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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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OF THE

SECOND VOLUME.

PART THE FOURTH.

CHAP.

E R R A T A.

Page 17, line 20, for *he*, read *it*.
—— 98, —— 11, for *banks of the Amazons*, read *banks of the river of the Amazons*.
—— 201, —— 3, for *Sir Robert Pringle*, read *Sir John Pringle*.
—— 294, —— 24 and 25, for *lamina*, read *laminæ*.
—— 312, —— 26, for *which is wrapped in its substance*, read *in the substance of which it is infolded*.

PHILOSOPHICAL RESEARCHES

INTO THE

VENOM OF THE VIPER.

PART IV.

CHAP. I.

Examination of the Remedies employed againſt the bite of the Viper.

AFTER having examined the quality of the viper's venom, and being much better acquainted than hitherto with the nature of this poiſon, it appeared to be no longer difficult to find a remedy for it. This is the uſual way of reaſoning, and is the ſource of numberleſs remedies, which ſucceed each other, and are at length found by experience to be hurtful, or uſeleſs at leaſt. The fluid volatile alkali owes its greateſt repute to the re-

ceived

ceived opinion that the nature of the viper's venom was discovered. Jussieu, from the authority of Mead, deemed it an acid, and this was sufficient to give the volatile alkali the reputation of being a true antidote against it.

The authors who succeeded Jussieu copied from each other. They adopted the remedy, and its mode of action, and they found it a good one, because the bite of the viper is not always fatal in its consequences. For my part, I am of opinion, that even when the nature of a poison is well known, and the effects it produces on animals, we may very easily be at a loss for its remedy. Nothing is less difficult to conceive than this, if we reflect how very ignorant we still are as to the animal machine, and how much we are in the dark and in uncertainty, as to the qualities, or the virtues of bodies.

Be that as it may, it is clear that the authority of certain writers has brought into fashion a greater number of remedies, than has been introduced by the fallacious experiments of others, or the scarcity and price of the remedies themselves. The bezoar, for instance, the unicorn, and rhinoceros, are of the latter discription. Were we to examine by the rigid touchstone of experiment, the long list of remedies, to how few would they not be reduced? It is on this account that the best collection of receipes is invariably the shortest.

We have already seen above, in what estimation the fluid volatile alkali, regarded as a specifick, ſhould be held. All my experiments ſhow it to be totally uſeleſs, even when taken inwardly. It does not appear that it can have the ſmalleſt effect when applied to the part bitten. It is almoſt needleſs to obſerve, that the volatile alkali, whether given internally, or applied to the part, can in no way ſerve to correct the acidity of the venom, ſince the venom is certainly not acid; whence it is that theſe boaſted qualities of an alkaleſcent principle, this neutralization of ſalts, are mere fancies and errours produced by ill experiments. I believe beſides, that even though the venom of the viper ſhould be acid, and deſtructive to animals as an acid principle, we ought to expect little or nothing from the volatile alkali applied externally. To the end that the volatile alkali ſhould ſaturate the acid of the venom, it would be neceſſary that it ſhould find its way into the part bitten, and ſhould there mix and unite itſelf with it. It appears to me, that in general, the volatile alkali does not reach the muſcles through the ſkin, at the place where the poiſon has penetrated. 'Tis at leaſt what I obſerved in thoſe animals that have a compact ſkin, like that of the human body.

Experiments on the effects of the Volatile Alkali upon the bite of the Viper.

I cut a piece of the ſkin under the belly of a guineapig, the inciſion being in the ſhape of

 a parela parellelogram, three ſides of which were ſeparated from the animal, the fourth ſtill hanging to it. I pierced the cut ſkin with the dried teeth of a viper, which penetrated it through and through. Under the hairy ſide of this ſkin, thus prepared, I held a bottle filled with volatile alkali, and having a mouth of four lines diameter, but could never perceive that any ſmell was communicated to the inſide of the ſkin, how long ſoever I kept it over the bottle, and however great the ſtrength of the volatile alkali, which was active to a degree.

I repeated this experiment on rabbits, that have a ſkin ſtill thinner. The event was the ſame: no ſmell could be perceived through the ſkin.

I moiſtened the inner part of the ſkin of a guinea-pig, which I had previouſly pierced with the dried teeth of a viper, as before, with a little diluted nitrous acid. Whatever quantity of the volatile alkali I threw upon the outer part of the ſkin, the nitrous acid never appeared to be ſaturated, or in the ſmalleſt degree weakened. At another time I moiſtened the ſkin of another guineapig, prepared as above, with a diſſolution of copper in well diluted nitrous acid, and kept the outer part of the ſkin wet with the fluid volatile alkali: the diſſolution of copper did not change its colour to blue.

It is then certain, that in general the volatile alkali does not penetrate through the compact ſkin of a quadruped; the reaſon of which is, that the tooth of a viper being very ſmall, does no more

than

than difplace the parts a little, and in proportion as it is withdrawn, the fkin, by its elafticity, recovers its former fituation, and clofes the hole. It on this account frequently happens, that the bite of a viper does not caufe the animal to bleed. If a confiderable veffel has been punctured by the tooth, the blood iffues out, coagulates, and prevents the entry of other fubftances.

The human fkin is thicker than that of rabbits and guineapigs; it is of a very clofe contexture, and very elaftick. If the fluid volatile alkali were a true fpecifick, whenever it could unite itfelf with the viper's venom, it would be inefficacious againft the bite of a viper in man, or could at beft ferve only for bites in the fkin altogether fuperficial, which are never to be dreaded in any quadruped, however fmall, and much lefs in man.

But if the volatile alkali is ufelefs when applied externally to the part, from the difficulty it finds to mix with the poifon; why may it not be ufeful, if we contrive to introduce it into the parts bitten? greater or fmaller incifions may be made in animals, and the volatile alkali conveyed by this means to the wounded mufcles. Will it in this cafe be a fpecifick? Will it at leaft be ufeful?

To fully elucidate this I made the following experiments.

I had feveral animals, fuch as hens, rabbits, guineapigs, &c. bit in the leg, and fome minutes after made deep and extenfive incifions into the wounded parts. I wafhed thefe incifions with pure volatile alkali, and covered the legs with linen bandages. I got ready an equal number of animals of the ſame ſize, and of the ſame kinds, to ſerve as a compariſon. Theſe were likewiſe bit in the leg, but I neither made inciſions, nor applied to them the volatile alkali. The reſult of twenty-four experiments was not favourable to this medicine applied to the inciſions, and the violence of the diſeaſe was even more conſiderable in the former than in the latter.

I cannot here paſs over an experiment which was ſuggeſted to me by the *Duc de Chaulnes*, and which I made a little time after, with his aſſiſtance, on a pigeon, in the preſence of a celebrated phyſician and chymiſt, Monſieur Darcet. I mixed together equal proportions of venom and volatile alkali, and introduced a part of this mixture into the pectoral muſcles. The pigeon died at the end of eleven minutes. As I had ſome ſuſpicion that in the operation, I had penetrated into the cavity of the thorax, I thought proper to repeat the experiment on other animals. Beſides this, I varied the quantities of the venom, and of the volatile alkali, and likewiſe made uſe of fluid volatile alkalies, prepared without lime. Of ſix pigeons who were poiſoned in the breaſt, and ſix others in the leg, not one recovered, and all died in a very ſhort ſpace.

I put into a ſmall glaſs, three drops of viper's venom, and twelve drops of fluid volatile alkali. After having mixed theſe two fluids together, I put

put half a drop of them on the divided fibres of one of the mufcles of a pigeon. It died at the end of thirty hours, with the fymptoms of poifon, but in a moderate degree.

I repeated this experiment on another pigeon, the mufcles of which I had laid bare, and wounded in feveral places. Into thefe mufcles I introduced a fmall piece of wood well covered with the venom taken from the glafs. The pigeon furvived, although the fymptoms of poifon difcovered themfelves.

I repeated this fecond experiment on another pigeon, and introduced as above, the piece of wood covered with the venom. The animal lived, and had fcarce any aliment.

I made a frefh experiment with the piece of wood dipped in the venom, and in lefs than an hour the pigeon died.

The little uniformity in the refult of thefe experiments made me fufpect, that the poifon had not well communicated itfelf to the mufcles, and that the ufe of the piece of wood was not the beft way to communicate the difeafe. Some fucceeding experiments confirmed me in this fufpicion.

I formed then the idea of introducing into the mufcles of pigeons, the venom from the fmall glafs, by means of a thread repeatedly doubled. I paffed the threads well rubbed over with this poifonous mixture, through the mufcles, and left them there. Six pigeons who underwent this experiment, all died in the ſpace of thirty-ſeven minutes.

It is, however, poſſible, that the fluid volatile alkali contracts the veſſels in ſuch a way, that the venom cannot be eaſily abſorbed; but be that as it will, we ſee clearly that when it is well applied, it is mortal as before, and that the volatile alkali does not diminiſh its activity.

Theſe experiments not only demonſtrate the abſolute inutility of the volatile alkali againſt the bite of the viper, when applied externally; but they at the ſame time prove ſtill further, that it cannot have a direct and ſpecifick operation, when it is even taken internally. If the venom of the viper preſerves all its malignant qualities when in immediate union with the volatile alkali, how can it ever be deprived of them, by meeting with it, when it is itſelf united in the animal, with an immenſe quantity of fluids, and diſperſed in ſo many parts?

Experiments on the inefficacy of different Subſtances againſt the bite of the Viper.

The ſame experiments may ſerve equally to exclude from the number of ſpecificks, the many boaſted remedies againſt the venom of the viper. I united a great number of ſubſtances with this poiſon, but did not obſerve after all, that it loſt its hurtful qualities. I mixed it with acids, with alkalies,

kalies, with neutral ſalts, with oils; but it continued to deſtroy the animals, whenever it had found its way into their wounds.

I likewiſe made direct experiments on theſe ſubſtances, to be more certain of their inutility. I ſhall not here enter into a detail of theſe experiments, becauſe it would be too long, and becauſe I think it of little conſequence; it will be ſufficient for me to ſay in a general way, that I applied them to the parts bitten by the viper; even making inciſions, to give the venom a freer communication. In theſe experiments I tried oil of vitriol, the nitrous acid, the phoſphorick acid, and the mineral acid, (*l'acide ſpathique*,) and I found them all to be at leaſt uſeleſs. Alkaline ſalts, cauſtick and not cauſtick, whether mineral, vegetable, or animal, had the ſame effect. I was ſtill more attentive to the neutral ſalts, and above all to ſea ſalt, which ſeveral eſteem a good remedy, but which I alſo found uſeleſs. As to oils in general, and eſpecially that of turpentine, they appeared to me to be of ſome real uſe. The beſt mode of application is, to foment for a long time the part of the animal that has been bitten, in the oil made extremely warm. Some guineapigs that according to the probabilities that reſulted from my experiments on theſe animals, would have died, were perfectly cured by it. It is very true that they had been bit by a ſingle Viper, and only once, and that two of the feet bitten, had loſt the

ſkin,

ſkin, and were in part crippled, which was probably occaſioned by the too great heat of the oil.

I made repeated experiments by dipping the part that had been bit in various fluids. The plunging it into very warm water, and keeping it there, appeared to me to be truly advantageous. The pain was evidently leſſened, the inflammation abated, and the colour leſs livid and changed. I found the ſame effects from lime water, and from water mixed with common ſalt, and other ſaline ſubſtances. Although the immerſion is neither a ſpecifick, nor a certain remedy, it is always attended with a greater or leſſer degree of advantage, which I think is in theſe caſes due to the mere fomentation with warm water.

I obſerved in the courſe of my experiments, that dogs and cats recovered with a facility proportioned to the violence of their vomitings; and wiſhing to follow this indication of nature, I made a great number of experiments on dogs. Here I was very often led to believe the emetick a good remedy, as I ſometimes met with ſeven or eight caſes which terminated in the ſame way, and were altogether favourable. The emetick I made uſe of was the ſtibiated tartar, which I gave in water, in different doſes, and at different periods. The reſult of ſome of theſe experiments contradicted that of others, but ſeveral of them were very favourable and uniform. Amongſt a great number of other trials, I had a dozen dogs bit in the leg, each by three vipers, and by each repeatedly.

edly. To fix of them I gave emetick tartar, to the others nothing. All of the firſt recovered; four of the others died in leſs than three days. I cannot therefore take upon me to determine the tartar emetick entirely uſeleſs, but it is certainly no ſpecifick, no aſſured remedy.

I wiſhed to try cantharides, not becauſe I was ſtrongly induced to believe them good againſt the bite of the viper, but only becauſe I wiſhed to ſee how an active ſubſtance, and that too in ſome degree poiſonous, would act on an animal attacked with the ſymptoms of poiſon.

I applied the cantharides to the bitten part, and likewiſe cauſed them to be taken internally. I ſoon perceived that, applied to the part, they were evidently injurious, and that there was a ſpeedy diſpoſition to gangrene and ſphacelus. To introduce them ſtill better, I made inciſions.

Thoſe taken internally furniſhed me, like the emetick, with equivocal concluſions, in proportion to the uncertainty of which, I multiplied my experiments, and was at length aſſured that the cantharides, are neither a ſpecifick, nor an efficacious remedy, although, I can neither declare them hurtful nor uſeleſs.

I conceived greater hopes from the bark, which is avowedly a powerful antiſeptick, and very uſeful in caſes of gangrene. For the ſhort time the complaint laſts, the viper's poiſon produces a true local gangrene, and the bark was therefore promiſing. I began my experiments with the ſimple powder,

der, which I threw on the part bitten, into which I had made ſeveral inciſions. Not perceiving that it had a certain good effect, I had recourſe to the bark in infuſion. I bathed the wounded part of the animal lengthways, ſometimes keeping it for a long ſpace entirely plunged in the warm infuſion. At other times, I plunged it repeatedly in, but it was all in vain: I could never aſſure myſelf that it had a real and conſtant advantage, although I cannot condemn it as entirely uſeleſs.

It was neceſſary to make an incredible number of experiments, before I could determine the little certainty of emeticks, cantharides, and bark, oppoſed to the bite of the viper: theſe experiments are beſides extremely inconvenient, as one operates principally upon dogs, and they are for the moſt part of very long duration. A dog when he ſurvives, frequently remains ſick ten, fifteen, or even twenty days.

I wiſhed to try beſides, if ſcarifications more or leſs deep, and the actual cautery, were certain remedies. The concluſions I obtained, which were very numerous, were not favourable to theſe two methods, which are however propoſed by authors with great confidence. It appeared to me on the contrary that the ſcarifications, very far from being uſeful, rather did harm; the part bitten, and afterwards ſcarified, being the more diſpoſed to gangrene. In a word, neither the ſcarifications nor the cautery had any uſeful effect.

It

It remained for me to try two other remedies which many celebrated phyficians prefer to many others.---The theriaca and the fat of the viper itfelf.

I employed the theriaca by laying it on the part, which had been bit, and afterwards fcarified. I renewed it feveral times, and kept the part well covered with pieces of linen fpread with the fame. I likewife gave it internally, but all without effect: it did not appear to me to be of any ufe in diminifhing the animal's complaint.

Mead in his treatife on poifons, fpeaks of a remedy which was reckoned in his time, a real fpecifick againft the bite of the viper. He fays that the viper catchers in England ufed it with fo much confidence, that they were no more afraid of the bite of a viper than of a common prick.

Mead contrived to ferret out this remedy, which was yet a fecret. He difcovered that it was the fat of the viper itfelf, which they rubbed over the bitten part. To be ftill furer of the efficacy of this remedy, he had the nofe of a dog bit by a viper, and applied the fat to it. The animal recovered, and on repeating the experiment, the refult was the fame. Being thus affured of the efficacy of the remedy, he fat about explaining philofophically how it ought to correct the action of the poifon. He found that the glutinous corpufcles of the viper's fat were calculated to enclofe the volatile falts of the poifon, and fo to prevent their uniting into chryftalline falts,

to

to which the poiſon owes its force of activity.

Mead's principal miſtake conſiſts in having ſuppoſed that the bite of a viper on a dog's noſe was abſolutely mortal. On another hand what dependance ought we to place on two experiments only? It has been ſeen above, how different the conſequences are when the circumſtances even appear the ſame, and how little truſt ſhould be placed even in uniform conſequences, provided the number of experiments be not very great.

Bites of the viper on the noſe, are leſs dangerous than in all other parts of the body. If Mead had multiplied his experiments more, if he had varied them as he ſhould have done, he would not have been deceived, or would have been ſooner convinced of his errour. From hence principally ariſes the ſlowneſs in progreſs of the ſcience of natural philoſophy, and this is the ſource of an infinite number of miſtakes which continue to disfigure medicine, and to prevent its advancement.

I have likewiſe tried electricity againſt the bite of the viper, and have not only found it uſeleſs, but it has even appeared to be hurtful. It is at leaſt certain that in the animals to which I applied it, the complaint became more violent, and that they died ſooner. In many, I threw ſparks from the conductor, on the part bitten; and in others drew ſparks from the part, keeping the animal faſtened to the conductor: in both ways I found electricity more hurtful than beneficial.

Applica-

Application of Leeches to the Bite of the Viper.

I had a pigeon bit in the leg by a viper, and inſtantly applied to the part three leeches which faſtened very well. In twenty minutes it died, the leeches being ſwoln with the blood they had ſucked from it.

I repeated the ſame experiment on two other pigeons, which were but juſt bit when the leeches were applied. They both died in eighteen minutes.

The ſucking the parts bit by the Viper.

I was deſirous to ſee if by ſucking the part immediately after its being bit, the poiſon could be prevented from diffuſing itſelf. I met with a perſon who made no difficulty of ſucking it.

I had the bites of two pigeons ſucked, without dilating them, and thoſe of two others, after having dilated the holes made by the teeth, and begun to ſcarify. All four died in leſs than twenty-ſeven minutes.

The ſame experiments terminated the ſame way with quadrupeds; after which I am not afraid to pronounce that neither ſuction by the mouth, nor the application of leeches, is a certain remedy againſt the bite of the viper.

I ſhall not dwell on ſeveral other methods I have tried againſt the venom of this animal, and which

I have

I have found more or leſs uſeleſs, and ſometimes hurtful. I have applied ſeveral earths, chymical preparations, and vegetable ſubſtances, to the bitten part, and have ſometimes even given them inwardly to the animals. I think it uſeleſs to inſert a catalogue of ſuperfluous remedies.

On the utility of amputating the Limbs bit by the Viper.

We have already ſeen that the action of the viper's venom is not inſtantaneous; that it requires a certain time for its effects to be perceived in the bitten parts; and that the external malady does not communicate itſelf ſuddenly to the animal. We have alſo ſeen, that if the part bit by the viper be ſuddenly amputated, the animal ſurvives. All theſe experiments together ſupply a certain remedy againſt the bite of the viper, when one can practice it with facility. It is natural to ſuppoſe, that by lopping off the diſeaſed parts, the life of the animal may be ſaved; but the amputation ought not to be much retarded, becauſe it is at leaſt certain, that the ſooner it is performed the ſurer its effects. In pigeons it begins to be even fatal at the end of fifteen ſeconds, at which time the internal malady is communicated, which the amputation encreaſes, and haſtens death, inſtead of diminiſhing one and retarding the other, as I have been ſatisfied by ſeveral experiments.

Before

Before I examined into the advantages of amputations on animals bit by the viper, I wiſhed to ſee whether the internal malady would be communicated in a ſenſible way, and ſo as even to occaſion death in other animals, in the ſame ſpace of time as in pigeons. It was neceſſary to try it on animals that die with much greater difficulty than theſe laſt, but who would die to a certainty, and in a ſpace not too diſtant from the introduction of the poiſon. I made choice of very ſmall guineapigs, becauſe I knew by experience, that they had all theſe qualities.

I had a guineapig bit ſeveral times at the extremity of the foot, which at the end of twenty ſeconds I cut off betwixt the tarſus and the tibia. The animal lived, and ſeemed to have no other complaint than that cauſed by the operation.

I had another guineapig bit repeatedly at the extremity of the foot, by a viper, and at the end of forty ſeconds cut off the leg as above. He recovered in the ſame way with the former.

A third guineapig received ſeveral bites in the foot, from a viper, a minute after which I cut off its leg. It recovered as well as the others.

I had another guineapig bit repeatedly by a viper, in the foot, which in eighty ſeconds I cut off; this one likewiſe recovered.

I had another guineapig bit repeatedly in the foot by a viper, and two minutes after cut off its leg; it recovered likewiſe.

I had another guineapig bit ſeveral times by a viper, in the foot, which at the end of three minutes I cut off: this one recovered too.

I had another guineapig bit ſeveral times by a viper, at the extremity off the foot, and at the end of four minutes cut off its leg: it died three hours after, having a lividneſs of the muſcles of the leg, and the auricles and heart filled with clotted blood.

I had another guineapig bit repeatedly in the foot by a viper, and at the end of four minutes cut off its leg: it recovered.

It muſt be obſerved, that the feet amputated at the end of three or four minutes, have unequivocal ſigns of local malady; theſe ſigns are even obſerved before, although with more difficulty, are not ſo certain, and do not exiſt always.

Neither of the guineapigs bitten in the feet, and mutilated in leſs than three minutes, died; but of the two mutilated at the end of four minutes one died, and the other ſurvived. There are even here then, as well as in many other caſes we have ſeen above, circumſtances in which the bite of the viper produces greater or leſſer effects; but what is more important, and deſerves all our attention, is, that the internal malady is not communicated to the animal till very late, in compariſon to that in the caſes of the pigeons, or more properly, that it does not become mortal till after a long time, and that the cutting off the part bitten may be made with all poſſible advantage and ſafety, within the limits of a much greater time.

But let us continue our experiments, the number of which has been too ſmall to ſupply us with certain concluſions.

I had a guineapig bit ſeveral times in the foot by a viper, and cut off its leg at the end of four minutes: it recovered.

I had another one bit repeatedly in the foot by a viper, and cut off its leg at the end of five minutes: it recovered.

I had another guineapig bit in this way in the foot, and at the end of ſix minutes cut off its leg; it died ten minutes after.

I had three guineapigs bit in the foot by a viper, each one ſeveral times, and in four minutes cut from each a leg: they all recovered.

I had three others bit in the foot in the ſame manner, and in five minutes cut off a leg from each of them: they all three recovered.

I had three others bit in the ſame way, and in ſix minutes performed the operation: one only recovered.

I had three others bit as above, and in ten minutes cut off a leg from each: they all died.

From all theſe experiments it appears that this deduction may be made, that every thing is to be expected from the amputation of the leg, if performed on guineapigs before ſix minutes are elapſed after their being bit by the viper.

It is natural to ſuppoſe, that in larger animals the amputation may be made much later ſtill than ſix minutes, and experience has demonſtrated it in very large rabbits; but we may ſtumble on another inconvenience which confines this method very much: pigeons are not endangered by the amputation of the leg; ſmall guineapigs bear that of the extremity of the foot, but not always that of the leg; larger animals are more apt to die when a great part of them, ſuch as the leg, is lopped off; ſuch an operation in theſe caſes is not only uſeleſs but dangerous.

It does not, however, follow that amputation, even in large animals, may not be uſeful againſt the bite of the viper; in general it is ſo when the animal bears it eaſily, provided it be done at a convenient time. As amputation may be very uſeful in a great number of caſes, I thought it incumbent on me to make experiments, and to vary them ſeveral ways, on different animals.

Rabbits and Dogs that had their ears bit and cut off.

I had a rabbit's ear bit a ſingle time by a viper, and in thirty ſeconds cut it off ſix lines below the part bitten. The animal bled a great deal, but did not die, nor even appear more diſordered than another rabbit, the ear of which I cut off, without having it bit.

I had

I had a fecond rabbit bit feveral times by a viper, and a minute after cut off the two ears, fix lines below the bite. It recovered without any fymptom of poifon.

I had a third rabbit bit in both ears, by two vipers, each of them repeating its bites, and in two minutes cut off each ear, eight lines below the parts bitten; it recovered as well as the other two.

I had two other rabbits bit feveral times in both ears, each by two vipers. At the end of fix minutes I cut off the ears, eight lines below the bites. Both recovered, without any intermediate fymptom of poifon.

I had a fmall dog bit in the ear, which in a minute I cut off fix lines below the part bitten. The dog recovered without any other fymptoms than thofe common to fuch an operation.

In the fame way I had another fmall dog bit repeatedly in the ear by two vipers, and in fix minutes cut off the part. It recovered without having had any other fymptoms than thofe refulting from the operation.

Again, I caufed a young and fmall dog to be bit in both ears by two vipers, and by each repeatedly. In twenty minutes I cut off the parts. It recovered without having any fymptoms of poifon.

I repeated this laft experiment on two other dogs, and it fucceeded equally, fince both of them lived. 'Tis very true they were much difordered, but not

 more

more ſo than dogs are uſed to be when their ears are cut off, without having been bit.

As it is not common for either rabbits or dogs to die when bit in the ears, above all, if they are ſomewhat large, the experiments on theſe animals only ſerve to prove that the effects, at leaſt local, do not ſubſiſt after the bitten parts are cut off.

Animals that have received bites on the Skin, into which inciſions have been afterwards made.

I had a very ſmall guineapig bit repeatedly by a viper on the ſkin of the back, and to prevent the viper's wounding the muſcles, I kept the ſkin raiſed with pincers, the teeth piercing it through and through. I held it thus for four minutes, and then diſſected it away in ſuch a manner that none of it remained for ſeveral lines round the part bitten. The animal recovered in twenty-four hours, the inciſion in its ſkin being covered with an eſchar. It eat as uſual, and appeared to ſuffer nothing except what was ſimply cauſed by the inciſion, as I aſſured myſelf by preparing another guineapig, to ſerve as a compariſon, and which recovered in the ſame time, but had not been bit by a viper.

I had another guineapig bit ſeveral times on the ſkin by a viper; I kept the ſkin raiſed for four minutes after its being bit, and at length cut it. It had already the marks of poiſon upon it, that is

to

to ſay, livid and black ſpots, and theſe ſpots extended for ſome ſpace round the part bitten.

I had another guineapig bit repeatedly by a viper on the ſkin, which in four minutes I diſſected away. The creature recovered without any ſymptoms of poiſon.

I had three rabbits bit as above, but made no inciſions; they all died, one in ſixteen hours, another in twenty-ſix, and the third in thirty-two. In this laſt the fleſh bitten was gangrened internally, and the cellular membrane, the pectoral muſcles, and thoſe of the abdomen, were filled with black and extravaſated blood. The two other rabbits had likewiſe evident marks of diſeaſe and gangrene, but in a leſſer degree.

I had two ſmall guineapigs bit as uſual on the ſkin, and in twenty minutes made the inciſion. Both recovered very well.

As the bite of the viper is uſually mortal in theſe animals, even when it does not penetrate beneath the ſkin, the inciſion into the part bitten becomes a ſure remedy for them againſt its venom. I repeated the ſame experiments on dogs and rabbits, and the reſult was the ſame. The cure is certain, and the local complaint avoided, as likewiſe the internal one, in a great meaſure at leaſt, although the amputation of the parts bitten be made much later.

Combs and Gills of Fowls bit, and afterwards cut off.

It has been ſeen above that the bite of a viper in a fowl's comb does no injury to the comb, but a great deal to the gills. This fact is not the leſs true for being ſingular, and is the reſult of many uniform and conſtant experiments.

As the venemous effects do not diſcover themſelves in the comb, but in the gills, which are attacked with a complaint generally fatal to the hen, it was natural to ſuſpect that the gills being cut off, the animal would recover perfectly.

I had a fowl bit, then, and that repeatedly, in the comb, by a viper, and in twenty ſeconds cut off the gills.

It recovered without any ſymptom of illneſs, continuing to eat and drink as uſual.

I had the comb of another fowl bit repeatedly by a freſh viper, and in forty ſeconds, cut off the gills. It did not appear to have any complaint.

I had a fowl's comb bit ſeveral times as above, by two vipers, and in ſixty ſeconds cut off the gills. It recovered without any ailment.

I had the combs of three fowl's bit repeatedly by two vipers, and cut them off, one in four minutes, another in eight, and the third in ten. They all recovered, the gills of the third were diſeaſed at the end of the ninth minute.

It has been ſeen that when the gills, and not the combs, are bit by the viper, the complaint does not

not find its way to the combs, but remains in the gills, is more dangerous than if the viper had bit the comb; and is ufually fatal.

I had a fowl's gills bit feveral times by a viper, and cut them off in twenty feconds. It recovered without any apparent illnefs.

I had the gills of another fowl bit repeatedly by a viper, and cut them off in fixty feconds. It recovered, without any external fymptoms of complaint.

I had three other fowls bit repeatedly in the gills by a viper, and in three minutes cut off the parts. The fowls did well, without betraying any ailment.

I had the gills of three other fowls bit in the fame way, each by two vipers, and that repeatedly, and cut them off in four, fix, and eight minutes. The fowls all recovered, without any fymptom of poifon, as if the gills had not been bit by the viper, but only cut off.

All the experiments hitherto made, tend in themfelves to give the moft flattering hopes that a remedy more eafy, more univerfal, and lefs painful than amputation, may be found againft the bite of the viper.

It has been feen that the nerve does not ferve to communicate the difeafe of the venom to the animal; that this difeafe communicates itfelf by the blood; and that wounds of the fkin, venomous, but fuperficial, are attended with no dangerous confequences. The two firft truths point out with cer-

tainty

tainty that to prevent the complaint from communicating itſelf to the animal, it is ſufficient to ſtop the circulation of the blood; the third demonſtrates that it is not neceſſary to ſtop it totally, and in the very ſmall veſſels. Nothing, as I ſee, can be more comformable to the theory of the poiſon, and its modes of acting on the human body.

This great and uſeful truth ought to be founded on a number of incontrovertible experiments. I thought that the pigeon, of all animals, would furniſh me with the leaſt equivocal proofs, and therefore preferred it to all others. I knew that the bite of the viper is certainly mortal to it; that it dies in a few minutes; and that an imperceptible quantity of poiſon is capable of killing it in a ſhort time. A ſingle bite of the viper is capable of introducing as much poiſon into a pigeon, as would kill 200 of them.

I had a pigeon bit once in the leg by a viper, the part having been previouſly tied with a ſilk ribbon, juſt above the joint. The ſymptoms of local malady appeared very ſoon in the leg: in four hours it was quite livid and ſwelled beneath the ligature; but all above remained in its natural ſtate. I took off the ligature, and a ſhort time after obſerved that the leg was leſs ſwelled and livid. In ten hours its colour was almoſt natural, and it was but little ſwelled. In twenty-two hours nothing remained but a few diſcoloured ſpots at the place where the teeth had entered. In ſixty hours there was a bluiſh caſt over it. In three days it was perfectly ſound.

I tied

I tied a ribbon about a pigeon's leg, and had it bit ſeveral times by a viper. In ten hours the leg was ſwelled and quite lived, diſcharging in ſeveral places a blaçkiſh humour. I removed the ligature. In twenty-two hours the leg was ſwelled as before, and black as a coal. In forty hours, all the muſcles ſeemed to border upon ſphacelus. In three days the leg was leſs ſwelled, and diſcharged leſs matter. In five it ſeemed in a healing ſtate. In ſeven it had in a great meaſure regained its natural colour. The animal recovered in ten days.

I repeated this experiment on four other pigeons, but fearing that the ligature in the former one had been too tight, and had in a degree increaſed the local malady, I made the ligature much looſer. Neither of the pigeons died, but their legs were ſomewhat ſwelled and livid. In ten hours I took off the ligatures. Two of the pigeons recovered the fifth day, the other two on the ſixth.

It is an experienced truth, then, that the ligature made in the part bit by a viper, prevents the complaint from communicating itſelf to the animal, no internal diſeaſe ſupervening, during the time it remains on. It is likewiſe an experienced truth, and equally important, that at the end of a certain time, the venom does not communicate itſelf to the ſyſtem.

Although it be true, as it indeed ſeems very probable, that on taking off the ligature, the venom is partly abſorbed by the veſſels, and carried with the blood into the circulation, it is however obſerved that

that it is no longer in a ſtate of poiſon, and capable of killing the animal. It is known that the ſmalleſt poſſible quantity of venom kills a pigeon in a few minutes, and experience ſhows, that none of them die when the ligature is made, although taken off at the end of a certain time.

Beſides, it is not difficult to conceive that when once the venom has produced its ordinary effect on the blood, and on the parts bit by the viper, it ceaſes to be hurtful. Moſt bodies operate in this way, and the viper's venom may very poſſibly decompoſe itſelf in producing the local malady, and unite with the blood: but it requires a certain time to be brought into this ſtate, and to be rendered inactive and innocent. In the caſes cited above, the ligature was left on for ten hours. 'Tis very true, that all this ſeems contradicted by the experiments I have myſelf made on the venom, which, when mixed with the blood, does not on that account ceaſe to be a poiſon. We have ſeen beſides, that the venomous ſymptoms excited in the muſcles of a pigeon's leg, communicate themſelves very readily to the bared muſcles of the leg of another pigeon, if they are placed for ſome time in contact with each other. But in all theſe caſes, the trial has been made a few minutes after the pigeons were bit by the vipers, or after the venom had united itſelf with the blood. To determine after what time the ligature may be removed without danger, I made the following experiments.

I had

I had a pigeon bit in the leg by a viper, and in twenty feconds applied a ligature. In four hours the leg was fwelled and livid, and difcharged on all fides a blackifh humour. I now removed the ligature, the parts above which were in a natural ftate. In ten hours the leg was lefs fwelled and almoft of its ufual colour; there was, however, a degree of fwelling above the ligature. In twenty-two hours the leg was fcarcely fwelled, although ftill a little livid; but the part above the ligature was both livid and fwelled. In fixty hours there was fcarce any mark of ailment, and on the fourth day the pigeon feemed perfectly recovered.

I had a pigeon bit in the leg by a viper, and in fixty feconds made a ligature. It died in three quarters of an hour, its leg having had a livid appearance before it was bound.

I bound the leg of a pigeon at leaft as tight as in any of the cafes related above; in a fhort time it fwelled, but not confiderably; in feven hours it was more fwelled, but with fcarce any difcoloration. On taking off the ligature at the end of ten hours, the tumour in the leg fpeedily fubfided, but in return it fwelled a little above the ligature. In twenty-two hours it was fcarcely fwelled or difcoloured. In thirty hours the pigeon was perfectly recovered.

I had a pigeon bit repeatedly by a viper in the leg, it being previoufly tied; in thirty minutes I took of the ligature; the leg was fwelled and livid. In fix hours its livid appearance was leffened, but a fwelling a ſwelling appeared above the ligature. In twenty-four hours the leg was ſtill ſwelled, and had a bluiſh caſt: the ſwelling above the ligature had reached the abdomen and breaſt. In forty hours the pigeon died, the parts above the ligature being much diſcoloured.

I had a pigeon's leg bit repeatedly by a viper, and tied immediately after. I then had it bit ſeveral times by a ſecond viper, and in an hour took off the ligature. In twenty-four hours the leg was ſwelled, but inconſiderably; in forty the pigeon was perfectly well.

I had a pigeon's leg bit twice by a viper, and inſtantly tied with a ribbon in the uſual way. In four hours I took off the ligature, and found the leg exceedingly ſwelled and livid. In twenty-four hours theſe ſymptoms bordered upon ſphacelus, and in thirty-ſix the pigeon died, with ſymptoms of diſeaſe above the ligature.

I had a pigeon's leg bit repeatedly by a viper, and immediately tied it. In twenty minutes I took off the ligature, and found the leg in a livid ſtate, but ſcarcely ſwelled. In eight hours it was violently ſwelled, and livid. In twenty-four hours all was nearly in a gangrened ſtate, and in thirty-nine the pigeon died.

I had a pigeon's leg bit three times by a viper, and immediately made a ligature, which in thirty minutes I took off; the leg was ſwelled and diſcoloured, but in eight hours the ſwelling was diminiſhed.

nifhed. In twenty-four it had almoft regained its natural ftate, and in fifty was perfectly found.

I had a pigeon's leg bit repeatedly by a viper, and immediately paffed a ligature round it, which in forty two minutes, I removed, and found the leg livid and fwelled. In eight hours thefe fymptoms were much diminifhed, but a fwelling and difcoloration appeared above the ligature. In twenty-four hours every fymptom of complaint was leffened, and in thirty-fix fcarcely perceptible. In fixty hours the pigeon was fully recovered.

I had a pigeon bit once in the leg by a viper, and inftantly made a ligature, which in two hours I took off. The leg was much fwelled and difcoloured, but in twenty four hours had almoft refumed its natural colour, except at the places where the teeth entered, and which were covered with fmall dark black fpots. In fixty hours all difappeared, and the pigeon recovered in three days.

I had a pigeon bit repeatedly in the leg by a viper, and immediately paffed a ligature round it, which in an hour and an half I took off. The leg was at firft livid and fwelled, but in eight hours thefe fymptoms were abated, and very confiderably fo in twenty-four. In thirty-fix hours the part was fcarcely difcoloured, and in three days the pigeon was perfectly well.

I had a pigeon's leg bit repeatedly by a viper, and paffed a loofe ligature round it, which when done, I had bit feveral times by a fecond viper.

In

In thirty minutes I took off the bandage. The leg was ſwelled, and diſcoloured in the parts pierced by the teeth. In twenty-four hours theſe ſymptoms were abated, and the pigeon recovered on the third day.

I had another pigeon's leg bit ſeveral times by a viper; and after paſſing a weak ligature round it, had it bit by a ſecond. In an hour I removed the ligature; the leg was livid and ſwelled; but theſe ſymptoms almoſt diſappeared in the ſpace of twenty-four hours. In forty-two the natural colour was well reſtored; the pigeon recovered in three days.

It ſeems that from all theſe caſes this deduction may be made, that the ligature applied immediately, and left for a determined time on the part bitten, is a certain remedy againſt the venom of the viper. It entirely prevents the internal complaint, and we obſerve the animal to recover, although the external and local one continues to exiſt.

I have obſerved in general, that the local malady is violent in proportion to the tightneſs of the ligature, and the time it remains on the part. It is at leaſt what ſeveral experiments, which, for brevity's ſake I omit here, have indicated to me. It is of the greateſt importance to know with ſome preciſion, the leaſt poſſible time the ligature ſhould be left on, and the leaſt poſſible degree of force required to prevent the venom's communicating itſelf to the animal, without bringing on a gangrene of the parts.

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As to the preſſure of the ligature, it was ſo very ſlight, that I could not have thought it capable of preventing the progreſs of the venom. I generally made uſe of a very thin ſoft ribbon, of four lines breadth at moſt, which I paſſed ſeveral times round the thigh, above the articulation of the tibia with the femur, and tied it with a knot. Sometimes I forbore the knot, tying the ribbon with a piece of fine thread. In this way I had a great number of pigeons bit in the leg, and removed the ligature an hour after; they all recovered, and none of them have ſince died.

This experiment conſtantly ſucceeds, when well made, and when the ligature is paſſed round the thigh as it ought to be.

Now, if we conſider that a very ſmall quantity of venom kills a pigeon, and that in a few minutes, it will ſeem clear, that the ligature ought to be a ſtill more certain remedy in animals larger than pigeons, and which die with far greater difficulty.

I was ſo fully perſuaded of the efficacy of this method againſt the bite of the viper, that I did not heſitate to addreſs a detail of it, encloſed, in a letter, to the Marquis de Condorcet, ſecretary to the Royal Academy of ſciences, begging that he would be pleaſed to depoſit it in the Academy. I informed him in this letter that my method was ſo very certain, that of 100 pigeons bit in the leg by vipers, I ſcarcely riſked the loſing one, although it is ſo delicate a creature that the ſmalleſt quantity of venom is ſufficient to kill it. It will be ſeen by and by, what led me into an errour as to the *generality* of this remedy, and how much we ought to be guarded againſt the moſt promiſing analogies, as I have already hinted ſeveral times in this work.

CHAP. II.

If the Bite of the Viper is naturally mortal to Man?

TILL now I have treated of the bite of the viper in animals; it is my preſent buſineſs to ſpeak of it as it relates to man himſelf; and this forms the moſt uſeful part of the work. And here I am not afraid to advance freely, that the bite of the viper is not abſolutely mortal to man, and that thoſe have been miſtaken who have regarded the diſeaſe cauſed by the viper's venom as one of the moſt dangerous, and from which it is impoſſible to recover.

We have ſeen that the leſſer animals, ſuch as ſmall birds, die in a few minutes, from the venom of the viper, if they have but been bit effectually, or rather, if the viper has penetrated with its

teeth

teeth far enough into the body, to leave there the quantity of venom it ufually forces from them when it bites: this circumftance I have verified in more than 200 fmall birds. The fmaller pigeons themfelves are in the fame predicament; not one of them efcapes death, if the viper is in good order, and they have been effectually bit: we muft except here the fmall number of cafes in which the venom introduced into the animal, is thrown out again with the blood, as the animal in thefe cafes furvives from not having had the complaint. Pigeons when bit, live longer than fmall birds, and the period of their exiftence after being bit, has an agreement with their fize and weight, compared with thofe of the fmall birds themfelves.

If from pigeons we pafs to fowls that have been bit, we already find a very great difference, both as to the intenfenefs of the complaint, and the time they live. We have feen that many of them do not die, although feveral times bit, and that others die much later than the pigeons and fmall birds.

If a great number of experiments made on fowls be fufficient to eftablifh an inductive proof, I do not hefitate to affert that the time of their death has likewife fome relation to the fize of their bodies.

What I have faid of fmall birds and pigeons, in their relation to fowls, may be equally faid of fmall guineapigs and rabbits, compared with the greater animals of their fpecies. Thefe fmall animals infallibly died when bit effectually, but a great number of the larger ones recovered, however they had been

 bit,

bit. They had very dangerous illneſſes proportioned to the number of vipers employed, and the number of bites they had received from them, but uſually recovered.

I have obſerved that very ſmall dogs die eaſily, even though bit by a ſingle viper, and but once; but amongſt the middle ſized dogs, a vaſt many not only ſurvive, but reſiſt the poiſon perfectly well, without any remedy being adminiſtered. In proportion as the dogs become larger, they the better reſiſt this poiſon; five bites from three vipers were not ſufficient to kill a dog that weighed nearly ſixty pounds.

Now let it be noticed than a man is about three times as big as the dog of which I ſpeak, and let any one judge if a viper can kill him with a ſingle bite!

