummy ſubſtances do not give a dark tinge to the blood, and that they have not the property of keeping it in a diſſolved ſtate, and preventing its natural coagulation. Thus, then, the changes that are wrought in the blood by the venom of the viper, are not the effect of a gummy principle, but of ſome other principle yet unknown to us, probably the very one that conſtitutes it a venom; and indeed we have hitherto

hitherto been able to diſtinguiſh nothing more in this humour, than a gummy principle, and a venomous principle deſtructive of animal life.

I was afterwards deſirous of trying whether the venom of the viper would be no longer a venom when mixed with the blood. For this purpoſe, I put into a glaſs thirty drops of the blood of a pigeon warm from the veſſels, and three drops o venom. I blended the liquors well together, and after twenty-four hours were elapſed, applied ſeveral drops of the mixture to the muſcles of a pigeon. The pigeon ſurvived, and at the end of thirty hours ſeemed ſcarcely to have any ſymptom of diſeaſe.

I prepared a mixture of venom and blood in the ſame way, in another glaſs, but made it with an equal quantity of each fluid. Two minutes after, I covered with it the wounded muſcles of a pigeon, which, although it ſurvived, had certain ſymptoms of the diſeaſe of the venom.

I repeated this experiment on four other pigeons. Three of them died in leſs than eighteen minutes; and the fourth had ſo violent an attack of the diſeaſe, as not to recover till the end of the ſixth day. I got ready two other pigeons in the ſame way, and did not employ the venom till half an hour after its union with the blood in the glaſs. Both pigeons died.

It appears from all theſe experiments, that the venom does not loſe its deadly qualities in conſequence of being united with the blood.

We have ſeen that the venom of the viper is a true gum, and that it has all the eſſential properties of a gum. Why may not this venom prevent the coagulation of the blood in warm animals, and likewiſe in ſeveral of the cold ones, as a ſimple gum, and not as a venom? And why likewiſe may not the blood of the viper be different from that of other animals, ſince we ſee that the venom is innocent to the viper, and not ſo to the others?

In this caſe again it belonged to experiment to decide.

As it did not appear to me that the experiments I have hitherto related were ſufficient to explain this difficult phenomenon, of blood which coagulates in the encloſed veſſels of an animal, and which does not coagulate in glaſſes in the open air, I thought it neceſſary to examine more attentively than before, the effects of the venom on the legs of animals cut off, and bit by the viper; and likewiſe on thoſe on which a ligature had been made previouſly to their being bit. I was apprehenſive that I might have made ſome miſtake in my prior experiments, and that ſome neceſſary attention might have eſcaped me. It was natural to conceive, that after all I had ſeen in my laſt experiments, I was better prepared for nice obſervance than before.

With this preſumption, I made the following experiments.

I had a pigeon bit in the leg repeatedly by a viper, and a few ſeconds after cut off the limb. There was a degree of lividity at the preciſe ſpot wher where the teeth had penetrated; but this was ſcarcely perceptible.

On repeating this experiment with the ſame circumſtances, the conſequences were perfectly ſimilar.

I had the leg of another pigeon bit by a viper, a moment after I had cut it off. There was no ſymptom of diſeaſe, nor any livid appearance.

I wounded the leg of a pigeon with a venomous tooth, and cut it off immediately after. There was ſome appearance of grumous blood in the muſcle the tooth had pierced.

I wounded the leg of a pigeon with a viper's tooth that had been dry a long time, and at the ſame time wounded the pigeon's other leg with a venomous tooth. The wounds made with the venomous tooth became livid, and this lividity penetrated into the whole ſubſtance of the muſcle. There was no appearance of any change in the part of the other leg that had been pierced by the dried tooth.

I wounded the leg of a pigeon with venomous teeth, and cut it off immediately after. There was a dark ſpot, which however I could ſcarcely diſtinguiſh, at the part the teeth had penetrated.