It has perhaps never happened that a man has been bit by more than one viper, and ſhould it ever happen, ſuch caſes will be always very rare, ſince it ſo ſeldom occurs that a viper bites a man more than once. A very few contrary caſes do not create an obſtacle. It does not appear then that the uſual bite of a viper can be mortal to man; but there is an obſervation which I have verified in almoſt all the countries through which I have paſſed, and from which I could draw any information, that ſeems clearly to demonſtrate that the venom of the viper is not naturally ſo. I obſerved that it is very rare to find two perſons bit by the viper, above all in the country or on the mountains, who make

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uſe of the ſame remedy. I have known ſome cured with the theriaca alone, taken internally or applied to the part; others with common oil; others with ſtimulants, ſuch as the ſtrongeſt ſpirituous liquors; and others on the contrary with ſedatives. In ſhort, there is no kind of ſubſtance or medicine, that ſome one has not tried in this diſeaſe. What is very certain is, that with all this none have died; at leaſt I could never ſubſtantiate any caſe in which a grown perſon died with the ſimple bite of a viper. Now if we conſider that perſons bit by the viper recover, however they are treated, and even with remedies altogether contrary to each other, we ſhall ſoon ſee that the bite of the viper cannot be ſo dangerous as has hitherto been imagined. A complaint yielding to all theſe remedies, even to thoſe of oppoſite natures, can never be a dangerous one.

I have informed myſelf very carefully, in all the countries through which I have occaſionally paſſed, of perſons bit by the viper; I have myſelf examined more than ten or a dozen, and have heard mention of more than fifty, either from phyſicians, ſurgeons, or perſons who were preſent and aſſiſted the ſick. Of ſo many bitten, not one is dead; and I have never heard but of two perſons who are ſaid to have died from having neglected to take remedies. It was not poſſible for me to gain any information of one of theſe people, whatever enquiries I made into the ſubject, ſo that I even doubt the truth of the fact; but I know that the other died at the end of twenty days, with a gangrened arm. He was ſcarcely bit, when very deep ſcarifications were made, and in three days the parts had already gangrened. The Count de Carburi, conſulting phyſician to the King, at Paris, ſaw eight perſons in the hoſpital at Turin, all of them bit by the viper, and ſaw them all recover, although each had a particular treatment. Of theſe eight, he treated one with the volatile alkali.

It remains now to reply to a difficulty, if it can however be called ſo.

Some one may perhaps object that my experiments were made on animals, and that the argument does not hold good, betwixt an animal and a man, betwixt a man and a dog. Difficulties of this kind have been made at all times, either through ignorance or envy, by thoſe who are galled at the ſeeing others add to the number of new truths, or by thoſe who are ignorant of the laws and correſpondence which nature has eſtabliſhed betwixt animals.

I ſhould be aſhamed to endeavour to prove that there is a perfect analogy in the caſes I have adduced, and that we may very well draw concluſions betwixt an animal and a man: it is ſufficient to peruſe what Boerhaave, Mead, Albinus, and Morgagni, have written on the ſubject, and the uſe theſe great men have made of it.

Common oil was believed in England to be a certain remedy againſt the bite of the viper, and experiments were made with it on a man, in the preſence of ſeveral members of the Royal Society of London.

The Royal Academy of Sciences of Paris having been

been informed that an English peasant had found a pecifick in olive oil, and that he had made the experiment with it on himself, in the presence of several members of the Royal Society of London, the Academy deemed the discovery of such importance, that it deputed two of its members, Messieurs Geoffroi and Hunauld, to verify the experiment. These two Academicians had several pigeons bit, and likewise several chickens, two cats, a goose, a turkey cock, and eight dogs. The result of their experiment was that olive oil could not be regarded as a specifick. No experiment was made upon the human species, and this illustrious body declared however that oil is in no way a specifick against the bite of a viper, and that it has not the smallest efficacy in curing the disease caused by it (*a*).

The two Academicians made a few general observations on the animals bit by the viper, which are as follow.

I. That there is no coagulation in the blood, but on the contrary, every sign of fluidity.

II. That the serosity is extravasated into the cellular membrane, and has a bloody appearance.

III. That the arteries are empty and the veins filled.

IV. That the blood is coagulated in the auricles and ventricles of the heart, but without any consistence.

In the report Messrs. Geoffroi and Hunauld made on the subject, we likewise find the recital of two

(*a*) Mem. de l'Acad. Roy. des Sc. de Paris. Annee 1737.

persons cured, who had been bit by the viper; but they were treated in a way more likely, in my opinion, to occasion their deaths, than to afford them relief.

To one was given a great quantity of Burgundy, and repeated scarifications made. This patient was actually ill for two months, whereas he would otherwise have probably recovered in two days, having only been bit in the finger.

The other was likewise bit in the finger: ligatures and scarifications were tried, and after all he was very ill. These two cases evidently demonstrate to me, that the bite of the viper is not much to be dreaded, since though so improperly treated, it did not occasion death.

Mead, a few years after, having read the experiments made by the Academy at Paris, on the inefficacy of oil in the bite of the viper, adopted them entirely without difficulty, and believed with this illustrious body, that oil could have no efficacy, and that they had decided well in the affair. After a judgment pronounced by so illustrious a body; after Mead has subscribed to it; I do not believe that any one will be sufficiently hardy to throw doubts on the application of my experiments, made on so many different kinds of animals, and repeated on so great a number of each species.

The venom of the viper is poisonous to all animals with warm blood; at least I have not as yet been able to find any exception. Being in Italy, I extended my experiments to all the animals I was

able

able to procure, and the complaint discovered itself in each of them when the viper had actually deposited its venom in the parts bitten. In every species of animal the complaint is violent and deadly in proportion to the smallness of the creature bitten; and its violence also depends on the quantity of the venom introduced. However, to suppose that what deranges the animal economy with so much activity and strength, in different animals, and encreases its effects in proportion to the encrease of its bulk, can at the same time be not at all injurious to man, would be hazarding a supposition at once absurd, incredible, and unlikely. Let a single example be brought of a poisonous substance, whether animal, vegetable, or mineral, which kills, or is productive of violent complaints in so short a space, in animals with warm blood, and has with all this no baneful effects on man, and it may then be said that the analogy betwixt the effects of a poison on man, and of the same poison on animals, is not admissible; or rather it may be said that this is a singular case, an exception to the general rule. The examples of cats, which survive the bites of several vipers, instead of favouring, destroy this hypothesis. It is true that the cat, a fierce animal, makes a strong resistance to this poison; but it resists it precisely because it is in comparison with other animals, very strong and robust, and is nevertheless, in spite of its strength, itself attacked with the complaint caused by the bite of the viper; and this complaint becomes violent, painful, and lasting in proportion as the quanti-

ty

ty of the venom introduced into the body of the creature by the viper is increased. I doubt not, if instead of five or six vipers, ten or fifteen for example had been employed, but the cat would have died, because the disease would then have been very violent, and more than equal to the strength of the animal. In fact, kittens die as well as other creatures, although bit by very few vipers, precisely because they are less robust than when grown up.

Reply to Monsieur Jussieu.

It now remains to reply to a difficulty which favours the volatile alkali, and which, previous to my experiments, might have appeared very great and unanswerable. This difficulty solely consists in the cases where persons bit by the viper were happily cured by the volatile alkali; such for example as the fine cure we read of in the history of the Academy of Sciences of Paris, by M. Jussieu, of a young man who was bit by a viper, and treated with eau de luce.

I think I ought to enter upon my reply by remarking that there are very few cases so complete in their circumstances, as that of Monsieur Jussieu undoubtedly is. Sannini indeed speaks of three persons bit by snakes, and cured by the volatile alkali alone; but we are ignorant of the quality and effects of the venom of these snakes, which certainly were not vipers, although he believed them more noxious than the

the rattlesnake itself. Besides, Mead is of opinion that the rattlesnake kills in a very short space of time, even in a few seconds. The first of the three persons Sannini treated with the volatile alkali, had been bit several hours before the remedy was applied, and was notwithstanding so well recovered on the following day, that he returned to his usual employment of fishing. This Author likewise speaks of a little insect called centipedes, the bite of which he deems mortal, and says that he was himself cured of it by the volatile alkali. But neither is the strength of the venom of this animal well known, nor have there been sufficient experiments on the subject.

Monsieur de Mascena speaks of a cure made with the volatile alkali, in French Guiana, on a person bit by a snake. He was treated with eau de luce, and recovered *(a)*. The snake is not named, and it is not known whether or no it is really noxious; but could it even be proved that a person bit by the rattlesnake had recovered on using the volatile alkali, ought we thence to conclude that the volatile alkali is a specifick against the bite of that creature? I have already proved in a demonstrative way, that it is certainly not so against the bite of our viper, which does not differ essentially from the rattlesnake, except in its size. It is true that the latter being seven or eight times as large as our viper, is consequently capable of conveying seven or eight times the quantity of venom, from whence the dif-

(a) Journal de Phys. août, 1777.

case

caſe may become proportionably greater and more dangerous. Doctor Mead ſpeaks of a man who was bit in London by a rattleſnake, and who recovered by having the part ſucked, and by bringing on vomitings with oil and water. This caſe may lead one to ſuſpect that the bite of the rattleſnake itſelf is not always mortal, ſince the bite of our viper is not cured to a certainty either by ſuction or by vomitings brought on by ſwallowing oil. But why ſhould the bite of the rattleſnake be invariably mortal to ſo large a creature as man, when we have ſeen that five bites received from ſeveral vipers were not ſufficient to kill a dog, weighing only a third of a man's weight? I cannot indeed ſee why the bite of a rattleſnake, which may be reckoned equal to that of ſeven or eight vipers, ſhould always be fatal to the human ſpecies.

The greater quantity of the rattleſnake's venom is not then a concluſive argument that it muſt invariably kill ſo large a creature as man; and why ſhould it be equally dangerous, whatever part of the body may be bit, at what time ſoever it may happen, and in whatever ſtate it may be found?

It has been ſeen above that wounds or bites in the noſes and ears of animals are attended with little danger; the ſame may be ſaid of thoſe of the ſkin, which heal better than the muſcular ones.

But were it even granted that the rattle-ſnake may by its bite introduce a ſufficient quantity of venom to kill a man, how many cauſes may there not be to prevent the introduction of this quantity of venom into the wound ſo as to produce ſuch an effect?

In

In the courſe of our experiments on European vipers, we have ſeen many caſes in which there has been little or no complaint in conſequence of the venom introduced: and what is the poiſon that may not become innocent when diminiſhed in quantity? The rattleſnake may be deſtitute of venom, a circumſtance which I have ſometimes obſerved in our vipers. It may bite ſo ſuperficially, or ſo ill, that the poiſon introduced may not be ſufficient to occaſion death. A vein, an artery, torn by its tooth, is ſometimes capable of forcing out either all the the poiſon, or a part of it at leaſt. We have obſerved all theſe caſes as they relate to our vipers, and they may be equally applicable to rattleſnakes.

To form a proper judgment of the ſtrength of a rattleſnake's venom, and that of the other ſnakes of French Guiana, and likewiſe of the advantages of the volatile alkali in the diſeaſe cauſed by it, it would be neceſſary to make a great number of experiments, as I have hitherto done on the European vipers. Beſides, if the volatile alkali is totally uſeleſs in the bite of our viper, how can it have any efficacy, how can it be a certain remedy in the bite of ſnakes which they pretend to be much more venomous? I am inclined to believe that the common bites of theſe ſnakes are not deadly in their nature, but that they become ſo in ſome particular caſes and by accident, as by the number of bites received, and the injudicious manner of treating the part bitten.

If attention be paid to the complaint produced

by

by the viper's venom, it will ſoon appear that the animal bitten may eaſily die if badly treated, or if diſorders are excited in its economy by perſons little ſkilled in this diſeaſe, which is not unlikely. A large tumour is generally formed round the part bitten; there is an extravaſation of black and livid blood in the cellular membrane, even at a great diſtance from the bite; and laſtly, a conſiderable gangrene is frequently formed, which deſtroys the ſkin and cellular membrane, and extends to the muſcles. Who does not ſee that in theſe caſes the gangrene of itſelf, and uninfluenced by the venom, may be fatal, if the patient be by accident badly treated? And this may be the caſe in the inſtance of any one's dying from the bite of the viper; there remains a large local wound, which may be conſidered as cauſed by the mere mechanical action, and this wound may be well or badly treated. We have ſeen above that the venom of the viper kills by its general action on the animal, which does not die from the ſimple local malady of the part bitten, but is deſtroyed by the venom, although the part injured be wholly extirpated.

Although I have not had the good fortune to diſcover a certain ſpecifick againſt the bite of the viper, I have however the pleaſure to aſſure the publick, that this bite is not ſo dangerous as has been hitherto univerſally believed, and that in an inſtance where a perſon may have the misfortune to be bit, life ſhould not be deſpaired of, even though no remedy be applied.

If

If I have demonſtrated the inutility of a remedy which was believed a certain one, if I have deſtroyed the hope of diſcovering a ſpecifick againſt the viper's venom, I have at leaſt the conſolation of having ſubdued the frightful idea that has been entertained, that the bite of the viper is uſually mortal.

I am of opinion, that of 100 men bit, each of them once by a ſingle viper, either in the foot or the hand, parts which are uſually expoſed to the bite of this animal, not one of them would probably die, even though they ſhould make uſe of no remedy.

After having ſeen the effects of the viper's bite on eight different kinds of animals, as well thoſe with warm blood, as thoſe with cold; after having had more than a thouſand of them bit in ſo many different parts of the body, by ſeveral vipers, and that repeatedly, I believe no one will deem the opinion I have delivered a raſh one; an opinion which even becomes the neceſſary conſequence of what we have hitherto obſerved.

The ligature made uſe of againſt the bite of the viper in the pigeons, was the immediate conſequence of theſe very experiments. After having diſcovered that the viper's poiſon did not attack the nerves, that its whole action was exerciſed on the blood, and that the diſeaſe was only communicated by means of the circulation, it was eaſy to percieve that this circulation being ſtopped, the diſeaſe cauſed by the venom would be ſo too. I

tried

tried this method with the greateſt ſucceſs, and found it a certain remedy in the caſes of the animals for which I made uſe of it. It did not however appear poſſible to me but that the ligature had been propoſed by ſome author, ſince it is an idea that ſeems to preſent itſelf ſo readily. It is very true that Redi, who treats of the venom of the viper in two diſtinct works, makes no mention of the ligature, and Mead himſelf, who ſpeaks of a great number of remedies, ſome even that are without efficacy, takes no notice of it.

I have for ſeveral years made reſearches into the remedies uſed in different countries againſt the bite of the viper, and could produce more than fifty recipes given me for the moſt part by country people, or by ideots. In one of theſe recipes mention is made of the ligature, but this ligature is joined to ſo many other things to be done before and afterwards, and all of them ſo abſurd, that no one would ever take the pains to examine, if any thing were to be expected from the uſe of this method. In general theſe recipes not only contradict each other, but what is ſtill more, one remedy is oppoſed to another in the ſame preſcription. Some are introduced, having a ſedative tendency, and others on the contrary which diſpoſe to irritation. Some amongſt them are cooling, others heating. The remedies themſelves are in a great part abſurd and ridiculous, notwithſtanding which the perſon who gave me theſe recipes aſſured me of their efficacy, and ſeveral of them had been bit by

vipers,

vipers, and treated with them. I confeſs that I have not had the patience to verify any of them by experiment, except ſome few that ſeemed the leaſt abſurd, and theſe I found totally uſeleſs, and ſome of them even hurtful. But at length I found in an authour, a method of treating the diſeaſe cauſed by venomous ſnakes, in which the ligature has a conſiderable ſhare. This authour is the celebrated Kempfer, who ſays he made uſe of it with the greateſt ſucceſs, in his voyages to India, and cured ſeveral perſons by this method.

Had I known Kempfer's method before I made my experiments on the pigeons bit by the viper, and which were cured by the ſimple ligature, I ſhould never have made uſe of it, and could not have believed it a certain remedy. I was too much perſuaded that the nerves had a great ſhare in the diſeaſe of the venom: it was neceſſary for me to know that all was brought about by the circulation; it was moreover neceſſary that the internal diſeaſe ſhould not be communicated to the animal ſo as to occaſion its death, till at the end of a certain time. I was then ignorant of all this, and was even perſuaded to the contrary. The bite of the viper in pigeons, convinced me that the internal malady was already communicated to the animal in twenty ſeconds, and that its death was not occaſioned by the external and local malady, but by the internal one. In theſe circumſtances it was eaſy to believe that Kempfer's method would come too late, and would be uſeleſs on that very account alone.

alone. But I ſhould have had other reaſons for not trying it: Kempfer, in all his voyages, makes no mention of vipers, but ſpeaks very much of venomous ſnakes which are not yet well known. It is not yet determined whether the venom of theſe is analogous to that of the European viper, and whether it cauſes a like diſeaſe.

I could not beſides have any confidence in a method which I ſhould rather have thought a hurtful than a good one. That of Kempfer conſiſts in ſeveral things which he deſcribes as equally neceſſary againſt the bite of a ſnake. He begins by a ligature above the part bitten, and then proceeds to ſcarifications. He ſqueezes the blood from the wounded part, covers it amply with theriaca, and applies above all pieces of linen ſpread with the ſame. During the whole courſe of the diſeaſe he gives his patient ſudorificks.

Kempfer's method conſiſts then, as we ſee, in five or ſix particular remedies, and it cannot be determined what good or what harm each of them may be productive of. Beſides, every thing concurs to give me a ſuſpicion of this method: there remains a doubt whether the ligature is proper or improper; and I know by experience, that ſcarifications in the part are more hurtful than ſalutary, and that ſudorificks, like the volatile alkali, are of no efficacy.

How many experiments ought not Kempfer to have made, to be certain that his method was good and effectual againſt the bites of the many ſnakes he he speaks of, since to exclude to a certainty the fluid volatile alkali from the remedies employed in the bite of the European viper, I was obliged to make more than 600 experiments! he ought to have assured himself whether the venom of these snakes was in its nature mortal; he ought to have known the average number of animals destroyed by it; and he ought to have multiplied and varied his experiments a thousand ways on all the different kinds of these snakes.

But what is still more, Kempfer himself would have made me doubt his remedy, and his authority for it: in the same work in which he speaks of this remedy, he asserts that *the stone of Pedro de Cobra* cures equally well the bite of these snakes.

It is in the first place difficult to conceive why Kempfer has preferred the use of a long, complicated, painful, and difficult method, whilst he had one so easy, commodious, and certain as this stone, which is found every where in those countries: it must be confessed that this is far from giving any confidence. It is besides known from the experiments of those two great Italian naturalists, Redi and Valisnieri, that this stone has no efficacy in curing the bites of our vipers; whence it follows, either that the venom of these snakes Kempfer speaks of is totally different from that of our vipers, or that Kempfer asserts the truth of particulars absolutely false and badly attended to, and that his authority can on that account have no weight.

I now think myself at the close of my present work, and flatter myself that my labours may be of some utility.

The bite of the viper conveys to the persons bitten the dread of its being fatal, and terrifies whole families. The persuasion of the disease being mortal, and that not a moment is to be lost, causes the application of remedies either violent or hurtful. The dread itself may increase the complaint; and there have been persons who have received almost imperceptible bites in the hands or feet, and who perceiving an instant after a viper near them, have suddenly fainted away.

I knew a man who seeing himself bit by a viper, fell into a swoon from the simple fright; he remained in this state more than an hour, till he was observed by accident, and brought to himself by means of cold water thrown upon his face. Animals in general who seem the most to dread the bite of the viper, and who tremble at the very sight of this creature, die the soonest. Dogs, who are enraged when bit, and attack the vipers furiously, make a great resistance to this poison: I have at least thought I perceived so in the course of my experiments on these animals. It cannot be doubted but that violent affections of the mind, and the dread of approaching death, must strongly influence the state of a sickness in man.

A person may very well die under these circumstances, who would not have died from the complaint alone caused by the venom. A simple bite

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of a viper is not in its nature mortal; and were there even two or three vipers, although the complaint would become more violent, it would not be fatal in its effects. Should a man even be bit six or seven times by a viper, and should all the venom contained in its vesicles be conveyed into the wound, yet ought we not to despair of him. The complaint would become violent, but there is as yet no certainty that it would be mortal. A real consolation then, and a truly useful discovery results from having better examined than heretofore, the effects of the viper's venom on animals of different sizes, and on man himself.

Experiments on the utility of the Ligature against the bite of the Viper in small Birds.

Mere curiosity, and perhaps again the vanity of being able to cure with the simple ligature the smaller animals bit by the viper, induced me to undertake various experiments on sparrows that had been bit; and the result of these experiments created doubts as to the ligature itself in larger animals, which I should otherwise never have entertained. I had not even a momentary doubt, since a pigeon could be cured by the ligature, but that a rabbit, a dog, and even a man, might be cured with still greater ease. The analogy was not only applicable in this case, but the nature of the pigeon, the action of the venom on its blood, and the effects it produces on the animal, were so many direct proofs that the ligature ought to be a remedy certain in proportion to the largeneſs of the animal, and the difficulty with which it dies. I was nevertheleſs deceived: ſo true it is that nature does not permit us to gueſs at her operations; that we know very little beyond experiment; and that we ſeem even to be forbid to reaſon on experiments themſelves. But let us proceed to thoſe on the ſparrows.

I had a ſparrow bit by a viper once in the leg, which had ſcarcely been bit when I bound it with a ſilk ribbon above the part. In thirty-five minutes I removed the ligature, and the ſparrow died twenty minutes after.

I repeated this experiment in all its circumſtances: the ſparrow died ten minutes after the ligature was removed.

I had another ſparrow bit once in the leg by a viper, and immediately applied a bandage above the bite: I took it off at the end of an hour, and half an hour after the ſparrow died.

I repeated this experiment on another ſparrow without varying the circumſtances, except that I removed the ligature at the end of fifteen minutes: in fifteen more the ſparrow died.

I bound a ſparrow's leg, and kept it in that ſtate for four hours, when it was ſcarcely altered in its appearance. I then contrived that a viper ſhould bite it repeatedly above the ligature, which after three hours I took off. At the end of twenty hours the

the ſparrow ſeemed very lively, and fed. In eight days I found it dead, although the leg was entirely healed.

After all the caſes hitherto related of the ſparrows, it appears that the ligature may be ſometimes an effectual remedy againſt the bite of the viper. The laſt ſparrow, which died at the end of eight days, proves nothing againſt the ligature, ſince without it, it would have been dead in a few minutes. I obſerved beſides, that the ſparrows I kept caged, died of themſelves, and that the ſmalleſt movements, or the leaſt violence they ſuffered, in having them bit, or in applying a ligature, was ſufficient to kill them. Amidſt theſe doubts I thought it neceſſary to multiply the experiments, and to vary them ſtill more.

I bound a ſparrow's leg with a ribbon in the uſual way, and had it bit by a viper. The bite was juſt above the ligature: in ſeven minutes the ſparrow died.

I bound the leg of another ſparrow as above, and had it twice bit by a viper. It died in five hours, although the ligature was not removed.

I tied the leg of another ſparrow, and had it bit by a viper below the ligature. In eight hours it died, the ligature continuing on the leg.

Having tied the leg of another ſparrow, I had it bit twice by a viper, and removed the ligature in four hours after. It died at the end of eight.

Theſe new experiments ſhew that the ligature preſerves the lives of ſparrows bit by the viper, but not always. The ſparrows that die ſeveral hours after its being removed, certainly cannot appear to die of the internal diſeaſe, ſince the ſmalleſt quantity of the venom introduced into their blood is ſufficient to kill them in a few minutes. It is likewiſe probable that ſome of them die becauſe in having them bit by the viper, in binding their legs, and above all, in the removing the ligature, thoſe who are thus employed, as well as thoſe who hold them, muſt inevitably do them ſome injury. When the ligature is taken off, the leg is already livid and ſwelled all over. This can never be ſo well done but that the creature muſt clearly ſuffer from it. There are ſome of them who no longer ſupport themſelves, but beat their breaſts and legs againſt the cage, and in this ſtate can neither eat nor drink.

I cannot doubt, after having had a great number of them bit, and having wounded ſeveral others with venomous teeth, but that all theſe different cauſes concur, more or leſs, to render the ligature uſeleſs in ſparrows. Some were tied before they were bit or wounded, others immediately after. In ſome the ligatures were removed at the end of four hours, in others ſooner. Three I left conſtantly bound, and neither of them died, but I took care to nouriſh them, without doing them any injury. Their legs became black, and intirely dried up. At the end of twenty days they flew about the chamber, ſupporting themſelves as well as they could with the extremities of their feet.

Five

Five others died in my hands almoſt at the moment I had taken off the ligature, and immediately after I had given them drink. Twelve others recovered perfectly, and the four laſt died in between ſix and ten hours. There were in all twenty-four.

However favourable the reſult of this laſt experiment was to the ligature, and however probable it appeared that ſeveral of theſe animals died from quite another cauſe than that of the venom, I was not yet altogether eaſy, and I thought it proper to repeat my experiments on animals of a larger kind, and of a different nature.

Ligatures made in the caſes of Fowls bit by Vipers.

I had a fowl's leg bit ſeveral times by three vipers, and in three minutes bound it with a ſtrong ſilk ribbon. In an hour I removed the ligature, and found the leg ſwelled and livid in all the parts beneath it. In three hours the fowl died. The whole ſubſtance of the muſcles bit was diſeaſed, and there were ſome marks of complaint even above the ligature, at the ſide of the belly, and in the breaſt.

I had a fowl's leg bit repeatedly by two vipers, and after four minutes tied it over the part bitten. In two hours the leg was very livid and much ſwelled. I took off the ligature, and at the end of twenty-two hours the leg ſtill remained a little livid. The fowl recovered in two days.

I had another fowl bit in the leg repeatedly by two vipers, and after four minutes bound the limb which

which in an hour became ſwelled and livid. Two hours after I took off the ligature; in twenty-two the leg was leſs livid and ſwelled; and in four days the fowl recovered.

I bound a fowl's leg very tight, and had it bit repeatedly by two vipers. It could not ſupport itſelf on this leg, which in two hours became ſwelled and livid. In eight hours I took off the ligature; after twenty-two the lividity ſtill remained, and even appeared above the part that had been bound, in forty-ſeven the fowl died.

I had a fowl's leg repeatedly bit by two vipers, and at the end of a minute tied it. In three hours I took off the ligature, and found the leg ſwelled and very livid. Three hours after this the ſwelling and lividity had extended to above the ligature, and in ſix more the fowl died.

I had another fowl bit repeatedly by three vipers in the leg, which I tied two minutes after. In ſix hours I took off the ligature, and in ſix more the fowl died. The ſwelling had reached above the ligature.

I had the leg of another fowl bit repeatedly by three vipers, and tied it three minutes after. At the end of nine hours I took off the ligature; the leg was ſwelled and livid, and bled from every part of it. The fowl recovered by degrees, the leg having a yellow and green hue which continued ſeveral days.

I had the leg of a fowl bit by two vipers, and bound it immediately afterwards, but ſlightly; in twelve

twelve hours I removed the ligature, three hours after which the fowl died. The ſwelling and livid colour of the leg had reached above the ligature.

I had another fowl bit repeatedly in the leg by two vipers, and immediately bound it, but tighter than in the preceeding experiments. I took off the ligature after twelve hours; in ſix more the fowl died, with the ſwelling and lividity extended to above the ligature.

I had the leg of another fowl bit repeatedly by two vipers, and tied it immediately, but ſtill tighter than in the laſt experiment. In twelve hours I removed the ligature, and in two more the ſwelling and diſcoloration had extended above it. The fowl recovered in five days.

I had three fowls bit in the leg, each by two vipers, and bound the parts immediately. Within the ſpace of ſix hours I removed the ligature from one of them, ſuffering it to remain on the other two for twenty-four. In two hours after removing it from the two laſt, one of them died;—the other recovered. The fowl whoſe ligature was removed in ſix hours, died in ſix more.

I had a fowl bit repeatedly by two vipers, in the leg, which I had previouſly well bound. It died at the end of twenty hours, although the ligature had not been removed.

Theſe experiments made on fowls, raiſe great ſuſpicions as to the efficacy of the ligature againſt the bite of the viper; it has even appeared to me, and I do not think myſelf deceived, that the local malady

malady is greater with the ligature than without it. I obferve this difference throughout the whole journal of my experiments, and it is difficult for me to have been miftaken, fince I have regularly compared the local malady of the limbs tied, with that of thofe which were not fo. Several fowls, as we have obferved, die before the ligature is removed, and that at pretty regular intervals. After the experiments made on the pigeons which recovered with the ligature, all this appeared paradoxical to me, and I could not conceive it poffible but that it ought to recover larger animals. I dreaded the not having made my experiments on the fowls as I ought to have done; that the ligatures were either too tight or too loofe; or that I had removed them either too foon or too late. In a word every thing feemed to me more probable than that the ligature was either ufelefs or hurtful.

In the midft of thefe doubts I determined to make my experiments on fome other kinds of animals more extenfive, and for this purpofe made choice of rabbits and guineapigs.

Experiments on Guineapigs.

I bound the foot of a guineapig very tight, and had it bit repeatedly by two vipers. After twenty-four hours I removed the ligature, and found the foot fwelled and livid. In thirty hours it was lefs livid

livid, but more ſwelled: the creature recovered in four days.

I had the foot of a guineapig bit by a viper, and applied the ligature a few ſeconds after. In the ſpace of an hour there were marks of diſeaſe in the part bitten. I removed the ligature, and after ten hours there were ſcarce any appearances of its having been bit.

I had the leg of a guineapig bit repeatedly by a viper, and a minute after I bound it. In fifteen minutes the foot was ſwelled and livid. I removed the ligature, and in ten hours the animal had ſcarce any marks of diſeaſe. It recovered in twenty-four.

I had the foot of a guineapig bit repeatedly by three vipers; and bound it a minute after. In two minutes the ſymptoms of diſeaſe had already diſcovered themſelves. At the end of twenty hours the foot was very much ſwelled and very livid. In twenty-four the leg diſcharged blood and *ſerum*. In two days there was ſome ſwelling above the ligature, and ten hours after, the part bitten was covered with an eſchar. In ſix days a total recovery took place.

I had a guineapig bit repeatedly by a viper, in the foot, which in two minutes I tied. After twenty minutes I removed the ligature, and found already marks of diſeaſe in the leg. In an hour the tumour had reached the ligature. The creature died in twenty hours, with the leg ſwelled and livid; and

and the lividity had extended to the muſcles of the belly and breaſt.

I had the leg of a guineapig bit ſeveral times by a viper, and bound it at the end of two minutes. In twenty minutes I took off the ligature, and found the leg diſeaſed in the uſual way. In ſix hours it was ſtill a little ſwelled, but not livid, and was healed in twenty-four.

I had a guineapig ſimilar to the former one bit ſeveral times by two vipers in the leg, which I bound a minute after. In thirty minutes I removed the ligature. The animal had ſymptoms of diſeaſe in its foot, but recovered in leſs than three days.

I had a guineapig's foot bit repeatedly by a viper, and at the end of twenty ſeconds bound it. I removed the ligature fifteen minutes after, and already found marks of diſeaſe in the part. In two minutes more I perceived a motion of its head, as if it was convulſed, and in four hours it died. All the muſcles of the leg, abdomen, and breaſt, were livid and inflamed.

I had another guineapig bit ſeveral times in the foot by a viper, and immediately after bound the part. In fifteen minutes I removed the ligature, the local malady diſcovering itſelf in the leg. In thirty-two hours there were ſcarcely any ſigns of complaint, and in the ſpace of forty-two, a perfect recovery took place.

It appears by the experiments hitherto related, that the ligature is capable of curing guineapigs that have been bit in the foot by the viper.

A com-

A comparative experiment was neceſſary to convince me that the bite of the viper was fatal to this ſpecies of animals. I had ſix guineapigs ſimilar to thoſe I have juſt ſpoken of, bit each of them in the leg by a ſingle viper. They all died in leſs than twelve hours.

Although ſatisfied of the utility of the ligature, I thought however it would not be amiſs to multiply my experiments ſtill more, and to vary them in ſome of the particulars.

I had the foot of a guineapig bit ſeveral times by a viper, and bound it, but very looſely. In thirty minutes I removed the ligature, and found every appearance of diſeaſe in the part bitten: however in ten hours there was ſcarcely any ſwelling or lividity; in thirty it was quite well.

I had another guineapig bit ſeveral times by a viper, in the foot, and bound it ſtill more looſely than in the preceding experiment. In an hour I removed the ligature; in ten there were ſcarcely any marks of diſeaſe, and in forty it recovered.

I had a guineapig bit repeatedly by a viper in the foot, and bound it, perhaps ſtill leſs forcibly than in the laſt. In two hours I removed the ligature, and found the foot very livid and much ſwelled. In ten hours all was diminiſhed, and in twenty-four there were ſcarcely any marks of diſeaſe.

I had the foot of a guineapig bit ſeveral times, and at the end of two minutes bound it. Soon as it was bound, I had it bit again repeatedly by a ſecond viper. In thirty minutes I took off the ligature,

ture, which was very loofe. In twenty-four hours the foot was fwelled and very livid; in five days the animal recovered.

I had the foot of another guineapig repeatedly bit by a viper, and in ten minutes tied it. I then had it bit by a fecond one. In twenty minutes I removed the ligature, which was very loofe. In twenty-four hours there were fcarcely any figns of difeafe.

I had another guineapig bit in the foot repeatedly by a viper, and in three minutes bound it, caufing it to be bit afrefh by a fecond. In twenty-four hours there fcarcely remained any appearance of difeafe in the foot.

The utility of the ligature feems to be more and more demonftrated, and it appears alfo that a very weak one is fufficient. It is true that it muft be left on for fome time, otherwife the internal malady is excited in the animal, and it dies in a fhort fpace after.

Various experiments made on guineapigs, bit as above by vipers, have pointed out to me that when the ligature is removed in ten minutes, or perhaps more, after they have been bit, the animal dies very fuddenly, and that of the internal malady.

It is not difficult to difcover when the guineapigs die of the internal malady. When the complaint begins to communicate itfelf internally, the guineapig moves its head in all directions, and feems to be convulfed. In this cafe death is certain, and follows in a fhort time. I made thefe experiments

 on

in very ſmall guineapigs, and I choſe them ſo, that the conſequences might be the leſs equivocal.

Experiments on Rabbits.

Not content with having tried the ligature on guineapigs, I wiſhed to try it once more on rabbits. I generally employed ſmall ones, beneath the middling ſize.

I had a rabbit's leg bit repeatedly by two vipers and tied it immediately after. In nine hours it was greatly ſwelled, with a diſcharge of blood. In this ſtate I removed the ligature; twelve hours after which the leg was livid and gangrened, in thirty hours the rabbit died.

I had the leg of another rabbit bit repeatedly by two vipers, and tied it three minutes after. In an hour and an half I took off the ligature. In ſix hours the leg was very much ſwelled and livid about the part bitten. In thirty hours the leg was ſcarcely ſwelled, but remained livid. In three days the animal appeared to be recovered.

I had a third rabbit bit ſeveral times in the leg by two vipers, and in two minutes bound the part. After an hour I removed the ligature, and found the leg ſwelled. In the ſpace of twenty-four a diſcharge enſued, and in three days the ſkin was corroded, and a ſore formed. In ſix days the rabbit was well recovered.

I had the leg of a rabbit bit repeatedly by two vipers, and four minutes after I bound it. In an hour and an half I removed the ligature. In four hours the leg was very much ſwelled, and diſcharged a good deal. The rabbit died in thirty-ſix hours; the tumour of the leg having extended to above the ligature, where there was alſo a livid appearance.

I had three rabbits bit in the leg as above, but as they were deſtined for a compariſon, made no uſe of the ligature. Two died in thirteen hours. The third had a ſore in its leg, which was otherwiſe much diſeaſed; but it ſurvived.

The experiments as yet made on rabbits ſeem to prove that the ligature is not a certain remedy againſt the bite of the viper in theſe animals, ſince we have ſeen them die when it has been applied, and recover ſometimes without it. I repeated theſe experiments on eight other rabbits, which I had bit in the leg, each by two vipers. The ligature was not removed till after ſix hours. Five of the eight died.

Seeing that the ſimple ligature was not uſeful to all animals, I wiſhed to try if, joined to ſcarifications, it would be more ſo; and as the local diſeaſe is formed of blood, partly diſſolved, partly coagulated, which corrodes and gangrenes the ſolids, I thought of uniting to the ſcarifications an antiſeptick, ſuch as the bark.

Liga-

Ligatures and ſcarifications tried on Fowls and Rabbits.

I had a rabbit's leg bit by two vipers, and immediately tied it: in two hours the leg was ſwelled, livid, and bloody. In this ſtate I made four longitudinal inciſions into it, at the part where the vipers had bit, and abſorbed with linen the blood that flowed from the inciſions. In this experiment I found that the muſcles were gangrened. The rabbit died in ten hours.

I had a fowl bit in the ſame way by two vipers, and immediately applied the ligature. In two hours the leg was ſwelled and livid: I made ſcarifications as above. In four days the leg was covered with an eſchar, and in ten the fowl recovered.

I had another fowl bit by two vipers, in the leg, which I immediately tied. After two minutes I made the ſcarifications, waſhing the blood from the wounds for a long time with warm water, and covering the leg with linen. In two days a black eſchar formed on the part; in three days and an half the fowl died.

I had the leg of another fowl bit by two vipers and immediately tied it, making the ſcarifications, and waſhing it with warm water. This being done, I ſtrewed a great deal of powdered bark on the inciſions, and covered the whole with linen. In twenty hours I removed the ligature, and the fowl recovered in a few days.

I repeated the above experiment in all its circumſtances, on another fowl. After twenty hours I removed the ligature, and in twenty more the fowl died.

I had two chickens bit, each of them by two vipers, ſeveral times in the leg, which I bound a ſhort time after. I made the ſcarifications, and bathed them lengthways with the volatile alkali, very much diluted with water. After eight hours I removed the ligature from one of them, which died in three hours more. At the ſame time I looſened the ligature of the other, and two days after it died.

I had a fowl bit in the leg by two vipers; I bound the part, ſcarified it, waſhed it, and covered it abundantly with the cortex. It died in the ſpace of ſeven hours, even before the ligature was removed.

I had another fowl bit in the leg by two vipers, and immediately afterwards tied it. I made ſcarifications into it, and moiſtened it with warm water in which common ſalt had been diſſolved. The fowl died in ſixteen hours, the ligature ſtill remaining on.

This experiment I repeated on two other fowls, and made uſe as above, of the diſſolution of ſea ſalt. In twenty-four hours I removed the ligatures, and in twenty-four hours more the fowls both died.

After making the ſcarifications, I tried the infuſion of the bark on two other fowls bit as above.

In twenty hours I removed the ligatures, and in twenty more the fowls both died.

I had another fowl bit by two vipers in the leg, which I immediately tied. I ſcarified it, waſhed it, and kept it immerſed in lime water which I had made warm, for twenty-five minutes. After twenty hours I removed the ligature. In three days the fowl died.

I made the ſame experiment on another fowl, keeping its leg for two hours in warm lime water. In twenty hours I took off the ligature, and in thirty-five the fowl died.

I repeated the ligature and ſcarifications on twelve other fowls, the leg of each having been bit by two vipers, and immediately tied. Four were ſcarified, and the parts kept during an hour in a ſtrong infuſion of bark in warm water. In four others they were kept for an hour in ſimple warm water, and thoſe of the laſt four in a mixture of warm water and the volatile alkali. I covered the legs with linen, and in ſix hours removed the ligatures. Three of thoſe treated with the bark, two treated with warm water, and three with the volatile alkali, died.

The reſult of the numerous latter experiments on the uſe of the ligature againſt the bite of the viper, neither affords us that certainty, nor that generality, we might have expected on beginning them. Not that the ligature ought to be rejected as totally uſeleſs, ſince we have found it a certain remedy to pigeons and guineapigs. It might become ſo to

 other

other animals, and perhaps would be uſeful to all, were the circumſtances with which it ought to be tried, better known. It appears in general, that nothing ought to be expected from ſcarifications, whether larger or ſmaller, or more or leſs ſimple, ſince we have ſeen the very animals ſink under this operation, who would the eaſieſt have recovered with ligatures only.

The ligature, by confining the blood to the part, produces a greater local complaint, and a ſtronger diſpoſition to gangrene: for this reaſon it ſhould be pretty looſe, and ought to be removed as early as poſſible.

I cannot determine what utility it may be of in man, becauſe I have made no direct experiments; but as I am of opinion that the viper's bite is not mortal to the human ſpecies, the ligature in this caſe can do no more than diminiſh the diſeaſe: a very ſlight one may perhaps be ſufficient, and it may probably be removed in a little time. But experiments are wanting to enable me to ſpeak to a certainty, and theſe experiments are very rarely to be made on men.

I wiſhed to ſee if the diſeaſe which the viper's venom cauſes in animals, diminiſhes when the incisions are made above, below, or around the part bitten.

It ſeemed natural to ſuppoſe, that as the venom of the viper finds its way into the blood by the circulation, it would alſo introduce itſelf into the parts purpoſely wounded, at leaſt into thoſe that ap-

proached

proached the neareſt to the part bitten. In this caſe it likewiſe ſeemed very probable, that the quantity of venom being thus diminiſhed by its more extenſive diſtribution, not only the internal diſeaſe, but alſo the external malady would be diminiſhed too; and that by this means the gangrene of the part would be ſtopped, or at leaſt rendered leſs dangerous.

But the following experiments ſhow the little dependence to be placed on analogical proofs, and probable reaſonings in matters of fact.

I had the leg of a fowl bit repeatedly by a ſingle, but very large viper; I made two ſmall inciſions into the inner part of the leg, above and below the part bitten. The fowl died at the end of an hour, with the part bitten conſiderably diſeaſed, but without any change in the two artificial wounds.

I had another fowl bit repeatedly in the leg by a viper, and made a little inciſion into the muſcles oppoſite to the part bitten, and a ſecond one into the muſcles of the other leg. In ſix hours the fowl was much diſeaſed. In thirty hours the leg was livid, even at a great diſtance from the part. In ſixty hours the fowl died, with the leg in a gangrened ſtate. In all this time I obſerved no change in the two inciſions.