I forced the venomous tooth into the leg of a pigeon, and cut it off immediately after. There was no ſymptom of the diſeaſe of the venom.

I cut off the leg of another pigeon, and wounded it immediately after with a venomous tooth. There was a ſlight appearance of dark extravaſated blood.

I again forced a venomous tooth into the leg of a pigeon, and cut it off immediately after. This was not succeeded by any ſymptom of the diſeaſe of the venom.

I pricked the leg of a pigeon repeatedly with the point of a needle, and cut it off immediately after. I obſerved dark and extravaſated blood at the part that had been pricked.

Although the greater part of theſe experiments ſhow, that the venom of the viper has no action on the parts of animals that have been cut off, there are ſome of them, however, in which we find a ſlight appearance of dark and extravaſated blood.

The experiment made with the needle rendered what I was deſirous of deducing from the others ſtill more uncertain. One would conceive that every time the large veſſels are ruptured, and there is a ſenſible hemorrhage from them, the ſpots and dark colour may appear, without the intervention of venom.

It is in general true, that there exiſts a notable difference betwixt the effects of the venom of the viper introduced into a leg that has been cut off, and the effects of the ſame venom communicated to a leg that continues to make a part of the animal. This difference may occur, either becauſe the quantity of blood in the amputated leg is leſſened, becauſe the blood receives ſomething from the air, or becauſe it, on the contrary, loſes ſomething when in contact with the air, To diſcover which

of

of theſe hypotheſes is the moſt probable one, I made the following experiments.

Experiments on the Venom of the Viper on Limbs that were ſheltered from the Air.

I placed a pigeon in water in ſuch a way, that I could cut off one of its legs without there being any communication betwixt the divided part and the air. A moment before I cut it off I had wounded the leg with a venomous tooth. At the end of four minutes I drew it out of the water. At the part where the tooth had pierced the muſcle, there was a ſmall livid ſpot, on which I immediately made an inciſion, and found it to have penetrated juſt as far as the tooth and venom had reached.

I repeated this experiment twice, and the conſequence was the ſame. The livid ſpot had extended in the ſame way into the ſubſtance of the muſcle.

The blood of the leg amputated in the water, flows from the veſſels in the ſame way as if the part had been cut off in the open air. The ſymptoms, therefore, of the diſeaſe of the venom in the leg ſtill adhering to the animal, and the abſence of theſe ſymptoms when it is detached, do not depend on the different quantity of blood that is found in the two different ſtates of the leg.

This experiment ſeems likewiſe to determine that the blood meets with no eſſential loſs when it is expoſed to the air, ſince it does not ſeem probable that the water which ſuffers the blood to flow from the leg, prevents this ſuppoſed principle from eſcaping with it.

It remains probable, then, that the contact of the air cauſes ſuch a change in the blood of the leg, and that the air itſelf unites in ſuch a way with it, as to produce the diverſity of effects we have obſerved, although it is certain that we cannot explain what this change conſiſts in, and how, in theſe caſes, the air unites itſelf with the blood.

New Experiments on Parts that were cut, after the Circulation had been interrupted in them by a Ligature.

I had now an important experiment to make, to determine the effects of the venom of the viper on parts of animals tied, and afterwards cut off.

I had the leg of a pigeon bit by a viper at the ſame inſtant that I had it tied and cut off. The whole operation was made in three ſeconds, and required the aſſiſtance of three perſons. The amputation was made over the ligature, which was ſo very tight as to prevent the ſmalleſt hemorrhage. The leg when cut off had the moſt decided ſymptoms of the diſeaſe of the venom. There were livid ſpots on it, the veſſels were black and ſwelled, and the blood black, and partly condenſed. Having opened the muſcles, I found that the livid colour

extended

extended for the whole depth of thoſe that were wounded.

I immediately made a ſecond experiment, ſimilar in every reſpect to the foregoing, except that the leg was not bit by a viper. It had not the ſmalleſt ſymptom of diſeaſe.