I repeated this experiment on different animals with the ſame ſucceſs, and never perceived that the artificial wounds were affected by the venom; ſo that it ſeems to be a truth eſtabliſhed by experiment, that the venom once introduced into the blood, and circulating with that fluid, may occaſion death; but that it cannot communicate itſelf to ſimple inciſions, even when made in the vicinity of the parts bitten.

I feel that I have been two prolix. I might have been leſs ſo, and perhaps even more clear, had I followed the ſynthetick, inſtead of the analytical method. I preferred the latter, preſenting my experiments in the order in which I made them. I have not even dreaded the diſcloſing my errours, and the ſhewing how often I have been obliged to begin over again. The analytical method is certainly neither the ſhorteſt nor the moſt favourable to a writer; but it is the moſt certain, the moſt luminous, and the only one which leads immediately to a diſcovery. It inſpires the reader with confidence, ſhows in what way the naturaliſt has ſearched into nature, and in what way ſhe has anſwered to his reſearches. His faults are at the ſame time obſerved, the efforts he makes to come at the truth, and the difficulty of attaining it.

Works that preſent ſomething new, ought all to be written in the way I have made choice of in proceeding thus far. When the methods are ſeen which have led to diſcovery, the merit of the work, and the authour's opinions will be the beſt judged of; all will be free from that myſtery and reſerve which abound in writings formed on the ſynthetick plan, in which the traces are wanting that have guided to a diſcovery. But man loves rather to be admired

than

than uſeful, marvellous than true, difficult than important.

I have made more than 6000 experiments; I have had more than 4000 animals bit; I have employed upwards of 3000 vipers, and may have been deceived; ſome eſſential circumſtance may have eſcaped me: I may have neglected ſome other, not thinking it neceſſary; my conſequences may have been too general, my experiments too few in number. In a word, I may very eaſily have been miſtaken, and it would be almoſt impoſſible that I ſhould never have been ſo in a matter ſo difficult, ſo obſcure, and likewiſe ſo new. It is ſufficient for me to declare the having written nothing but what I have ſeen, or at leaſt have believed to ſee.

In reviewing my journal of experiments, I perceived ſome miſtakes in it, and found that I had written in ſome places, what I could not poſſibly have obſerved. It likewiſe happened to me ſeveral times, in copying the experiments from the journal, to read in one way, and write in another. This is a new ſource of errours into which I may eaſily have fallen. How little certain are we, even in things we believe ourſelves beſt ſkilled in, and in which we have the leaſt apprehenſion of being miſtaken! I know but one claſs of people who never err; thoſe who do nothing, obſerve nothing, and make no experiments. All others are led into errours; and the more experiments they make, the more they are deceived: but we muſt not deſiſt on that account from conſulting nature, and ought not

to

to bluſh when a Newton himſelf, who was ſcarcely ever miſtaken in the ſublimeſt calculation, has been deceived in matters of mere fact and experiment.

It is incumbent on me likewiſe to obſerve, that a part of my experiments on the venom of the viper, was made in the rudeſt ſeaſon, in winter. It is natural to conceive, that the vipers I employed could not be in full vigour; that they would exert leſs ſtrength in biting the animals; and that not having been nouriſhed for ſeveral months, their venom would be in a leſs quantity. I can eaſily conceive, that in a more favourable ſeaſon, as in the ſummer, and in a warmer climate, the effects would be in ſome way different, and in general greater.

I might likewiſe have been deceived by thoſe who furniſhed me with the vipers. I had a cuſtom in the beginning, of returning thoſe I had employed in biting the animals, when I had no occaſion to kill them. I have every reaſon to believe, that the vipers already made uſe of, have been ſold to me a ſecond time: this I no ſooner ſuſpected, than I determined to kill them without reſerve, after having employed them in my experiments.

On all theſe accounts, and perhaps on many others which I am ignorant of, my experiments might be liable to ſome variation, were they to be repeated; but this does not render the principal truths I have deduced from them leſs certain. I hope that my work will be diſtinguiſhed by the experiments, inductions, obſervations, and conſequences it contains. If my conſequences are falſe,

and

and my inductions unjuſt, my readers will ſoon perceive it, and no errour will be occaſioned by them. But if I have been miſtaken in the facts themſelves, if I have not made my obſervations well, my miſtakes will be communicated to others, and will ſerve as a baſis to a falſe theory. It is for this reaſon that I have endeavoured to be as exact as poſſible in the facts, and have entered into a long detail on ſeveral of them. In ſeveral places I have deſcribed my experiments at length, and in a great number. I might have been leſs tedious; I might have given the ſimple reſults; but I muſt then have been believed on my word, and my readers have been deprived of the pleaſure of judging for themſelves, which can alone lead to evidence and conviction.

Beſides the greater part of my experiments relate to queſtions entirely new, and in regard to which either nothing has been done before, or the obſervations badly made. It was therefore neceſſary to extend them in ſome meaſure, and I hope my readers will thank me for ſo doing.

Now, that a baſis of experiments, and of determined facts, is formed, on the bite of the viper, the naturaliſt will continue his reſearches with more eaſe, and will preſent them with more brevity.

APPENDIX

TO THE

RESEARCHES

INTO THE

VENOM OF THE VIPER.

TWO years after having made at Paris, where I then was, the experiments on the viper's venom which have been related in this work, I was informed in London, on my making ſome ſtay there, that a certain ſpecifick againſt the bite of this creature was juſt diſcovered in Italy. The little ſucceſs I had met with in France and in Italy ſome years before, in my ſearch into an efficacious remedy againſt the venom of this creature, gave me a very earneſt deſire to enrich my work with ſo important a diſcovery.

His

His Excellency, Count Belgioyoſo, Ambaſſador from the Court of Vienna to London, who eſteems the ſciences becauſe he knows the importance of them, had the civility not only to procure me the treatiſe publiſhed in Italy on this remedy, but likewiſe to give me one of the *ſtones* which are the ſubject of this treatiſe, and to which are aſcribed the faculty of curing the bite of the viper. This had been ſent from Milan, and had been prepared by the author himſelf. On this occaſion I was ſhown ſeveral letters from Milan and Vienna, which related wonders of this new, but already famous remedy. Miracles had been done with it, they ſaid, at the firſt of theſe two places, and they affirmed that the beſt phyſicians of that famous city had a knowledge of them. They likewiſe added, that the rare and very important diſcovery had been made, that the ſo much boaſted *cobras ſtones* were nothing more than calcined hartſhorn.

The treatiſe I read was entitled, *Treatiſe on the Efficacy of an Alexipharmick againſt the Venom of the Viper, by the Abbé de Tecmeyer* (*a*). It contains divers experiments which deſerve attention, and which tend to prove that calcined hartſhorn is a certain remedy againſt this creature's bite.

The peruſual of this treatiſe gave me a ſtill greater deſire of verifying, myſelf, the utility of this boaſted remedy; ſince the only way to be aſ-

(*a*) This treatiſe was printed in the RACCOLTA DI OPUSCOLI SCELTI, of Milan.

ſured

ſured of an experimental truth, is to have recourſe to experiment itſelf. The different cures related by the Abbé Tecmeyer, however brilliant and extraordinary they may be, are nevertheleſs neither ſo numerous nor ſo varied, as I at leaſt ſhould have deſired them in ſo important a matter. Neither could I conceive that hartſhorn, calcined only to blackneſs, as the Abbé Tecmeyer will have it, could be a certain remedy, whilſt calcined white, in which way I had tried it in France, I had not found it of any value. I however thought it neceſſary before I ventured my opinion, to make a great number of experiments on different animals with this *ſtone*, which I ſhall agree with the authour in calling ſo.

It is likewiſe true that on reviſing this treatiſe, it appeared to me that the authour of this new ſpecifick gave too much extenſion to his remedy, and that many things are there advanced too readily, which have either not been ſufficiently proved, or are not altogether certain.

For example, he is of opinion that the ſmall piece of burnt hartſhorn applied externally to the wound made by the viper, heals it by virtue of the volatile alkaline ſalts contained in the hartſhorn from which it is prepared.

He maintains that the viper's venom is principally compoſed of acid ſalts, and quotes the authority of Mead, and his own obſervations made with a microſcope. He even ſays, that with this venom

venom he has changed the blue dye of the turnesol red.

He believes that burnt hartshorn absorbs the venom of the viper, because it turns milk yellow, after it is removed from the part bitten.

He finds his remedy efficacious against this venom, ten hours, and even more after the animal has been bit by the viper, and when the swelling is very great, the symptoms very violent, and the indications of approaching death very certain.

He finds it equally useful against the bite of a mad-dog; and such is his good opinion of these marvellous *stones*, that he believes by virtue of them, the having cured wounds made in the posteriors of a man with the teeth and claws of a tiger.

Lastly, he does not think it impossible but that the tooth of the caiman, a species of crocodile, simply carried in the pocket, will cure the bite of a viper.

He asserts afterwards, that Redi is mistaken in supposing that the *cobras stones* are no specifick against the bite of the viper; and he believes that this celebrated physician made his experiments on false ones. The thing is certainly possible; but if the true and efficacious stones are nothing more than pieces of badly calcined hartshorn, I do not see what could have induced them to impose on Redi, in giving him false for real ones, since the latter might so very easily have been made. Besides, it appears that Kempfer thought but little of these true stones, called by the Indians *de cobras di cabello*, and

and that he did not place the ſmalleſt confidence in them. In his *Amænitates Exoticæ* he ſpeaks of them in the following manner: *de efficacia hujus lapidis, et quæ in dies cum ipſo diſtinguuntur in India experimentis multa dicenda, inquirenda, dubitanda venirent* (*a*). *Saltem fateor ingenue penès me valorem lapidis ſemper manſiſſe in ſuſpenſo, dùm quid erroris, et fallaciæ ſublatere poſſet propriis experimentis non exploraverim* (*b*). And he knew them ſo well that he deſcribes them, and neither believes them natural ſtones, nor engendered in the brain of the ſnake; he even ſeems diſpoſed to think them made of hartſhorn: *ſubſtantiam*, ſays he, in ſpeaking of theſe ſtones, *obtinet firmam et duram, levem tamen, hic ibi poroſam, et quodammodo corneam, ita ut appareat formatus ex cornu cervi in vapore vel liquore aliquo macerato tinctoque; niſi fortè fragmentum ſit lapidis Connoor variegati ita hic lapis dictus à patria Connoor multatriæ provinciæ, luſitanis ibidem, Pedra frigue dicta à qualitate refrigirante, eſtquè triplicis differentiæ ſive coloris, nimirùm albus, citrinus et obſcurè cæruleus, qui poſtremo nephritico lapidi in omnibus præter levitatem ſimillimus eſt. Quotquot videre mihi contingit per Indiam firmam et inſularem prædictæ conditionis et figuræ fuerunt. Qualiſcumque figuræ fuerint prima fronte apparebunt haud quaquàm naturales, et in cerebro viperæ, quod vulgo creditur, genitos eſſe, et ut fruſtra fuerit, qui illos in anguium capitibus quærerent* (*c*).

(*a*) Kempheri Amænitates Exoticæ. Lemgoviæ, 1712. Faſc. III. p. 579.

(*b*) P. 580.——(*c*) P. 581.

Such

Such are the opinions of the Abbé Tecmeyer in this treatise, and I confess they have appeared to me very singular.

But should it even be true that burnt hartshorn cures the bite of the viper, I can never be induced to believe that such an effect is due to the alkaline salts of this substance. I have demonstrated, so as to place it beyond a doubt, that the fluid alkali itself is of no utility in this disease, and that the venom of the viper in substance, mixed with alkaline salts, preserves all its activity, and kills as heretofore.

It is besides an errour, that the venom of the viper is composed of salts, and that these salts are acid; and it is also false that it changes the tincture of violets red. In the course of my work I have already pointed out the errour of Mead, and of other naturalists, who have succeeded him, as to the salts in the venom of the viper. It is singular to see mistakes repeated by others, which were refuted more than ten years ago.

The slight change of colour observed in the milk, which is in itself in a degree yellow, certainly cannot be caused by the hartshorn's absorbing the venom, on being applied to the part bitten; for a quantity of milk scarcely sufficient to cover the bit of hartshorn, will not become yellow if the venom of several vipers be united with it. This colour of the milk is caused by the blood's being absorbed by the hartshorn when applied to the bitten

bitten part; and in fact it tinges it equally when applied to a part wounded but not envenomed.

But it is time to proceed to experiments, which can alone determine whether a piece of burnt hartſhorn is, or is not, a certain remedy againſt the venom of the viper. It has been ſeen in ſeveral parts of this work, how little truſt ſhould be repoſed in experiments, even when they appear the moſt conſtant. I have ſometimes ſeen five, ſix, and even more animals recover, one after another of the bite of the viper, and ſhortly after as many of them die, without my having done any thing to them in either inſtance: and I have ſometimes met with the ſame conſequences in applying the ſame remedies to the ſame animals in the ſame circumſtances. In one inſtance I ſhould have judged that ſuch a ſubſtance was a ſpecifick againſt the bite of the viper, and in another that it was either hurtful or totally uſeleſs. This is the riſk we incur in not ſufficiently multiplying experiments. I do not pretend to have myſelf guarded againſt this inconvenience in all the parts of the preſent work on the venom of the viper, although it is true that I have varied and multiplied the experiments exeedingly, as much at leaſt as my circumſtances at the moment permitted me.

But in the preſent caſe I imagined that a limited number of experiments would ſuffice to determine the utility of this remedy. The many obſervations I had already made on the bite of the viper, and the knowledge they had given me of the animals I wiſhed

I wiſhed to employ, enabled me to forbear the multiplying them ſtill more.

The firſt thing to which I applied myſelf to ſucceed in my experiments, was to procure a ſufficient number of pieces of hartſhorn, prepared in the way deſcribed in the above cited treatiſe, and quite black. I had taken them from that part of the horn which is fixed in the animal's head. Applied to the tongue, they adhered ſtrongly to it. I prepared many of them, and of this number I choſe the beſt, to the end that my experiments might be made at the ſame time, on the ſame animals, and with the ſame circumſtances. After having made uſe of them, I put them in milk or in wine, as the authour directs, and having left them there for ſeveral hours, I afterwards expoſed them a great while to the ſun, or to a gentle fire, till they adhered to the tongue as before. I had likewiſe the advantage, as I have mentioned before, of having had one of them brought from Italy. I made uſe of theſe *ſtones* ſeveral times, with the ſucceſs that will be ſeen.

Before I give the principal conſequences of my experiments, I think it proper to obſerve, that I began them in London in the month of March, and did not finiſh them till the latter end of the month of May. Although it was the mildeſt ſeaſon that had been felt for ſome years in England, it did not prevent ſeveral days being very cold, in conſequence of which my vipers ſeemed to me to be much benumbed and very inactive. In general I thought that I found them lefs active there than in France, and lefs fo in France than in Italy; fo that the confequences of my experiments on thefe animals muft differ fenfibly, but only from greater to lefs. It is however true that vipers are venomous in all countries, and that their venom can kill. To accomplifh this aim in cold climates, with as much certainty as in warm ones, it will be fufficient to make them bite the fmalleft animals, or to employ feveral to bite a fingle one. In this way the effects may be rendered nearly equal in all countries, and at all feafons. Thus then, the action of the viper's venom is in proportion to its quantity, when all other circumftances are abfolutely equal; they however vary to fuch a degree, that one can fcarcely pronounce any thing certain on them, even when every poffible ftep has been taken to fucceed, and to caufe all the trials to be quite alike in their circumftances. Let us proceed to experiments.

I had a pigeon bit once in the right leg by a viper, and at the fame inftant applied to it the Italian *ftone*, which immediately faftened, and remained on. Seven minutes after the pigeon gave tokens of difeafe, and was dead in twelve. I detached the ftone by force, and put it in milk for other experiments.

To make a comparative one, I had another pigeon bit in the leg by a viper, and it died in fixteen minutes.

I expreffed the venom from the teeth of a viper forced into the mufcles of a pigeon's leg, and applied

plied the Italian *ſtone*, which faſtened immediately to the wounds. The pigeon died in eighteen minutes, without the *ſtone* having detached itſelf.

I made the ſame experiment on another pigeon, with teeth taken from a ſecond viper, and it died in twenty-two minutes.

I had a pigeon's leg bit once by a viper, and immediately applied the Italian *ſtone*, which did not looſen of itſelf. The pigeon died in four hours.

To make a comparative experiment, I had another pigeon bit once in the leg by a viper, and applied the *ſtone* wrapped in a piece of bladder, binding it to the part. The pigeon died in eight hours, the bandage having probably retarded the action of the venom.

Another pigeon bit in the leg by a viper, died in two hours, although the Italian *ſtone* ſtill adhered to it.

I had another pigeon bit twice by a viper, and made a very ſmall opening with a lancet at the place where the teeth had penetrated, immediately applying to it the Italian *ſtone*. The pigeon died in ſix minutes, the *ſtone* ſtill adhering.

I had ſix other pigeons bit by as many vipers. To four I applied the *ſtone*, to the other two I did not. One of the laſt died twenty minutes after, the other in an hour. The firſt four all died in leſs than twenty minutes, and one of them at the end of eleven. The *ſtones* were ſtill attached to the parts bitten.

This experiment was repeated on ſix other pigeons, to each of which I applied a *ſtone*. They all died; three in ſixteen minutes, and three in twenty-ſeven. Five of the ſtones continued to adhere: the one which fell, belonged to a pigeon that was one of the lateſt to die.

Experiments on Quadrupeds.

Perſuaded of the little efficacy of theſe *ſtones* in the caſes of pigeons, I wiſhed to ſee if they would be of more uſe to quadrupeds. I made choice of ſmall guineapigs and very ſmall rabbits.

I had a guineapig bit in the leg by a viper, and having dilated the wound a little, applied the Italian *ſtone* to it, which faſtened very well. It died an hour after, the ſtone ſtill remaining on.

I had the leg of a guineapig bit by a viper, as above. This one died before the ſtone was applied to it, and almoſt at the moment of its being bit: a very rare caſe, and ſuch as I have obſerved only once in the courſe of my experiments on the venom of the viper.

I immediately had another bit in the ſame way, and applied nothing to it: it died four hours after.

At the cloſe of theſe laſt experiments I had ſix guineapigs bit ſucceſſively; to four I applied the *ſtone*, to the other two I did not. Three of the firſt died in two hours, and one of the two laſt in twenty-

twenty-ſix minutes. The two others had no perceptible complaint.

Theſe experiments on the guineapigs likewiſe bear witneſs to the inutility of the propoſed remedy.

I however made ſtill ſome others on rabbits, and can certify that the reſult of them was entirely conformable to that of the preceding ones. The detail of them here would be tireſome. The fact is, that they not only did not prove the *ſtone* uſeful againſt the bite of the viper, but gave the cleareſt evidence on the contrary, of its being totally inefficacious.

Let not particular caſes be urged againſt me, either of animals recovered, or of men who have been bit and have not died, after the application of the *cobras ſtone*. Experiments on men prove nothing, ſince the viper's venom is not uſually mortal to them, any more than it generally is to large animals. To determine if this *ſtone* is uſeful or not, experiments muſt be oppoſed to others made on animals on whom no remedy has been tried, and it is neceſſary to make a great number of them. For example, let one hundred animals, ſuch as pigeons, ſmall rabbits, and guineapigs, be collected, and let them be bit by as many vipers, an equal number of times in the ſame parts. Let half of theſe animals be treated with *cobras ſtones*, or other boaſted remedies, and let thoſe which remain be left to themſelves. Let the number of thoſe that die on each ſide be remarked, and if there is a very ſenſible difference in favour of the remedy applied, I ſhall ſay that that remedy is probably uſeful. If the ſame experiment is repeated two or three times on the ſame number of animals, and the conſequences are always the ſame as in the firſt caſe, I ſhall then ſay that the utility of the remedy is a truth demonſtrated by experience, but it will not, on that account, be a ſpecifick, a certain remedy. To be ſo, it will be neceſſary that none of the animals, or at leaſt a very few of them, die. But after the many experiments I have made, I look upon this ſpecifick as impoſſible, or at leaſt I do not think it will ever be diſcovered. This is not a conſoling idea, but it ſeems juſt. I do not wiſh to diſcourage any one, or to keep others from making new reſearches; but too ſanguine a hope frequently cauſes that time to be loſt, which might have been uſefully employed.

I hope that certain perſons will not be ſo eaſily diſpoſed to believe in prodigies, and to trumpet forth dreams as very important diſcoveries; and that certain other perſons will miſtruſt their own ſtrength a little, and even ſometimes their own experiments: for it is eaſier to believe than to judge, and it is likewiſe eaſier to ſee imperfectly than clearly.

Experiments according to the method propoſed by Kempfer.

I ſhall finiſh my experiments on the venom of the viper, by a detail of what I have obſerved in making

making trial of Kempfer's method againſt the bite of that animal, that is to ſay, in employing ſcarifications and the theriaca. I relate it the more willingly, ſince in trying Kempfer's method, I thought it proper to make ſome experiments which ought to be rendered publick.

Kempfer propoſes theriaca, ſcarifications, and ligatures, as a certain remedy againſt the venom of the viper and other ſnakes. In the courſe of my experiments I have already proved the inutility of theriaca applied to the part bitten, or taken internally; and have obſerved that ſcarifications and ligatures, inſtead of being uſeful, are conſiderably injurious. It is true that I never have united theſe remedies; it would, however, appear to me very ſingular, could they be only uſeful when joined together. It is the more neceſſary to have recourſe to experiment, ſince Kempfer, a very grave authour, aſſures us that he has found his remedy conſtantly efficacious, and has cured all the perſons to whom he could apply it in time.

I had a guineapig bit once by a viper, either in the leg or foot. Having applied a ligature, I made ſlight ſcarifications in the part, ſqueezed out the blood, and covered the whole with theriaca, obliging the animal to ſwallow the ſame, mixed with water. The guineapig lived, but a part of the foot gangrened, and it was never afterwards of any uſe.

I had another guineapig bit twice in the foot by a viper. Having made the ligature, I ſcarified the part ſlightly, preſſed the blood from it, and co-

vered

vered all with theriaca, with which I rubbed the foot well. The animal ſwallowed a great deal of theriaca mixed with water; it loſt the uſe of its foot, but recovered.

By way of making comparative experiments, I prepared two guineapigs as above, but only made the ligature and ſcarifications, neither applying the theriaca, nor giving it internally. One died in five hours; the other lived, but like the former ones, loſt its foot.

The conſequences of theſe experiments are neither uniform nor in ſufficient number, to decide as to the inutility of Kempfer's method. I thought it neceſſary to make freſh experiments, and to vary them a little, operating likewiſe on different animals.

I had the leg of a guineapig bit twice by a viper. It was tied and ſcarified, the blood ſqueezed from it, then well covered with theriaca, which, when diſſolved, I made the animal drink repeatedly. It died in two hours.

Another guineapig, ſomewhat ſmaller, was treated in the ſame way, and died in four hours.

I had another guineapig bit as uſual, and did no more than ſcarify and apply the ligature: it died in four hours and an half.

I had another, a much larger one, bit, and did not treat it all. It died in three hours.

I had four others bit by as many vipers, each twice in the foot, and treated all of them according to Kempfer's method. Two died in leſs than four

four hours; the other two came off each with the loſs of a foot.

Having had ſix other guineapigs bit as above, I treated three as uſual, but not the other three. Two of thoſe treated died, and the third recovered without loſing its foot. As to the others, one of them died, another was very ill, and the third recovered, but loſt its foot.

With ſome ones I afterwards had bit, I tried ſcarifications and ligatures, and covered the wounds with theriaca, without making them ſwallow any; others, on the contrary, ſwallowed the theriaca, without having ſcarifications made, or ligatures or theriaca applied to the part bitten. The conſequences appeared to me to indicate the inefficacy of the theriaca applied to the part, and to admit concluſions that the ſcarifications and ligatures do a great deal more harm than good, becauſe in general they diſpoſe the parts to gangrene the readier. I could not determine that the theriaca taken internally was uſeleſs, ſince the events, though neither ſufficiently conſtant nor numerous, were more favourable to it than not: but to be better aſſured of this, a greater number of experiments are neceſſary, than that I was enabled to make; and although the utility of it ſhould be ſhown, I believe that many other ſubſtances capable of accelerating the motion of the blood, would be equally uſeful.

I made many other experiments on pigeons and ſmall rabbits, conforming myſelf to Kempfer's method, but did not find them more favourable to his

his ſyſtem, than thoſe cited above; ſo that I do not heſitate to declare that this method is neither certain nor uſeful, and that on the contrary it muſt appear in the higheſt degree dangerous, eſpecially to large animals.

But whatever may be the inefficacy of the remedy propoſed by Kempfer, I however found it ſingular that ſeveral of the pigeons recovered, although the diſeaſe of the venom declared itſelf with the moſt violent ſymptoms. This appeared to me ſo ſtrange, that I determined to repeat ſeveral experiments, and to examine afreſh whether different ſubſtances I had tried before, and found totally uſeleſs againſt this poiſon, were really ſo.

Subſtances employed againſt the Bite of the Viper; to wit, quicklime, magneſia, cauſtick alkali, abſorbent earths, and calcined hartſhorn.

I had a pigeon bit in the leg by a viper, and having made two ſlight ſcarifications, I covered the part with quicklime, which I kept on with a looſe bandage. The pigeon had the diſeaſe of the venom; the leg ſwelled and blackened, and a ſore formed itſelf: but in ſix days all came about.

Having had another pigeon bit as above in the foot, and having ſcarified, I applied quicklime; it died in twenty minutes.

I repeated the ſame experiment on two other pigeons; they were both very ill, but neither died: in ſeven days they were perfectly recovered.

I was deſirous of repeating the ſame experiment on ſix other pigeons; two only died, although all the ſix had the diſeaſe: however one of them loſt its foot by a gangrene.

I took two of theſe recovered pigeons, and had them bit each ſeveral times in the leg, by two vipers. Having made the accuſtomed ſcarifications, I applied the quicklime; one died in twenty-ſeven minutes, the other in ſix hours.

Of ſix other pigeons bit and treated as above, with ſcarifications and quicklime, two died, the other four all recovered in nine days. Two of them had the muſcles of their legs ſo gangrened, that they never could uſe them afterwards.

I repeated the ſame experiments on ſmall guinea-pigs and rabbits, and the conſequences of them were far leſs favourable to the uſe of the quicklime, than in the caſes of the pigeons. I however in my firſt attempts fancied it not to be totally uſeleſs; but however it be with quadrupeds, it is certain that I deemed it of uſe to pigeons, all of which uſually die when the diſeaſe of the venom communicates itſelf to the part bitten: ſuch is at leaſt the reſult of experiments made at Paris. The pigeons' legs when bit, became ſwelled and livid, with ſymptoms of gangrene; and ſeveral of the abdominal muſcles, as well as all thoſe ſurrounding the wound, were black.

It is besides true that I have had consequences very analogous to those of the lime, on applying absorbent earths, such as the different boles, but above all, English pipe-clay, to the parts bitten. Many of the pigeons recovered, although the greatest number died, with all the symptoms of the disease of the venom.

However I very much suspect the utility of these remedies, and the cures they have effected, because I have met with several recoveries without applying any remedy at all. I have had pigeons repeatedly bit, and the venom was well communicated to the parts, since several of them lost their feet by the gangrene which supervened; the inflammation and stoppage of circulation were extended to a great part of the body, and the animals recovered, although not perfectly till at the end of eighteen or twenty days. I generally observed at Paris, that the smallest quantity of venom was sufficient to kill a pigeon, since it had symptoms of the disease, so that I am now persuaded that there may very easily be a difference betwixt the venom of one viper, and that of another; betwixt the venom of vipers of one country, and of those of another; and betwixt the venom of the same vipers at different seasons. In this way may be conceived why great scorpions are mortal in summer, and not in winter; and why the pigeons bit by a single viper, and treated with oil before several of the Members of the Royal Society at London, recovered. It must, however, be observed, that it is not impossible but that

that the poiſon introduced into the animal was not ſufficient in quantity to produce a dangerous complaint. We have ſeen ſeveral ſimilar caſes in the courſe of the preceding experiments.

TREATISE
ON THE
AMERICAN POISON
CALLED
TICUNAS*,
AND ON SOME OTHER
VEGETABLE POISONS.

THE experiments I made at Paris four years ago on the venom of the viper, and which are a continuation of many others I had publiſhed ten years ago in Italy on the ſame ſubject, have enabled me to ſpeak with certainty on the nature and properties of this poiſon. The unexpected and important effects which I have obſerved in applying

* This poiſon is thus called from the name of the Indians amongſt whom it is prepared. Mem. de l'Acad. des Sciences. Ann. 1745, p. 490.

the

the venom of this animal to living bodies, have prefented me new truths as to the animal economy; and thefe new truths have by degrees conducted me to a doubt of certain medical theories which are not fufficiently proved, or which have become too general amongft practitioners.

I have fince wifhed to extend my refearches to other venomous fubftances, and was defirous to examine, if poffible, one of the moft active vegetable poifons. I figured to myfelf that animal poifons, as for example, that of the viper, applied to a wound, do in truth diffufe themfelves in the body of the animal, but do not on that account become augmented, as does on the contrary the *variolous* venom *(a)*, or that of a mad animal. I figured to myfelf, I fay, that thefe poifons had a great analogy betwixt them, and that they acted in the fame way, and on the fame parts of the animal: but on the other hand, I could conjecture nothing as to the action of vegetable poifons, which I had not yet examined, and it did not feem to me poffible to eftablifh any certain principles on them, even after reading the principal fymptoms they excite. The mode of making experiments, which had been followed, was very different from that I had employed in examining the venom of the viper, and the deductions drawn from them feemed to be too vague and uncertain.

(a) The matter of the fmall-pox.

On my arrival in London, I was enabled to ſatisfy myſelf with eaſe on the ſubject. Mr. Heberden, a celebrated phyſician there, and member of the Royal Society, procured me a great number of American arrows well preſerved, and well covered with the poiſon; he had beſides the complaiſance to get me a good quantity of the poiſon itſelf. I found it encloſed and ſealed in an earthen jar, the tin caſe of which ſtill remained on it. Within the caſe was found a paper, on which was written as follows: *Indian poiſon brought from the banks of the Amazons by Don Pedro Maldonado: it is one of the ſorts mentioned in the Philoſophical Tranſactions*, vol. 47, No. 12.

In this volume of the Philoſophical Tranſactions mention is made of two poiſons of pretty much the ſame activity; one called *Lama*, the other *Ticunas*.

The poiſon contained in the earthen jar, which I made uſe of, is the *Ticunas*. It is not well known with which the poiſoned arrows were prepared; but I found by experiment, that it had the ſame ſtrength as the *Ticunas*, ſo that I thought it needleſs to diſtinguiſh one from the other.

Many things have been written on the activity of the American poiſons, ſo that I thought it adviſeable to begin my experiments gradually, taking all poſſible precautions. It is thought that the ſmell alone on opening the veſſel containing it is hurtful, and grievous diſeaſes, and even death, are apprehended from the eſcape of ſome of the particles of it it into the air; this at leaſt is what we read of in the graveſt authours.

I began then, as ſoon as the jar of poiſon was opened, to make a young pigeon inhale the impregnated air, and kept it with its head in the jar for ſeveral minutes; when I drew it away, 'twas as well as before. I looſened with a chiſel ſeveral bits of poiſon, to raiſe a little duſt in the jar, and again plunged into it the pigeon's head; it did not ſuffer in this ſecond experiment, any more than in the firſt.

From this moment I did not heſitate to expoſe myſelf to this vapour, and to examine the ſmell of it, which ſeemed to me nauſeous and diſagreeable. Several very fine particles of it entered with the air into my mouth, and I found them to taſte pretty much like liquorice. Thus then is the ſmell of the dried poiſon innocent, and ſo likewiſe are the particles which find their way with the air into the mouth or noſe, and reach the lungs.

But it appears that the circumſtance in which they dread this poiſon the moſt, although it is ſtill external, is when it is reduced to vapours by live coals, or when it is boiled a long time, and riſes in a thick ſmoke. I caſt ſeveral ſmall pieces of the dried poiſon on burning coals, and made the pigeon inhale the ſmoke of them, in the midſt of which I kept its head: it did not betray the ſmalleſt ſymptom of uneaſineſs. I did more; I conveyed this ſmoke into a glaſs tube ſix inches in height and four in diameter. When filled, I introduced the pigeon, and it did not feem to fuffer more than if I had expofed it to the vapour of burnt fugar. I then fat about boiling a good quantity of it in an earthen veffel, and expofed the pigeon to the vapour that rofe from it. I did this when the poifon began to form a confiftence; I expofed it when it was become more folid, begining to burn at the fides of the veffel, and to reduce itfelf entirely into a very thick vapour, and into coal. The animal did not fuffer in any of thefe trials, and I made no longer a difficulty of fmelling to it, and of expofing myfelf to its vapour. The fmell of the dried poifon on the live coals is very difgufting, and refembles that of burnt excrements.

From all thefe experiments, I infer that the vapours which rife from the fmoke of the American poifon are innocent, whether fmelled to, or inhaled; and Monfieur de la Condamine had certainly been impofed upon when he wrote that this poifon is prepared by women condemned to death, and that they determine its having attained its point of perfection, when the vapours it emits, during the boiling, kill the perfon who attends it.

Not one of the well informed travellers who have vifited the American continent, fpeaks of this tale they propagate of the accidents which happen to the old women deftined to prepare the *Ticunas*. Monfieur de la Condamine himfelf only fpeaks of it after the doubtful relation of fome native of that country; and on the like authorities he believes that falt and fugar are fpecificks againft this poifon. My experiperiments have however taught me that they are of no ufe, and that it would be in vain to be flattered with the hope of obtaining a cure by thefe remedies, fhould any one have the misfortune to be really poifoned by the *Ticunas*.

There is no fufpicion of the poifon I employed in my experiments having fuffered or loft its activity through age, fo that we cannot afcribe to thefe caufes, that the vapours which exhaled from it were not deftructive, even to the moft delicate animals. It had very well preferved its effential property of killing very ftrong ones, in a very fhort time, and in a very fmall dofe; and I was always unfuccefsful in my endeavours to oppofe *fugar* or *falt* to it, which are however Monfieur de la Condamine's two fpecificks, who has likewife in this adopted the opinion of the American natives.

This poifon diffolves readily and perfectly in water, even cold; as alfo in the mineral and vegetable acids. It diffolves much flower in oil of vitriol than in the other acids, and becomes black as ink, which does not happen in the other cafes.

It neither effervefces with acids or alkalies, nor caufes any kind of change in milk.

It neither turns the juice of radifhes red nor green; and when examined with a microfcope, difcovers nothing regular or faline, but feems chiefly compofed of very fmall irregular fpheroidal corpufcles, like thofe of the juices of vegetables. It dries without cracking, differing in that from the venom of the viper, and when put on the tongue has a very bitter tafte.

From all this I conclude that it is neither acid nor alkaline, and that it is not compofed of falts vifible to the microfcope.

The order I meant to purfue in my experiments, rather than curiofity, engaged me to examine whether this poifon would be mortal, if put in immediate contact with the eyes, or if it would bring on difeafe or irritation. I had already found that the venom of the viper is altogether innocent when in any way applied to the eyes, as it alfo is to the mouth and ftomach; and was curious to fee what agreements there might be betwixt two poifons fo active, but fo different in their origin. I began then by putting a fmall quantity of the *Ticunas* diffolved in water, on the eye of a guineapig: the animal neither feemed to fuffer at the moment, nor afterwards, neither was the eye at all enflamed. I repeated this experiment two hours after on both eyes of the fame animal, applying a greater quantity of the poifon: its eyes retained their natural ftate, and it did not fuffer the fmalleft inconvenience. I repeated this experiment on the eyes of two other guineapigs, with the fame fuccefs; and fuch was likewife the refult of all the experiments I afterwards made on the eyes of feveral other animals, and particularly on thofe of rabbits: I never could obferve the fmalleft change in the part, to which I found that the poifon was no more offenfive, than if I had bathed it with

water:

water: whence I think I may conclude that the American poiſon is not a poiſon when applied to the eyes, and that it has no action on theſe parts.

But will it be as innocent when received into the ſtomach?

Monſieur de la Condamine, and all the other authors who have ſpoken of this poiſon, believe it altogether innocent, taken internally, and ſuch is the opinion of the Americans. The reaſon it is thought ſo, is that the animals killed with this poiſon, or rather with the poiſoned arrows, can be eaten without danger. Such an argument is more ſpecious than convincing, becauſe this ſubſtance may be a poiſon when introduced into the blood, even in the ſmalleſt quantity, and may not be ſo when received into the mouth in a much greater quantity.

There is a relation in the Britiſh Journal, dijeſted by Mr. Cleaby, (Vol. 13. p. 85) that a ſmall bird which had been made to ſwallow this poiſon, died inſtantly. But this obſervation, iſolated, and deprived of its neceſſary particulars, did not influence the minds of the authors who have treated of this poiſon, which continued to be notwithſtanding regarded as entirely innocent when taken internally.

Here follow the experiments I have made on this ſubject: they ſtill ſerve to make us cautious how we pronounce, even after having had recourſe to experiment.

I made a ſmall rabbit ſwallow two grains of poiſon diſſolved in water, and forced it to drink a teaſpoonful of water to waſh its mouth, and carry down all the poiſon into the ſtomach. This animal did not appear to ſuffer, neither at the time or afterwards.

I made another ſmall rabbit drink, as above, three grains of the poiſon; it did not ſuffer any more than the former.

Four grains fo the poiſon ſwallowed by another ſmall rabbit, were attended with no ill conſequences. I made the ſame trial on three ſmall rabbits, to the third of which I gave ſix grains of the poiſon, and it continued well like all the others.

I thought theſe experiments might be ſufficient to aſſure me that the American poiſon is innocent taken internally, as is the venom of the viper; but I was miſtaken. I had the curioſity to try it on a young pigeon, which I made ſwallow ſix grains of it, and it died in leſs than twenty-five minutes. I repeated this experiment on two other pigeons, both of which died in thirty.

Theſe laſt experiments, which ſeem to contradict the preceding ones, obliged me to make a great many new ones on rabbits and guineapigs. I made a ſmall guineapig ſwallow five grains of the poiſon, and found it dead in twenty five minutes.——I made a ſmall rabbit ſwallow even to eight grains of the poiſon; in thirty minutes it had no apparent complaint; in thirty more it began to totter on its feet; in four minutes more it fell inſenſible; and in four others was quite dead.——I made two rabbits and two guineapigs ſwallow each about ten grains of the poiſon: one of the rabbits died in leſs than forty-

five

ſive minutes, and the two guineapigs in twenty minutes.

Theſe conſequences led me to imagine that a greater doſe of the poiſon would cauſe a more certain death, and that the ſame quantity of it would produce different effects on the ſame animals, according to the ſtate of the ſtomach. I had generally obſerved in making the above experiments, that animals, when they ſwallowed this poiſon on a full ſtomach, either were much longer in dying, or had no complaint. I made trial of this on three rabbits and two pigeons, which I kept a long time without nouriſhment: they all died in leſs than thirty-five minutes with only three grains of the poiſon. I repeated this experiment on five more of theſe animals, with their ſtomachs filled, and only one of them died.

I deduce from this as an eſtabliſhed fact, that the American poiſon taken internally, is a poiſon; but that a conſiderable quantity of it is required to kill even a ſmall animal.

The particulars related above of its being innocent in a ſmall doſe, and mortal in a greater one, induced me to believe that the viper's venom, which is innocent when taken internally in a ſmall quantity, would be mortal if the doſe of it were increaſed. The numbed ſenſation it cauſes on the tongue, and which continues ſo long, ſeems a ſufficient proof that this venom is not altogether innocent in this way, and that taken in a greater quantity it may readily occaſion death.

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I reſerve to ſome other occaſion the making this experiment, and ſhall then employ the venom of eighteen or twenty vipers, to be ſwallowed by a ſmall animal on an empty ſtomach, and I venture to pronounce beforehand that it will probably die; ſince, if ſo very ſmall a quantity of venom deprives the tongue of motion and feeling, that is to ſay, deſtroys the principles of animal life in this organ, a much greater quantity ought to deſtroy them in the organs more eſſential to life.

If we conſider that the venom taken into the mouth muſt extend itſelf over a very great and almoſt moiſt ſurface, and mix itſelf with the aliments in the ſtomach, and that the inhalent veſſels are very ſmall, it will appear no longer ſurpriſing that it is not hurtful when taken in a ſmall quantity, which is what we obſerve in the American poiſon.

I began my experiments on the latter, and employed a lancet, covered with a diſſolution of it in water. With this inſtrument I wounded a ſmall guineapig three times in the leg, at certain intervals; the lancet was well ſupplied with poiſon, but the animal did not ſuffer at all. I made the ſame trial on three other ſmall guineapigs, and on a rabbit, and neither of them died or was diſordered by it. In all theſe caſes the blood iſſued freely from the wounds, whence I ſuſpected that the poiſon, inſtead of communicating itſelf might have been forced out, as I had obſerved of the viper's venom, which was often prevented in that way from doing harm.

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My ſuſpicion was ſoon confirmed by the experiments that followed. I impregnated a ſingle thread with the poiſon, and paſſed it through the ſkin of a guineapig, near one of its teats: it had no complaint. I impregnated another thread three times doubled, and letting it firſt dry a little, as I feared that the poiſon was prevented from lying on the ſkin by the threads paſſing acroſs, I paſſed it through the ſkin of a ſmall rabbit's thigh, near its belly. In ſix minutes it appeared feeble, and began to tremble, and fell motionleſs a minute after; from time to time it was ſeized with ſlight convulſions, and in ſix minutes more was dead.

I repeated the ſame experiment with the doubled thread, on two other rabbits and three guineapigs; in ſix or ſeven minutes they became feeble, fell down, were ſeized with convulſions, and died in the ſpace of thirty.

I wiſhed to ſee if the American poiſon could be communicated to animals and be mortal to them, being ſimply applied to the ſkin ſcraped, or juſt entered with the point of a lancet. I had obſerved at Paris, that the venom of the viper did actually produce a local malady in theſe caſes, and that it changed and corroded the ſkin, but did not go ſo far as to kill. The American poiſon, on the contrary, never produces the local malady, as I obſerved in making the experiments related above, and it leaves the wounded parts in their natural ſtate, which eſſentially diſtinguiſhes it from the viper's venom.

I cut off the hair with ſciſſars from the ſkin of

a ſmall

a ſmall guineapig's thigh, and ſcraped it ſlightly with a file. It did not bleed, but was moiſt, and covered with ſmall red ſpots. I wet it with a drop of a ſolution of poiſon in water, and in ten minutes the creature was convulſed, and a ſhort time after fell down motionleſs, being from time to time more or leſs convulſed: it died in twenty minutes, the ſkin to which the poiſon was applied not being changed. This experiment made on two other guineapigs, and a ſmall rabbit, ended the ſame way; they all died in leſs than twenty-ſeven minutes with very violent convulſions.

I wiſhed to ſee if the larger animals could reſiſt this poiſon, when ſimply applied to the punctured or ſcraped ſkin. I ſhaved the ſkin of a large rabbit, and punctured it ſlightly in ſeveral places with the point of a lancet, moiſtening it with ſeveral drops of the poiſon. In fifteen minutes the creature drooped, bowing its head at intervals, and being ſcarce able to ſupport itſelf; however in leſs than twenty minutes more it became as lively as ever. I repeated this experiment on another, but ſmaller rabbit; in ten minutes it began to move to and fro' its head, and could ſcarcely walk or ſupport itſelf on its feet; but twenty minutes after it recovered its uſual ſpirits.

I ſhaved about an inch of the ſkin of a very large rabbit, a little blood oozing from it. Upon this ſkin I put about three drops of a ſolution of the poiſon. In ſix minutes the rabbit was very feeble and much diſordered, and a minute after fell as if dead,

dead, being ſcarcely obſerved to breathe. From time to time it was convulſed, but in leſs than forty-ſix minutes was able to walk very well; ſoon afterwards it began to eat, and appeared perfectly recovered.

I ſcraped the ſkin of a fowl's thigh, and applied the poiſon to it: no illneſs enſued, although I twice repeated the experiment on other parts of the ſkin.

I ſlightly ſcarified the ſkin of a pigeon's thigh, and applied to it the poiſon diſſolved in water. In twenty-five minutes the pigeon became ſo weak as not to ſupport itſelf, and at intervals was convulſed. Soon after it fell inſenſible, and remained in this ſtate more than three hours. However it afterwards recovered gradually, ſo that in half an hour more it did not ſeem to have at all ſuffered.

This experiment was repeated on five other pigeons: three died in leſs than twenty minutes; the other two fell into convulſions, but afterwards recovered.

From experiments ſince made as well on birds as quadrupeds, I conclude that the American poiſon may be mortal, when applied to the ſkin ſlightly ſcratched; though not always, nor in all circumſtances. The largeſt animals eaſieſt reſiſt the action of this poiſon, and even the weakeſt, when they do not die, ſoon become as well as before.

I wiſhed to know what quantity of poiſon would kill an animal. I had made a like ſearch in France in regard to the viper's venom, and had determined the quantity. I had great reaſon to preſume that a

very

very ſmall portion of the American poiſon would be fatal to a ſmall animal, ſince a drop or two of its ſolution, applied to the ſcraped ſkin, was capable of killing more than one; but I wanted the preciſe quantity.

I moiſtened a very ſmall bit of cotton with the fiftieth part of a drop of the ſolution of the poiſon, containing ſuch a proportion of water, that the poiſon ſcarcely compoſed a fiftieth part. I introduced this into one of the muſcles of a pigeon's leg; the pigeon felt nothing from it.

Two hours after I introduced into another muſcle an atom of the dried poiſon ſcarcely viſible to the naked eye; here again the pigeon did not ſuffer. I repeated this laſt experiment on three other pigeons, one of the bits of the dry poiſon being pretty large; neither of them died, or appeared to ſuffer: however I found the bits of poiſon whole and undiſſolved by the humours of the wounded part.

I applied to the muſcle of another pigeon a bit of cotton much larger than that above, and moiſtened it with about eight times the quantity of poiſon: in ſix minutes the pigeon fell, and died ſoon after. I applied bits of cotton moiſtened with the poiſon, pretty much in the manner of the foregoing, to two guineapigs; one died in twelve minutes; the other fell inſenſible in ſix, but ſoon after came to itſelf.

I conclude from theſe experiments, that it requires about the hundredth part of a grain of poiſon to kill a ſmall animal, and that the poiſon muſt neceſſarily diſſolve, to occaſion either death, or ſome ſenſible derangement of the animal economy.

I made ſeveral experiments to determine whether the American poiſon would be mortal or dangerous, when applied to wounds in the combs of fowls, or to the ſcratched ears of quadrupeds. The venom of the viper is not uſually mortal in theſe parts, but the diſeaſe, which does not attack the envenomed comb, ſeizes the gills, which ſwell ſo violently as frequently to kill the animal.

I began by repeatedly wounding the combs of fowls, and applying to them the ſolution of the American poiſon; I did this twice by the medium of cotton wetted with it, without producing any complaint. But my experiments on the ears ſucceeded otherwiſe: after ſeveral fruitleſs attempts to communicate the poiſon by ſcraping or wounding the ears of rabbits, which did not appear to ſuffer, I at length ſucceeded in killing two in leſs than thirty minutes after my having applied a great deal of poiſon to the moſt fleſhy part of the ear, which I had wounded in ſeveral places with the point of a lancet.

The experiments on the ears convinced me, that when there are but few blood-veſſels, the complaint either does not communicate itſelf, or is not mortal. The American poiſon is very analogous in this to the venom of the viper: they are both entirely innocent, in whatever way they are applied to the tendons, above all if the latter are free from blood-veſſels; and likewiſe to the other parts of the body that

that are ſo, ſuch as the cellular membrane, or the ligaments: it would be needleſs to enter into a detail of theſe experiments, which would not only be tedious, but, as will be ſeen by what follows, unneceſſary.

I wiſhed to know whether the American poiſon was more deſtructive when inſinuated into the muſcles, than when applied to the ſkin with inciſions made quite through it. A large guineapig which two days before had twice undergone the operation of poiſon applied to the inciſed ſkin, without any ſubſequent complaint, and a third time with very trifling ſymptoms, died in leſs than twelve minutes after the application of the poiſon to the divided fibres of one of the muſcles of its leg. In three minutes it fell motionleſs, and with ſcarce any ſign of life.

I repeated this experiment ten times, and all the animals, as well guineapigs as pigeons, and moderate ſized rabbits, died; ſo that I have no doubt but that the poiſoned wounds of the muſcles are more mortal than thoſe of the ſkin and ears, and of the combs of fowls. The way to ſucceed the beſt is to take a pointed piece of wood of a ſpungy ſubſtance, well ſteeped in the poiſon, and to force it, when almoſt dry, into the ſubſtance of the bared muſcle; but this method did not ſucceed on my making trial of it on the combs of fowls; I could obſerve no complaint, although the wood was well ſteeped, and left for ſeveral hours in the combs, pierced through and through.

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I now had recourfe to the arrows, employing feveral in piercing the fkins of animals, and many others in wounding the mufcles. In the former cafes feveral of the animals furvived, the large rabbits making the ftrongeft refiftence; but in the latter not one efcaped death.

I found in general, that the arrows are more deadly in their effects than the poifon diffolved in water, and fimply applied to the part wounded.

I obferved that the poifon of the arrows is more active and certain, if they are previoufly dipped in hot water. Their activity is ftill increafed, if they are fteeped in a folution of the poifon boiled to a firopy confiftence. Several pretty large animals, fuch as rabbits, tumbled motionlefs in lefs than two minutes, and before eight, were dead. Some of the fmaller ones became ill in lefs than a minute.

I paffed one of thefe arrows well dipped in the boiled poifon, into the comb of a fowl, and left it there a whole day, without the animal's betraying any fymptoms of pain. The day following I pierced the comb and gills of the fame fowl through and through with two frefh arrows prepared as above, and left them there ten hours. The fowl ftill continued well in the fecond operation, and I afterwards paffed an arrow into one of the mufcles of its leg, when in forty-two minutes it died.

Are Acids and Alkalies capable of destroying the deadly quality of the Ticunas?

Amongst the researches I proposed to myself in examining this poison, that of the changes it might undergo when united with acids or alkalies was one, as it had also been in examining the venom of the viper. I had found that neither the strongest mineral acids, nor the most active alkalies, deprived this venom of its hurtful qualities, and wished to see if they would act as little on the American poison. For this purpose I dissolved it in the three mineral acids, as also in distilled vinegar, and rum, and in a few hours made the following experiments.

I made slight incisions into the skin of a small guineapig, and wet it several times with the solution of the poison in nitrous acid. What the animal suffered seemed to result from the wounds and acid alone, and in an hour it became as lively as usual. Two hours after, I repeated this experiment on another part of the skin prepared in the same way, employing a solution of the poison in rum; in less than four minutes the animal died.

I wounded the skin of a small rabbit slightly, and applied to it several drops of a solution of the poison in oil of vitriol: the rabbit felt no ill effects from it. In four minutes I wounded another part of the skin as before, and applied to it several drops of a solution

tion of the poiſon in diſtilled vinegar: in four minutes more the animal fell, and died in ſix.

I prepared as uſual the ſkin of a ſmall rabbit, and wet it with a ſolution of the poiſon in marine acid: the animal did not ſuffer from it. Six hours after, I applied to another part of its ſkin a ſolution of poiſon in rum, and in forty-five minutes it fell into convulſions, but recovered in leſs than an hour.

From theſe firſt experiments it ſeems probable, that mineral acids render this poiſon quite innocent, and that vinegar and rum on the contrary, do not alter it. I continued my experiments on the ſolutions of it in vinegar and in rum, and the conſequences were a little varied. Of ſix animals treated with the ſolution in vinegar, only two died; two others had all the ſymptoms of the diſeaſe cauſed by the poiſon; and the other two were not affected by it. Of ſix others treated with the ſolution in rum, five died, and the ſixth had an attack of the diſeaſe; this ſeems to demonſtrate that the *ticunas* diſſolved in theſe two fluids, preſerves its deadly qualities.

On the contrary, I repeated the experiments on the ſolution of the poiſon in mineral acids, on ſix animals, neither of which died, nor diſcovered ſymptoms of complaint.

I ſuſpected that the poiſon might probably be innocent in this caſe, not becauſe it had loſt its deleterious qualities, but rather becauſe the too great action of the mineral acids on the ſkin, and on the veſſels which are cauterized and hardened by them, might prevent its inſinuating itſelf into the parts wounded. To clear up this doubt, I evaporated over the fire the ſolution in the mineral acids, and when the poiſon became dry, I applied it to different parts of the ſkin in ſeveral animals, but without any one being diſeaſed by it.

It appears then, that the mineral acids deprive the American poiſon of its hurtful qualities. I ſay ſimply that it appears ſo, becauſe it ſtill may be ſuſpected, that a little of the acid may remain in the poiſon after the evaporation, and that this acid may produce its uſual effect on the veſſels of the ſkin. I ought to have repeated theſe experiments after having waſhed the poiſon in ſeveral waters, but at that time I wanted animals to examine into the truth of this freſh ſuſpicion, and have never ſince been able to return to the ſubject.

As to alkaline ſalts, I can venture to ſay, that I have not found them in any way to change this poiſon, or to render it leſs deadly than before. It is true that I neither repeated nor varied theſe exriments ſo much as neceſſary. I ſhould have done this, if I had not found great difficulty in procuring animals, and had not had an eye to much more important experiments.

It was natural to ſuppoſe, ſince acids prevented the action of the poiſon on animals, that they might alſo be a remedy againſt it.

I got ready the ſkin of a ſmall guineapig in the uſual way, and covered it well with poiſon; in about

about forty feconds I wafhed it with the nitrous acid, and afterwards with pure water: the animal had no complaint. Two hours after I laid fome of the poifon on one of its mufcles, and immediately applied the nitrous acid: it inftantly fell convulfed and without ftrength, and in two minutes died.

I repeated this experiment on the mufcles of another guineapig, and had fcarcely applied the poifon, when I wafhed the parts with nitrous acid a little diluted with water. Two minutes after it became convulfed, and died in four.

I poifoned the mufcles of four pigeons as above, and wafhed them immediately after with nitrous acid. In a minute all the pigeons expired. Fearing this might be the effect of the nitrous acid, rather than of the poifon, I tried nitrous acid very much weakened on four other pigeons, all of which died, though much later.

I wifhed to fee if the fimple application of nitrous acid to the mufcles, would kill pigeons and fmall guineapigs. I made the experiment on two of each fpecies. Both of the pigeons died foon after, but the guineapigs furvived, although one of them fuffered a good deal.

It feems then that acids are ufelefs and dangerous, when applied to the poifoned mufcles of an animal.

How long is the TICUNAS *in discovering its deadly effects in Animals that have been poisoned with it?*

I ſhall not ſpeak of other remedies I have tried, becauſe experience has convinced me that all are uſeleſs, whether applied ſoon or late, exteriorly or interiourly. When the poiſon is deeply fixed, when it is already introduced into the humours, every remedy comes too late.

A very nice enquiry, and one that might be uſeful in certain caſes, remained to be made. My experiments on the viper's venom were the occaſion of this enquiry into the American poiſon. I had determined the time the former requires to diffuſe itſelf in the body of an animal, and in what time the cutting off the envenomed part may be uſeful, to prevent the venom from communicating itſelf by the circulation, to the animal.

I introduced an American arrow, previouſly dipped in hot water, into the muſcles of a pigeon's leg, and left it there. In four minutes I made a pretty tight ligature about the part wounded, and juſt below the femur. In twenty-ſix hours the animal had no other complaint than that cauſed by the ſimple ligature, which on my taking out the arrow, I untied. The part was a little ſwelled and livid, and continued lame for ſeveral days, but did not occaſion the animal's death.

I pierced

I pierced the mufcles of another pigeon with a frefh arrow, as above, and in fix minutes made the ligature, leaving the arrow in the part. In four minutes the pigeon could not fupport itfelf, and fhortly after fell infenfible, dying at the end of fix more.

I repeated this experiment on another pigeon, leaving the arrow in the mufcles. In eight minutes I bound the leg. Three minutes after, the creature became ill, but in a fhort fpace recovered. In twenty-fix hours it was ftill alive, although the mufcles were become livid. I took off the ligature, and it died two hours after.

I fubmitted a fourth pigeon to the fame trial, and made the ligature five minutes after, leaving the arrow in the mufcles. It died in two hours.

I repeated this experiment on four other pigeons, and in two minutes made the ligature: not one of them died. Ten hours after I took off the ligature, when three died, and the fourth recovered perfectly.

I made the fame experiment in all its circumftances on four other pigeons, except that I did not take off the ligature till thirty hours were elapfed. One only of them died, in two days. Its death was certainly owing to the tightnefs of the ligature, which produced a gangrene of the mufcles.

I repeated this experiment on much fmaller pigeons, that can bear the lofs of the leg beneath the femur; not one of thofe died that had their legs cut off in the ſpace of two minutes, and only two out of ten, when they were taken off at the end of three.

Fewer pigeons die with this method than with the ligature, when the ſame time is obſerved: the reaſon is, that the amputation neither cauſes death, nor any remarkable derangement in the animals; inſtead of which the ligature frequently brings on a gangrene, which is ſometimes mortal, in the parts wounded by the arrow.

I made the ſame experiments on ſmall guinea-pigs and rabbits, ſometimes cutting off the wounded leg, ſometimes making the ligature: the conſequences were partly analogous to thoſe I noticed in the pigeons, though ſomewhat leſs conſtant and certain. I have in general obſerved that the American poiſon requires a certain time to communicate itſelf to the animal; that this time is much more conſiderable than that required by the venom of the viper; that the effects of the former on animals are more vague and uncertain; and, laſtly, that the effects of both may be remedied by amputating the parts, when it can be attempted without danger, and is done in time.

In the experiments I made on the venom of the viper, I found that its effects are not alike to all animals, and that there are animals with cold blood to which it is quite innocent. I was curious to ſee if it would be the ſame with the American poiſon.

The authours who have ſpoken of this laſt, tell us that it is poiſonous to all animals; but the be-

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lief of a thing is very far from the proof of it. Experiments, and thoſe very numerous, muſt be made, and I do not perceive that they have made a ſufficient number from which a general conſequence may be drawn.

Experiments on Animals with Cold Blood.

I began by inſinuating the poiſon into the muſcles of frogs, which died in a ſhort ſpace of time. I proceeded to eels, introducing the arrow near the tail; they all died though very late.

I had found that the venom of the viper is quite innocent to the viper itſelf, and to adders. I could procure only two of theſe laſt, and made but few experiments, which I however think deciſive. I wounded one of theſe ſnakes towards its tail, with an arrow well covered with the poiſon, which had the conſiſtence of a ſirop, and left the weapon in the muſcles. At the place where I inſinuated it, I had previouſly made an inciſion, to the end that the diſſolved poiſon on the arrow might the eaſier enter the muſcles, into which I afterwards made ſmall inciſions about the wound, and introduced freſh poiſon. The adder continued well for ſeveral hours, when I ſhut it in a chamber, and on entering ſix hours after, found it fled, and have never ſince met with it.

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I repeated this experiment ſeveral times, at certain intervals, on a ſomewhat ſmaller adder. The laſt time, I forced two poiſoned arrows into the muſcles of its tail, and left them there for twenty-four hours. I applied the poiſon brought to a ſiropy conſiſtence, repeatedly about the wounds, introducing a great deal of it into them with a bit of wood; the adder neither died nor ſuffered perceptibly.

I was enabled to repeat this experiment ſeveral times on vipers, not one of which died of the poiſon, although I wounded ſeveral in the muſcles near the tail with arrows well covered with it in the above conſiſtence, leaving them in the muſcles for twenty, and even thirty hours. It is true that ſome ones, a ſhort time after the application of the poiſon ſeemed leſs lively than uſual, and that the hinder part of the body which was wounded, became benumbed, and loſt its natural motion in a ſenſible degree, and that for ſeveral hours. On the others the poiſon had no perceptible effect.

After this I can venture to ſay, that the American poiſon, as well as the venom of the viper, &c. is totally innocent to animals with cold blood. Theſe two poiſons have here a very great analogy, although one is an animal gum, the other a vegetable juice.

It remained for me to examine the action of this poiſon on living animals, and to ſee what parts of the animal are ſo changed by it, as to bring on death.

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Every thing concurred to the belief that it excites one of thoſe diſeaſes which modern phyſicians call nervous, and that it acts immediately on the nervous ſyſtem. The ſymptoms of the diſeaſe are the moſt preciſe and deciſive in favour of this ſpecies of complaints. Convulſions, faintings, total loſs of ſtrength and motion, the feeling weakened ſo as to be almoſt totally deſtroyed, are the moſt common ones produced by this poiſon, on animals. We often obſerve that the creature, juſt now very lively, in a moment finds itſelf deprived of motion and feeling, and at the point of death. I uſually obſerved a ſymptom which effectually ſeems to demonſtrate, that the diſeaſe produced by the *ticunas* is purely nervous. The animal, if it ſurvived, was as well in a few minutes as before, not appearing to have ſuffered at all. It notwithſtanding remained in a lethargick ſtate, ſometimes for ſeveral hours, without any apparent ſign of life. This is exactly what happens in diſeaſes called nervous, the attack of which is frequently very ſudden. Now they excite commotions, and now entirely exhauſt the ſtrength; but ſcarcely do the ſymptoms of diſeaſe begin to diſſipate, than the perſon finds himſelf recovered, and ſcarcely recollects the having been ill.

But all theſe ſymptoms could not impoſe upon me, after having made my experiments on the venom of the viper. The diſeaſe occaſioned by it has likewiſe the ſymptoms common to nervous complaints, and the principal affection ſeems to lie

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in the nerves, although experience has decided to the contrary. It was likewiſe neceſſary then in the preſent caſe to have recourſe to experiment, and not to be carried away by falſe theories and probable reaſonings.

Effects of the TICUNAS *on Blood drawn from Animals.*

To proceed methodically in ſo important an enquiry, I thought it proper to begin by examining whether the American poiſon produced any ſenſible alteration in the blood of animals, if mixed with it on its iſſuing warm from the veſſels.

I cut off a pigeon's head, and received the warm blood in two ſmall conical glaſſes a little heated, about eighty drops in each glaſs. In one of them I put four drops of water, and in the other four drops of an aqueous ſolution of the *ticunas*, containing ſcarcely a grain of the dried poiſon. I ſhook each of the glaſſes for a few ſeconds, ſo as ſeverally to unite their contents; in two minutes the blood mixed with the ſimple water was coagulated; that with the poiſon did not coagulate, but became darker coloured, and in three hours was ſtill in a fluid ſtate, whilſt in the other glaſs the ſerum and coagulum were diſtinct.

I examined with a microſcope, as well at this time as afterwards, the blood in the two glaſſes, and found that the red globules were alike in each, having preſerved their primitive ſhape.

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This experiment was repeated ſeveral times with the ſame ſucceſs, ſo that the American poiſon, in the circumſtances related above, ſeems clearly not to change in a perceptible way the red globules of blood. It however deſerves to be noticed, that far from coagulating this fluid, it on the contrary abſolutely prevents the natural coagulation of it, when it is drawn from the veſſels. Neither again can it be ſaid to attenuate or diſſolve it, ſince nothing of this is remarked on examining it with a microſcope: the red part is ſhaped as uſual, and does not ſeem to be more ſubtil or more fluid.

The conſequences were the ſame on trying the viper's venom; ſo that the effects of theſe two poiſons, or the changes they cauſe, in the blood drawn from the veſſels, ſeem altogether ſimilar. Both prevent it from coagulating, and neither of them diſſolves or changes the globules of it: the only difference betwixt the two poiſons is that the venom of the viper turns the blood darker than does the *ticunas*.

The former does not act on the globules of blood, even when it is communicated to the living animal, and kills it. I made the ſame obſervation as to the blood of animals which died of the American poiſon, ſo that in all theſe caſes there is an admirable agreement betwixt the two. But it has been ſeen that the venom of the viper produces a ſenſible alteration on the general maſs of blood in animals bitten. I thought it proper to examine with the

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ſame attention, the blood of animals killed by the *ticunas*.

On doing this it appeared that the muſcles of animals ſo killed, were in general rendered pale by it. The venous veſſels near the heart ſeemed more ſwelled than uſual, the blood a little darker, but not coagulated. The abdominal viſcera were not ſenſibly changed; the heart and auricles in a natural ſtate: the external veſſels of the heart were ſometimes more viſible, and as if injected.

But I obſerved a great change in the lungs, a viſcus very eſſential to life. I generally found it more or leſs ſpotted; the ſpots were frequently very large and livid, and ſometimes the part ſeemed quite putrid. This change in ſo noble an organ deſerves the utmoſt attention; it ſeems to become conſiderable in proportion to the length of time the animal lives after being poiſoned. I found that the lungs of ſome animals were here and there tranſparent, above all towards the ſides. The air within was very viſible through the external membrane, which I examined with a microſcope, and very clearly obſerved through it the ſmall pulmonary veſicles moiſtened by a diſcharge from veſſels that had quite emptied themſelves.

However great the change in ſo important a part, I could not be perſuaded that it could alone produce ſo violent and momentary a diſeaſe, and that the poiſon entirely exerciſed itſelf on the blood, and the lungs. 'Tis true that I had the example of the viper's venom acting ſomewhat in this way; but it brings

brings on an almost general coagulation of the blood, which is certainly not the case with the American poison.

Effects of the TICUNAS *introduced into the Blood Vessels of Animals.*

In an enquiry so important, and at the same time so obscure, I thought it proper to recur to experiment, and to examine the effects of this poison introduced immediately into the blood.

I had recourse to the means I had employed in introducing into the jugular vein the venom of the viper, making use of a small glass syphon bent at the point, by way of syringe. Into this syphon I drew up by suction a solution of the *ticunas*, and having opened the vein, injected it. As I have already described the way of making these experiments, in the treatise on the venom of the viper, I do not think it necessary to repeat the description of them here. They are so contrived that the poison mixes with the blood through the medium of the jugular vein, without touching the part of the vessels where the incisions are made, or even the jugular itself.

I put into the syringe for the first experiment, four drops of an aqueous solution, which scarcely contained half a grain of the dried poison. Having introduced the point of the syringe into the jugular vein of a very large rabbit, I perceived the liquor to flow back on the moment of my puſhing the ſucker, owing to its not being exactly fitted to the ſides of the ſyringe: I obſerved to the perſons preſent, that the experiment had failed, but was ſurpriſed to hear in reply, that the animal was dead. I do not think ten ſeconds paſſed betwixt the time of the liquor's flowing back, and the death of the animal, which had actually taken place. I cannot eſtimate the quantity of poiſon introduced into the blood, but as the animal died, ſome muſt neceſſarily have found its way thither; had not this happened, I ſhould have ſuppoſed from the quantity which flowed back into the tube, that not a ſingle drop of it had entered the jugular vein.

The death of this animal was much more ſudden than in the caſes of introducing in a ſimilar way the venom of the viper into the blood; and the whole body was more ſunk and relaxed, than it is obſerved to be in animals that have been dead a long time.

Having put my ſyringe in better order, I introduced two drops of water, with which I had previouſly mixed about a quarter of a drop of the aqueous ſolution of the poiſon I have ſpoken of. I ſcarcely began to inject this liquor by the jugular, when the rabbit fell, without motion and without life, as if ſtruck by lightning. I do not think half a drop of the liquor was introduced.

I can ſay in general, after other experiments I have made ſince, that this poiſon introduced immediately into the blood by the jugular vein, kills

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more ſuddenly than the viper's venom, and in a much ſmaller quantity. Death ſucceeds the introduction of it ſo quickly as uſually to prevent convulſions. If a ſmaller proportion of it is employed, the convulſions and uſual ſtruggles are obſerved, and the death retarded. It is trúe that the blood is neither coagulated, nor ſo changed in its colour as when the viper's venom is mixed with it, but this does not delay the death of the animal, and the *ticunas* in immediate contact, kills to as great a certainty as the viper's venom. This is an experienced and an inconteſtible truth, however obſcure and difficult the cauſe of the deaths may appear, in the caſes I have related.

The American poiſon introduced into the blood kills inſtantly; from whence it ſeems beyond a doubt that when applied externally to the wound of a living animal, it muſt cauſe a derangement of the animal economy, and even death.

Effects of the TICUNAS *on the Nerves.*

The ſudden death which follows the introduction of this poiſon by the jugular vein into the blood of an animal, ſeems an unanſwerable demonſtration, that in theſe caſes the action of the poiſon is on the blood alone, and that the nervous ſyſtem is neither affected nor deranged by it. This does not however prove but that the nerves may be more or leſs affected by this poiſon, when the death hap-

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pens at ſome diſtance, and when it is applied externally to parts that have been wounded. In theſe caſes convulſions, and all the ſymptoms of a nervous diſeaſe, are principally obſerved. The nerves then may very probably be affected by the poiſon, and be the principal cauſe of the animal's death.

Here it was again neceſſary to recur to direct experiment, as I had done in regard to the venom of the viper, and to ſee what derangements and what complaints the American poiſon would produce, on being applied immediately to the nerves, without touching the blood veſſels.

Effects of the TICUNAS *applied to the Surface of the Nerves.*

I made my experiments on the ſciatick nerves of very large rabbits, preparing the nerves in the way I had done at Paris in operating with the viper's venom, and ſhall therefore enter into no detail here as to that particular, but ſhall relate a few of the principal experiments, to ſhow the variety I met with on my firſt trials: a variety that might have led me into an errour, had I not perſiſted in multiplying my experiments, and varying them till I found the conſequences ſomewhat uniform. 'Tis to this conſtancy, or if you will, to this obſtinacy that I owe in a great part the new truths I think I have diſcovered, as well in relation to the viper's venom, as to the *ticunas*.

Having

Having cleared the ſciatick nerve of a rabbit, I paſſed under it ſeveral doubles of fine linen, covering it with lint well wetted with the *ticunas* of a ſiropy conſiſtence. I covered the nerve with the ſame linen, to ſecure the bared muſcles of the animal from the poiſon, and ſewed the ſkin in the uſual way. In twenty minutes the rabbit began to be convulſed, and could not ſupport itſelf; it was then ſeized with all the ſymptoms of poiſon, and died ſoon after.

I repeated this experiment on another rabbit, and contrived to encloſe the nerve in linen ſtill better than before. This ſecond rabbit continued well for ten hours, but viſiting it on the twelfth I found it juſt expired.

I ſuſpected that the poiſon applied to the nerve in a certain quantity, might in time penetrate through the linen, with the humours of the wounded part, and convey its action to the muſcles and other adjacent parts. It was proper then either to diminiſh the poiſon, to prevent its ſoaking through, or to cover it with more linen. I choſe the latter as the ſurer method.

I cleared the ſciatick nerve of a rabbit in the uſual way, paſſing under it repeated doubles of very fine linen. I covered it with lint well ſteeped in the poiſon, putting bits of linen over the whole. The rabbit lived twenty-four hours, when it died ſuddenly, but I could not ſuſpect its death to be cauſed by the poiſon.

I got ready the ſciatick nerve of another rabbit as above, covering it with poiſon and bits of linen as uſual. It died in forty hours without any ſymptoms of the diſeaſe of the poiſon.

I made the ſame experiment on the ſciatick nerves of three other rabbits, taking the utmoſt care, after applying the poiſon, to have them well covered with linen, to prevent all ſuſpicion of the poiſon's having ſoaked through. One of theſe rabbits died in three days; the other two were alive at the end of eight.

I got ready the ſciatick nerves of two other rabbits exactly in the ſame way, but without applying the poiſon, for a comparative experiment. One of them died in thirty-ſix hours, the other was ſtill living at the end of the eighth day.

I thought theſe experiments ſufficient to determine whether the *ticunas*, applied externally to the nerves, is capable of producing any diſeaſe or derangement in the animal; but I wanted to know whether it would be equally inactive when applied to wounded nerves, or rather to the very ſubſtance of them.

Experiments with the TICUNAS *on Nerves cut or wounded.*

I prepared the ſciatick nerve of a rabbit as above, and having pierced it ſeveral times through with a lancet, laid the poiſon exactly on the wounded part.

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The rabbit lived five days, when it died without any ſymptoms of diſeaſe. I repeated this experiment in all its circumſtances on another rabbit, which was ſtill alive eight days after.

I varied this experiment a little on the nerves of three other rabbits. Inſtead of making ſeveral wounds with a lancet, I made a longitudinal opening into the nerve of more than five lines in length, into which I introduced threads well ſteeped in the poiſon, and covered all ſecurely. One died in ſixty hours without any ſymptoms of the diſeaſe of the poiſon; the two others were living eight days after.

I thought it proper, again, to vary this ſecond experiment, by cutting the nerves of a few rabbits, in the way I had done in examining the venom of the viper. I cut the ſciatick nerve as far as I could from its origin, that ſo I might encloſe it in linen. The part of it cleared in very large rabbits was about an inch and an half: having put linen beneath the nerve, I covered the cut part well with the poiſon, and placed linen over all as uſual.

I made this experiment on ſix rabbits, two of which died in forty hours, two in three days, and two were ſtill living on the fourth.

To make a comparative experiment, I prepared the nerves of two rabbits as above, but did not apply the poiſon. One died in thirty-ſix hours, the other ſurvived the third day.

The uniform conſequences of theſe experiments on the nerves, made me think any further ones unneceſſary; and I believe thoſe will agree with me, who are accuſtomed to make experiments, and are not prejudiced by ill-founded hypotheſes. The American poiſon, as is here ſeen, in whatever way it is applied to the nerves, is not a poiſon to them, and does not produce in theſe caſes any ſenſible derangements in the economy of a living animal: this is proved by immediate experiment. To ſuppoſe what we do not ſee, to believe what is contradicted by experiment, is ſubſtituting dreams to real objects, embracing errour for truth, and adopting chimæræ for facts.

The American poiſon then, agrees with the viper's venom, in being quite innocent to the nerves, in whatever way it is applied to them; like the viper's venom it kills in the ſmalleſt quantity, and that inſtantly, if introduced into the blood by the jugular vein; and its action is altogether on the blood, let the principle or mechaniſm by which it cauſes death be what it will.

The effects the viper's venom produces on the blood are more clear and decided; there is an unqueſtionable coagulation of it, not to be perceived in the caſes of the *ticunas*: we however obſerve in this laſt a great change in the lungs, which become violently diſeaſed.

The ſudden death of an animal, when the poiſon is injected into its veſſels is really ſurpriſing; it ſcarcely ſeems to have had time to reach the heart. Neither can we conceive how animals with cold blood; frogs, for inſtance, that exiſt with an impeded

impeded circulation, can be killed by it; although it is true that they die much later than animals that have the blood warm. The blood or any humour, changed by a poiſon, may indeed gradually produce in theſe animals, more conſiderable derangements than are cauſed by an impeded circulation.

The death which immediately ſucceeds the introduction of the *ticunas* into the blood, may induce a ſuſpicion that this humour contains a very active, ſubtil, and volatile principle, which eſcapes the niceſt eye, and even the microſcope. Agreeable to ſuch an hypotheſis, this principle ſeems highly eſſential to life, and to be particularly acted upon by the poiſon.

What may lead one to ſuſpect that a very active and volatile principle does really exiſt in the blood is, that the viper's venom prevents its coagulation when 'tis drawn from the veſſels, and on the contrary, produces it in the veſſels themſelves. One would ſuppoſe in the firſt caſe, that ſomething had flown off from the blood, which exiſts in it when 'tis encloſed by the veſſels.

This active and vital principle, agreeable to this hypotheſis, may be conſidered as reſulting from the whole animal economy, without excluding the nerves, which may even contribute greatly towards it.

But theſe are mere conjectures, more or leſs probable, and not demonſtrated by experiment. We muſt ſtick to certain facts, in whatever way we explain them: it is certain then, that the *ticunas* does not act on the nerves, and that it acts altogether on the blood.

Previous to my experiments, no one would have doubted its immediate action on the nerves, which was announced by every outward symptom. These symptoms then, which the physicians improperly regard as a certain proof that the disease is purely nervous, are equivocal, and may present themselves without the smallest nervous affection: the mere change in the blood is sufficient to produce them in an instant. The greatest physicians have regarded the diseases caused by the viper's venom and the American poison, as derangements in the nerves: it is now their business to examine whether other diseases, ascribed to the nerves, are not rather diseases of the fluids, and of the blood. There are grounds for this suspicion; the symptoms are equivocal, and the principles not generally demonstrated.

I do not pretend to deny but that diseases of the nerves may occur; this would be running from one extreme to the other. There are without doubt, diseases nervous in their origin, and many others which become so by the changes that take place in other parts, even in the fluids. The passions of the mind shew us the influence the nerves have on the body; but this does not prove that all the diseases ascribed to the nerves, are really nervous, and that the usual symptoms of these diseases may not be equivocal. Besides, it is certain that the

poisons

poisons we have examined, have no immediate action on the nerves, whatever may have been hitherto believed.

Some one may object, that probably the viper's venom and the American poison, act only on the extremities of the nerves, and on that account are innocent when applied to the trunks of them. But every thing serves as an objection to those who busy themselves in imagining difficulties; and it is very rare to find two things in every respect alike. For my part, it appears to me, that the internal substance of the great nerves, is the same as that of their extremities; and that the former are subject to pain as well as the latter. I do not create hypotheses which are not confirmed by facts.

I may have been deceived in some of the consequences I have deduced from my experiments, and may likewise have been deceived in some of the experiments themselves, although I have strove to succeed, and have searched for truth in an unprejudiced way. I do not doubt but that any one who will apply himself after me, to these researches, will find something to add, and even perhaps to correct. It is sufficient for me to have opened a road to new truths, and to be able to attest the veracity of the principal facts I have advanced.

The greater part of these experiments was made in the presence of my particular friend Dr. Ingenhausen, Physician to their Imperial Majesties, who has displayed in several publications the talents of a real naturalist. M. Tiberus Cavallo has likewiſe aſſiſted at ſeveral of the moſt important of them. I conceived that the authority of two perſons known to the learned, would give an additional credit to my experiments.

On the poiſoned Arrows brought from the Eaſt Indies.

After I had finiſhed my experiments on the *ticunas*, one of my friends at London procured me a number of arrows from the Eaſt Indies, on which I made ſome experiments, but neither greatly multiplied nor varied them, as well becauſe the arrows were but few in number, as becauſe they ſeemed to differ from the American poiſon, only in having leſs activity. This laſt circumſtance was probably owing to their not having been ſo well preſerved as thoſe from the Weſt Indies, which indeed ſeemed to be the caſe, or becauſe the poiſon had been prepared many years ago.

I could not ſucceed in killing even ſmall rabbits by applying this poiſon to the ſkin, ſcraped or ſlightly ſcarified; although I laid it on in a greater quantity, and upon larger portions of the ſkin, than in my experiments with the *ticunas:* when given inwardly, in twice or thrice the quantity in which I gave the latter, it produced no ſenſible change, even in rabbits that ſcarcely weighed a pound.

I pierced the ſkin of ſeveral animals with theſe arrows, leaving them ſticking in it for whole days, without

without my perceiving any poifonous affection. But on piercing the mufcles, and leaving the arrows plunged in them, I foon found the effects. feveral of the animals poifoned in this way, died with the fame fymptoms that are produced by the *ticunas*. They were feveral hours in fickening, fo that this poifon does not feem to differ effentially from the other. It exactly refembles it when examined with a microfcope, when thrown into the eyes of animals, when tafted, and when chewed. It diffolves with greater difficulty in water, in which the greater part of it remains in an infoluble ftate.

The only inferences to be drawn from the particulars I have related are, that this poifon communicated to the mufcles, acts with far greater force than when applied to the fkin; that it agrees very well with the other poifons, and ferves ftill to perfuade us that the immediate action of poifons is not on the nerves; fince it is certain that the fkin is more fenfible than the mufcles, and is quite interwoven with nerves.

Experiments made with the TICUNAS, *after my return to Italy in* 1780.

I forced an American arrow into the body of one of the fnakes, called *anguis miliaris*, near its tail. The fnake lived, and fcarcely feemed benumbed

numbed by it. I repeated this experiment with a fresh arrow on the same snake, which neither died, nor suffered a great deal. These two arrows had been previously dipped in the *ticunas* softened before the fire to the consistence of honey.

I passed another arrow in the same state, through the tail of a snake: in four hours it was motionless, and as if dead. On pricking the body with needles, there were notwithstanding some marks of irritability, which at length disappeared, so that I judged the creature dead. In thirty-six hours it however began to move, and continued in a very feeble state for five days more. In its first condition 'twas apparently dead; in the second certainly alive: nothing has surprised me so much as this kind of return to life, in so large an animal, in which the suspension of all its vital motions continued so long a time.

I repeated this experiment afresh on another snake of the same kind, steeping the arrow in warm water before I forced it into its tail: I left it there twenty-four hours, without its producing any effect. Soon after I forced another of these arrows into its body, where I left it for twelve hours, but without effect.

I pierced one of the fore feet of a land turtle that weighed four pounds, with an American arrow, which I left there for half an hour. In another hour it scarcely seemed to live, and in two appeared quite dead. After an interval of ten hours, I removed with a sharp instrument the inner shell, taking

taking care that the flefhy parts fhould be as little torn as poffible. The heart was ftill, and I fcarcely found any motion in the auricles. But in a little time the heart, together with the auricles, recovered all its force, and continued in action for fix hours inceffantly; the auricles kept in motion for two days, that is to fay, as long as they were moiftened by the blood which flowed from the neighbouring veffels.

I pierced the fore foot of a land turtle that weighed a pound and an half, with an American arrow. In eight minutes it could fcarce move, and in a quarter of an hour was dead. When the feet and neck were ftimulated, they difcovered a flight degree of fenfation. Having opened the thorax, I found the heart and auricles quite motionlefs. I touched the heart thrice, and it contracted itfelf once each time. On freeing it from the membranes, it began to move very brifkly, and continued to do fo for feveral hours. I covered it with the inner fhell, and in twenty-four hours found it again motionlefs. I pricked it once with a needle, it contracted itfelf a fingle time; I pricked it again, it contracted itfelf afrefh, and continued to do fo once every time I pricked it. I left it expofed to the air for three minutes, and it then began to move of itfelf, continuing a very brifk motion for feveral hours. I covered it afrefh with the fhell, and on uncovering it four hours after, found it motionlefs. I left it in the air for a few minutes, and in a fhort time, and of itfelf, it recovered

vered its oſcillations, which continued for ſix hours. I again covered it with the ſhell, and on uncovering it two hours after, found it without motion. I then covered it with water, which I kept on it for ten minutes, without its producing any change. I drained off the water, and the heart was ſcarcely left in the air a minute, when it began afreſh to move briſkly, and continued to do ſo for ſeveral hours. Laſtly, I put it in the ſun, where it ſoon dried. The auricles likewiſe dried in a degree, and all was then ſtill. I now wet both heart and auricles; the former continued always motionleſs, but the latter began to move, and continued their motion for eighteen hours, when becoming dry, they loſt it for ever.

This ſucceſſion of motions confirms ſtill more the doctrine I have eſtabliſhed on the irritability of animal fibres (*a*), and ſhows that the air is a very active principle in awakening the ſenſibility of muſcular fibres, and of the heart.

It cannot be doubted, but that the *ticunas* attacks the principle of irritability in the muſcles, although it has no action on that of the heart. It agrees in this with the other poiſons, which do not uſually act on this muſcle or on the inteſtines: the motion of theſe laſt continues, even after the death of the animal, and when the irritability of the other muſcles is totally deſtroyed.

(*a*) De Legibus Irritabilitatis nunc primum ſancitis. Lucca, 1775.

FIRST TRACT

ON

CHERRY-LAUREL WATER.

TO close my refearches on poifons, I fhall relate various experiments I have made on cherry-laurel water, a poifon which for fome years has been celebrated in Europe. It does not yield to the moft active amongft them, when the great derangements it caufes in the animal economy, and the fuddennefs of its action on being given internally to animals, are confidered. It not only produces the moft violent convulfions and death, in animals even of a middling fize; but if given in a fmaller dofe, the animal writhes itfelf, draws its head and tail together, and forces its vertebræ out in a way frightful to the view.

In

In this ſtate the convulſions and agitations of the body are very violent, and in the midſt of them the animal at length meets with a ſpeedy death.