I had the leg of another pigeon bit once by a viper, and, after four ſeconds were elapſed, made the ligature and the amputation at one and the ſame inſtant. In leſs than a minute the ſymptoms of the diſeaſe were apparent, the whole ſubſtance of the wounded muſcles being in a livid ſtate.

I tied and cut off the leg of a pigeon, and immediately after had it bit once by a viper. The ſymptoms of the diſeaſe of the venom in the part were very violent, and the muſcles were livid throughout their whole ſubſtance.

I tied and cut off the leg of another pigeon, and had it afterwards bit by a viper. The whole ſubſtance of the muſcles was livid.

Theſe experiments ſeemed to me ſufficiently uniform to diſpenſe with my multiplying them any further. They demonſtrate, that the venom of the viper acts as a venom on theſe parts, although they are detached from the animals, provided there is no hemorrhage from them.

We likewiſe ſee, that it is not neceſſary that the uſual circulation of the blood and other humours ſtill continue in the part, ſince I have ſince obſerved that the venom acts on the legs that have been

been tied, even when they have been bit a pretty confiderable time after the ligature has been made.

Experiments on Animals with warm Blood, deprived of the Head.

The experiments made on the frogs that had been deprived of the head, in which it appeared to me that the difeafe of the venom was communicated with difficulty, gave me the idea of feeing whether it would be the fame with warm animals. Thefe experiments have fome relation to thofe that were made on the legs of animals cut off and afterwards bit, and only differ from them in this particular—that here the greater part of the body continues in conjunction with the leg, notwithftanding the blood flows in a great quantity from the neck, where the incifion has been made.

I divided the trachea arteria of a fowl, and having fixed in it the nozzle of a fmall pair of bellows, inftantly cut off the head. I now began to work the bellows, and at the fame time had the leg of the fowl bit repeatedly by two vipers. The fowl lived for upwards of fifteen minutes. There were deep livid fpots in the leg, at the part the teeth had penetrated.

I repeated this experiment on two rabbits, and on a guineapig. They lived beyond all comparifon longer than the fowl, and their life was not equivocal, as was eafy to be diftinguifhed by the volun-

voluntary motions of the parts. It is true, that in these last I prevented the loss of blood, in a great measure at least, by tying the vessels; and it is certain that they might have lived much longer, if the total effusion of blood could have been prevented. The symptoms of the disease of the venom were manifest in all the three, and in all of them the muscles that had been bit were livid.

This experiment shows, that the head of warm and perfect animals is not necessary to life, although it is so to a continuation of it. In a word, an animal may live very well, although deprived of its head, and may be sensible to external objects. The pulmonary respiration, and the circulation of humours in the parts, are sufficient for this effect. This principle of life is still sustained in the animal, which may reasonably be said to be not altogether dead, but only so in part.

CHAPTER VI.

On the Cause of the Death of Animals bit by the Viper.

MY experiments on animals in which the nerves were bit by vipers, show that the venom is a substance perfectly innocent to these organs, that it does not occasion in them any sensible change, and that they are not even a means or vehicle of conveying it to the animal. In a word, it appears that the nervous system does not concur more to the production of the disease of the venom, than does the tendon, or any other insensible part of the animal. On the other hand, all the experiments on the blood, the injection of venom into the vessels, and so on, constantly evince that the action of the venom of the viper is on the blood itself. This fluid is alone changed by the venom, and this fluid conveys the venom to the animal, and distributes it to its whole body. The action of the venom, and its effects on the blood, are almost instantaneous. The colour of the latter is suddenly changed, and the bright red colour that is natural to it, becomes livid and black. This first effect is succeeded by a second. The blood coagulates very suddenly in the lungs, heart, auricles, liver, and in the large venous vessels. Sometimes the

the heart ſtill continues its oſcillatory motions, notwithſtanding the blood it contains is, at leaſt in part, coagulated. At other times, the heart beats with greater force, as if it wiſhed to ſtop the principle of coagulation that exiſts in the blood.

The coagulation of the blood of animals is certainly the moſt remarkable effect of the venom of the viper, and it is this which ought principally to occaſion the derangements in the viſcera, and in their functions. But the whole maſs of blood is not coagulated in the animal, ſince a part of it appears in a diſſolved ſtate. The red and lymphatick parts alone form the coagulum, the ſerous part is more fluid and diſſolved than before. It is certain at leaſt, that the latter is thrown in great abundance on the venomed parts, and ſheds itſelf in great plenty on the adipoſe membrane.

If the coagulated part of the blood is left for ſome time in water, it loſes the black colour it had contracted, depoſits the red part, which unites with the water, and leaves a tenacious, white, fibrous, ſubſtance, ſimilar to the polypus.

The blood, partly coagulated, and partly diſſolved, produces a very violent derangement in the organs of the animal. The part bit by the viper ſwells inſtantly, and becomes livid by ſucceſſive degrees. The blood in the large veins ſtops and coagulates. The ſerous part tranſudes into the adipoſe membrane, which it entirely fills. The circulation is deranged in the viſcera, diminiſhes by degrees, and at length ceaſes. The lungs are the

viſcus

viſcus in which the circulation ceaſes ſooner than in the other parts. In a moment after the injection of venom into the jugular vein, the blood coagulates in the lungs, the veſſels of which are filled and diſtended with this humour, in a black condenſed ſtate. In a word, the circulation is totally impeded and ſtopped, and the animal dies. It is a known fact, that as ſoon as the circulation is ſtopped in an animal with warm blood, death enſues in a few minutes, whatever the principle may be that binds and unites together the circulation and the life, the motion of the fluids and the ſenſitive faculty.

It will not be foreign to the purpoſe to ſpeak here of the animal irritability, or of that property of the muſcular fibres, by which a muſcle contracts on the ſlighteſt touch. We muſt conceive this property of the muſcular fibres, as ſomething that differs from the nerve, or from ſenſation; notwithſtanding it is true, that the nerve is the organ of the voluntary motions of the animal, and that when it is touched, it excites irritability in the muſcle. The nerve, in whatever way it is ſtimulated, is always motionleſs, and the muſcle continues to contract after it is ſeparated from the animal; whence it follows, that the nerve is rather the occaſion than the cauſe of the contraction of the muſcles.

In my work entitled *De Legibus Irritabilitatis nunc primum Sancitis*, printed at Lucca in 1767, I demonſtrated that the nervous fluid cannot be the *efficient cauſe* of muſcular motion. The arguments I adduced in that work are drawn from the hypothesis, that the nervous fluid acts agreeably to the laws of fluids in general. If the nervous fluid was different from fluids in general, if it had laws altogether different to theirs, or if it was analogous to electricity, my reasons would be no longer applicable to the present case.

However this may be, it is certain that the motion of a muscle separated from the animal, does in no way depend on the animal, or on the sensitive principle that resides in it, and that the irritability in the fibres subsists from itself alone. The irritability of the fibres is therefore distinct from the sensibility of the animal, and two things which appear so different, and which seem to have been separated by nature, ought no longer to be confounded.

But if this sensitive principle, which constitutes the life of the animal, is different from the irritability of the fibres, why, in a part separated from the animal, may there not subsist an obscure sensation, an imperfect life, relative to the size and to the nature of the part separated from the animal, and to the nerves that are found in that part?

In this supposition, there is no agreement, no harmony, betwixt the life of the entire animal, and the obscure sensation of the part that has been separated: but I do not see why, in this case, the irritability may not likewise depend on the sensation of the part. The irritability would then depend on the partial sensibility, or would be the same as the latter, that is to say, it would depend on the sensibility

lity of the part cut, and not on the sensibility of the animal.