If given to an animal as an injection, it equally produces convulſions and death.

With leſs than two teaſpoonfuls of this water taken internally, I have ſeen middle ſized rabbits fall into convulſions in leſs than thirty ſeconds, and die in a minute. If ſwallowed by animals in a large quantity, they die almoſt inſtantly without convulſions, and with their bodies entirely in a ſunk and relaxed ſtate.

When given in a ſmall quantity, convulſions more or leſs violent ſucceed, and the body and extremities of the animal become lifeleſs: the hinder feet firſt loſe their uſe, then the fore ones, and ſo on. When the animal can neither ſtir its legs or body, it continues to move its neck and head very well, raiſing the latter without difficulty, and turning it freely about. In this ſtate the animal retains its ſmelling and ſight, and though it does not ſtir its feet of itſelf, it draws them back when they are pricked or violently ſqueezed: a proof that they have not wholly loſt their ſenſibility.

Cherry-laurel water then is a very ſtrong poiſon, either taken by the ſtomach, or introduced into the body as an injection. Its action is ſo ſudden and violent, that the animal gives ſymptoms of pain almoſt on the moment of its being ſwallowed. It is true that a ſmall doſe of it is innocent; that is to ſay, that a few drops of it given to a ſmall animal

animal which would be killed by the ſame quantity of the *ticunas*, produces no ſenſible complaint: but this does not make an eſſential difference betwixt the water of the cherry-laurel and the other poiſons that are better known.

On diſtilling a great deal of water from a few of the leaves, I found it to be quite innocent; it becomes more active if repeatedly diſtilled over again from the ſame leaves, but is not mortal. If inſtead of adding water to the leaves of the cherry-laurel, the diſtillation is made *in balneo mariæ*, the liquor that runs off is a very ſtrong poiſon, and very ſudden in its effects. This is what I have principally employed, and I doubt not but that it may be brought to ſuch a degree of activity, as to kill in a ſmall doſe, in the ſame way with the *ticunas*. For this purpoſe the liquor that riſes the firſt time ſhould be diſtilled over again ſeveral times on freſh leaves, well wiped and almoſt dry. I believe that if it were evaporated over the fire, an oily concrete ſubſtance would be obtained, which would not only equal in activity any of the known poiſons, but would probably far exceed them all. I reſerve this experiment for another occaſion, when I ſhall alſo ſpeak of bitter almonds, and the degree of poiſon to which their water may be carried, when diſtilled dry.

We have ſeen that cherry-laurel water kills animals when introduced into the ſtomach or inteſtines; let us now ſee the effects it produces when applied to wounds. One of the many experiments I made on this ſubject will here be ſufficient.

I made a cut of about an inch in the ſkin of a large guineapig's belly, and ſlightly ſcarified the bared muſcles in ſeveral places, applying to them two or three teaſpoonfuls of the water mentioned above. In leſs than three minutes the animal fell into convulſions, and died ſoon after. This experiment ſhows that the cherry-laurel water agrees with the other poiſons in its action on the body, when introduced through the medium of wounds.

The ſucceſs of this experiment was the ſame on other animals with warm blood; I however obſerved, that the water of the cherry-laurel acts invariably with greater quickneſs and force, when taken internally, even in a ſmaller quantity.

This laſt circumſtance deſerves, in my opinion, the greateſt attention, ſince it is a determined fact, that a large wound has infinitely more veſſels than the mouth or ſtomach, to abſorb the poiſon almoſt at the very inſtant: beſides which, the nerves contained in the wound, both from their number and the ſtate they are then found in, ought to be very ſenſible to the action of this poiſon.

Not only animals with warm blood die very ſuddenly when made to ſwallow this water, but thoſe with cold blood die too. What appeared to me very ſingular, ſince it is quite different with the other poiſons, is that they die in a very ſhort time, perhaps even quicker than the firſt. It will be

ſufficient

ſufficient juſt now to ſpeak of eels, creatures very hard to kill, and the parts of which, when they are dead, continue to move a long time: they die in a few ſeconds after ſwallowing this water, contracting themſelves at firſt, but becoming motionleſs the inſtant after their death, when their bodies are not even ſenſible to ſtimulation. The heart however continues to move, but not ſo forcibly as before; and this motion ceaſes much ſooner than when they are killed by cutting off the head. It cannot here be denied but that the muſcular irritability muſt be ſtrongly acted on, and that in a particular way. All the animals with cold blood I have been able to procure, were killed by this poiſon, and I doubt whether any of them are capable of reſiſting it: if they are not, it ought on that account to be diſtinguiſhed as the moſt terrible of all known poiſons, from its univerſal faculty of deſtroying every ſpecies of animals.

But how does it kill in ſo ſhort a ſpace, when introduced into the ſtomach, where we perceive no veſſels capable of receiving it? This difficulty requires ſome further experiments: we muſt ſee the effects it produces when applied immediately to the nerves, and when introduced into the blood without touching the parts that are cut.

I employed large rabbits, making my experiments on the ſciatick nerve, as I had done with the venom of the viper and the *ticunas*. For brevity's ſake, I ſhall relate here a ſingle experiment, omitting all the others, which I do not think very ne-

 ceſſary,

ceſſary, after the many experiments on the nerves already related.

Having laid bare the ſciatick nerve of a large rabbit, for the length of more than an inch and an half, I placed under it a piece of very fine linen, ſixteen times doubled, to prevent the water from penetrating to the parts beneath it. I then wounded it lengthways ſeveral times with a lancet, and covered the wounded part, more than eight lines in length, with a bit of cotton about three lines thick, well ſteeped in cherry-laurel water, of which it abſorbed more than fifteen drops: all of this was communicated in an immediate way by the wounds, to the medullary ſubſtance of the nerve, which I covered for ſeveral minutes with freſh linen, ſo that it was impoſſible for the poiſon to be communicated either to the parts beneath or thoſe adjacent. Having made the ſuture in the ſkin, and left the animal at liberty, it ran about, eat, and was as lively as before, and neither then nor afterwards, ſeemed at all affected by this poiſon, which kills ſo ſuddenly when taken by the ſtomach. This and ſeveral other facts, analogous to thoſe of the viper's venom and *ticunas*, ſhow us that cherry-laurel water, in whatever way it is applied, whether immediately to the nerves or even introduced into their medullary ſubſtance, has no action on them, either as a poiſon or otherwiſe.

After all the experiments related in this work on the venom of the viper, and on the American poiſon, which is ſtill more powerful; after having ſeen

ſeen that neither of theſe two poiſons has any action, when applied in an immediate way to the nerves, whilſt they kill the ſtrongeſt animals the moment they are introduced into the blood; there cannot be a more natural inference than that the water of the cherry-laurel, innocent like the others when applied to the nerves, ought to be likewiſe deſtructive when introduced into the blood; and yet the caſe is altogether different: ſo true it is that we ought to miſtruſt analogies, even when they are moſt uniform.

I ſat about introducing this water into the jugular vein of a large rabbit, beginning with five or ſix drops, as I had done with the viper's venom and *ticunas*. The animal giving no ſymptom of pain, I thought I had not ſucceeded in the attempt, and that the ſyringe had found its way into the cellular membrane. I repeated this experiment, introducing afreſh, perhaps three or four times the quantity of the water, and aſſuring myſelf previouſly that the point of the ſyringe had entered the vein, and that the liquor could in no way force itſelf back: the animal ſtill continued to be unaffected by it. I was more ſurpriſed than ſatisfied at what I ſaw. I could not perſuade myſelf but that the cherry-laurel water would be a poiſon, and even a very powerful one, when introduced into the blood, ſince applied to the nerves, it was quite innocent. I returned then to my experiments, and now introduced a teaſpoonful of the water into the jugular vein, from which the animal felt no ill effects. I repeated this experiment on another rabbit, and introduced the ſame quantity of the poiſon, the creature neither ſuffered at the moment nor afterwards.

The unexpected reſult of theſe experiments threw me into the greateſt uncertainty as to this poiſon; I was not only puzzled at its mode of operation, but even as to the parts on which it acts, when taken internally or applied to wounds. It does not act perceptibly on the nerves; it has no action on the blood; and yet it kills when ſwallowed, and that in an inſtant. The deaths of animals may then be brought on in another way than by the blood and nerves! The loſs of motion in a few ſeconds in creatures ſuch as eels, which continue to move for whole hours after their heads are cut off and their bodies in pieces, would induce a belief that this poiſon acts on the irritability of the muſcular fibres. It is true that in theſe animals the heart ſtill continues to move, but its motion is much diminiſhed, and of very ſhort duration. In animals with warm blood, poiſoned by the water of the cherry-laurel, a very feeble motion ſtill exiſts in the other muſcles, and if the heart continues to beat for ſome time, its motion is feebler than when they are killed in any other way. There is certainly a very great diminution of irritability in many animals, and in many others a total loſs of it, in whatever way this loſs or this diminution may contribute to bring on ſo ſpeedy a death, and however obſcure the mechaniſm may be by which this action is wrought on the muſcular fibres.

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We muſt avow our ignorance in ſearching into nature; when we think we have done every thing, we frequently find ourſelves returned to the ſpot we ſat out from. Experiment, which is the only guide we have in our reſearches, undoubtedly ſecures us effectually from falling into errour; but it does not always draw us into an acquaintance with remote truths; neither does it always lead us to a knowledge of the ſecrets of nature, nor carry us invariably as far as we propoſed to go.

But if we do not know how the cherry-laurel water acts, and on what parts it exerciſes itſelf in deſtroying animals, we however know that when it is applied immediately to the nerves, and even to the medullary ſubſtance of them, it is quite innocent: and it is not leſs true, as the many experiments hitherto related have clearly demonſtrated, that the viper's venom and the *ticunas* are not mortal in whatever way they are applied to the nerves, but are always ſo when introduced into the blood. Theſe truths, which we were formerly ignorant of, are now cleared up in ſuch a way that they cannot be called in doubt, and deſtroy all the ſyſtems which writers have invented on the action of theſe poiſons: it is from theſe facts that we muſt proceed to an intelligence of them, and of their action.

The applying the water of the cherry-laurel to the different parts of the brain of a living animal may probably throw ſome light on its action; I poſtpone the doing it till I have more conveniences for that purpoſe than at preſent, and ſhall then, I hope, furnifh fome new and more interefting particulars as to this poifon, and the parts of an animal on which it acts when it deftroys it.

I referve to this opportunity the examining whether it acts on the lymphaticks, or rather on the lymph itfelf. I fufpected this after trying the experiments I have recited, but could not then enter into an inveftigation of it. My experiments on cherry-laurel water are confequently lefs complete than I wifhed: They muft be multiplied and purfued more than I have been able to do, and this is ftill another reafon that I continue my refearches on this truly interefting fubject.

On the action of Poifons on the Nerves.

It has been feen in the courfe of this work, that the venom of the viper, and the *ticunas*, in whatever way they are applied to the nerves, are innocent; and on the contrary, that when they are introduced into the blood, without touching the wounded parts adjacent, they bring on a fudden death. It has likewife been feen, that thefe two poifons throw an animal into very violent convulfions, and excite the moft decided fymptoms of thofe difeafes, which the phyficians call nervous, becaufe they believe them to arife fimply from a nervous affection. It appears, that there can be no doubt in future of thefe poifons being abfolutely innocent when in immediate contact with the nerves;

nerves; and that they have no immediate action on the solid parts, such as the muscular fibres, the bones, cellular membrane, and tendons. But this is not sufficient to lead to a perfect knowledge of them. The nerves are certainly excluded in the diseases they occasion, and the blood alone seems to be acted on. But how many different humours do we not find mixed with the blood? It has even been believed, that it is animated, and it seems more than probable that the nerves are perpetually secreting a humour which is mixed and circulates with it. May not this humour be essential to life, and be attacked by these poisons, when they are introduced into the vessels? In reply to this query, frogs are known to live, as the learned Spalanzani has observed, even after their vessels are emptied of the blood, and in this state die very readily, if made to swallow the spirit of the cherry-laurel, as I have observed several times. Thus is the first of these two hypotheses proved to be false, and the second is by no means sufficient to explain the action of this poison on the parts of animals.

The spirit of the cherry-laurel, which acts but feebly when applied to wounds, which is innocent when applied to the nerves, and which kills when simply applied to the mouth and eyes, throws us into fresh uncertainties, and scarcely leaves room for conjectures. A substance which is a poison in the stomach, in the mouth, and in the œsophagus, and which is almost innocent when applied to wounds, is a true paradox, and reiterated experiments are

scarcely

ſcarcely ſufficient to convince us that the fact is real.

The only ſure conſequence to be drawn from ſo many experiments is, that an animal may have all the ſymptoms of a nervous diſeaſe, without our being able to aſſure ourſelves that the nerves are affected. The convulſions likewiſe that are obſerved in animals from the diminution of the blood alone, or the unequal diſtribution of this fluid in the different parts, in the way I have demonſtrated in ſeveral parts of my different works, leave no room to doubt it (*a*).

I have only to wiſh at preſent, that ſome ſkilful phyſician, gifted with the rare talent of nice obſervation, and diveſted of all prejudice for hypotheſes that want the neceſſary ſupport of facts and experiments, would find leiſure to examine with a critical nicety this important ſubject, which conſtitutes at this time one of the principal branches of modern medicine. Such an inveſtigation may at length decide with certainty, whether all the diſeaſes that are ſtiled *nervous*, and which are ſaid to originate from a change in the ſyſtem of nerves, have really ſuch an origin; whether there are characteriſtick ſymptoms, invariable and conſtant, of this ſpecies of diſeaſes; whether there is a certain criterion, a ſufficiently faithful touchſtone, to aſſure

(*a*) This firſt tract on the cherry-laurel, and likewiſe, the treatiſe on the *Ticunas*, were printed in the 68th volume of the Philoſophical Tranſactions.

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us of it; and whether the greater part of these diseases may not be caused by the blood, or other vitiated humours, rather than by the nerves. Is the perceiving the nerves to be in some measure affected, really sufficient to characterize a *nervous* disease? Can we determine a disease to be *nervous*, because several of its effects manifest themselves on the nerves? An affection of the nerves may very easily be attached to a particular class of diseases, and yet the nerves not be the cause of them; ought we thence to say that such diseases are *nervous*? We do not here demand sublime and abstracted theories, which a singular effort of genius frequently gives birth to: we are in need of nice observations; new and well imagined experiments; direct and useful inductions, drawn by a calm mind, and one capable of assembling and combining in the best manner, the most luminous particulars. Three of the most learned physicians of our days have by their writings fully satisfied the first of our demands; we have now to desire that a fourth will at length finish this important work, by applying himself assiduously to the last.

SECOND TRACT

ON THE

CHERRY-LAUREL.

SHORTLY after my return to Florence in 1780, I had an opportunity of re-examining the effects of the ſpirit of cherry-laurel on different animals. I therefore thought it proper to give a greater extenſion to my experiments on this poiſon, than I had done in England; and my labours have not altogether been without ſucceſs. I have at leaſt eſtabliſhed truths I was before ignorant of, and have excluded many uſeleſs or falſe hypotheſes, which might have retarded the reſearches of thoſe who may wiſh to buſy themſelves hereafter on this ſubject. I very juſtly obſerved ſome time ago, that in diſtilling the leaves of the cherry-laurel without water, a ſpirit was obtained capable of killing

killing animals in a very ſhort ſpace, although it was given to them only in a ſmall doſe. I likewiſe obſerved that if water was put on the leaves, the ſpirit would become altogether innocent, and preſerve nothing but an agreeable flavour. But I did not know whether the eſſential oil drawn from the cherry-laurel by diſtillation was innocent, or acted as a poiſon; and whether, ſuppoſing it hurtful, it was ſtill more ſo than the ſpirit. I likewiſe did not know whether the deadly effects of this ſpirit were cauſed by its having in it more or leſs of this oil in a diſſolved ſtate. It was owing to the want of experiments that I was ignorant of all theſe particulars, and of many others which the authours who have written on poiſons have not examined into. None of them, that I know of, have made experiments on the *empyreumatical oil*, and on the *extract* of the cherry-laurel; neither have I met with any one who has made direct experiments on the greater part of the above particulars, to ſerve me as a guide, and yet they appear to me neceſſary to the right underſtanding the nature and qualities of ſo very ſingular and active a poiſon.

To proceed methodically, I ſhall give a conciſe detail of the different productions I have drawn from the cherry-laurel, and of the method I purſued for that purpoſe. In diſtilling the leaves in the uſual way in glaſs retorts, and without adding water, I obtained the ſpiritous part, (what the chymiſts call *rectified ſpirit*). This ſpirit was tranſparent, fragrant, poignant to the taſte, and at the

bottom

bottom of the receiver there was a certain quantity of a heavy, coloured, odoriferous oil, which was bitter, pungent, and burning to the taſte. I ſeparated this very attentively from the ſpirit, after letting it ſettle for ſeveral days. I likewiſe employed the ſpirit not entirely deprived of the oil. I ſhall call this oil, *oil of the firſt diſtillation*, and the ſpirit, *ſpirit of the firſt diſtillation*.

I took a quantity of the ſpirit of the firſt diſtillation, and diſtilled it afreſh till there remained about one third in the retort. The ſpirit came off more tranſparent, fragrant, and poignant, more bitter and pungent, than that of the firſt diſtillation. Being left in a bottle to ſettle, it depoſited a very tranſparent, odorous, burning oil, very like in its external qualities the oil of the firſt diſtillation. I ſhall call this, *oil of the ſecond diſtillation*, and the ſpirit, *ſpirit of the ſecond diſtillation*. What remained in the retort I ſhall call *reſiduum*, or *phlegm of the ſecond diſtillation*. I prepared another phlegm by evaporating two thirds of the ſpirit of the ſecond diſtillation in the ſun.

I mixed a quantity of the ſpirit of the ſecond diſtillation with the ſame quantity of decrepitated and well dried ſpirit of ſea ſalt. I diſtilled this mixture with a ſlow fire, and drew off one half: this was of the colour of common oil, and leſs ſpirituous, pungent, and fragrant than before. It precipitated an oily coloured ſubſtance, of an earthy appearance, and divided into ſmall grains or globules. I continued the diſtillation, and there came

off

off a phlegm without ſmell, and not ſenſibly pungent, although put on the tongue, or rubbed on the noſtrils. I ſhall call it *phlegm of the third diſtillation*. The oil I have juſt ſpoken of, I ſhall call *oil of the third diſtillation*, and its ſpirituous part which came off the firſt, *ſpirit of the third diſtillation*. Both oil and ſpirit have the ſmell of bitter almonds.

I drew off in the ſame way the *extract* of the leaves of the cherry-laurel, purſuing the ſteps laid down by chymiſts; and likewiſe prepared a good quantity of *empyreumatical oil* with freſh leaves. After having furniſhed myſelf with all theſe preparations, I began my experiments on animals, chuſing rabbits, guineapigs, pigeons, frogs, and adders. I thought it neceſſary to try them on animals of different natures, ſome with warm blood, and ſome with cold, becauſe I knew by experience how much the action of poiſons varies in different animals, and above all in the larger ſpecies of them, in which the economy of the various movements is ſo different.

Spirit of Cherry-laurel of the firſt diſtillation, given internally.

I let this ſpirit ſettle for ſome days to ſeparate the oil from it, ſo that it became clear and tranſparent. I gave a teaſpoonful to a pigeon of a middling ſize, which

which in four minutes fell into convulſions, and was for ſome time not able to ſupport itſelf; it however ſurvived.

I gave three teaſpoonfuls to a ſmall guineapig, which felt no ill effects from it.

I gave two teaſpoonfuls to a pigeon, which in a minute could not ſupport itſelf; in another minute it fell into ſtrong convulſions, and died on the third.

I repeated all theſe experiments on the following day, on the ſame animals, and the reſult of them was the ſame.

As the pigeon died which ſwallowed two teaſpoonfuls of the ſpirit, I wiſhed to try it before the oil had ſunk to the bottom. In this ſtate it is leſs clear, more pungent, and more fragrant. The want of activity in the ſpirit employed above, ariſes, as will be ſeen, from my having left the oil to precipitate for ſeveral days, and in reality that which I had employed in London was thick, and acted with far greater violence.

I gave then to different animals, ſuch as rabbits and guineapigs of a middling ſize, three teaſpoonfuls of the ſpirit in the ſtate I have ſpoken of, and the greater part of them died convulſed in a very little time: a proof that this ſpirit is much ſtronger than the other.

Spirit of the second Distillation given internally.

I gave to a small guineapig a teaspoonful of this twice rectified spirit: it died almost instantly.

I made a large rabbit swallow a spoonful of this spirit. It immediately fell, and died a short time after.

I gave four drops to a guineapig of a moderate size. In the very act of deglutition, a liquor of a yellow and green hue poured from its mouth. This frequently happens when the spirit is swallowed by these animals, but never when they swallow the oil. It was otherwise without complaint.

I gave a large rabbit six drops of this spirit mixed with forty drops of water. The creature lay down several times on its belly, seemed very uneasy, but did not die.

I gave four drops to a pigeon, which expired in less than a minute.

I gave four drops to a frog, which in two minutes was to appearance dead, and the parts of which in two more, although stimulated, were totally without motion.

Phlegm of the Spirit of the second Distillation.

This phlegm was scarcely spirituous or pungent, but was very transparent.

 I gave

I gave it to ſeveral animals, ſuch as pigeons, rabbits, and guineapigs. The pigeons to which I gave a teaſpoonful all died: of thoſe to which I gave only a few drops, ſome had no complaint, ſome died after a long time, and others were only convulſed.

A few of the very ſmall rabbits and guineapigs died, others were convulſed, and others again had no ſenſible complaint. Thoſe which died, had ſwallowed three or four teaſpoonfuls.

The phlegm then is leſs deſtructive than the ſpirit.

Phlegm of the Spirit of the ſecond Diſtillation, obtained by evaporating two thirds of it in the Sun.

I put about three ounces of the ſpirit of the ſecond diſtillation in the ſun to evaporate. The reſiduum being an ounce, was liquid, tranſparent, and almoſt without ſmell: when put on the tongue it was ſtill pungent, although much leſs ſo than before. I gave half a teaſpoonful to a pigeon, which fell inſtantly into violent convulſions and died. Five other pigeons died in the ſame way. Theſe experiments would lead one to ſuppoſe, that the poiſon neither conſiſts in the fragrant nor the pungent quality of the cherry-laurel, ſince both of them were ſo trifling, and the animals notwithſtanding died ſo ſuddenly.

I gave a teaſpoonful of the ſame phlegm to two pigeons, both of which immediately died.

I gave

I gave three drops to a pigeon, without its having any effect, whence we may infer that this phlegm is alſo leſs deſtructive than the ſpirit.

Spirit of the ſecond Diſtillation applied to the Mouth.

I wiſhed to know whether this very active and deſtroying ſpirit would kill, when ſimply applied to the inner part of an animal's mouth.

I wetted a piece of linen with it, and introduced it into a pigeon's beak, without the poſſibility of a drop of the ſpirit paſſing into the ſtomach, or even the œſophagus. In thirty ſeconds the pigeon fell into convulſions, and died immediately.

I wetted another piece of linen with it, and kept it a long time in the mouth of a middle ſized guineapig, which was not annoyed by it.

I repeated the ſame experiment on two pigeons, both of which died in leſs than two minutes.

I repeated it on two guineapigs, which continued free from complaint.

This ſpirit then is capable of killing weak animals without touching the œſophagus or ſtomach.

Spirit of the ſecond Diſtillation put on the Eyes.

It remained to know whether this ſpirit would be likewiſe deſtructive, when applied to other tender parts of the body. Here it occurred to me to make my experiments on that very ſenſible organ, the eye.

I let ſeveral drops of the ſpirit fall on the naked eyes of a guineapig. It ſuffered ſeverely; but had neither convulſions, inflammation, nor any other ſymptom of poiſon.

I made the ſame experiment on the eyes of two other guineapigs, and the conſequence was the ſame. I repeated it on the eyes of two rabbits, but although the ſpirit clearly incommoded them, they neither died, nor were convulſed; and their eyes were not apparently inflamed.

Theſe experiments do not yet prove that the ſpirit of the cherry-laurel is entirely innocent when applied to the eyes of theſe animals, becauſe they are hard to kill, and make a ſtrong reſiſtance to the action of the ſpirit, when it is ſimply applied to the mouth.

It is true that I covered the eyes of two very ſmall guineapigs which weighed only three ounces each, with the ſame ſpirit, and repeated the application of it more than twenty times, but in vain: they had no ſymptom of poiſon, and their eyes were free from inflammation, although they were very ſenſible of pain when I applied the ſpirit to them.

But I thought it would be right to make ſome experiments likewiſe on pigeons. I moiſtened a pigeons eyes ſeveral times with a piece of linen wetted with the ſpirit: after a ſhort time it vomited repeatedly, and fell on its breaſt. The iris in the vicinity of the tranſparent cornea was a little inflamed, the pupil moveable, and of its natural ſize.

I let

I let fall ſome drops of the ſpirit on the eyes of another pigeon, keeping them on the part for more than two minutes; it was ſeized with convulſions, and died in a few inſtants, without the eyes being inflamed.

I put ſeveral drops of the ſpirit on one of the eyes of a third pigeon, keeping it on for three minutes. The iris was much inflamed, as were the eyelids in a degree. A ſhort time after, the pigeon was ſeized with convulſions, and appeared as if dead. In a quarter of an hour, it recovered by degrees, and at length ſeemed quite well; however it fell afreſh into convulſions, appeared for a ſecond time as if dead, and very ſoon after revived again. The iris of the eye to which I applied the ſpirit, was as red as if it had been injected, the pupil was immoveable and much enlarged: the iris of the other eye was likewiſe a little red, the pupil in its natural ſtate. After the animal's relapſe and ſecond recovery, the pupil and iris were in the ſtate they were the firſt time; but after the third attack, from which it recovered itſelf perfectly, the pupil became moveable like the other, the inflammation of the iris was much leſſened, and both pupils recovered their natual ſize.

I let fall ſeveral drops of the ſpirit into the eye of another pigeon, and kept them there for ſeveral minutes: the creature fell into convulſions, and could not ſupport itſelf. The iris was inflamed; that of the other eye was a little ſo, but infinitely leſs. The pigeon recovered by degrees, and I then found the iris immoveable, enlarged and inflamed; the other was moveable in the light, a little inflamed, and of its natural ſize. The pigeon fell inſenſible three times, and as often recovered. The pupils and iriſſes, which were affected as I have deſcribed, came to their natural ſtate in a few hours.

Spirit of the ſecond Diſtillation applied to Wounds.

It was natural to conceive that this ſpirit would kill ſtill more readily, when applied to wounds. I made large inciſions in the legs of a pigeon, and introduced a great quantity of it, but the creature had no ſymptom of complaint.

I repeated this experiment on two other pigeons, and the reſult was the ſame.

I applied a piece of linen wet with the ſpirit, to the wounds of two others, and left it there ſeveral minutes: the pigeons were not diſordered.

I wiſhed to ſee whether it would be as inactive, when applied to the pectoral muſcles. Having laid them bare, and wounded them in ſeveral places, I immediately applied the ſpirit, and fomented them with linen wet with it. The pigeon had not the ſmalleſt complaint.

On repeating this experiment on three other pigeons, the event was the ſame.

I was on the point of concluding that the ſpirit of the cherry-laurel, in whatever way it is applied to wounds, is neither poiſonous nor mortal, although it is highly ſo when put on the eyes, or applied to the

the mouth. The ſingularity of this particular induced me to continue my experiments, and they convinced me that I had been deceived.

I raiſed a large portion of the ſkin of a pigeon's breaſt, and applied about an hundred drops of the ſpirit. A ſhort time after the pigeon fell into convulſions, and died.

I laid bare the muſcles of the leg of another pigeon, and wounded them in ſeveral places. I bathed the wounds repeatedly with the ſpirit, and kept them wet with it for eight minutes. The pigeon did not ſuffer during this interval, but two minutes after, fell on its breaſt and died.

I made a large opening in the ſkin of a guineapig's back, and made ſeveral ſuperficial wounds in the muſcles: I introduced at the opening in the ſkin, a great quantity of the ſpirit, and that repeatedly for ſeveral minutes. It was at firſt ſlightly convulſed, ſoon after it fell on its breaſt incapable of ſupporting itſelf, and died in a very little time.

I laid bare the muſcles of a pigeon's breaſt, for a great length, and made ſeveral deep wounds in them. I applied the ſpirit in ſuch a way that it could not reach the ſkin, and renewed the application to the wounds more than thirty times, keping it applied for at leaſt twelve minutes. It at length vomited, then fell into convulſions, and died very ſoon after.

It is an eſtabliſhed truth then, that the ſpirit of the cherry-laurel kills when it is applied to wounds, although it is likewiſe true that it acts much later than when applied to the eyes, and mouth, and received into the ſtomach, in which caſes a ſmaller doſe is deſtructive. It is very ſingular, and ſtill as certain, that the quantity which kills an animal, a pigeon for example, in the latter caſes, brings on no ſenſible complaint when it is applied to wounds.

Spirit of the third Diſtillation.

I gave half a teaſpoonful of this ſpirit to a large guineapig, which did not ſuffer ſenſibly from it: but three pigeons, to each of which I gave ſcarcely three drops, died; as did alſo three rabbits and four guineapigs, to each of which I gave a teaſpoonful. A large guineapig and a large rabbit ſurvived however, after ſwallowing this quantity, although each of them was viſibly diſordered.

Spirit of the Cherry-laurel of the third Diſtillation, made by mixing a quantity of decrepitated Sea Salt with the Spirit of the ſecond Diſtillation.

This ſpirit has ſcarcely any ſmell, and is almoſt inſipid. I gave not quite a teaſpoonful of it to a pigeon, which inſtantly fell into convulſions, and died ſoon after. Two other pigeons died on ſwallowing a much ſmaller quantity; ſo that the ſea

ſalt

ſalt does not ſeem to have changed the natural qualities of the ſpirit.

Phlegm of the third Diſtillation, with ſcarcely either ſmell or taſte.

I gave three drops of this phlegm to a pigeon, which did not ſuffer from it.

I gave a teaſpoonful to a large guineapig; it fell in a moment into convulſions, but afterwards raiſed itſelf and continued well.

I gave the ſame quantity to a guineapig of a middling ſize, which felt no ill effects from it.

I gave a teaſpoonful to a very ſmall guineapig, which continued well.

I made a large guineapig ſwallow a ſpoonful and an half. It vomited a little foul matter, but recovered.

I gave the ſame quantity to a ſmall guineapig: it fell inſtantly into convulſions, but ſhortly after recovered itſelf perfectly.

I gave a ſpoonful to a pigeon, which fell immediately into convulſions, and died in leſs than a quarter of an hour.

Oil of the Cherry-laurel given internally.

It remained for me to examine the oils of the cherry-laurel, and after having aſſured myſelf by reiterated

reiterated experiments, that there is no effential difference between them, whether they are of the firft, fecond, or third diftillation, I did not think it neceffary to diftinguifh them, and have therefore employed them indifferently. What was of the moft confequence to me was, to know whether this oil is likewife a poifon, and if fo, whether it is ftronger or weaker than the fpirit. I fhall on that account relate fome of the experiments I made with it on different animals, and which will fuffice to decide as to its poifonous qualities, and to fhow the frequent anomalifms which refult from the fame fubject.

I made a large rabbit fwallow two drops of the oil, united with two drops of fpirit. It died in a few feconds, flightly convulfed.

I made a land turtle of a pound weight fwallow about two drops of pure oil. Two hours after, it was become very feeble: in fomewhat more than fix hours it died with all the fymptoms of a lofs of irritability.

I gave a large guineapig four drops of the oil, from which it felt no ill effects.

I gave fcarcely three drops to a pigeon, which died in two minutes.

I made a large guineapig fwallow half a tea-fpoonful. It remained well for more than half an hour, but afterwards fell into violent convulfions, and died half an hour after.

I gave

I gave a pigeon a third of a teaſpoonful of this oil; in a very little time it could not ſupport itſelf, and died in leſs than half an hour.

I made a large guineapig ſwallow about ſix drops of the oil mixed with forty drops of ſpring water. It immediately gave ſymptoms of uneaſineſs; had ſeveral reachings to vomit; but a ſhort time after became well and continued ſo.

I gave a ſmall guineapig ſix drops with forty drops of water. It was very uneaſy; but neither fell, was convulſed, nor died.

I made a frog ſwallow three drops, and in two minutes it was quite dead: the heart ſtill preſerved its motion, and the feet ſtirred, on ſtimulating the crural nerves.

This experiment was repeated on two other frogs with nearly the ſame conſequences.

Notwithſtanding the little conformity in all theſe experiments, we may conclude that the oil of the cherry-laurel is a violent poiſon, and kills both animals with warm blood, and thoſe with cold. We may determine it to be far leſs active than the ſpirit, and that accidental circumſtances, and a diverſity in animals, are ſufficient to prevent its noxious effects. It is very ſtrange that it kills an animal with cold blood much quicker, as we have ſeen, than one that has the blood warm.

Oil of the Cherry-laurel applied to the Mouth.

I wiſhed to ſee whether the oil of the cherry-laurel, which is a violent poiſon when received into the ſtomach, would be ſo too when only introduced into the mouth and palate, without entering the œſophagus. The experiments I have related on the ſpirit of the ſecond diſtillation, led to a ſuſpicion that this may be the caſe.

I made a piece of linen a little moiſt with oil, and introduced it into the mouth of a very ſmall guineapig. I prevented it from ſhutting its mouth, to avoid the linen's being compreſſed ſo as to force the oil into the ſtomach. After keeping it in this ſtate for two minutes, the animal was as well as before.

I repeated this experiment on another ſmall guineapig, and rubbed the inner part of the mouth ſeveral times with the linen. The animal ſoon became very dull; but neither died, nor was convulſed.

I repeated this experiment on two other pretty large guineapigs, neither of which diſcovered any ſymptom of complaint: but theſe experiments are not deciſive, becauſe theſe animals are very hard to kill, and becauſe a greater quantity of the oil was perhaps neceſſary. I had recourſe then to pigeons, which are ſo eaſily killed.

I wet

I wetted a piece of linen with the oil, and introduced it into a pigeon's beak, in ſuch a way that it could not reach the ſtomach or even the œſophagus. The pigeon died ſoon after.

I repeated this experiment on four other pigeons, three of which died very ſoon. The fourth had ſcarcely any complaint.

I think that we may conclude that the oil of the cherry-laurel acts as a poiſon, even when it neither touches the œſophagus nor ſtomach; and that for this end it is ſufficient for it to be in contact with the inner part of the mouth.

The conſequences of theſe experiments are entirely analogous to what we have ſeen above in making uſe of the ſpirit of the cherry-laurel.

Oil of the Cherry-laurel applied to Wounds.

We can no longer doubt but that the oil of the cherry-laurel is a poiſon, and a very violent one too, when taken internally. It now remains to know, whether it is a poiſon when applied to the wounded parts of an animal. Experiments alone can decide this, and thoſe we have already made on the ſpirit of the ſecond diſtillation lead us to preſume, that it is likewiſe a poiſon in theſe circumſtances.

I thruſt a bit of wood dipped in this oil, into a pigeon's leg, and obſerving that after fifteen minutes the creature was not diſordered by it, I took

it

it out, and introduced a great deal of the oil into the wound, which was very deep; the pigeon notwithſtanding, neither died, nor was convulſed.

I made a wound in the body of a ſmall tortoiſe, towards its tail, and introduced the oil freely; the tortoiſe was not diſordered by it.

I made a wound in a pigeon's leg, bathing it ſeveral times with the oil, with which, put on linen, I likewiſe covered the wound. The pigeon was not diſordered by it.

I wounded the legs of a pigeon in ſeveral places, and rubbed the wounds with this oil. It did not ſuffer ſenſibly.

The conſequences of experiments on two other pigeons, three rabbits, and four guineapigs, were the ſame, notwithſtanding that I did not ſpare the oil, with which I covered the wounds repeatedly, as I had done after wounding the muſcles of theſe animals.

I wounded the pectoral muſcles of three other pigeons, and covered the wounds with the oil; neither of the pigeons were at all diſordered by it.

It appears beyond a doubt, that the oil of the cherry-laurel, which is a poiſon when ſwallowed, has not this quality when applied to wounds, of the parts at leaſt, on which I made my experiments: this is quite contrary to the nature of the viper's venom and other poiſons, which are innocent when ſwallowed, and deſtructive when applied to wounds. If any thing probable can be advanced to account for this difference betwixt the

the oil of the cherry-laurel, and the venom of the viper, I do not fee what can explain the variety of action in the former, on the different parts of an animal; this is a very fingular circumftance, and what one would leaft expect. I muft however confefs that my experiments are not altogether decifive, although I made them on pigeons; becaufe I did not employ the fame quantity of the oil, as I had done of the fpirit. I was in want of this fubftance in the greater part of my experiments, and have not till now had conveniences to make it afrefh. Notwithftanding this, it is fingular that what is poifonous when taken inwardly, fhould be innocent when applied in a much greater quantity to wounds.

Oil dried in the Sun.

I placed about two drachms of the oil of the cherry-laurel in the fun to dry, till about half was wafted; the refiduum was yellow, bitter, fragrant, and pungent. I gave about three grains of this in weight, mixed with twenty drops of water, to a pigeon, which fell a moment after, was convulfed violently, and died very foon. I repeated this experiment on three other pigeons, and the confequences were the fame. It is therefore certain, that this concrete refiduum is a ftrong poifon, and that the evaporating a part of it in the ſun, does not at all deprive it of its hurtful qualities.

The reſiduum of the oil of the cherry-laurel dried in the ſun, is a true reſin; when precipitated from ſpirit of wine, by the means of water, it is no longer poiſonous.

It has been ſeen that the concrete part of the oil of the cherry-laurel expoſed to the ſun, is a ſtrong poiſon. It does not diſſolve in water, but is eaſily and perfectly diſſolved in ſpirits of wine, It is a reſinous ſubſtance then, which retains a deleterious quality. To know whether it would ſtill retain it, if diſſolved in ſpirit of wine, and precipitated by water, I threw a great quantity of diſtilled water on this ſolution of it, and when it had ſunk in the form of a white farinaceous powder, I waſhed it in ſeveral waters. It had ſcarcely preſerved any ſmell, but when put on the tongue, and chewed, it was ſtill pungent. I gave twenty grains of this ſubſtance, whilſt ſtill wet, to a very ſmall guineapig, and as much to a pigeon; neither of which either died or felt any ill effects from it.

I repeated this experiment on two other guinea-pigs, and the event was the ſame. It follows then, that this reſin, after being diſſolved in ſpirit of wine, and precipitated by the means of water, becomes innocent, although it preſerves in a degree its ſmell and pungency; and it appears that this ſmell which is left, together with the pungent and cauſtick quality, is not ſufficiently deleterious, to kill, or produce any ſenſible derangements in animals.

Extract of the Cherry-laurel.

I made a guineapig ſwallow about thirty grains of this extract; it had no effect on it.

I made the ſame trial on a rabbit, which felt as little.

I gave about fifteen grains to a pigeon, without any ſenſible effect.

I repeated this experiment on two other pigeons, with the ſame ſucceſs. We may infer from all theſe experiments, that the extract of the cherry-laurel is quite innocent.

Empyreumatical Oil.

I made a guineapig ſwallow about twenty drops of the empyreumatical oil of the cherry-laurel; it vomited ſoon after, but ſpeedily recovered itſelf, and continued well.

I gave a ſmall pigeon twelve drops of this oil. It vomited ſeveral times, was very feeble, but ſoon recovered.

I gave about thirty grains to a rabbit. It vomited repeatedly, but was ſoon re-eſtabliſhed.

I gave to two pigeons twenty drops each; they vomited ſeveral times, were very weak, but neither died nor were convulſed.

Two guineapigs and three rabbits had the ſame vomitings, but neither of them died nor ſuffered a great deal.

We may conclude from hence, that the empyreumatical oil of the cherry-laurel, is rather an emetick, than a poiſon, ſince it does not kill even the moſt delicate animals, although exhibited to them in a very ſtrong doſe.

From what has hitherto been ſaid, the following facts may be deduced.

1ſt, That the ſpirit of the cherry-laured is a poiſon.

2dly, That the oil of the cherry-laurel is likewiſe a poiſon.

3dly, That the ſpirit of the cherry-laurel, almoſt entirely deprived of its ſmell and pungent taſte, is likewiſe a poiſon.

The poiſonous qualities do not ſeem to conſiſt in the fragrant and pungent particles, and this is further proved by the oil, dried, and afterwards diſſolved in ſpirit of wine and precipitated from that menſtruum, which is ſtill fragrant and pungent to the taſte, but is no longer a poiſon.

Beſides, the dried oil is a true reſin, and continues to be a poiſon even in that ſtate. The deleterious principle then in this reſin is deſtroyed by the ſpirit of wine.

As

As a little oil and ſome fragrancy remain in the ſpirit of the cherry-laurel, even when dephlegmated, the deleterious principle of this ſpirit may be the ſame with that of the oil, and that which is found in the reſin.

It is not beſides proved, that the oil is more powerful and active than the ſpirit. It has not however appeared invariably ſo, when given to animals.

It is true that we find the ſpirit of the cherry-laurel to be leſs active than before, when it is evaporated, and reduced to a third, or deprived of its moſt ſpirituous parts by diſtillation; in this ſtate it has loſt a good deal of its original ſmell and taſte, which may be ſaid to contribute in a great meaſure to its poiſonous qualities: but on the other hand, after the oil has been precipitated by the ſpirit of wine, it ſtill preſerves its taſte and ſmell, and is however no poiſon.

So that after all theſe experiments, notwithſtanding that they have been greatly varied and multiplied, we are ignorant in what the poiſon of the leaves of the cherry-laurel really conſiſts; we are ignorant of the mechanical action of this poiſon; and we are ignorant on what part of an animal it acts, when it cauſes its death; we have notwithſtanding learned by our experiments, and by the particulars we have related, many truths we had no previous knowledge of, and which we could not have divined: thus is the knowledge of man always accompanied by ignorance. We do not ſeem permitted to go beyond ſimple experiment, and it is to that alone that we ought to confine ourſelves. But how many caſes are there where either experiments are ſilent, or where we cannot ſucceed in conceiving any thing deciſive from them!