But the opinion that an obscure sensation of life subsists in the parts separated from animals, is founded on an immense number of observations and experiments, which I have promised to give in the second volume of my Philosophical Enquiries on Animal Physicks, *(Recherches Philosophiques sur la Physique Animale)* the first volume of which, in quarto, was printed in Italian at Florence, in 1775. In the mean time I can venture to assert, that I know a very great number of animals, even amongst those that are called perfect, that is to say, that have humours, heart, and viscera, in which the hypothesis of the continuance of animal sensation, in parts that have been divided, is verified.

But whatever opinion may be adopted on irritability, it is still certain that this property exists in the muscular fibres, that it is the principle of all the motions of the animal, and that without it, all would be still, the organs would become useless, and the functions would be suspended.

When I wrote the first part of the present work, I was of opinion that the venom of the viper attacked the irritability in an immediate way, and that the animal died from the loss of irritability in the fibres. But I did not then know, that the venom of the viper has no action on the nerves, and that when it is introduced into the blood, it kills an animal in a few instants. This hypothesis ought now to be partly modified. It is not that in effect the

the irritability is not diminiſhed, in the animal that has been bit, and that it is not even deſtroyed in a little time; but this is rather an effect than a cauſe, and is a conſequence of the change cauſed in the blood by the venom, rather than an effect of the venom on the muſcular fibres. It ſometimes occurs that we ſee an animal, at the moment of its being bit, loſe all its voluntary motions, and ſcarcely diſcover any of the lateſt ſymptoms of life.

The debility of an animal, after it has been bit, is in general very great; but this ſhows equally, that the ſenſibility is affected: and as the venom does not act on the nerves, but on the blood, this diminution of ſtrength and ſenſation, and likewiſe the diminution of the irritability itſelf, may depend on the blood.

I have had frogs bit in the leg by the viper, and have found upon pricking the crural nerves a little time after the bite, or upon drawing electrical ſparks from them, that they had loſt but little, if any, of their irritability. It is very true, that this irritability diminiſhes with time, and that, on the death of the animal, it is frequently loſt altogether; but in theſe caſes, the ſenſibility is likewiſe diminiſhed and loſt. It is beſides certain, that if the crural nerves of the leg that has not been bit are ſtimulated, they contract with greater force than thoſe of the other; and that they frequently contract ſtill, when thoſe of the venomed leg have no longer in any degree that property.

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The irritability of the fibres, in animals bit by the viper, diminiſhes in proportion as the diſeaſe is more conſiderable, and as it continues a longer time. An animal that dies in a few minutes, preſerves in its muſcles more irritability, than one that dies at the end of ſeveral hours, or of ſeveral days. The irritability ceaſes much later in the heart, ſtomach, and inteſtines, than in the other parts. It particularly ceaſes very late in the inteſtines, which continue to move, notwithſtanding the animal has been dead ſome time. The irritability of the diaphragm, or the motion of the thorax, ceaſes much later than that of the other muſcles that depend on the will.

I made all theſe obſervations on animals with warm blood, in which it appeared to me, that the electrical ſparks were drawn with greater difficulty from the parts bitten, than from the other parts of the animal. This experiment ſucceeds particularly in fowls, in which there is no difficulty in laying the muſcles of the leg bare, and in having them bit.

The diminution of irritability in the muſcular fibres, is occaſioned by the changes the venom cauſes in the blood. The latter in this ſtate, in which it is partly diſſolved and partly coagulated, is diſpoſed to a ſpeedy putrefaction, and being pent up in the veſſels, diſſolves the texture of them, paſſes through their coats, and ſheds itſelf in the adipoſe membrane, corrupting and decompoſing whatever it meets with. In animals, the parts that have been bit by the viper, paſs in a ſhort time to the

the ſtrongeſt putrefaction, and preſent gangrenes and ſphacelations. The ſkin is ſpeedily corroded and deſtroyed; the muſcles black and fœtid; and the adipoſe membrane diſſolved.

I have known a rabbit die in leſs than three hours, with the muſcles of the leg already gangrened throughout their whole ſubſtance; they were black and offenſive, and were divided by a knife without any reſiſtance. In a word, this putrefactive tendency of the muſcles, in animals that have been bit by the viper, cannot be denied, and is occaſioned by the change wrought in the blood by the venom.

It is very true, that when the animal dies in a few minutes, there is as yet no actual putrefaction in the ſolid parts, although the humours have a true tendency to this ſtate. The diſeaſe reſides ſolely in the humours, and the ſtoppage of theſe humours in their natural courſe, occaſions the death of the animal. Whatever tends to impede the motions in the animal machine, neceſſarily tends likewiſe to deſtroy in it the ſenſitive principle and life; and we cannot conceive life there, where every thing is in a perfect repoſe.

Senſation is an active principle, and neceſſarily expreſſes an action, and we cannot conceive action without motion. We ſay in effect, that an animal is dead, when it is no longer ſenſible; and we ſay, that it is no longer ſenſible, when there are no longer in its organs, the ſigns, the external motions, that indicate ſenſation. The moment theſe motions ceaſe, we ſay that an animal is dead. This manner of judging is founded on obſervation itſelf. We have ſeen that when an animal is reduced to this ſtate of repoſe, it does not return again to life; and think, we may, on the other hand, reaſonably conclude, that an animal, when it is dead, can no longer revive in any manner whatever. This ſecond opinion, if we pay attention to it, actually appears to be derived from the firſt, ſince, after all, we do not know the principle that conſtitutes life and ſenſation in animals; it is, however, contradicted by obſervations and experiments of more modern date.

The obſervation that an animal deprived of motion does not return to life, appears to be combatted, as I have ſaid, by modern obſervations of a quite contrary nature. We have heard of ſtrong aſphyxies, in which there was no longer any ſign of motion. We are likewiſe told of drowned perſons, who have preſented the ſame phenomenon, although death in them was nothing more than apparent. I therefore do not ſee, why a certain obſcure motion may not ſubſiſt in the organs of an animal, which may not come within the reach of the evidence of our ſenſes. A motion to be inſenſible is not the leſs real; and when a motion ſubſiſts in an animal, there may ſtill ſubſiſt in it a principle of ſenſation.

I cannot deny but that, when there no longer ſubſiſts any principle of ſenſation, the animal is in all phyſical rigour dead; ſince we cannot poſſibly have any conception of life, in an animal without ſenſation. In the ſame way it ſeems equally clear, that

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a total repoſe in the organs of an animal, ought to cauſe this ſenſation to ceaſe, and conſequently to occaſion the death of the animal. But is there any method by which we can aſſure ourſelves of the total immobility of the organs of an animal, in which the humours are ſtill in a fluid ſtate? I cannot conceive any one. A very ſmall motion is entirely imperceptible to us, and we ſee only the greater ones. Every thing in nature is in motion, and it is not poſſible that a body, or any of its parts whatever, can be found for a ſingle inſtant in a total and perfect repoſe. Perfect repoſe is beſides repugnant to the general laws of gravity, and to the nature of fluids, which are in a greater or leſs degree penetrated by heat. Hence ariſes the difficulty of pronouncing on the death of animals, ſince in ſhort there may ſtill ſubſiſt in them a motion which may be inſenſible to us, but which may yet be ſufficient to maintain in them an obſcure ſenſation, to prevent their being altogether dead, and to enable them to return to life.

The motion of the heart being ſuſpended, and the reſpiration and circulation ſtopped in an animal, it is ſoon reduced to that ſtate in which we ſay of it that it is dead; notwithſtanding that this may probably not always be the caſe, when we believe it to be ſo. I know of only two ſtates of an animal, that can make us certain of its being really dead. One of theſe is the total putrefaction of its organs; the other, the abſolute deſiccation of its humours.

The first renders all animal function impossible; the second destroys all principle of motion.