What however deſerves our principal attention is, the ſeeing that this poiſon, when ſimply applied in a very ſmall quantity to the eyes or to the inner part of the mouth, without touching the œſophagus, or being carried into the ſtomach, is capable of killing an animal in a few inſtants; whilſt applied in a much greater quantity to wounds, it has ſo little activity, that the weakeſt animals, ſuch as pigeons, reſiſt its action.

This circumſtance appears to me a very particular one, and deſerving of a further and very nice inveſtigation. I do not deſpair of an opportunity of returning to the ſubject, and ſhall then endeavour to give a greater certainty and extenſion to my other experiments on this poiſon, particularly to that of injecting the water of the cherry-laurel into the veins of a living animal. I found it quite innocent to the few animals on whom I tried it in this way in London, in ſmall doſes: this is very different from the action of the other poiſons.

EXPERI-

EXPERIMENTS

ON SOME OTHER

VEGETABLE SUBSTANCES.

On the Toxicodendron.

I WAS defirous of making a feries of experiments on the Toxicodendron, which the moft celebrated writers have in general regarded as a very ftrong poifon, although fome modern naturalifts have not found it fo to certain animals. But I was forced to abandon my enquiries into this plant in the very beginning, having had the misfortune to poifon myfelf three times fucceffively with the leaves of it. Thus have I dearly paid for my fcepticifm and want of precaution, in becoming, myfelf, the fubject of my experiments.

 I began

I began by expreſſing the juice of the leaves, which I made ſeveral animals ſwallow; they neither died nor fell ſick, although they ſwallowed a pretty large doſe of it. I likewiſe gave it in the form of an extract, and this preparation had no more effect than the other; the perſon indeed who gathered the leaves, had a complaint which very much reſembled an eryſipelas, particularly in the beginning. Whilſt he detached the leaves from the plant, two very ſmall drops of their milk fell on the back of his hand. Three days after, two ſmall dark ſpots made their appearance at the place where the milk had fallen, and in three days more, his whole face, eyes, and neck, began to ſwell, becoming red and inflamed; as did alſo his breaſt, and hands. He had no fever, but was neverthel eſs obliged to keep his bed for upwards of fifteen days, and the epidermis fell off by degrees in ſmall portions, cauſing a continual troubleſome ſenſation, at once of ſmart and itching. It ſeemed to me very ſtrange that ſo ſmall a quantity of this milk ſhould occaſion ſo extenſive and troubleſome a complaint, and that it acted ſo late. Other poiſons have no action when applied to the epidermis, at leaſt none that has been obſerved. I was inclined to think, I muſt confeſs, that it was an accidental complaint, ariſing from ſome other cauſe.

I was ſtill more confirmed in this opinion, on applying a great deal of this milk to the bared ſkin of ſeveral rabbits, guineapigs, and pigeons, after having made theſe animals ſwallow it mixed with the

crumb

crumb of bread, and after I had at length applied it to wounds I had purpofely made in the fkin and mufcles. I could not perceive on any of thefe trials and experiments, that it created the flighteft inconvenience to the animal. I was at length fully grounded in my perfuafion, on letting feveral large drops of this milk fall on the hands of two gardeners, who indeed at the end of three days had the black fpots I have mentioned, but were not fick. I was no longer afraid to make the fame experiment on myfelf. I fcarcely touched the back of my hand with a leaf of the toxicodendron which I had cut near the ftalk, and could with difficulty perceive the fkin to be wet at the place where I had applied it.

Three days after there appeared a dark fpot, and in three days more, my whole face began to fwell, particularly the eyelids and tips of the ears. I experienced a terrible fmart for fifteen days, and an intolerable itching for fifteen more; even the hands fmarted and itched, above all betwixt the fingers, which were become red, and were covered here and there with fmall veficles filled with a tranfparent and fharp humour: I had no fever, but my pulfe was very quick. The fkin of my face, particularly about the eyes and eyelids, was extended and filled with an aqueous fluid, and eafily retained the print of any thing that touched it. The epidermis fell off in fmall fcales, and I felt a moft troublefome itching through the whole procefs of the difeafe.

At the end of fome days, and when I thought myfelf perfectly recovered, I was making experiments on the air of the leaves of the toxicodendron, and whatever precaution I took, could not avoid touching ſome of the leaves with my fingers; this was in the parts where they had not been cut, and where there could be no ſuſpicion of there being any milk. Six days after, all the parts that had ſwelled the firſt time, ſwelled a freſh, although not ſo violently, nor did the ſwelling laſt ſo long. My eyes and eyelids however gave me a great deal of pain, and got well the lateſt.

After an interval of twenty days, I wiſhed to examine the air of ſome leaves of this plant, which I had cauſed to be got ready by another perſon, and I touched a few of the leaves when under water. In four days, my face and eyes ſwelled a third time, although not ſo violently as on the two firſt.

It cannot be believed after this that the milk of the leaves of the toxicodendron is innocent when applied to the human ſkin; but on the other hand, it is very ſingular, that an atom of this poiſon produces in a few days ſuch remarkable ſymptoms, and in ſuch remote parts, whilſt the juice and milk of the leaves are quite innocent to animals, as well externally as internally, and even when applied to wounds. Its not acting on the two gardeners, was certainly owing to their hands being extremely callous, and I did not think it proper to apply it to the parts where the epidermis was more delicate; this firſt trial was however ſufficient to aſſure me that parts which are become callous, reſiſt this poiſon.

EXPERIMENTS

With the Oil of Tobacco.

I made a ſmall inciſion in a pigeon's leg, and applied to it the oil of tobacco. In two minutes it loſt the uſe of its foot.

I repeated this experiment on another pigeon, and the event was exactly the ſame.

I made a ſmall wound in the pectoral muſcles of a pigeon, and applied to it the oil of tobacco; in three minutes the animal could no longer ſupport itſelf on its left foot.

This experiment repeated on another pigeon, ended in the ſame way.

I introduced into the pectoral muſcles of a pigeon a ſmall bit of wood covered with the oil of tobacco; the pigeon in a few ſeconds fell inſenſible.

Two other pigeons, to the muſcles of which I applied the oil of tobacco, vomited ſeveral times, all that they had eaten.

Two others with empty ſtomachs, treated as above, made all poſſible efforts to vomit.

I obſerved the vomiting to be the moſt conſtant effect of this oil, and that the loſs of motion in the lower part of the extremity to which it was applied

applied was merely accidental. Not one of the animals to which I applied the tobacco died.

Confiderations on the Nerves in Difeafes.

Let me be permitted to object for the laft time, to the too great readinefs in the modern practice of phyfick, of having recourfe to the nerves, to explain the greateft part of the difeafes incident to the human body. The ancients had fcarcely an idea of this fource of fo many difeafes, to which fome of our moft modern writers have even believed that the whole of them fhould without any exception be attributed.

My doubts only relate to the too great extenfion that has been given to the functions of the nerves, in the difeafes of the human body; and I flatter myfelf that my arguments may make fome impreffion on thofe who do not fuffer themfelves to be carried away by hypothefes which have moft frequently been adopted, merely becaufe they have never been fufficiently examined

Hoffman in the third volume of his *Medicina rationalis*, afferts that all the difeafes of the human body are nervous: and amongft the more modern writers, Mufgrave, a learned Englifh phyfician, has fupported the fame opinion. The moft moderate of the recent authors who have written on thefe difeafes, have each according to his particular fancy or

fyftem

fyftem, either diminifhed or fwelled the catalogue of them, and it is very curious to obferve that fome take the fame pains to exclude feveral difeafes from this number, which others do to prove them to be nervous.

But I muft firft eftablifh fome truths, which will ferve to make me better underftood in a matter in which there is fo much confufion and obfcurity. There is no organ in the living body, which may not be difordered by internal and external caufes, and afterwards give rife to fome difeafe. Hippocrates and the other phyficians of antiquity were well perfuaded that if any part of the human frame were difordered, it might diforder another part in confequence; but they did not on that account believe in the *confenfus nervorum*, nor in the nervous difeafes of the moderns, of which we have made mention in this work. Hippocrates was not ignorant of the power of affections of the mind on man, and how many diforders and changes they are capable of caufing in the animal economy; all this I do not to deny, and thefe are not the nervous difeafes now under examination. Befides, the nerves, as every one knows, are the inftruments of motion and fenfation in animals, and it is on them that the moft noble functions, and thofe the moft neceffary to life, depend. It cannot then be doubted but that many difeafes are nervous, and that the nerves are in many cafes the fource of very great derangements. But if this cannot be doubted, let me afk phyficians what are the certain fymptoms by which to diftinguifh a difeafe

purely

purely nervous; let me aſk why it may not be ſimply a diſeaſe cauſed by the groſſeſt humours, and how they contrive to know that the nerves are immediately attacked in the derangements of the animal economy, which they wiſh to attribute entirely to them. I do not take upon me the phyſician, but I have heard ſeveral very ſkilful ones ſay, that the ſymptoms of nervous diſeaſes are for the moſt part equivocal and deceptious.

The moderns have formed a claſs of motions, and of *ſympathetick* diſeaſes, availing themſelves of the motions of ſneezing and of the iris. We know that the famous anatomiſt *Meckel*, thought that he explained ſneezing, by the ſhocks ſuſtained by the olfactory nerves, which proceed from the optick nerve, which ariſes from the *maxillaris* ſuperior, from which is derived another nerve, which in conjunction with a branch of the ſixth pair, forms the intercoſtal. *Meckel*, then, aſſerts that the ſhocks ſuſtained by the olfactory nerves, ought neceſſarily to communicate themſelves to the whole of the intercoſtal nerve, from thence conſequently to the phrenick, and to all the muſcles of the neck, back, and loins. In fact, true ſympathetick motions ſhould be the conſequence of mechanical ſhocks ſuſtained by all the nerves, and by the communication of their organs, and it is thus that the beſt phyſiologiſts have conſidered it; but theſe two motions of ſneezing and of the iris, are purely voluntary

tary (*a*), not organical, nor ſympathetically nervous, and are not produced by external ſhocks, as anatomiſts in general have hitherto believed. It will ſuffice to read on this ſubject, my work *on the motions of the iris* (*b*), to be perſuaded of it. At leaſt I think I have produced both evidence and demonſtration on this very obſcure ſubject.

Beſides, theſe pretended nervous ſympathies are ſupported on a principle, the falſity of which has been demonſtrated by experiment: if you irritate a nerve, you communicate its motion to the branches it throws out above the part ſtimulated, and it is on this account that the great Haller, when he became a more ſkillful anatomiſt, and a nicer obſerver, either doubts, or openly denies the pretended nervous ſympathies he admitted in his youth.

Undoubtedly no one will ſay that theſe motions are nervous and ſympathetick, becauſe the mind in producing them, employs the nerves, which are the organs of motion and ſenſation. This is not the opinion of *Meckel*, nor of thoſe who explain theſe motions in a way different from mine.

There are phyſicians who account for all theſe nervous diſeaſes, by ſuppoſing that the nerves are become hard, dry, and tough: others on the contrary, believe them flaccid and relaxed in theſe caſes. "I have always found," ſays the great

(*a*) The ſenſe in which the authour wiſhes the word *voluntary* to be underſtood, may be ſeen in the work cited below.

(*b*) Printed at Lucca.

Boer-

Boerhaave, "that however eaſy it may be to imagine a cauſe for a diſeaſe in explaining it, it is afterwards as difficult to prove that this cauſe is real, and to be fully perſuaded of it."

Let not thoſe who favour the ſyſtem of nervous diſeaſes, object that the nerves accelerate and retard the motion of the blood in a thouſand caſes, as is ſeen in fear, pleaſure, and in ſo many other conditions. It is true that we cannot deny but that, after theſe affections of the principle of ſenſation take place, we obſerve changes and movements in the living body, which were not obſerved before; but this is not ſufficient to aſſure us that theſe changes are brought about by the nerves alone, and that they act immediately on the blood veſſels. The celebrated Haller, completely ſkilled in anatomy, was of opinion, as is ſeen in his excellent treatiſe, "*De Imperio Nervorum in Arterias*," that theſe veſſels were bound by nervous rings, with which he found the arteries provided in ſeveral places. But as he was alſo a great experimenter, as well as an excellent anatomiſt, he ſoon abandoned this hypotheſis, which is contradicted by occular demonſtration. A nerve, in whatever way it is ſtimulated, is not ſeen to contract, even with a microſcope, and the minuteſt blood veſſels are not obſerved to ſhrink or oſcillate when irritated by a mechanical ſtimulus; neither does anatomy convince us that there are nervous and muſcular fibres in theſe minute veſſels, which therefore ſeem

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to be destitute of all the instruments of animal motion.

Besides, we frequently see persons violently convulsed, without fever, or any sensible change in the pulse: it is true that the contrary is sometimes observed, but physiologists know that in convulsions, the motion of the blood may be accelerated by the contraction of the muscles that force it from the veins into the heart. The celebrated *Spalanzani* has observed, that the spinal marrow of frogs may be irritated, without accelerating the motion of the blood in the mesenterick vessels of these animals. I have tried this experiment on several other species' of animals, both with cold and warm blood, and have always found the event to be the same; so that the nerves appear to have no immediate action either on the veins or arteries, notwithstanding it is true that the passions of the mind excite very violent disorders in the animal economy.

The derangements I have mentioned, happen, it is true, after certain sensations have taken place in the animal; but this does not prove them to be derived from the nerves, and that the nerves have an immediate action on the organs that are in these cases disordered. It is likewise true, that the followers of Stahl, wishing to establish a general principle which might be applied to all the motions of the living machine, as well in health as in sickness, have had recourse to the mind, as the prime mover of the whole animal economy: I do not pretend to combat the exiſtence of the nervous diſeaſes in Stahl's hypotheſis, according to which all the animal motions are to be regarded as purely nervous, and all the derangements excited in any way whatever in the animal economy, as nervous diſeaſes. Nervous complaints are the immediate conſequence of ſhocks which the nerves may have ſuſtained, or of extraordinary affections excited in the principle of ſenſation; and I here principally conſider the firſt claſs of theſe diſeaſes. We ſee indeed that Boerhaave admits of nervous diſeaſes, although he afterwards taxes Stahl's ſyſtem with falſehood. Haller himſelf is of the ſame opinion; and the two moſt modern authours who have written on nervous complaints, De la Roche and Tiſſot, do not heſitate even to reject the more rational ſyſtem of the learned Engliſh phyſician, Whytt, on the principle of animal motion, and both of them notwithſtanding give a greater or leſſer ſupport and extenſion to nervous diſeaſes.

In a word, I do not believe any one will aſſert, that any motion whatever, any accidental and ſecondary derangement, is a true nervous diſeaſe, becauſe it ſucceeds to a ſenſation excited in the mind. In this caſe it muſt likewiſe be ſaid, that the agitations which are cauſed by fear, pleaſure, and grief, are nervous diſeaſes: thus, for example, a laborious and painful reſpiration, which renders the dilatation of the thorax neceſſary, will be a nervous diſeaſe, notwithſtanding there is no affection of the nerves in this caſe, and that the phyſi-

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cian would not certainly ſeek to apply a remedy to an organ, which was not abſolutely injured.

I have pointed out in ſeveral parts of this work, that there are poiſons, which when applied immediately to the nerves, do not occaſion any complaint in a living animal. I have likewiſe pointed out that theſe poiſons when introduced into the blood, without touching any ſolid part, ſuddenly bring on violent convulſions, and the moſt decided ſymptoms of what are believed to be nervous affections. Laſtly, I have ſhown, that when the wounds of animals are envenomed with theſe poiſonous ſubſtances, the animals are ſeized with all the ſymptoms of nervous complaints.

On another hand, I have already made it appear, that convulſions may be excited in animals, without the nervous ſyſtem being in the leaſt affected, and that the want of an equilibrium in the ſtrength and in the humours, is ſufficient to produce the moſt violent convulſions in the muſcles (*a*).

Here are then all the ſymptoms of nervous diſeaſes, without the nerves having any ſhare in them; and we ſee at the ſame time, that theſe contractions are excited even when the cauſes of them only ſeem to act on the humours of the animal, and whilſt theſe cauſes are found innocent and ineffectual to the nerves, in whatever way they are applied to them. It is not ſufficient then to ſee

(*a*) See the firſt part of my work on the motions of the iris, quoted above.

all theſe ſymptoms, to enable us to pronounce with certainty, that the diſeaſe is purely nervous.

But there is an argument which ſeems to leave no refuge to the moſt prejudiced and moſt obſtinate ſkeptick. It is obſerved in regard to the motions, as well involuntary as ſpontaneous, which are wrought by the nerves, that if you ſtimulate the nerves which are carried to the organs of theſe motions, the latter will conſtantly and neceſſarily follow. This is a general law for all the muſcles and nerves, in all animals, and admits of no exception. It becomes then a certain law, a ſure principle, and an infallible criterion of the nature of theſe motions.

The heart, of all others, is the organ firſt affected in the paſſions of the mind and in nervous affections; and it is on this firſt change, that a number of others which accompany it depends. Let the breaſt of an animal with cold blood be opened, (this experiment is ſubject to leſs uncertainty in theſe animals than in thoſe with warm blood, on whom the effect is however the ſame,) and let the nerves that are ſent to the heart be ſtimulated, in any way whatever; it will not on this account quicken its contractions if it is in motion, nor will recover its motions if it is at reſt, although it is ſtill in a ſtate of contracting on the leaſt ſtimulus offeredto its fibres. It is in vain that long pins are introduced into the vertebral canal, and that the ſpinal marrow and brain are mangled, the heart continues ſtill inſenſible. The nerves then

then which are carried to the heart, are in no way the organs of motion in this muſcle, although they are certainly ſo in all the others. They can therefore never cauſe any ſenſible change, however the animal may be affected. The experiment is certain, and the conſequences of it direct *(a)*, and it would be a downright contradiction, that the motions of the heart ſhould be brought about by the nerves, whilſt the latter are not capable of exciting theſe motions, as has been experimentally demonſtrated.

It cannot then be advanced with certainty that the changes in the heart which uſually accompany the affections of the mind, are wrought by the immediate channel of the nerves, and not otherwiſe; and the only inference which an unbiaſſed philoſopher can draw from all that we have ſaid is, that we are ignorant through what medium, and by what mechaniſm, the affections of the mind influence the heart.

It may perhaps be ſaid that the *ſenſitive principle* in the animal may make certain impreſſions on the nerves, which mechanical ſtimuli cannot imitate; but this ſuppoſition is contradicted by daily experience, ſince the leaſt ſtimulus to a very ſmall nerve at the extremity of a muſcle, is ſufficient to give it motion: this is a demonſtrated truth which has never been contradicted by obſervation.

(a) This important truth has been demonſtrated by the author in his treatiſe on Animal Phyſicks, Volume. 1 ſt p. 92. publiſhed in Italian in Florence at 1775.

It may be oppoſed that the experiment of the immobility of the heart to the ſhocks that are given to the nerves, is contradicted by ſeveral of the moſt famous naturaliſts: there is no other reply to this than the recurring to experiment. Any one in doubt, may eaſily ſatisfy himſelf by taking a frog and, after opening its breaſt and cutting off its head, wait till the heart is ſtill, or moves but ſlowly, to render the experiment more deciſive, and then thruſt a pin deep into the ſpinal marrow: he will ſoon ſee the event. Care muſt be taken not to leave the frog to itſelf, but to ſecure the feet well, otherwiſe there will be a riſk, amidſt the violent convulſions that will be excited in all the other muſcles, that the heart itſelf may be agitated, and cauſed to move in quite another way than by the action of the nerves. This is undoubtedly what has deceived ſo many good anatomiſts, who have believed that this motion was occaſioned immediately by the nerves. I refer my readers to the experiments related on this ſubject, in the work cited above.

It is in my opinion then, a matter demonſtrated by the fulleſt evidence, that no motion of the heart can be in any caſe brought about by the medium of the nerves, although the heart is, of all the muſcular organs, the moſt ſuſceptible to the affections of the mind. Some grounds may after this be permitted me for doubting that whilſt the motions of the heart are never influenced by the nerves, thoſe of the other muſcles are invariably cauſed by their immediate action.

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A thoufand like arguments may be brought in favour of the hypothefis of the nerves; but they are all indirect, and only prove that a fenfation in a living animal, is followed by a motion in its body: this is not yet fufficient to demonftrate that the effect has been immediately produced by the action of the nerves. Fear retards or quickens the motion of the heart; and yet, as has been feen, the nerves have no immediate action on that organ, notwithftanding it be true that this change is the confequence of a fenfation.

The learned tranflator in French, *(a)* of the new edition of Whytt's Englifh publication on nervous difeafes, introduced in the 151ft page of the firft volume, a note againft his author, which feems to me fo very appofite to the prefent fubject, that I give it entire. Whytt had maintained that hyfterical difeafes, (or rather their fymptoms) and the hypochondriacal difeafes of men, were in effect the fame; and that both of them were fimply nervous. The tranflator obferves in his note that "this is a fure me-
"thod to confound and perplex." He adds that
"the fymptoms Whytt has related, are effects
"which in a greater or lefs degree are common to
"all difeafes. There is not any one in which the
"nerves are not affected; but the bufinefs of a
"phyfician confifts in knowing, as well as he can,
"the caufe of the difeafe. It is certain then, that
"three fourths of the difeafes of women have their
"origin in the matrix. The ancients were therefore

(a) The tranflation was printed at Paris in 1777.

" not miſtaken when they beſtowed the epithet of " hyſterical, on thoſe diſeaſes in women in which " the nerves diſplay the greateſt number of ſymp- " toms."

Several ſubſtances are announced in the *Materia Medica*, as *nervines*, becauſe they are ſuppoſed to have a benign influence on the nerves. For my part, I believe it would be very difficult to prove clearly, that a remedy has an immediate action on the nerves, and not on the other parts of the body. I do not mean however to ſpeak here, of certain ſubſtances which, being capable of diſſolving, corroding, and gnawing the nerves, have aſſuredly a true immediate action on them. It is true, that ſpirituous ſubſtances applied to the noſtrils, ſeem to act inſtantly on the nerves, and in a very different way; but it muſt be conſidered, that they not only cauſe a ſimple mechanical percuſſion on the pituitous membrane, but excite a particular ſenſation in the animal organization. The natural light of the ſun, in whatever way it is applied to the body, is quite innocent, and is not felt by it; but meeting in ſudden contact with the eyes, may make ſuch an impreſſion on them, as to cauſe a violent pain, and even tears. The eye alone is ſuſceptible of the impreſſions of light, and the other parts, although endowed with ſenſation and life, are inſenſible to it. The difficulty then I have juſt mentioned, only proves that a ſtronger percuſſion on a determined organ, excites a ſtronger and quicker ſenſation in the animal machine, than a weaker

weaker one, which is altogether natural, and argues nothing in favour of the hypothesis of the nerves.

But who can assure us that the particles of scents may not penetrate, even in a few instants, through a quite porous body, filled with canals and fluids incessantly in motion?

I know it is commonly believed that opium produces effects, when introduced into the stomach, which are not observed when it is applied to the other parts of the body. But who again will here assure us, supposing it an incontestible fact, that certain juices which are only found in the stomach, are not requisite to detach the most active parts of this substance, and that this viscus does not contain very fine vessels or porosities, proper to receive them, which are not found elsewhere? I am not ignorant of its having been asserted, that opium, when it is immediately applied to the nerves, produces a palsy of the muscles: but I recollect to have seen some years ago, a disorder of this kind clearly occasioned by the spirit of wine in which I dissolved the opium, since it did not ensue when I tried it dissolved in water. This circumstance appears to me so interesting, that I shall not fail to repeat the experiment at my leisure, particularly as the authority of Monro, who has found the contrary, may be opposed to mine.

There are in the living body passages we are yet ignorant of, unknown powers, and latent principles. We see the necessity of admitting them, but we are ignorant of their nature and mechanifm. If it is a misfortune to be in the dark as to a truth, it is a ftill greater one to fubfcribe to an errour. We do not attempt to draw erroneous confequences from things we are ignorant of; but a miftake neceffarily leads us into many others. It is much better then to be ignorant of a truth, than to admit an errour.

I have frequently, in many parts of this work, fpoken of the influence of the nerves in difeafes. I have faid too little on this fubject, in proportion to its importance, and certainly a great deal too much in a work that has quite another object in view; but I could not refift the evidence my experiments held out to me. They have led me, in fpite of myfelf, to make fome applications to certain phenomena in the animal economy.

I do not pretend here to undeceive thofe who are prejudiced in favour of a principle which fupplies all the wants of a fyftem of medicine, with a readinefs proportioned to the uncertainty and obfcurity of that fyftem; and I am not to learn that it is a great help to thofe who profefs it. The character of obfcure and indeterminate hypothefes is, that they adapt themfelves to every thing, becaufe they are capable of being modified according to particular cafes; but this is exactly what ought to make them fufpected.

I ought however to make an exception here in favour of feveral very fkilful phyficians, who have frankly acknowledged that my experiments render the nature of nervous difeafes in general very fuf-

pected.

pected. I ſhall ſelect from the names of many others, that of the celebrated Engliſh phyſician Sir Robert Pringle, who told me that he had never too great a belief in nervous diſeaſes, and that for the future he ſhould have leſs faith in them than ever.

I repeat that I will not deny, as the great Albinus has done, but that the nerves may in general be the organs of ſenſation, or of motion, in animals: this would be going too far; but it may be doubted that all the motions we obſerve in animals depend immediately on the nerves, and that the ſubſtances which have excited them, as has been pretended of the venom of the viper, the *ticunas*, and the cherry-laurel (to oppoſe the action of which all my reflections ultimately tend) have immediately acted on the nervous ſyſtem. Neither can it be denied, but that when the *principle of ſenſation* is affected, a thouſand diſorders in the animal economy enſue, any more than it can be affirmed that the nerves can be acted on with impunity; but it does not yet follow from this, that all the diſeaſes commonly aſcribed to the nerves, are owing to the nerves alone; that they may not rather depend on the humours; that both medicines and poiſons act immediately on the nerves; and laſtly, that the nerves (it is to this that I principally confine myſelf) have an immediate action on the other ſolids.

Irritability ſeems independent of ſenſation in an animal; and there is beſides nothing which demonſtrates

monſtrates that the muſcles are only capable of moving by the action of the nerves. The principle of ſenſation and the nerves, may have agreements with the blood and other humours, which we are yet ignorant of, and theſe humours, more or leſs changed, may exert their influence againſt the ſolid parts of an animal. It is permitted to ſuppoſe every thing, rather than to reſiſt a direct and luminous experiment. It is permitted, when we are about to eſtabliſh a truth, to imagine a new conſtruction of parts and of organs, and to ſurmiſe new agreements. But to invent a new conſtruction, and to admit unknown agreements, in ſupporting an hypotheſis, is building the enchanted palaces of Arioſto, for the reception of Roger and Alcine.

EXPERI-

EXPERIMENTS

MADE AT LONDON IN 1778 AND 1779.

On the Reproduction of the Nerves.

THE knowledge I had acquired of the true ſtructure of the nerves, and of the primitive cylinders of which theſe organs are formed, as will be ſeen in the following treatiſe, made me deſirous of applying this knowledge to the animal economy. During my ſtay in London, I did not fail to examine the muſeum of the celebrated Dr. Hunter. Mr. Cruikſhanks, a young man very promiſing in the ſcience of anatomy, and diſſector to the Doctor, there ſhowed me a glaſs, in which he told me was preſerved a reproduction of a dog's nerve, one of the eighth pair, which he had cut. The circumſtance appeared to me altogether new, and deſerving the utmoſt attention.

He

He added, that the nerve had been cut in the living animal, and that he had removed a portion about an inch in length; and indeed, for the ſpace of about an inch, it appeared very different from what it was in all the other parts. It was much enlarged, quite irregular and uneven, and ſeemed formed of a different ſubſtance from the reſt.

On ſeeing this preparation of Mr. Cruikſhanks', I had two reaſons for doubting the truth of the fact. One was, that I had never obſerved in any one of the experiments I made at Paris on the venom of the viper, a true reunion of parts in the ſciatick nerves, which I had ſo often cut. The other, that in the nerve in queſtion, there might very well be a reunion of one part to the other, but not a true reproduction of the two extremities, ſo as to form a ſingle nerve as it was before.

Theſe ſuſpicions made me deſirous of a particular converſation with Mr. Cruikſhanks, during which I aſked him, amongſt other things, what was Dr. Hunter's opinion on the ſubject. He told me ingenuouſly, that the Doctor did not perceive a real reproduction of the nerve in theſe experiments, and ſuſpected very ſtrongly, from the great difference betwixt the external ſtructure of the part cut, and that of the other parts, that it was not the caſe. I then underſtood from Mr. Cruikſhanks, that he had at different times, in the ſpace of eighteen or twenty days, divided the two nerves of of the eighth pair, and the two intercoſtal ones, in the ſame animal, and that they all ſeemed to be equally reproduced.

It is beyond a doubt, that the cut extremities of the eighth pair, and of the intercoſtal nerves, are capable of reuniting, although a part of them be taken away, and Mr. Cruikſhanks' excellent experiments demonſtrate it in ſuch a way, that it cannot be doubted a moment; but it is not yet certain that theſe nerves return to their prior ſtate, by forming a continuance of a true nervous and medullary ſubſtance, and continue to perform their uſual functions. This abſolutely remains to be proved. It is true, that a continuance of life in the animal, after the nerves have been cut, as well as there being no ſenſible change in the operations of the heart, leads one to ſuſpect that the eighth pair of nerves has been truly and full reintegrated; but as it has not been yet proved that the nerves are abſolutely neceſſary to the motion of the heart, and as it is known that this viſcus receives nerves from other parts, it is to be doubted, whether this ought to be regarded as a true renewal of the nerves, or ſhould be only deemed a ſimple union of parts, brought about by the means and interpoſition of an heterogene ſubſtance, compoſed of the cellular membrane. My obſervations on the ſtructure of the nerves enabled me to aſcertain with certainty, whether they are really reproduced or not, and this has engaged me to make ſeveral experiments on the ſubject. I preferred rabbits, as the moſt convenient for ſuch trials, and eaſily to be procured. I deſtined a great number for the cutting out the ſciatick and crural nerves, many others for that of the eighth pair, and ſome for that of the intercoſtal and eighth pair together.

In ſix rabbits, I ſimply cut the right ſciatick nerve; and removed a portion of about ſix or eight lines in length, in ſix others. Some of them lived eighteen or twenty days, and others died in the ſpace of four or ſix. Others lived ſo as to enable me to examine the nerves I had cut, at the end of thirty days or more.

I could not perceive the ſmalleſt appearance in any of theſe animals, of a nervous reproduction. In all of them the extremities were as ſmooth and even as when I firſt cut them. The nerves were in every part white, and neither thickened nor uneven. In a word, I had the fulleſt aſſurances that in the animals I had employed, there was no reproduction of the nerves.

I muſt obſerve here, that I might eaſily have been deceived in two particular caſes, had it not been for the knowledge I had acquired of the ſtructure of the nerves and muſcles. In one of theſe I had ſimply cut the ſciatick nerve, in the other I had removed a portion of it of about ſix lines in length. In both I could not diſcover the two extremities of the nerves, and I found them perfectly covered and bound together with a ſubſtance partly cellular and partly fleſhy. What was very

ſingular,

fingular, the more I removed this fubftance with a fcalpel, the more the union and reproduction of the nerves feemed to be really brought about. But the microfcope foon freed me from this fufpicion, and I at length found that this fubftance was not formed of the primitive nervous cylinders of which I fhall fpeak in the enfuing treatife, but of a cellular membrane, and of primitive flefhy cylinders.

The obfervation of thefe latter particulars made me fufpect that the intercoftal nerve and that of the eighth pair, had only prefented an apparent reproduction, becaufe in all the cafes where I had cut the fciatick and crural nerves, there was not the fmalleft appearance of a reunion or reproduction of the parts.

'Tis true that the conftant reftleffnefs of the animal may prevent a reunion of the divided nerves in thefe cafes; but the two extremities of them fhould at leaft appear a little changed and rounded, as happens in all the parts where there is a reproduction or reunion, after their being cut.

But it ftill belongs to experiment to decide; and we muft not form conjectures where that can be recurred to.

I cut one of the eighth pair of nerves in a dozen rabbits, and in a dozen others removed a portion of it of fix to eight lines and more in length; in thefe laft I alfo removed an equal portion of the intercoftal nerve. One of the firft died in four days, two of the fecond in three, and a third in eight. Thofe

which

which ſurvived, had no perceptible complaints, and até as uſual a ſhort time after the operation.

In ſome of theſe rabbits, I cut at the end of twenty-five days the other nerve of the eighth pair, and in ſome others removed a portion, as well of the nerve of the eighth pair as of the intercoſtal. Of ſix of theſe laſt, three died in a few days.

This is not the place to give a detail of all I obſerved in theſe animals; I ſhall for the preſent content myſelf with relating a few general obſervations.

In two of them I could obſerve no change in the divided nerves, although I examined one at the end of eighteen days, and the other at the end of twenty-ſeven. In a third I obſerved that the cut extremities of the nerve had changed their ſhape and colour, but there was no true reunion, nor apparent nervous reproduction.

In a fourth, which died twenty-three days after the operation, the extremities of the nerves were a little elongated in a conical ſhape, but were not united: there was indeed a flat membrane betwixt the ends of the nerves, which united them imperfectly. In all the others where the nerves had been ſimply cut, the parts ſeemed to be united, had changed their colour, and were thickened. They were in general covered with a cellular membrane, enlarged, and a little red.

As to the nerves of which I had removed a part, there was a reunion which likewiſe ſeemed to be

cauſed

caused by a cellular substance much swelled, unequal, and full of blood vessels. The cut extremities of the nerves were whiter than in any other part.

I sought for the spiral form of the nerves in these reproduced and unequal parts, and although I thought I distinguished in a greater or less degree, in more than one, the white spires or bands *(a)*, I could not however distinguish them from one extremity of the nerve to the other, so that I was uncertain whether the part reproduced was not rather cellular than nervous; neither could I assure myself by cutting the cellular membrane, a part of which I likewise removed, whether the primitive nervous cylinders really passed from one part of the nerve to the other, although I saw them extended through this reproduced cellular membrane. I confess that I had not all the conveniences necessary to render me certain in so important an enquiry, and which I found at the same time a very difficult one.

All that I can say with certainty is, that the cut ends of the nerves are prolonged, that they alter their shape and colour, and that they are united by a substance betwixt them, which is a prolongation of the cellular membrane itself belonging to the two cut parts of the nerves. The winding cylinders and blood vessels pass from one part to the other, and

(*a*) It will be seen in the following treatise, what this spiral shape or these white bands are, of which mention is here made.

there is an union of the whole, as if the cellular coat of the nerves, which is much thicker and more unequal than in the reſt of the nerve, was quite entire.

The difficulty of eſtabliſhing by immediate and ocular demonſtration, whether in the above caſes the nerves are really reproduced, or are ſimply faſtened together by a cellular membrane, made me redouble my attention, and multiply my experiments.

I do not think I am wrong in advancing at this time, as a certainty, that a like reproduction of the nerves may take place; although it cannot be always demonſtratively proved, and perhaps is not always brought about, although the nerves ſeem to be reunited, and reproduced.

I can ſay to a certainty that I obſerved it in two particular caſes, and in one of the two in ſo evident and ſure a way, that I think it proper to give here the figure and deſcription of it. But I repeat again, that a ſimple continuity of parts betwixt the cut ends of a nerve are not ſufficient to determine whether the nerves are really reproduced, and that it is moreover not ſufficient that a cellular ſubſtance is produced and elongated, althought it be a continuation of that of the nerves themſelves. We muſt be certain that the nervous cylinders paſs without interruption from one part to the other.

Plate VII. Fig 3. repreſents a nerve of the eighth pair belonging to a middle ſized rabbit; I viewed it with a lens which magnified about three

time s

times in diameter. The part of this nerve I had removed, was about ſix lines in length, and I diſſected the animal twenty-nine days after the operation. I found a reunion of the two cut extremities of the nerve, but that the nerve was ſmaller than elſewhere at the part where the reunion took place, which I have marked *r*, *r*.

At ſome diſtance from the point *r*, *r*, preciſely where the nerve was cut, were ſeen two white ſpots *n n*, *n n*, as they are repreſented in Fig 3 and 4. Theſe two ſpots formed two opake rings round the nerve, and cloſe to theſe rings the nerve began to decreaſe on each ſide in a conical ſhape, and to prolong itſelf in this way as far as *r*, *r*, where both cones met. In Fig. 3, and ſtill better in Fig. 4, are ſeen the ſpiral bands of the nerve, and theſe bands were continued as far as *r*, *r*, where they were not ſo diſtinctly ſeen.

At the two ſpots *n n*, *n n*, the bands ſeemed to be interrupted, or rather, the white colour of the nerve in this place prevented their being ſeen.

The nerve was ſmooth throughout, and was likewiſe ſo all over the two nervous cones. I wiſhed to ſee the nerve through a ſtrong lens, and to examine the cellular membrane. Fig 5, repreſents the nerve obſerved with a very ſtrong lens. I found it covered with the uſual cellular membrane. I then examined it with the ſtrongeſt glaſſes, and as is ſeen in Fig. 6, found it formed of primitive nervous cylinders, of which as will be ſeen in the enſuing treatiſe, every nerve is compoſed. Theſe cylinders diminiſhed in diameter as they approached each other to the point *r*, *r*, of the two cones, and they were plainly diſtinguiſhed to be continued, and to paſs from one ſide to the other. Fig. 7, repreſents the ſame nerve, but partly torn by needles, to ſhow more diſtinctly the continuity of the primitive nervous cylinders.

Two things concur to perſuade me that a true reproduction of the nerve is brought about. One is, the appearance of the ſpiral bands, which are found even in the renewed and ſmalleſt part of the nerve; the other is, the continuity of the primitive nervous cylinders, which removes the ſmalleſt ſuſpicion of doubt.

I have had another inſtance of a reproduction, almoſt exactly ſimilar to that I have deſcribed. The two white ſpots were likewiſe ſeen here, and at the part cut, the two cones which met at their points. The ſpiral bands were continued in the cones, and in the continuity of the primitive nervous cylinders was diſtinguiſhed in every part of the nerve.

It is an eſtabliſhed truth then, that the nerves of the eighth pair are capable of a reunion, not only when they have been cut, but likewiſe when a portion of them, ſeveral lines in length, has been removed. In the firſt caſe, there is a true reunion of parts, a real continuity of ſubſtance, and in a word, a complete continuance of the primitive nervous cylinders and of the external coats which encloſe them. In the ſecond caſe the nerve is reproduced, that is to ſay, its nervous ſubſtance is encreaſed at the two extremities, and in the prolongation of it, theſe extremities meet, ſo as to form an homogeneous, continued, and uniform whole.

It is ſingular that the two extremities of the divided nerve, meet ſo exactly as to be capable of uniting together; particularly when a very large portion of it, an inch for example, has been removed. In theſe caſes, it ſeems altogether improbable that the cut parts ſhould meet ſo well, and this junction is rendered ſtill more difficult by the great derangement in the ſituation of the nerves, that attends the operation. But we muſt in the firſt place conſider, that all the parts of the neck, the muſcles particularly, continuing their accuſtomed functions, oblige the nerves to regain that ſituation which theſe parts and theſe motions require.

Again, I muſt here obſerve, that having twice expreſsly changed the direction of the cut extremities, ſo that they preſented themſelves contrary ways, I did not find in the ſequel that theſe parts were reunited, or had met each other.

Had I had more time, I ſhould have endeavoured to determine whether this faculty of reproducing, in the nerves of the eighth pair, and the intercoſtal ones, be common to many other nerves; which ſeems probable. The ſciatick nerves, which are probably of the ſmall number of thoſe that have not the advantage of reproducing, are perhaps prevented from it, from there being too much motion in the parts where they are ſituated, and it would in all likelyhood enſue if that motion were diminiſhed. It may likewiſe be a property only belonging to thoſe nerves that are the moſt eſſential to life; but all these points may easily be cleared up by immediate experiments.

Every one must now see, that a great many interesting truths to the practices of physick and surgery result from what I have just related. We may now conceive how sensation and motion itself have returned to certain parts which were almost entirely detached from the bodies of animals. The nerves were in these cases reunited, and continued to be the instruments of motion and sensation. In many cases of urgent necessity, there will be less dread of cutting some particular nerve, in doing which it will be only necessary to take care that the cut extremities be placed opposite to each other.

There is a physiological experiment to prove the the reunion of the divided nerves, which want of time has prevented my making. After cutting the phrenick nerves, if once a reunion of the divided parts takes places perfectly, and there is a true continuity of the nervous substance, the diaphragm should contract itself, on irritating these nerves, in the part towards the head, above where they were cut.

OBSERVATIONS

ON THE

PRIMITIVE STRUCTURE

OF THE

ANIMAL BODY.

TO WHICH ARE ADDED,

REMARKS ON VEGETABLES AND FOSSILS.

Obſervations on the Structure of the Nerves, made at London in 1779.

OF all the organical parts of which a living animal is formed, there is no one in my opinion, the ſtructure of which is leſs known, and the knowledge of which at the ſame time is more important than that of the brain, and the nerves that are derived from it.

The beſt authours have advanced nothing but mere hypotheſes on theſe parts, and the moſt penetrating naturaliſts have made obſervations which have been contradicted by other naturaliſts equally ſkilful; ſo that after an unprejudiced examination of the whole, we are forced to confeſs the having learned nothing, and that the texture of theſe organs is obſcure and uncertain.

The celebrated Haller, after comparing the different opinions of anatomiſts as to the ſtructure of the nerves, and principally examining Lewenhoeck's obſervations on the ſtructure of theſe parts, ingenuouſly confeſſes that nothing but ſimple conjectures can be advanced thereupon. He however is diſpoſed to believe that the ſtructure of the nerves may be cellular.

Among the latter naturaliſts who have examined the animal body, there are two particularly, who deſerve our notice, the learned Father della Torre, known by ſeveral philoſophical works, and ſtill more by the microſcopical obſervations he has at different times publiſhed, and M. Prochaſka, a ſkilful profeſſor of anatomy at Prague, who has given us two very intereſting microſcopical works, one on the fleſhy fibres, the other on the ſtructure of the nerves.