The total deficcation of the fluids and solids of an animal, not only forbids the use of the organs, but even conveys an absolute immobility into all the parts. An animal, in this state of a total deficcation of parts, and of an immobility of its organs, is in my opinion certainly dead, and ought to be so in the opinion of every body; else we should be exposed to a capricious and unreasonable pyrrhonism. A fish, for example, dried in the sun, or by artificial heat, during twenty years, so as to become hard as wood, might still pass for being alive. I must confess that I cannot conceive life without action, nor action without motion, nor organical motion when the organs are dry; and this state is therefore to me a state of death. The naturalist ought not, however, to confound with each other these two different states of death, that is to say, the putrefaction of the parts, and the deficcation of the organs. In the first the animal is dead for ever; in the second, it may yet again return to life. We do not know any power, nature herself does not disclose any, that can recompose an organ that is destroyed, and entirely decomposed by putrefaction, or by the concussions of external bodies. This is what has never yet either been accomplished or seen. We have therefore every possible reason, not only to believe an animal that is reduced to this state dead, but likewise to believe it dead for ever. But if the animal is simply dry, if there is no physical disease in its organs, if the component particles of the different parts ſtill preſerve their reſpective ſituations, the animal may in this caſe very well return to life, to which effect it is only neceſſary, that the organs are reſtored to the ſtate they were in when the animal was alive. And why ought not an animal to revive in theſe caſes, provided it has every thing that concurred to make it live before? Whoever had reaſoned in this way a century ago, would have advanced matters both probable and reaſonable, but would not have been liſtened to, even by philoſophers, and would have riſked the paſſing, at leaſt for an extravagant perſon, or for a viſionary.

But let us return to the animals that die by the bite of the viper.

The blood coagulates in the veſſels of an animal bit by the viper, and the animal itſelf is in a ſtate of death. The blood, changed by the venom, corrupts and deſtroys the organs of animals, and renders the leaſt ſuſpicion of life altogether improbable.

It is true that, in proportion as the circulation of the blood ſtops in the veſſels, and as the death of the animal approaches, we likewiſe ſee a perceptible diminution of the ſenſibility; but this does not yet demonſtrate to us, that the nerves are either changed, or have received an injury.

There may perhaps be ſuch an harmony or agreement betwixt the circulation of the blood, the air of the lungs, the principle of ſenſation, and the nerves, that the one being removed, the other may diminish, although one may not act on the other.

My experiments have demonstrated, that an animal may lose its sensibility from quite another cause than from that of an injury to the nerves; and I therefore think that any one would reason ill, who should say that the death of an animal depends on the nervous principle alone, because in proportion as the animal draws towards its death, its sensibility is found to diminish. The diminution of sensibility in the nerves may be a secondary effect of the cause that kills an animal; and indeed, if the repose, if whatever puts a stop to motion in the animal, produces death, it must likewise be productive of a loss of sensation, which cannot subsist without motion.

Such is the death of animals with warm blood bit by the viper; but in cold animals it is not exactly the same. Animals with cold blood, such for example as frogs, may live a certain time without the circulation of the blood, and without respiration. It is precisely on this account, that the venom of the viper operates on them with less activity than on warm animals, and that they survive much longer than these last, in proportion to the size of their bodies. The action of the venom of the viper is insensibly communicated to the whole animal; the muscles dispose to putrefaction, and the part bitten becomes in a little time livid and gangrenous. The death of the animal then follows, but happens much later than in animals with warm blood, be-

cause

caufe the principle of life is not fo intimately connected with the circulation of the humours.

Why the circulation is thus clofely connected with life in animals with warm blood, and why it is fo little fo in animals that have the blood cold, is a much nicer enquiry. I propofe to fpeak on this fubject in a work *On Factitious and Natural Airs*, (*Sur les Airs Factices et Naturels*) which I hope to publifh very foon.

End of the Firft Volume.

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