Father della Torre (*a*) examines the two ſubſtances, cortical and medullary, of the cerebrum

(*a*) Nuove Oſſervazioni Microſcopiche. Napoli, 1776.

and

and cerebellum; he likewiſe examines the medulla oblongata, medulla ſpinalis, and laſtly the medullary ſubſtance of the nerves. He finds that all theſe organs are nothing more than a maſs of numberleſs globules, tranſparent, and ſwimming in a diaphanous fluid. Theſe globules, he ſays, are very ſmall in the medullary ſubſtance of the nerves, in which they are placed in almoſt a right line, ſo that they ſeem to compoſe ſimple threads and fibres, inſtead of which in the cerebrum they are very large, leſs in the cerebellum, and ſtill leſs in the medulla oblongata and medulla ſpinalis, where they are not placed in a right line, but mixed confuſedly together.

Prochaſka (*a*) does not admit of any difference betwixt the cortical and medullary ſubſtances of the brain, finding both of them to be formed of an immenſe quantity of globules united together by an elaſtick and very tranſparant cellular membrane. He does not agree with Father della Torre as to the difference in ſize of theſe globules, but obſerves with him, that they are diſpoſed in the nerves in a right line, and appear like a longitudinal fibrous ſtructure.

Albinus, who joined the uſe of the microſcope to that of the moſt ſubtil injections, denies that the cortical and medullary ſubſtances of the brain are purely vaſcular. Others have not only conſidered the ſubſtance of the brain, but likewiſe that of the nerves, as a nonorganick ſubſtance, and a mucous

(*a*) Structura Nervorum. Vindoborn, 1779.

pulp.

pulp. Others again have taken it for a ſubſtance purely cellular. And all of them have thought the uſe of the microſcope neceſſary to aſſiſt the naked ſight.

By the little that has been ſaid, it is eaſy to perceive how great an uncertainty we are in, not only in regard to the ſtructure of the nerves, but that of the brain itſelf. The obſervations of Father della Torre and M. Prochaſka however deſerve our conſideration. Independent of their ſkill in obſervation, they were not ignorant of all that other naturaliſts had ſeen, or had believed to ſee, before them. They were therefore leſs likely to be deceived, in running over a beaten path, and ſo much the more deſerve our utmoſt conſideration, as entirely agreeing in regard to the primitive ſtructure of theſe parts, which both believe to be formed of ſimple globules.

Being in London in 1779, I heard that the celebrated anatomiſt, Mr. Monro of Edinburgh, had made ſome important diſcoveries in relation to the ſtructure of the nerves; but as I was ignorant not only of the detail, but likewiſe the conſequences of his obſervations, I addreſſed him in the following manner:

"SIR,

"Although I have not the honour to be perſon-
"ally known to you, I take the liberty to requeſt
"ſome

" ſome information as to the intereſting diſcoveries " in anatomy, which I am told you have made on " the ſtructure of the nerves. I am informed you " have publiſhed ſomething relative thereto in a " journal, and in two papers read to the Medical " Society at Edinburgh. As I am buſied on this " ſubject, I ſhall be glad to know how far you " have carried your reſearches, that I may be en- " abled to render you all the juſtice you deſerve, " provided I determine hereafter to give my ob- " ſervations to the publick. I regard your diſ- " coveries as already publiſhed, and conſequently " anteriour to mine, but ſhould I write on this ſub- " ject before I am fully acquainted with them, it " would be out of my power to attribute to you " all that is your due, and by paſſing over your " works in ſilence, I ſhould incur the ſuſpicion of " wiſhing to appropriate to myſelf the diſcoveries " of others. You can run no riſk in communi- " cating to me what you have done, ſince on one " hand, you have already made your diſcoveries " known to a publick body, and on the other, my " letter will be always a ſecurity to you, againſt " any improper uſe I might make of your com- " plaiſance. Men of real merit are rarely ſuſpi- " cious and ſhy: this induces me to hope that you " will beſtow ſome favour on one who renders you " the greateſt juſtice, and who deſires to be in- " ſtructed by the diſcoveries with which you have " enriched and advanced the ſcience of anato- " my."

Having

Having received no reply form Mr. Monro, and fearing that my letter had not reached him, I conveyed a copy of it by his pupil Mr. Crawford (*a*), who was then in London, begging him to have it delivered by ſome truſty perſon into Mr. Monro's own hands: it was quite ineffectual, as I had no reply from this celebrated profeſſor of Edinburgh.

I heard in the mean time, that mention was made of Mr. Monro's diſcoveries in the firſt part of the ſixth volume of a journal, entitled, Medical and Philoſophical Commentaries by a Society in Edinburgh, printed in London in 1779. Not being able to obtain any information on the ſubject from Mr. Monro himſelf, as I had flattered myſelf, and ſhould have wiſhed, to do him every juſtice, I am under the neceſſity of tranſcribing from the commentaries I have mentioned, the article relative to the profeſſor's diſcoveries, which, to be more certain, I do at full length. " Dr. Alexan-" der Monro, profeſſor of anatomy at Edinburgh, " has lately taught in his lectures many particulars " reſpecting the brain and nerves, which are en-" tirely new, and which muſt lead to very different " opinions reſpecting theſe organs, from what any " phyſiologiſts have hitherto entertained: he has " alſo read a paper on the ſame ſubject in the phi-" loſophical ſociety of Edinburgh.

(*a*) Authour of an excellent publication on occult heat.

,, Of

"Of his defcriptions, which are founded en-
"tirely on microfcopical obfervations, conjoined
"with nice diffection, and which are illuftrated by
"numerous engravings, we cannot at prefent pre-
"tend to give a fufficient account. We may only
"obferve, that he finds the ftructure of thefe parts
"to be very different from what was formerly ima-
"gined. He has difcovered that the brain and
"nerves, in all claffes of animals, in place of
"ftraight fibres, are every where compofed of con-
"voluted fibres, nearly $\frac{1}{9000}$ parts of an inch in
"diameter, which do not feem to be hollow, but
"folid.

"He finds that their extent in the fyftem is
"much greater than has even been believed; and
"that they not only enter the compofition of parts
"intended for fenfe and motion, but alfo of every
"other part of the body. Thus he has difcovered
"them penetrating to the very extremities of the
"longeft hairs; and in great numbers entering
"the compofition of the cuticule and nails. He
"farther alledges, that the bulk of all our organs
"depends chiefly on their nerves; and that when
"mufcles or bowels are cut tranfverfely, many
"more nerves are divided than when the fame ope-
"ration is done upon the cord, called by anatomifts
"the nerve of that part."

"He finds alfo, that a fyftem of convoluted
"fibres, in every refpect analogous to the nerves of
"the human body, is to be difcovered throughout
"the whole vegetable kingdom. Nay, that the
"metals,

" metals, ſemi-metals, earths, and ſalts, conſiſt al-
" moſt entirely of convoluted and ſerpentine fibres,
" ſimilar to the nerves of animals in ſize and
" ſhape.

" How far the teſtimony of future obſervers will
" confirm Dr. Monro's deſcriptions, is not for us to
" determine. But we may venture without heſita-
" tion to aſſert, that if the account which he has
" given ſtands the teſt of ſceptical ſcrutiny, it muſt
" be conſidered as the greateſt anatomical diſcovery
" which has been made for many years."

Mr. Monro's diſcovery ſeems to me principally to conſiſt in his having found that the brain and nerves are compoſed of convoluted, and not of ſtraight fibres; that theſe fibres are about the $\frac{1}{9000}$ part of an inch in diameter, and that they are not hollow, but ſolid. He adds, that theſe fibres not only enter into the compoſition of the organs of ſenſation and motion, but likewiſe into that of all the other parts of the body; and he likewiſe finds them in hairs, in the epidermis, and in the nails.

He will likewiſe have it, that the principal maſs of all organized parts is compoſed of theſe convoluted fibres, that is to ſay, of nerves.

Laſtly, he finds a ſyſtem of the like convoluted fibres, in all the nerves of the human body, and in the vegetable kingdom; and believes that foſſils are almoſt wholly compoſed of convoluted fibres, ſimilar in ſize and form to the nerves of an animal.

We

We may certainly conclude from all this, that Mr. Monro regards the convoluted fibres in animals, as of a nervous nature, although he agrees besides, that they are not found to be instruments of motion and sensation in all bodies, as they certainly are not in fossils or plants, any more than in the nails and hairs.

Although the greater part of these discoveries of Mr. Monro seems paradoxical, this is no reason for denying them; and the authority of the professor would alone be sufficient to induce me to examine his observations very attentively, even if the importance of the subject, which is in itself very interesting, did not require it.

The new discoveries of Mr. Monro, are altogether different from the observations on the structure of the brain and nerves, made by those who have preceded him. I thought it proper then to examine this matter as if it had been entirely new to me; and the opinions of writers have only served to make me more cautious in pronouncing, even where my observations were the most constant.

I wished to examine the nerves as they appear to the eye, in a living animal; without touching the parts that compose them. I separated alone, the parts altogether of a different nature, that were contiguous to them. It was not difficult for me to perceive that they appeared formed of bands more or less regular, or of alternate white and dark spots.

PLATE

Plate III, Fig. 1. Reprefents one of thefe nerves in which the bands were more regular and diftinct than in the others. On examining them with a lens which magnified fix times, every thing was feen better and more diftinctly. I detached the nerve from the animal, without pulling it in any way, and examined it on a glafs. The bands appeared exceedingly regular, they were all equally large, and the fpaces from one band to another, were equal to each other, and equal to the bands themfelves. I thought at firft, that thefe bands formed a true fpire in the nerve, or rather, that they turned fpirally, as a ribbon would do round a cylinder. This idea feemed to agree with what I obferved, and what feemed ftill more to confirm it, the nerve rolling in this way on itfelf, the bands feemed to continue their circumvolutions throughout, and I could not perceive them to be formed of detached rings, placed at equal diftances.

I wifhed to fee whether this fingular ftructure, or fpiral appearance, were common to all the nerves, and fpared neither time nor labour to affure myfelf of it. In the great number of nerves of animals I have examined till this time, I have feen very few in which the bands were as regular as in the above cited Fig. 1.

Thefe bands in general feem to form different angles, and to crofs each other, and are often feen of different fizes.

But whether great or fmall, regular or irregular, whether they crofs each other, or proceed in

a paa parallel direction, they are obſerved in all the nerves, to the very brain and ſpiral marrow, that is to ſay, to the part where the nerves form themſelves into threads, or cylinders. A degree of attention is required to obſerve the ſpires in many of the nerves, above all at the place of their origin.

When they are too abundantly covered with cellular membrane, or if it be in flakes, it muſt be removed to better diſtinguiſh the ſpires. In many of the nerves they are ſeen with the naked eye, without any need of preparation, ſo that this appearance of bands, is a conſtant and certain characteriſtick in the nerves; and theſe organs of motion and ſenſation appear to be compoſed in a great part of theſe white bands, ſince when ſimple and regular, they occupy about half the length of the nerve.

Theſe nervous bands are not deſtroyed, although they are not ſo diſtinctly ſeen, when the nerves themſelves are ſtrongly pulled out, provided the diſtenſion be not extremely great: in the latter caſe they may be ſo changed as not to be diſtinguiſhable. The tenacity in the nerves, to preſerve this appearance of bands, ſeems to confirm ſtill more the latter being true ſpires, and that they roll round the nerve, as a ribbon does round a cylinder.

I could not however conceive how the anatomiſts who have ſearched into the nature and compoſition of the nerves, and ſtill more the microſcopical obſervers, could eſcape the ſeeing theſe

 bands,

bands, which are so readily and constantly observed in all of them. I have at least found no one who speaks of them, although some one whose works I have not read, may have noticed them: this is of but little consequence; but it is very essential for us to know that the nerves present themselves in this form, a circumstance which may tend very much to a knowledge of the nature of these organs, that are so important to animal life.

Let us pass to the examination of the different appearances of these bands, and see with how many variations the nerves present themselves, to the attentive eye of an observer.

Plate III. Fig. 1. Represents a nerve magnified about six times, with a lens. *cc, cc, cc, cc*, are white bands, all equally large, and at equal distances. *oo, oo, oo, oo*, are the opake parts of the nerve, equal in every respect to the bands.

Fig. 3. of the same plate, represents a nerve magnified eight times by a lens. The bands in this one cross each other at different angles, and in different parts of the nerve. Fig. 2. is another nerve observed in the same way. The bands are more distinct, and approach each other in some places, instead of crossing, but without any regularity. The nerve in Fig. 6. likewise shows the white bands, some of which unite, and others cross. The little regularity in the bands led me to suspect that there were several orders of them in the same nerve, and that they perhaps took different directions. Fig. 7. almost fully confirmed me in this belief

belief. The bands croſſing each other are there ſeen in the midſt of the breadth of the nerve, forming both ſharp and obtuſe angles, perfectly equal to each other. But this hypotheſis, or this ſuſpicion, was not yet confirmed by obſervation, and might very likely be falſe. In multiplying my obſervations, I found in ſeveral of the nerves a double order of theſe bands, which met each other, as the cogs of two wheels do, that are fixed to each other.

Fig. 5. repreſents this double order of bands very perfectly, as I obſerved them in a nerve, with the aſſiſtance of a lens which magnified ſix times. The bands of the two orders *a r*, *o c*, were equally large, and every where at equal diſtances, and ran one into the other for more than a third of their length; as the band *o* is ſeen to enter into the band *a*, and likewiſe the band *c* into the band *r*. This new obſervation convinced me ſtill more that theſe bands ran along the nerve in the form of concentrated ſpires, equally diſtant, and every where of an equal diameter. It is true that I regarded them as formed of two nerves united together by a common cellular ſheath, and could in this way account for all the irregularities I obſerved in them. The nerve in Fig. 8. completed this perſuaſion. *r*, *a*, *r*, *a*, point out one of the two nerves, and *a*, *o*, *a*, *o*, the other. The line of sepraation betwixt the upper and lower bands is diſtinctly ſeen, and this line *a a*, can be nothing elſe than the union of the two nerves. I found it no longer difficult to aſſure myſelf of the truth of this ſuppoſition, all that

was required being to ſtrip this nerve of the common coverings, and afterwards to ſeparate the nerves of which it might be compoſed. Fig. 4, repreſents in effect, not only a ſeparation of the nerve from the common ſheath, but likewiſe of the nerves that compoſe it. There are four of theſe nerves, viz. *a b*, *c e*, *o r*, *ſ m*, and in each of them the bands are ſimple, without meeting each other, or forming angles. It muſt not however be thought that a nerve which preſents a ſingle order of bands is perfectly ſimple, that is to ſay that it may not be formed of other ſmaller ones; all my obſervations prove the fallacy of this. I have always found that the largeſt nerves are formed of ſmaller ones, and theſe again of ſmaller ſtill; and in the laſt, which I ſhall call ſimple, the bands always appear regular and without forming angles.

The nerves then are ſmall as hairs, and perhaps ſtill ſmaller; notwithſtanding which the bands are diſtinctly ſeen, even with the weakeſt lens'. When they are larger, and form other nerves, the cellular membrane unites them and wraps them up in ſuch a way, that the eye cannot penetrate far into theſe ſubſtances, and conſequently a ſingle order only of bands is diſtinguiſhed.

This wonderful ſtructure of bands is common then to all the nerves, even the ſmalleſt of them, and the irregularity of theſe bands is only cauſed by the bands of the ſmaller nerves, of which a large one is compoſed. I have endeavoured to ſtrip them of their ſheaths, as well particular as common, with the point of a ſharp needle, ſo as not ſenſibly to change their ſtructure, and am fully convinced that this operation does not deſtroy the bands, which therefore ſeem clearly to depend on the primitive ſtructure of theſe organs.

Perſuaded laſtly that theſe bands were not an appearance or opticle illuſion, and ſeeing that the very irregularities of them confirmed me ſtill more in the opinion that they were ſo many ſpires, that is to ſay, that they turned uninterruptedly round the nerve from one extremity to the other, I proceeded to a ſearch into the nature and compoſition of them.

The white colour may at firſt ſight induce one to judge that it is a pure medullary ſubſtance; but this can alone be decided by obſervation.

My firſt attention was to examine a ſmall nerve covered with its own natural cellular membrane. Fig. 10. repreſents it in the way I ſaw it with a very ſtrong lens, and covered with water. The two extremities of the nerve, *a*, *a*, were a little tranſparent, and ſeemed formed of very fine threads, interſperſed with a great number of very large oval globules. Theſe oval globules and ſmall threads are common to all the ſheaths of nerves. The opake and middle part of the nerve *a*, *a*, ſeemed formed of parallel winding threads, as is ſeen in the ſame figure. *m*, *m*, *m*, are oviform globules obſerved in the cellular membrane of the nerve; and *r*, *r*, *r*, are the filaments of the cellular membrane itſelf, ſwimming in the water.

 Every

Every time I examined the nerve in this manner, that is to ſay, with a very ſtrong lens, the bands were no longer viſible, and I could not account clearly for this diſappearance.

Inſtead of ſpiral bands obſerved before, I ſaw parallel winding fibres, every where of an equal thickneſs, running all along the nerve; and yet when I examined this ſame nerve with the naked eye, or with a weak lens, it preſented the white bands as uſual.

I at length removed the cellular membrane or ſheath of the nerve, without changing its texture, and examined it in this ſtate with the utmoſt attention, but could only diſtinguiſh the winding fibres, as they are deſcribed in Fig. 9. However often I repeated this examination, all my reſearches were uſeleſs: I could only find the wavy, winding fibres, when I examined the nerve with ſtrong lens'; and bands or ſpires, when I obſerved it with weak ones, or with the naked eye. If my firſt obſervations made me believe that a nerve was really compoſed of large and very white bands, the latter perſuaded me that it was formed of parallel winding fibres, ſo that I could not determine which of theſe two appearances was the right. It is very true that the firſt hypotheſis ſeemed to me leſs likely after the latter obſervations, becauſe with the ſtrongeſt lens' I could perceive nothing in the nerve conformable to the firſt appearances. I could find no remains of a thread which twiſted ſpirally round it, ſo that it was neceſſary to examine whether theſe bands were not a true opticle illuſion. In this ſtate of uncertainty, I ſaw

I ſaw no other ſtep to be taken than the continuing to obſerve this double appearance of bands on one ſide, and winding threads on the other. I diverſified the circumſtances of this examination as much as poſſible, and by acting in this way, if I did not ſucceed in diſcovering whence this double appearance of curvilinear bands, and winding threads, aroſe, every thing however concurred to perſuade me, above all when I examined a very ſmall nerve not compoſed of leſſer ones, that the nerves ſimply contain very fine winding threads, and are wholly compoſed of them. The microſcope would not have been capable of preſenting them to the eye in ſo conſtant a way, and in ſo many different circumſtances, in which, on obſerving the nerve with the ſtrongeſt lens', the winding threads inſtantly ſhowed themſelves, if theſe threads had not really exiſted. This ſucceſſive appearance and diſappearance of bands and winding threads, and *vice verſa*, perſuaded me at length that the appearance of bands, to the eye, either naked or feebly armed, was cauſed by the winding fibres themſelves. By this new hypotheſis I was completely enabled to account for the different appearances in the form of the ſtructure of the nerves, and there was no effect which I did not eaſily explain, nor obſervation that I did not immediately comprehend. I however ſought a more certain proof in a deciſive experiment. I wiſhed to take nature in the fact. An obſtinate application of ſeveral days, which I employed in obſervations, enable me at length to ſee clearly, and to diſcover the whole myſtery. Very ſtrong lens' cauſed the bands to diſappear, and very weak ones the winding fibres.

A ſtrong or weak light, directed on the object by the motion of a reflecting mirrour, produced ſenſible changes in the appearances of the nerve; ſo that I ſaw with the ſame lens, at ſometimes the bands alone, and at others the winding fibres alone.

I now employed a lens of a middling ſtrength, and threw a light upon the object in ſuch a way as to diſtinguiſh clearly the winding, wavy fibres; but without ſeeing the ſpiral bands. Without touching object or lens, I ſimply turned the mirrour a little, and ſo directed the light on the object, that at length the ſpiral bands appeared very diſtinctly. On ſcarcely touching the mirrour, the bands inſtantly diſappeared, and the winding fibres ſhowed themſelves in their ſtead. I again touched the mirrour, when the bands ſuddenly appeared afreſh, and by a motion of the mirrour, they once more gave place to the winding fibres. Thus I could at pleaſure produce this double appearance, of bands and fibres, by only throwing more or leſs light on the object.

Figures 9. and 10. repreſent theſe ſucceſſive appearances and diſappearances. Fig. 10. ſhows the nerve ſtripped of its outward ſheath, and viewed with the lens of middling ſtrength. It was ſo ſurrounded by light, that the bands or white ſpots, *c*, *c*, *c*, *c*, and the dark ſpots, *a*, *a*, *a*, *a*, were diſtinctly ſeen. This double claſs, of dark and white ſpots, forms the nervous bands.

Scarcely did I touch the mirrour, when the bands

ſuddenly

ſuddenly diſappeared, and I ſaw in their place the winding fibres of Fig. 9. When I moved the mirrour by almoſt inſenſible degrees, I obſerved the bands diſappear as leiſurely, and the fibres ſhow themſelves; or rather I ſaw the appearance of bands converted into true parallel winding threads: the bands, *c*, *c*, *c*, of Fig. 10. became the winding and convex fibres *c*, *c*, *c*, of Fig. 9. and the opake intervals *a*, *a*, *a*, *a*, of Fig. 10. became the concave fibres *a*, *a*, *a*, of Fig. 9.

It was no longer poſſible to doubt the reality of theſe obſervations; that is to ſay, that the bands were not real but apparent; and on the contrary, that the winding fibres were not apparent but real.

All the obſervations I have made ſince have confirmed me ſtill more in this, ſo that I can no longer doubt but that the ſpiral bands in the nerves are an opticle illuſion, and that this illuſion is produced by the wavy form of a great number of fibres, or parallel threads, running along the nerve.

Amidſt the numerous microſcopical obſervations I have made at different times on animals, or on other ſmall bodies, no one has coſt me ſo much trouble as this, or has been ſo near to the leading me into an errour. I have ſhown theſe bands to ſeveral perſons accuſtomed to obſerve the ſmalleſt objects, and the ſtructure of the human body; and have met with no one who did not ſuppoſe the ſtructure of them in the nerve to be real, and who did not ridicule my aſſertion, that they could be no other than a ſimple appearance.

After

After having thus ſurmounted this firſt difficulty, and aſſured myſelf that a nerve preſents to the view a great number of winding fibres, of which it is formed, I proceeded to farther reſearches. I wiſhed to know what the primitive ſtructure of the nerves is, that is to ſay, whether it is compoſed of channels, or of ſimple threads; whether it merely conſiſts of globules, or contains a non-organick, irregular, ſpongy matter. This reſearch is as important as difficult, ſince it tends to nothing leſs than the fixing, once for all, the ideas of anatomiſts on the nature of the nerves; that is to ſay, on the ſtructure of the organ of motion and ſenſation in animals. They have diſputed for more than three thouſand years, from Hippocrates down to Albinus, from the Greeks to the moderns, and ſeem during all this time to have done nothing more than multiply doubts and hypotheſes.

Without being very ſanguine in my hopes of diſcovering the firſt principles of nerves, I have undertaken the inveſtigation with ardour, perſuaded that the knowledge I have of their winding fibres, muſt be extremely uſeful to me in ſo difficult a ſearch.

I began my obſervations on a very ſmall nerve, which I had ſtripped of the cellular membrane. I obſerved the winding fibres narrowly with a very ſtrong lens, and determined the ſize of them. This done, I divided the nerve towards its extremity in a longitudinal direction, by means of a very ſharp needle,

needle, and divided the parts or threads of it, separating one from the other. I immersed the nerve in water, in which the threads floated. After several useless attempts, and several observations either suspicious or inconstant, I at length succeeded in finding many very small cylinders, more or less transparent, seemingly composed of a pellicle, and partly filled with a transparent, gelatinous humour, and with small unequal globules, or bodies. Plate IV. Fig. 3. represents three of these tubes, which I shall call *primitive nervous cylinders*; because these are the parts that constitute the nerve, or its medullary part. Fig. 5. represents another of these cylinders.

To distinguish their structure and shape the better, I examined a great number of these primitive nervous cylinders with a lens that magnified 500 times. Fig. 1. represents one that seemed to have here and there on its outside some fragments of winding threads; and some spheroidal corpuscles in the inner part of the cylinder. Fig. 2. describes another which appeared filled here and there with very small globular corpuscles, immersed in a gelatinous transparent humour. I have seen others which one would have supposed to be filled with a gelatinous substance broken confusedly, and separated into different fragments, so that the gelly of the cylinders may be looked upon as separated, or divided into large, transparent, irregular masses.

However, all the efforts I made to assure myself f the nature and reality of these irregular corpuscles belonging to the primitive cylinders, did not enable me to judge precifely of them. They fometimes feemed to me to be fpots or irregularities in their external coats, but this I could not determine, and my doubts multiplied in proportion to my obfervations. I had recourfe to a very ftrong lens, which encreafed 700 times the diameter, and after feveral fruitlefs attempts, I at length affured myfelf that the coats of the primitive nervous cylinders were very rugged and full of irregularities. Fig. 4. reprefents four of thefe cylinders, *ac*, *om*, *rs*, *ne*, in two of which *ac*, *rs*, the irregularities are apparent. Being at length affured of this new truth, it remained for me to make myfelf better acquainted with the true nature of thefe irregularities, and to know whether they contained globules or corpufcles differently fhaped.

To fucceed in fo difficult a fearch, I began by feparating the primitive cylinders of feveral nerves, with the point of a needle.

The extremities of the nerves were placed in water, and I ran the point of the needle along them, to break the cylinders, or deprive them in fome way of their irregularities: I fucceeded at length in meeting with one that had the form defcribed by Fig. 6. About half of this cylinder *ac*, was formed of a tranfparent and uniform thread, and the other half *ma*, was almoft twice as thick, lefs tranfparent, irregular, and rugged. I then fufpected that the primitive nervous cylinder was formed of a tranfparent cylinder, fmaller, and more uniform,

uniform, and covered with another ſubſtance, the nature of which was perhaps cellular.

The obſervations I made afterwards confirmed me invariably in this hypotheſis, which at length became an eſtabliſhed fact. I have very often ſeen theſe two parts, that compoſe the primitive nervous cylinder. The exteriour one is unequal and rugged; the other a cylinder which ſeems formed of a particular, tranſparent, and homogeneous membrane, that appears to be filled with a gelatinous conſiſtent humour.

Fig. 4. repreſents, as has been ſeen, a group of theſe primitive nervous cylinders, in the way I have obſerved them on examining the nerve of a rabbit. One of theſe cylinders, *o, m,* was entirely ſtripped of its external and rugged membrane, and had an uniform tranſparent appearance. Another was in a like way ſtripped, except at one extremity, *n, e,* which ſeemed covered and encloſed by an external rugged membrane. A third, *ac,* was almoſt altogether covered with this rugged membrane; the fourth, *r, s,* entirely ſo.

Fig. 7. repreſents a primitive nervous cylinder in which, *o, r,* the thick part, is covered with a cellular membrane compoſed of fine threads. The part, *r, s,* is ſtripped of this cellular membrane.

On examining this external covering of the primitive nervous cylinders with attention, it ſeemed to be compoſed of winding threads, running along

along the nerve, and ſo forming a cover to the interior cylinders. I was well aſſured of this ſoon after, by employing a lens that magnified 800 times. Fig. 8, repreſents a primitive nervous cylinder, covered with its exteriour ſheath, which is diſtinctly compoſed of very ſmall winding threads, running along the cylinder.

The progreſſion of theſe threads may in ſome meaſure be compared to the canal formed by the epididymis, which produces inceſſant folds. The threads are very ſmall, not appearing to be more than $\frac{1}{15000}$ part of an inch in thickneſs; and although they are thus fine, they form ſo thick a coat round the primitive nervous cylinder, that they almoſt triple its diameter; this is cauſed by their winding and heaping upon each other exceedingly.

Theſe winding threads, which cover the primitive nervous cylinders, I ſhall call the *winding cylinders* of the nerves; and conſidering them collectively, as a wrapper to the above primitive nervous cylinders, I ſhall call them the *external ſheath* of theſe cylinders.

Fig. 9, repreſents a primitive nervous cylinder, covered with its external ſheath. The ſheath is woven with winding threads, ſome of which are a little ſeparated from others by the point of a needle. Theſe winding threads have a perceptible thickneſs when viewed with very ſtrong lens', although much finer than the primitive nervous cylinders.

The primitive conſtruction of the nerves is as follows; a nerve is formed by a great number of tranſ-

parent,

parent, homogeneous, uniform, very ſimple cylinders. Theſe cylinders ſeem compoſed of a very fine uniform tunick, filled, as far as the eye can judge, with a tranſparent, gelatinous humour, not ſoluble in water. Each of theſe cylinders receives a cover in form of an external ſheath, which is compoſed of an immenſe number of winding threads. A very great number of tranſparent cylinders form together an almoſt inviſible nerve, preſenting the exteriour appearance of white bands; and ſeveral of theſe nerves united, form the larger nerves ſeen in animals.

I am fully convinced by my own obſervations, repeated a great number of times with the ſame ſucceſs, that the cylinders I have deſcribed, are the ſimple and firſt organical elements of nerves, for I have never been able to divide them farther, whatever trials I made with the ſharpeſt-pointed needles. I could eaſily tear and rend them here and there, but they always remained ſimple as before. I could ſtrip them of their ſheaths, and ſeparate the winding cylinders of which theſe were formed, although they were very ſmall. The primitive nervous cylinder then appeared tranſparent, homogeneous, and every where of equal diameter. We ſee by this how much even the beſt anatomiſts were in general miſtaken, when they maintained that the nerves were divided and ſubdivided without end, without there being any hope of ever ſeeing, or coming at a knowledge of, their firſt threads, or firſt organical elements.

This

This seems to me to be a great stride towards the understanding an organ so essential to life, and which till now has been hid to the eyes of the greatest observers.

I reserve the examining, till I am more at leisure, the matter of which the nerves are composed, or filled. This subject when well known, may throw a very great light on the knowledge of the animal economy, and perhaps likewise, on that of a great number of diseases.

On

On the STRUCTURE of the BRAIN.

AFTER having examined the ſtructure of the nerves, and their firſt organical elements, order requires me to turn my attention to the brain, whence they draw their principal origin. We know that the brain is compoſed of two ſubſtances, named cortical and medullary, which are diſtinguiſhed by their reſpective colour.

I have already related the various opinions of authors as to the ſtructure of the brain. Some believe its ſubſtance to be altogether vaſcular, and others will not have it to be ſo. There are ſome who ſuppoſe it ſimply compoſed of blood veſſels; others on the contrary believe it formed of veſſels much ſmaller again than theſe.

Malpighi believed the brain, as well as all the other viſcera in the body, deſtined for particular ſecretions, to be glandular. Of the more modern obſervers, ſome believe it to be formed of ſimple globules, others of a non-organical and ſpongy pulp.

The obſervations I had made on the medullary ſubſtance of the nerves, were a great help to me in examining the brain, although in the cortical ſubſtance I encountered very great difficulties, which I ſhould never, perhaps, have been able to ſurmount, if I had not been previouſly acquainted with the medullary ſubſtance of this organ, in which every thing is ſeen much better, and in a clearer way. 'Tis for this reaſon that I ſhall begin my obſervation by examining the medullary ſubſtance of the brain.

I ſhall not ſpeak of the blood veſſels that are ſeen in the medullary ſubſtance of this viſcus, and which are well known to anatomiſts, particularly ſince the uſe of injections.

I ſhall only treat of that part of the medullary ſubſtance which is quite white, and which is certainly not formed of blood veſſels. I cut a ſmall and very thin lamina of this medullary ſubſtance, and extended it on a glaſs wet with water. I exaamined it with the greateſt attention, purpoſely varying the light, and it ſeemed to me to be formed of a tranſparent cellular ſubſtance, covered here and there with very ſmall, rounded, winding cylinders. Plate V. Fig. 8, *r r*. repreſents this ſubſtance, which, when well obſerved, ſeemed compoſed, as it were, of a heap of inteſtines; but all was obſcure, and uncertain. At its ſide, at *a a*, there were ſeveral corpuſcles, detached from the cellular ſubſtance, ſwimming in the water. Some ones

ones were larger than the others, they were irregular, and more or less oviform. This intestinal form of the medullary substance of the brain, made me suspect that there might be canals or vessels, and that it might be wholly formed of them. And in reality, having observed this medullary substance afresh, as in Fig. 8, with a much stronger lens, it appeared absolutely formed of an heap of small, irregular, winding, transparent intestines, filled with a gelatinous humour. It was not possible for me to discover any more; it only appeared that these small intestines were very short, and that some of them terminated in small globules, or spheroidal bodies.

However certain this observation appeared to me, it still left me a great deal to desire. I wished to see things clearer, and to assure myself that these were canals, which ramified. I employed a lens that magnified in diameter upwards of 700 times, and after several fruitless attempts, I at length succeeded to observe the part I had before touched with the point of a needle, and which I had wetted afresh with water. I then found to a certainty, that it was really formed of a winding and vascular substance, which folded as the intestines do, making several turns and circumvolutions. Fig. 9, exactly represents its manner of appearing to the eye, armed with a microscope. Globules, which seemed surrounded by something, were spread about it; certain round, or blunt bodies, appeared in the intestinal substance itself; and some of these intestines ſeemed to terminate in theſe bodies. It is however certain, that round corpuſcles are tenaciouſly fixed to this ſubſtance, and that they are detached with difficulty by water.

The point of the needle, however, had detached ſeveral bodies from this ſubſtance, which are repreſent by Fig. 16. Some of them, particularly the largeſt, appear to be branched; others ſeem to end in the corpuſcles I have ſpoken of. I can nevertheleſs form no conjecture on them, and only repreſent the object as I have ſeen it. I have repeated this obſervation a great many times, but have not yet ſucceeded in ſeeing any thing more. 'Tis true, I have aſſured myſelf to a greater certainty, that the medullary ſubſtance of the brain, is not a ſimple collection of venous and artereal veſſels; that it is not ſimply formed of ſpheroidal globules or corpuſcles; but that it is an organiſed, particular ſubſtance, compoſed of irregular cylinders, or tranſparent canals, which fold as the inteſtines do, and which I ſhall call the *inteſtinal ſubſtance*, on account of the ſhape in which it is ſeen.

This particular, inteſtinal, ſubſtance, of which the marrow of the brain is formed, is not ſoluble in water, any more than is the tranſparent matter with which theſe inteſtines ſeem to be filled. The round corpuſcles likewiſe, that have been deſcribed, are not capable of being diſſolved in water.

Such is the ſtructure of the medullary ſubſtance of the brain, and in this way I have found it in all the animals I have examined. My obſervations on

 this

this ſubject conclude here, and I can venture to ſay, that I flatter myſelf with not having been deceived. All that I could further advance on this occaſion, would be nothing more than conjectural hypotheſes.

Cortical Subſtance.

The inteſtinal ſtructure I had diſcovered in the medullary ſubſtance of the brain, aſſiſted me in diſcovering ſomething like it in the cortical ſubſtance of this viſcus. I ſay ſomething like it, becauſe I could not after all trace the progreſs and inteſtinal ſtructure of this ſubſtance in ſo clear a way, as I had done that of the medullary one.

I began to examine the cortical ſubſtance, in the way in which I had examined the medullary one, viewing a thin lamina of it moiſtened with water. Fig. 6, *r r.* repreſents this lamina obſerved with a very ſtrong lens. It ſeemed to be formed of an irregular web, granated here and there, which I ſhould have ſuppoſed a cellular membrane, if I had truſted to a ſimple inſpection. Beſide this ſubſtance, were very ſmall, irregular, tranſparent, ſpheroidal corpuſcles, which ſeemed filled with a gelatinous humour, and which were not ſoluble in water; they were ſmaller than thoſe I have obſerved in the medullary ſubſtance, but in every other reſpect like them. I now had recourſe to a ſtill ſtronger lens, with which I at length ſucceeded in obſerving a ſtructure altogether like that I had obſerved in the medullary ſubſtance; that is to ſay, compoſed of a tranſparent, organical, vaſcular ſubſtance, formed like the inteſtines. On obſerving Fig. 7. its revolutions and windings in this ſubſtance, like thoſe in the medullary, will be ſeen at *m*, *a*; it only ſeems to differ from the latter in the fineneſs of its veſſels, which it is very difficult to ſee diſtinctly. About it were the corpuſcles, *r*, *r*, which likewiſe ſeemed to be ſurrounded by ſomething. The obſervations I have ſince made, have more and more convinced me, that the ſtructure of the cortical ſubſtance of the brain, is as I have deſcribed it, and that it does not differ ſenſibly from the medullary ſubſtance, although their colour makes them appear ſo diſtinct. I do not pretend to deny, but that the uſes of theſe two organized ſubſtances may be a little different; and the different dimenſions of their reſpective inteſtinal ſubſtances, give great reaſon to ſuſpect it.

I wiſhed to ſee the union of theſe two ſubſtances, and to obſerve the end of one, and beginning of the other; but could not ſucceed. It however appeared to me, that the fluid with which the inteſtines of the cortical ſubſtance ſeem to be filled, agrees in its nature with that of the medullary ſubſtance.

Retina.

After having examined the primitive ſtructure of the nerves, and thoſe of the cortical and medullary

dullary ſubſtances of the brain, I thought it proper to beſtow a particular attention on the parts in which the nerves terminate, or where they become the organs of a particular ſenſe in an animal. For this purpoſe I made choice of the retina, which appeared to me the moſt proper organ for my purpoſe, and the uſe of which is the moſt noble. I reſerved to another occaſion, the examining the nervous expanſions directed to the other ſenſes.

Authours are divided amongſt themſelves as to the primitive ſtructure of the retina, although they agree in general in other reſpects, as to the nature of this organ, which they believe to be formed of the medullary part of the optick nerve. Several have believed it to be a pure web of nervous fibres, the primitive ſize of which, as may be ſeen in the works of Portenfield and Geſner, they have even gone ſo far as to determine; but the exiſtence of theſe fibres, and their ſize, in theſe authours, are rather founded on theory, than on immediate obſervation; ſo that a nice obſerver, who does not content himſelf with hypotheſes, can have but little dependance on their calculations.

There are others indeed who have ſtruck into the path of obſervation, but their reſearches have been very ſuperficial, ſince they have contented themſelves with ſimple ocular inſpection, or with very common lens'. It hence has ariſen, that they have given us nothing more than vague and general opinions, and have conſidered the retina as no other than an expanſion of the medullary part of the optick nerve.

But others again, more venturous, and accuſtomed to obſerve very minute bodies with very ſtrong lens', have carried their reſearches ſtill farther, and have aſſured us that the retina is not formed of diſtinct nervous fibres, but rather of the mucous ſubſtance of the brain, which ſeveral of theſe obſervers imagine they have diſcovered, and which, as we have ſaid above, is nothing more than a maſs of very ſmall ſimple ſpheroidal corpuſcles, or as others have thought, of a confuſed non-organick pulp, or of ſimple threads, and cellular lamina.

Some obſervations I had previouſly made on the retina of rabbits, were very uſeful to me in the examination I afterwards entered upon, of the retina of other animals. Had I not known the qualities of the former, I ſhould perhaps like others have been deceived, and ſhould have denied a ſtructure to this organ, which is abſolutely proper to it.

The retina of rabbits obſerved with the naked eye, appears very different from that of other animals viewed in the ſame way. A particular ſtructure in the former inſtantly fixes the attention of thoſe who obſerve it. In the inner part of the retina, (ſee Fig. 12.) and oppoſite the lower entry of the optick nerve, a pretty deep hollow, the edges of which are well raiſed, forms itſelf; it is wider towards the head and upper jaw, than towards the two corners of the eye. From the bottom of this

this hollow arises a very great number of exceedingly small and very white nervous threads, which spread all around it, as rays that diffuse themselves from a common centre, and form the retina.

If the optick nerve be divided lengthways with a scalpel, into two equal parts, the threads of it, which are no other than a continuation of the above mentioned filaments, correspond perfectly with them. These nervous filaments on their leaving the hollow, continually diminish in size, and ramify; in proportion as they are more distant from their origin, they become more numerous, till at length, being so very fine as to be scarcely visible, they terminate in very subtle threads at the beginning of that part of the retina which I shall call *mucous*, to distinguish it from the other, which I shall call radiated, or *filamentous*. The nervous rays diffuse themselves in every direction round the hollow, but are very long in two parts, *r*, *r*, diametrically opposite to each other, and occupy the greater part of the internal portion of the eye, so that the unradiated part of the retina is very small in this place, in comparison with the other two parts which correspond with *m*, *m*.

These small nervous filaments, when very nicely examined, are of a crooked structure, and have the usual appearance of spires or bands, above all at the other parts where they are least divided; but this is very difficult to be distinguished clearly.

These

These ſmall nerves, or radiated fibres, which are ſo eaſily diſtinguiſhed in the eyes of rabbits, even without the help of glaſſes, are obſerved with great difficulty in the eyes of other animals. I candidly acknowledge, that if I had not firſt ſeen them in rabbits, I probably ſhould not have found them in other animals, ſince not having any ſuſpicion of them, I ſhould not have examined the retina with all the attention that is neceſſary to diſtinguiſh them. I muſt likewiſe obſerve, that it is very difficult to view theſe radiated fibres diſtinctly, and to verify their exiſtence; to ſee them with facility, very ſtrong lens' muſt not be employed. The beſt are thoſe that ſcarcely magnify ſix or eight times. The retina muſt likewiſe be examined at the entry of the optick nerve, where the medullary part of the nerve is the largeſt. It is only diſtinguiſhed well and to a certainty, in ſome lights. I have ſucceeded in diſtinguiſhing it ſtill better when I have thrown the marine or ſome other acid, well diluted with water, upon the retina, which then becomes white and opake, and the nervous fibres are more clearly ſeen in it. Sometimes I have viewed them very well, after throwing ſome drops of water on the retina at the entry of the optick nerve. Although I am very certain of the exiſtence of theſe ſmall radiated nerves in the eyes of all the animals, ſuch as oxen, lambs, and kids, that I have hitherto examined, I ſhall however be not at all aſtoniſhed, ſhould they be hidden to the eyes of many obſervers, even to ſome

ſome who beſt underſtand the uſe of the microſcope, and view the minuteſt objects in the beſt manner. Theſe ſmall nerves do not eſcape the eye on account of their extreme fineneſs, but rather on account of the great conformity they have to the pulp of the retina itſelf, which covers and conceals them, if I may be allowed the expreſſion, from the obſerver. They appear to me to be of the ſame length, in the eyes of all animals, except, as has been ſeen before, in thoſe of rabbits, and they diſappear when arrived at two thirds the ſpace that is found betwixt the entry of the optick nerve, and the *plexus ciliaris*, ſo that the unradiated retina in theſe animals only occupies one third, or thereabout, of the orbit of the eye.

Theſe radiated nervous fibres are very numerous, and ſeem to be formed of, or covered with, a medullary, dark, ſlightly tranſparent, pulp, which when examined with the ſtrongeſt lens', ſeems to be compoſed of very ſmall tranſparent ſpheroidal bodies, well united together, and tied as it were by very fine tranſparent membranes or filaments.

That part of the retina in which the ſmall nerves are ſeen in a radiated form, ought then to be conſidered as compoſed of two parts, one radiated, the other pulpous, or ſimply medullary. The radiated part of the retina, if the eye be examined at the pupil, is covered with a particular ſubſtance, like a non-organick mucus, and the part beneath this mucus is formed of ſmall, decreaſing, longitudinal nerves, that is to ſay, of nervous rays.

I found

I found the unradiated nervous part of the retina, to be likewiſe compoſed of very ſmall nervous globules, ſupported by a very fine tranſparent cellular web, in which it ſeemed in ſome way to encloſe itſelf. Theſe globules are ſmaller than thoſe of the blood. I found them in rabbits to be about $\frac{1}{3500}$ part of an inch in diameter, whilſt thoſe of the blood were the $\frac{1}{2500}$ part. The globules, which appear to be formed of a tranſparent gelly, in the retina of rabbits, do not diſſolve in water like the red globules of blood, and are ſtrongly attached to a cellular ſubſtance, that ſeems to ſupport them. Fig. 11. repreſents a ſmall portion of the retina, with the globules and cellular web.

The ſizes and reſpective forms of the globules of the nervous part of the retina are repreſented by Figures 10. and 11. The ſmall bodies in Fig. 10. repreſent the globules of the retina, and thoſe of Fig. 13. repreſent the globules of the blood.

The globules of the retina have a great reſemblance to thoſe that are found in the brain. Water and acids produce the ſame changes in them; they are equally tranſparent; and I have only remarked, that the globules in the retina are more regular and uniform than the others.

When the retina is kept a long time in water, and wiped a little, ſhreds of it are often found more or leſs deprived of thoſe globules, and it appears in theſe parts like an unequal, rugged, cellular web, formed with ſmall hollows capable of

receiving

receiving theſe globules. Fig. 15. exactly repreſents the retina in this ſtate.

Thus appears the retina when nicely obſerved, and we are ſcarcely permitted to penetrate farther into its nature. The part which correſponds to the entry of the optick nerve, and which extends a great way, is compoſed of very fine threads of nerves, and of a nervous pulp formed of very ſmall tranſparent globules, attached to a very looſe, tranſparent, and rugged web.

The other part of the retina is ſimply formed of the uſual globules, and cellular web; and as far as I could obſerve, ſeems to have no nervous threads in its compoſition.

Wiſhing to be better acquainted with the nature of theſe cellular webs of the retina, and how the globules of the medullary part are attached to them, I began afreſh to examine the retina with very ſtrong lens', and have at length, after many attempts, been able to diſtinguiſh more preciſely. I think I may venture to advance that theſe cellular webs are no other than a net-work of very ſmall tranſparent crooked veſſels, to which, as is ſeen in Fig. 14. theſe globules attach themſelves. Theſe crooked veſſels reſemble very much, in tranſparence, figure, and progreſs, thoſe of the medullary ſubſtance of the brain, and they only ſeem to differ in their ſize, which is ſomewhat ſmaller, ſo that there ſeems to be a particular inteſtinal ſubſtance, which is found in all theſe parts.

Since

Since the experiments of Mariotte, and the calculations of Daniel Bernouilli, we know that the part of the retina which correſponds to the entry of the optick nerve, is blind, that is to ſay, that the images of objects expreſſed there, are in no way perceptible to us; and this nervous part in man is nothing leſs than a ſmall diſk of the diameter of a paris line. The nervous fibres in this place, are larger and more heaped together than in any other parts; the pulp is likewiſe larger and more heaped. However incredible it may appear, it is certain that the ſmall nerves and pulp of the retina I have juſt mentioned, are not organs of ſight, and that they only become ſo in the part where all is finer, more rare and more open. Is this inſenſibility of the retina to the light, cauſed by the nerves being as yet too large and not well freed from the cellular membranes? Or is it cauſed by the pulp of the retina being too much heaped, preventing in this way the rays of light from reaching theſe nerves?

But it now remains to make another very important ſearch, by examining whether the Retina is in all its parts ſenſible to external objects.

At a ſmall diſtance from the entry of the optick nerve it is as has been deſcribed certainly ſo, and it clearly continues to be ſo at a great diſtance from the ſame nerve; the rays of light likewiſe extend themſelves to a great diſtance, when we regard ſubſtances in a natural way; but let me enquire whether this viſion is produced at the ſpot where the rays end, and

and the mucous part of the retina begins. Do external bodies there cause a sensation? In a word, how far does the organ of vision extend? The solution of these problems, however difficult they may be, is not impossible. It depends on an exact knowledge of the parts of the eye, and a few experiments purposely made; I however have not time to busy myself in this search. It is generally known that in proportion as images are expressed at a greater distance from the optick nerve, the objects are seen more confusedly; so that there is in all appearance, a spot or limit, at some distance from the optick nerve, where vision is the most distinct, without our being yet able to assure ourselves whether it be at the precise place where the nervous part of the retina begins.

The blood vessels of the part, and principally the venous ones, are generally covered with the nervous fibres of the retina, and with its mucous substance. They are at least very often observed to be so in the eyes of oxen; but these vessels are in many places totally deprived of every nervous substance, and in these parts frequently cross the retina, and its adjacencies, breaking its order and texture, and so rendering it in many places insensible to external objects, although it does not appear so to us, on account of the great mobility of the eye.

Fig. 10. and 11. of Plate IV. represent two singular canals, with intermissions here and there. It may be suspected that they are lymphatick vessels; above all that of Fig. 11. I cannot decide as to their

their nature, having ſo ſeldom met with them. I found theſe two veſſels, in examining the ſubſtance of the brain.

It remains for me to make an obſervation or two, on certain figures of Plate V. which have been improperly introduced, by an errour of the engraver, into that plate.

Fig. 1. repreſents ſeveral oviform bodies of different ſizes, which are found in the external cellular coat of the nerves.

Fig. 2. repreſents very ſmall corpuſcles which I have obſerved in examining the medullary ſubſtance of the nerves. In Fig. 3. are other oviform bodies, which are globules of the blood of a rabbit, and lead to a comparative judgment of the reſpective ſizes of one and the other.

Fig. 4. repreſents ſeveral winding cylinders of the adipoſe membrane.

Fig. 5. repreſents two threads, *m*, *a*, one placed at the ſide of the other, to ſhow their reſpective ſizes, *m* belongs to the adipoſe membrane, and *a* to the external cellular membrane of a nerve. They are of an equal ſize.

ON

On the STRUCTURE of the TENDONS.

THE obſervations I made on the ſtructure of the nerves, and of the undulating progreſs of their primitive cylinders which cauſes the uncommon appearance of bands I have before deſcribed, ſtimulated me to examine with all poſſible attention the ſtructure of the tendons. I did not find any difficulty in obſerving a certain ſpiral form in them, although it did indeed appear leſs regular to me than in the nerves. This apparent ſpiral form is obſerved, not only in regarding externally the larger tendons, but even the very ſmalleſt of them. Theſe bands however, when better examined, have rather the appearance of longer or ſhorter winding ſpots, which a nice obſerver will eaſily diſtinguiſh from the bands that are ſeen in the nerves, and which we have deſcribed. When a tendon is examined with a lens that magnifies but a few times, white ſpots are perceived through the cellular membrane that covers it, as they are repreſented by Fig. 1. Plate VI. in which the tendon is magnified ſix times. Fig. 2. repreſents another tendon likewiſe obſerved with a very weak lens, and in which the ſpires, or ſmall curvilinear ſpots, were more regular, and very much reſembled thoſe obſerved in the nerves. This ſpiral ſtructure of the tendons is likewiſe obſerved with the naked eye, though not ſo diſtinctly as with a microſcope.

My principal attention was to examine nicely the elementary threads of the tendons, their ſize and their progreſs. I at firſt ſuſpected that their progreſs was analogous to that of the primitive nervous cylinders, and that the ſmall white curvilinear ſpots owed their origin, or their apparent exiſtence, to it.

This laſt reſearch ſeemed to me the more important, ſince it tended to decide whether, beſides the whole nervous ſyſtem, other organical parts were to be found in an animal, of an undulated and winding texture, like that of the elementary parts of the nerves.

All the tendinous ſubſtance in general, or rather all the tendons, when examined with a microſcope, ſeemed to be formed of a vaſt many very ſmall, ſimple, longitudinal *faſciæ*, ſeparated one from the other by the cellular membrane. Each of theſe *faſciæ*, which I ſhall call *primitive faſciæ*, becauſe they are not compoſed of ſmaller ones, is formed of an infinite number of extremely fine threads, which I ſhall call *primitive tendinous cylinders*, becauſe they are not ſubdivided into ſmaller ones, in whatever way they are prepared or examined. Theſe primitive cylinders run along the tendon for its whole length, and are ſolid throughout; that is to ſay, neither vaſcular nor hollow. They are much ſmaller than the primitive nervous cylinders, and are tied together in the primitive tendinous *faſcia*, by an almoſt imperceptible, ſubtle, and elaſtick cellular membrane. Theſe primitive cylinders appeared to me of the ſame ſize throughout the whole of the tendon in an animal, and likewiſe in all the tendons. They are homogeneous cylinders, every where uniform, neither hollow, nor formed of ſmall veſicles or globules; in a word, they are canals.

All the reſearches I made to diſcover whether theſe cylinders were compoſed of other ſmaller parts, were totally fruitleſs, ſo that I am obliged to conſider them as non-organick, primitive ſolid threads. Theſe primitive cylinders then, which compoſe the tendinous ſubſtance in its laſt decompoſition, being many of them united together, form the primitive tendinous *faſciæ*; and it is from ſeveral of theſe laſt that the tendon is at length compoſed. As the cellular membrane which binds together the primitive tendinous cylinders yields eaſily, and as at the ſame time that of the primitive *faſcia* itſelf is tranſparent, it is eaſy to diſtinguiſh the progreſs of the primitive tendinous threads, and this progreſs is ſo very like that of the primitive nervous cylinders, that it would be difficult to diſtinguiſh them. The tendinous threads elongate, and form undulations in the whole ſubſtance of the tendon, and the appearance of a ſpiral ſtructure and of bands in the tendons, as well as in the nerves, is cauſed by theſe undulations.

Fig. 3. repreſents a primitive tendinous *faſcia*, which appears formed of a vaſt number of primitive tendinous threads. Theſe threads, parallel to each other, run along the tendon, forming regular undulations, from whence, as has been ſaid, ariſes the appearance of bands or ſpires. At *r*, *r*, two of theſe threads are ſeen, purpoſely ſeparated from each other with the point of a needle.

Fig. 4. repreſents another tendinous *faſcia*, compoſed of primitive threads, *r*, *r*, *r*, obſerved in water, and ſtripped of their cellular membranes. The cylinders here are neither undulated nor winding, becauſe they have been pulled out, and deranged from their natural ſituation by the needle with which I ſeparated them.

My reſearches into the ſtructure of the cellular membrane of the nerves, which, as we have ſeen, is no other than a web of very ſmall, winding, tranſparent cylinders, led me to think that the cellular membrane of the tendons ſhould be of the ſame nature, that is to ſay, quite filled and woven with the ſame cylinders. I have in reality obſerved them with very little difficulty, and found them of

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the ſame ſize and form as in the nervous membrane, and their progreſs to be alike in both.

Fig. 5. repreſents a ſmall portion of the membrane, or cellular web, of a primitive tendinous *faſcia*. It was formed of a great number of winding cylinders, of which a few only are expreſſed in the engraving, that they may be the better diſtinguiſhed, and are indicated by the letters *r, r, r, r, r, r, r*. I obſerved them with the ſame lens with which I obſerved thoſe of the cellular membrane of the nerves, and the primitive tendinous threads of Fig. 4. and 5. explained above. The ſizes of theſe threads of the tendon, and of the cylinders of its coat, are equal to thoſe of the winding cylinders of the nerves, and almoſt equal to thoſe of the primitive tendinous threads themſelves, ſo that they may be all regarded as pretty alike in ſize, without falling into any ſenſible errour.

On the Tendinous part of the Diaphragm.

Fig. 1. of Plate VII. repreſents a portion of the diaphragm of a rabbit. *a, p, q, r,* is the fleſhy part; *a, m, c, r,* the tendinous part; *n,* is the trunk of a nerve that enters the diaphragm, and *a, r,* is a vein. What deſerves attention is, that the nerve *n,* has all its ramifications towards the fleſhy part of the diaphragm, and none towards the tendinous part. I have however found nerves in other ani-

mals, which led towards the tendinous part; but they had no further ramifications in their progreſs towards it; and I have not till now obſerved in any caſe, any nervous branch which ended in the tendinous part, as happens in the fleſhy one, where they decreaſe rapidly and diſappear. *f, f, f,* are the branches of the nerve *n*. *o, o, o, o,* are the branches of the vein *a, r,* towards the fleſhy part. *y, y,* are very ſmall branches of the ſame vien, running in almoſt a ſtraight line along the tendinous part of the diaphragm, and forming, as is ſeen, a very few ſmaller branches. *u, u, u, u, u, u, u,* are very ſmall longitudinal veſſels without ramifications, which, riſing from the vien *a, r,* run along the tendinous part.

The tendinous ſubſtance is bright like ſilver, and tranſparent; and its branches, even the ſmalleſt of them, are opake. If the ſmalleſt thread of a nerve were received in it, it would be eaſily ſeen; and this very different mode of ramification which the nerve aſſumes in the two different parts of the diaphragm, is as complete a proof that the tendons do not receive nerves, as it is certain that the muſcles do receive them; it is a complete proof that theſe two animal ſubſtances are perfectly different from each other; and it proves the falſity of one ever having been, or having ever degenerated into, the other, as many anatomiſts have believed.

The phyſical reaſon why the nerves do not ramify towards the tendinous parts of the diaphragm, and why the veſſels ſend very few ramifications, and thofe fcarcely perceptible, to thefe parts, may, I think, be principally afcribed to the fubftance of the tendons itfelf, which prefenting a greater obftacle than the flefhy part, does not allow a great and free vegetation, either to the nerves or veffels.

When the tendinous part of the diaphram is nicely examined, as well with the naked eye as with glaffes, the ufual fmall fpots and bands are obferved in it, formed in the accuftomed way, of undulating threads. Fig. 2, reprefents a very fmall portion of this part of the diaphram, in which, obferved with a very ftrong lens, is feen the undulated progrefs of the primitive tendinous threads.

The ramifications of the blood veffels likewife deferve fome confideration. The vein throws out all its principal branches towards the flefhy part of the diaphragm, or rather, its principal ramifications; and thefe are very fmall. A vaft number however of very fine veffels, without the ufual ramifications, and almoft parallel, run in a right line by the tendinous part, and pafs into the oppofite flefhy part, where they ramify, and at length lofe themfelves.

On the Structure of the Mufcles.

My obfervations on the tendons led me to examine the mufcles, or rather their elementary fibres. The ftructure of the mufcles, when obferved, is more regular and lefs uncertain than that of the tendons, although obfervers do not agree as to all the particulars. The learned Prochaſka deſerves here again a particular homage, for having ſupplied us with a ſmall work, entitled, *de Carne Muſculari*: Vindob. 1778, in which he leaves us very little to deſire on this ſubject.

This ſkilful profeſſor finds an appearance of whitiſh wrinkles, on examining the muſcular fibres with a very ſtrong lens, and is perſuaded that they are no other than ſuperficial impreſſions made by the veſſels, cellular cylinders, and perhaps likewiſe the nerves, which ſurround the ſheath of the muſcular fibres themſelves. He is of opinion, that when a muſcle is boiled in water, theſe veſſels and very delicate threads ſhorten, embrace the fibre here and there, and imprint on it the whitiſh wrinkles or crevices.

Prochaſka repreſents the appearance of theſe wrinkles by Fig. 12, of Plate IV. in his work, and this Figure is perfectly ſimilar to Figures 1 and 2 of Plate VI. of mine.

I obſerved in breaking the muſcle by degrees, with very ſharp-pointed needles, that it is at length brought into very fine threads, which no pains can divide again into leſſer ones. I ſhall call theſe filaments *primitive fleſhy threads*,

Some hundreds of theſe threads united together, form a ſimple faſcia, which I ſhall call *primitive fleſhy faſcia*. The muſcle is at length formed of a great number of theſe faſciæ.

I examined theſe fleſhy faſciæ with all poſſible attention, and with lens' of $\frac{1}{90}$ part of an inch in the focus,

focus, but have never ſucceeded in finding a ſtructure altogether ſimilar, either to that of the tendons, or that of the nerves. The greater part of the ſmall white ſpots that ran tranſverſely acroſs the faſcia, were curvilinear, ſemicircular, uniform, and uninterrupted.

Fig. 6, Plate VI. repreſents four primitive fleſhy faſciæ, in contact with each other, and covered with their cellular membrane. Two *m*, *m*, *s*, *s*, are, as I have ſaid, ſpotted circularly, and in the two others, *r*, *r*, *a*, *a*, the ſpots ſeem in ſome places to form a certain number of ſmall angles, ſuch as are deſcribed in the Figure. This is all I could obſerve to a certainty.

Fig. 7 repreſents a primitive fleſhy faſcia, covered like the four others, with cellular membrane, but only in part. I ſucceeded in raiſing this cellular membrane, as it is deſcibed in the plate, at one of its extremities, and in obſerving the primitive fleſhy threads, and at the ſame time, the ſmall circular ſpots. The primitive fleſhy threads are ſolid cylinders, equal to each other, and very perceptibly marked at equal diſtances, with reſemblances to ſmall crevices or wrinkles. I could not perceive a true undulated progreſs in theſe threads, and the ſmall curvilinear ſpots of the primitive faſcia, appeared to me to be occaſioned by the ſmall indented marks of the primitive fleſhy threads. *m*, *o*, *r*, *c*, is the part ſtill covered with the cellular membrane; *a*, *e*, are primitive fleſhy threads, ſeparated from each other.

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Fig. 8, reprefents a primitive flefhy fafcia, covered with its fheath. After feveral attempts, I fucceeded in ftripping it entirely, as it is drawn in Fig. 9. It was compofed of a very great number of folid homogeneous cylinders, which were interrupted at equal diftances, by very fmall marks or lines, which, obferved in different pofitions, might have paffed for fmall globules. I cannot decide as to their true nature, obfervation not carrying me further. Sometimes one would fuppofe thefe apparent globules to be fo many wrinkles, formed by the contraction of the threads themfelves.

I obferved them, both immediately after the animal's death, and when they were on the point of becoming putrid. Fig. 9, does indeed reprefent thefe cylinders a little undulated, and fo they appear to the naked eye; but they could not be in their real ftate, after the preparation they underwent. The letters, *r*, *r*, *r*, *r*, exprefs primitive flefhy threads, a little feparated from each other, and forming a reunion at *a*.

Thefe fucceffive obfervations forced me to allow fome difference betwixt the progrefs of the tendinous cylinders, and that of the flefhy cylinders; the appearance of fpots in the primitive flefhy fafciæ likewife feems a little different from that of the fpots of the tendons.

I alfo paid a particular attention to the coats and cellular membrane of the mufcles, and found them to be formed, as in the tendons, of the ufual tranfparent winding cylinders. Fig. 10, reprefents a fmall ſmall portion of the cellular membrane of the muſcles, which is ſeen to be no other than a web of very ſmall cylinders. *m*, *m*, *r*, *r*, ſhows their progreſs and ſize, which are exactly the ſame as in the tendons and nerves.

Difference betwixt Nervous, Tendinous, and Muſcular Subſtances.

After all that has been ſaid on the ſtructure of the nerves, muſcles, and tendons, theſe three ſubſtances ought readily to be diſtinguiſhed one from the other. It has been ſeen that the nerves are compoſed of tranſparent primitive cylinders, which appear to be filled with a mucous ſubſtance. Theſe cylinders are much larger than the primitive fleſhy cylinders, and primitive tendinous cylinders, ſo that it is abſolutely impoſſible to confound them together. The primitive nervous fibres have another characteriſtick in their progreſs; they run ſerpentining and undulating, whilſt the fleſhy fibres tend very much to a right line.

The primitive fleſhy threads of the primitive nervous cylinders are inſtantly diſtinguiſhed, not only by their being ſo very ſmall, and by their progreſs, but likewiſe by their ſolidity. They are in no way either veſſels or canals, but ſolid cylinders, homogeneous throughout. The apparent ſtructure, not only of the ſimple primitive fleſhy thread, but

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likewiſe of the fleſhy faſcia itſelf, is beſides altogether different; and when we are a little accuſtomed to examine them, it ſeems no longer poſſible to be miſtaken, and to confound one with the other. It is true that the primitive tendinous threads have an undulated and winding progreſs, like the primitive nervous canals, but they are much ſmaller, and like the fleſhy threads, are quite ſolid, ſo that it is impoſſible to miſtake them for the primitive nervous cylinders, which ſeem to be filled with a diſtinct ſubſtance. Neither can the fleſhy threads be eaſily miſtaken for the tendinous threads, although both of them are ſolid and of equal bulk, becauſe the latter are evidently diſtinguiſhed by their winding progreſs, which is not ſeen in the fleſhy threads; and becauſe they do not alter in ſize or ſhape on their progreſs, in oppoſition to the fleſhy threads which are interrupted continually by ſmall criſpations and nodoſities.

Theſe characteriſticks being once well eſtabliſhed, I repeat that it is no longer poſſible to confound theſe three animal ſubſtances, the nervous, the fleſhy, and the tendinous, together. I have many times made the proof of this, without ever having been deceived. I employed a perſon to put the ſmalleſt imaginable particle of a nerve, a muſcle, or a tendon, at his choice, under my microſcope; theſe particles were detached with the point of a very fine needle, from the above ſubſtances ſwimming in water; a moment's examination enabled me to

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know them to a certainty, and to diftinguifh to which of the three fubftances they belonged.

It would be fuperfluous to dwell on the importance that the diftinctive characterifticks I have fixed on the primitive ftructure of the nerves, mufcles, and tendons, may be of to anatomy, and to animal phyficks. It is ftill doubted whether many of the parts endued with motion, have, or have not, mufcles. No one can be ignorant of the difpute on the flefhy fibres of the uterus, particularly on the exiftence of the *mufculus orticularis* of Ruifch. When we fee an Albinus pafs this mufcle in filence, when fpeaking of the uterus; a Haller who cannot find it; and Ruifch himfelf who appears to difvaow it in his old age, we remain in fufpence on viewing the excellent refearches a great Englifh anatomift has made into this mufcle (*a*).

All the difficulty lies in knowing whether what is by fome called a mufcular fubftance in the uterus, is really fo. The exiftence of a fubftance in this vifcus is inconteftible, and this fubftance by fome is faid to be flefhy, whilft others openly deny it that quality. A very fmall particle examined by a good obferver in a microfcope will decide the queftion. The characterifticks of the flefhy fibre are too clear to be confounded with other animal fubftances. The nature of it may then be determined in a few inftants, and the queftion, which to the great fcandal of anatomy has exifted for half a century, may be decided.

(*a*) Hunter *de Utero Gravido.*

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The ſame may be ſaid of the other parts of the animal machine, and a like trial may be made, when there is a doubt whether any particular part does, or does not, receive tendinous fibres. The characteriſticks of theſe laſt are no more equivocal than the fleſhy fibres, and a microſcope may determine to a certainty whether ſuch parts are tendinous or not.

In a word, I believe the well eſtabliſhing the characteriſticks of the three ſubſtances, the nervous, fleſhy, and tendinous, to be highly advantageous; and if I had found myſelf in favourable ſituations, I ſhould already have made applications which would have been uſeful to the knowledge of the human body, and ſhould probably have diſſipated many doubts, and terminated many queſtions, as to the ſtructure of many of its parts. Wherever, for example, there are tendinous fibres, it would not be difficult to explore them, and to aſcribe to every part the texture it really has.

I was deſirous in the mean time of examining, according to the rules I have eſtabliſhed above, what the ſtructure of the ſmall blood-veſſels is; and in ſpite of my attention and patience in the courſe of my obſervations, I have not been able till now to ſee any thing that might lead me to ſuſpect, that there were either nerves or muſcles in the textures of their coats. I do not pretend however to aſſert any thing deciſive on the ſubject, and even wiſh other obſervers to buſy themſelves upon it, and to ſee whether or no I have been miſtaken. But they will in the mean time allow me not to admit of thoſe theo-

theories that have a nervous or muſcular ſtructure, of which they ſuppoſe the blood veſſels to be formed, for their baſis, and which they do not ſee.

Many things have been written, and moſt of them uncertain, on the ganglions. It is generally believed that they ſerve to collect the medullary part of the nerves, and to furniſh an origin, as if each of them were a ſmall brain, to new threads of nerves. By entering upon a ſhort inveſtigation of the ganglions, a good obſerver may eaſily know the ſtructure of theſe organs, and form a truer conception of their uſes, which ſeem to be very important to the animal economy.

Were I in circumſtances more favourable to this ſpecies of obſervations, I ſhould not fail to examine all theſe parts, and ſtill many others, which I am obliged, for the preſent however, to leave to the induſtry of other obſervers. I ſhall only ſay two words on tendons. Anatomiſts diſpute whether they be or not, an elongation of fleſhy ſubſtance, that is to ſay, of the ſame nature as muſcular fibres. I can take upon me to ſay that I have never ſeen a primitive fleſhy thread, nor a primitive fleſhy *faſcia*, become tendinous, however I have multiplied my obſervations, particularly on the tendinous and muſcular parts of the diaphragm, in ſmall animals. I have ſeen primitive fleſhy *faſciæ* terminate as a fleſhy ſubſtance, and have ſeen primitive tendinous *faſciæ* introduce themſelves betwixt the fleſhy *faſciæ*, but it was not to unite their ſubſtances.

ſubſtances with that of the others. In a word, one *faſcia* does not begin where the other ends, but they introduce themſelves into each other, like the cogs of two wheels that faſten and riſe one within the other; the tendinous threads particularly, penetrate a great way amongſt the muſcular ones.

On the primitive winding Cylinders of the Animal Body, or on the Cellular Membrane.

The primitive winding cylinders I have diſcovered in the cellular membrane of the nerves, tendons, and muſcles, are of all the parts or organs that I know of, in the animal body, the ſmalleſt. They are, as has been ſeen, much ſmaller than the fineſt blood veſſels, that do not allow but one globule of blood to paſs at a time. All the attempts I have made to reduce them into ſmaller cylinders, have been ineffectual, and although they are obſerved with glaſſes of the greateſt ſtrength, they appear very ſimple, and not ſurrounded by other ſmaller veſſels.

The philoſopher who is not fond of hypotheſes, who admits no other ſtructures or parts in the animal body as certain, than thoſe which obſervation has diſcovered in it, will find no difficulty in conſidering theſe winding cylinders, as primitive ſimple principles, not compoſed of other leſſer ones. This is a *datum* of which obſervation demonſtrates

monſtrates the reality, and from which we muſt ſet out, to reaſon fundamentally on the functions of the organical parts of a living body.

One general purpoſe of theſe winding cylinders may be, if they are really veſſels, to nouriſh the parts in which they are found, or which are ſurrounded by them. According to this hypotheſis, they may perhaps ſerve for the nutrition of the primitive cylinders, as well nervous, as tendinous and fleſhy. But there is another purpoſe, ſtill more noble, and perhaps equally important, to be aſcribed to them; the principal functions of life may even depend on them: the ſmalleſt changes made in theſe organs, may cauſe the utmoſt diſorder in the animal economy.

My experiments on poiſons have ſhown that they bring about the death of an animal in an unknown way, and we ſeem to need the diſcovery of a principle, an organ in ſhort, on which theſe poiſons act. Who knows but this principle, this organ, may conſiſt in the winding cylinders we have obſerved? But what can be expected from an inſenſible ſubſtance, and one on which poiſons do not ſeem to have any action?

But before we proceed farther, we muſt ſee whether theſe canals or cylinders are found in other parts of the animal, and whether they form a ſyſtem of veſſels and organs, hitherto unknown.

We firſt obſerved them in the external cellular membrane of the nerves, tendons, and muſcles. I have ſince ſhown how they may with eaſe be found

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in all the cellular membranes of theſe organs; ſo that their whole cellular ſubſtance is a web of winding canals.

I have obſerved that when the primitive nervous cylinder is covered with winding cylinders, its ſize is more than double what it is when it is ſtripped. A great number of theſe nervous cylinders form nerves, which are larger or ſmaller, and ſeveral of theſe nerves are uſually united together, to form the very large ones. All theſe nerves have proper cellular coverings and common ones, and theſe coverings are made of winding cylinders. If I now ſuppoſe that the maſs of nerves is formed of two parts of winding cylinders, and of only one part of primitive nervous cylinders, I do not think I am much miſtaken. By applying the ſame mode of reaſoning to the tendons and muſcles, it will be found that the winding cylinders make up the greater part of theſe two ſubſtances, becauſe the primitive tendinous and fleſhy threads are of the ſame ſize with the winding cylinders, and theſe encreaſe their maſs by the numberleſs windings they make round the primitive cylinders of theſe parts, over which they heap themſelves.

Some hundreds of primitive threads, whether fleſhy or tendinous, form the primitive *faſcia*; and a great number of theſe at length form the muſcle or tendon; ſo that I think I ſhall not be led into an errour by the belief, that out of ſix parts, of which the muſcles and tendons of an animal are compoſed, five of them are winding cylinders,

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and one only the primitive threads of theſe two ſubſtances.

We already ſee that according to this, a great part of the animal ſolids is compoſed of winding cylinders; it remains to examine whether theſe cylinders are found in other parts. This reſearch, which will throw a great light into the knowledge of anatomy, is very important.

Firſt, it is natural to ſuppoſe, that if theſe winding cylinders are generally found in the cellular membrane of the nerves, muſcles, and tendons, they ought likewiſe to be found in the cellular ſubſtance of other parts; and as the membranes themſelves are no other than cellular, the winding cylinders ſhould conſequently be found in theſe membranes. It would be tedious to give a detail here of my obſervations on theſe parts; I reſerve the doing this for another occaſion. It will be ſufficient to ſay that I have found the whole cellular ſubſtance to be formed of winding cylinders, in whatever part of the body it is met with. I have found them in the membranes of the brain, in the pleura, the peritoneum, the meſentery, the mediaſtinum, the pericardium, the perioſtium, and the pericranium, and in the ligaments of the liver and other viſcera. The membranes of the arteries and veins are formed of theſe cylinders, and their internal coats which ſeem ſo compact, are woven with them. All the cellular ſubſtance, the ſacculi, and the veſicles which contain the fat, are a web of theſe winding cylinders. In a word, I know of no part in the body which has a cellular membrane, and does not present these winding cylinders. I should except the membranes of the vitreous and crystalline humours, in which I was not able to observe it; and also the lamina of the transparent cornea, which did not present them to me with certainty. If these membranes be destitute of winding cylinders, they must be distinguished from the ordinary cellular membrane, and are of another nature.

If it now be considered that the cellular substance is found in all the organs of the animal body, and that all the solids are principally composed of it, it will readily be concluded, that the winding cylinders form the greater part of these solids, and that all the rest is very trifling when compared with them.

The use of so great a number of cylinders is without doubt of the highest importance; but this is not the place to treat of it. The subject requires new observations, and very many experiments, the greater part of which I have not yet made. I have indeed thought that the matter of which they seem to be formed, is a glutinous substance similar in consistence and colour to a gelly or a mucous matter. I am somewhat apt to suspect, that the gelatinous matter which is drawn from the animal substance, is nothing else than the matter of which these winding cylinders are formed. But I repeat that I have not yet made a sufficient number of experiments to determine with certainty, either their real nature, or the uses to which they may be destined in the living body. It is enough for me at present to have established the existence, size, and extent of them.

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REFLECTIONS ON THE MOTIONS OF THE MUSCLES.

THE primitive nervous cylinder is abſolutely ſimple, is never found larger or ſmaller in its progreſs, and is not ſubdivided into leſſer branches. It appears beſides to be filled with an homogeneous, tranſparent, concrete, matter or fluid, which ſeems in different caſes, to form itſelf into irregular ſpheroidal corpuſcles, more or leſs elongated, and generally of a much ſmaller ſize than that of the red globules of the blood.

In animals, the nerves ramify much leſs than the arteries and veins, and their ramifications diminiſh in ſize much more than thoſe of the blood-veſſels; obſervation has therefore eſtabliſhed this truth, that there is a leſs number of ramifications of nerves, in any given part of an animal whatever, a muſcle for example, than of veins and arteries: it follows hence, that the ſpace occupied by the blood veſſels muſt be much greater than that occupied by the nerves.

The primitive nervous cylinder is about thrice as large as the primitive blood-veſſel, and this laſt is about four times as large as the primitive fleſhy thread. Thus is the primitive nervous cylinder about twelve times the ſize of the primitive fleſhy thread, and when the primitive fleſhy faſciæ are attentively examined, we can ſcarcely ſay that we obſerve blood-veſſels in them, and remain uncertain whether we really ſee nerves in them or not; and it is abſolutely impoſſible in any circumſtance, to ſee either veſſel or nerve proceeding to the primitive fleſhy threads. If there were any blood-veſſel or ſhred of a nerve betwixt the primitive fleſhy threads, theſe threads would not meet in contact, as they actually do for their whole length; their diſtances would be rendered four times greater by the blood-veſſels, and twelve times greater by the nervous cylinders. Theſe would be much more viſible to the microſcope, on account of their ſize; but we obſerve nothing of them. The fleſhy threads cling to each other in ſuch a way for their whole length, leaving ſcarce any ſpace betwixt, that we can ſcarcely perceive the very fine cellular web, or glutinous principle, that faſtens them together.

We may I think deduce from hence, that a muſcle is in the greateſt part formed of primitive fleſhy threads, and in the leaſt of primitive nervous cylinders.

ders. The blood veſſels preſerve a medium betwixt theſe two quantities.

We may likewiſe reaſonably deduce, that the primitive fleſhy threads are not accompanied throughout, nor environed on all ſides, by blood-veſſels, and ſtill leſs by nerves; and I am almoſt inclined to think, that an entire fleſhy faſcia, ſcarcely receives any primitive blood-veſſel, or primitive nervous cylinder. Hence it is ſcarcely probable, not to ſay abſolutely impoſſible, that every fleſhy thread can receive, either a venous canal, or a primitive nervous cylinder. Theſe ſeem to be the natural conſequences of immediate obſervation, and of the very great diſproportion there is betwixt the ſize of the primitive fleſhy threads, and that of the minuteſt blood-veſſels, and the primitive nervous cylinders.

It would be unaptly objected, that the primitive fleſhy threads could not vegetate, if there were not blood-veſſels to nouriſh them; that they could not contract, if there were not nerves throughout; and that they would not be ſenſible, if they were not accompanied throughout by primitive nervous canals. It is needleſs to remind, that to enable any part to vegetate or encreaſe, it is ſufficient that a fit humour be conveyed to this part; and that a humour may reach a part, by other veſſels than by thoſe of the blood, or by ſimple tranſudation. The contraction of the muſcles does not neceſſarily ſuppoſe that the nerves muſt penetrate into all the minuteſt part of a muſcle, and that they ought to touch, ſurround, and envelope every one of its elementary parts, which could not even be combined by the ordinary quantity of nerves. Senfibility is in the mufcles in general, and no experiment can demonftrate that the primitive flefhy thread is fenfible, and much lefs, that it is fo in all its parts. Our fenfes are too blunt to reach fo far as this.

The confequences we have juft drawn from the primitive ftructure of the nerves and mufcles, not only do not favour the different hypothefes naturalifts have imagined to explain the contraction of the mufcles; but likewife demonftrate that the greater part of them is abfurd. What however feems very clear is, that this undulated ftructure of the primitive nervous cylinders, and of the flefhy threads and primitive tendons, ferves admirably well to refift very violent efforts in the machine; thefe parts being able to bear a very great extenfion without breaking.

An important queftion now prefents itfelf, which is precifely occafioned by the very ftructure of the primitive nervous cylinders. It is, to know whether the nerves are irritable; that is to fay, whether they contract when ftimulated by any fubftance, or when any of the mufcles are contracted at will. All the obfervations and experiments hitherto made by the beft naturalifts, affure us that the nerves are not acted on by any ftimulus, that is to fay, that they do not diminifh in length, nor alter their fize, whether they are externally pricked, or whether the animal employs them in contracting the mufcles: but thefe experiments prove nothing, unlefs it be that the external coat of the nerves is in thefe cafes altogether ufelefs; they do not prove that the internal

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parts of these same nerves is motionless, and that the primitive nervous cylinders are not capable of shortening themselves. The external coat of the nerves is not so formed as in any way to prevent an oscillatory or contracting motion of their primitive canals, and the winding structure or progress of these cylinders would lead one to suspect the contrary.

But it must be left to experiment to decide on all occasions when it can be consulted; our reasonings very seldom exceed the limits of simple conjecture, even when they seem founded on the completest analogy.

If the primitive nervous cylinders change their situation whenever the nerve is stimulated, the spiral figure which results from the position of these cylinders, ought necessarily to be more or less changed, and the spaces betwixt one spire and another, or betwixt one band and another, must become greater or smaller: it is at least certain, that the progress of the primitive nervous cylinders in a nerve cannot be deranged, without the spires being deranged likewise. A common lens enables one to see the spires of the greater part of the nerves very well; thus is the observation easy, and the preparation of the parts for the experiment not at all difficult. I principally made my observations on the nerves of the diaphragm of small animals, such as rabbits, and on the crural nerves of frogs. As soon as I had opened the animal, I employed some one to stimulate the phrenick nerves

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in the thorax of the rabbits, and the crural nerves at their going out of the vertebræ. The diaphragm, and likewise the legs and feet of the frogs contracted themſelves. At this time I obſerved the ſpires of the nerves with ſuch attention, that the ſmalleſt change in them could not eſcape me. I examined the nerves of the diaphragm at their minuteſt ramifications, where the ſpires are the moſt ſimple. I repeatedly examined the ſmall nerves in frogs, that are ſent to the abdominal muſcles, and in which the ſpires are yet more viſible. But whatever diligence and attention I beſtowed on them, I never could perceive any alteration in their ſpires. However violently I ſtimulated the nerves, and however great the contraction of the muſcles, I invariably found them motionleſs, and at the ſame diſtance from each other as before.

Having loſt the hope I had conceived, of ſeeing ſome motion in the ſpires of the nerves, I proceeded to the laſt experiment I had to make on theſe organs, which was to examine in an immediate way, their primitive nervous cylinders themſelves. This obſervation coſt me a great deal of pains, and I cannot flatter myſelf with having ſo nicely obſerved the abſolute immobility of theſe threads, as I did that of the ſpires. But what I obſerved in them did not indicate motion, and as they have always appeared to me at reſt, I ſhall invariably believe, till ſome one ſhall be more ſucceſsful than myſelf, that theſe primitive nervous cylinders are in that ſtate, when the nerves are ſtimulated, and the muſcles contract; and I ſhall believe it the more readily, as I cannot conceive how the cylinders can undergo a ſenſible change and vary their ſituation, whilſt the ſpires or bands remain quite motionleſs.

This ſingular ſtructure of the nerves, and the regular progreſs of the primitive nervous cylinders, which ſeemed to promiſe ſome freſh diſcovery as to muſcular motion, and ſome new uſe of the nerves themſelves, leave us as much in the dark as before, and only ſerve to diminiſh the number of hypotheſes that have been imagined to explain the contraction of the muſcles: ſo true is it that before we can attain a knowledge of the truth, we muſt paſs through all the errours that ſurround it. The ſyſtem of the vibrations of nervous threads, for example, ſeems no longer capable of being ſupported, after my obſervations. I ſpeak of thoſe vibrations that are capable of being perceived by the microſcope, and I give up to metaphyſicians the privilege of inventing inviſible ones.

The pretended very great velocity of the nervous fluid ſeems to be contradicted by that inert, viſcous fluid or matter with which the primitive nervous cylinders appear filled. The conſiderable ſize of the nervous cylinders and blood-veſſels, when compared with the primitive fleſhy threads, leads me to ſuſpect that theſe threads are not put in motion, in an immediate way however, either by the blood, or by the nerves. In a word, we are not only ignorant of muſcular motion, but we cannot even imagine any

any thing to explain it, and we ſhall apparently be driven to have recourſe to ſome other principle: that principle, if it be not common electricity, may be ſomething however very analogous to it. The electrical gymnotus and the torpedo, if they do not render the thing probable, make it at leaſt poſſible, and this principle may be believed to follow the moſt common laws of electricity. It may likewiſe be more modified in the nerves than in the torpedo and gymnotus. The nerves ſhould be the organs deſtined to conduct this fluid, and perhaps alſo to excite it: but here every thing yet remains to be done. We muſt firſt aſſure ourſelves by certain experiments, whether there is really an electrical principle in the contracting muſcles; we muſt determine the laws that this fluid obſerves in the animal body; and after all it will yet remain to be known what it is that excites this principle, and how it is excited. How many things are left in an uncertain ſtate, to poſterity!

Microſcopical Errours; and Conſequences deduced from Microſcopical Obſervations.

Such have been my obſervations on the ſtructure of the nerves, tendons, and cellular membrane, and my conjectures on ſome of the uſes of theſe parts. I have ſo contrived as to give, to my principal obſervations at leaſt, all poſſible certainty. I have endeavoured

deavoured to render them as ſimple as poſſible, and have eſtabliſhed them by direct and diverſified experiments.

There is a great difference betwixt a microſcopical obſervation, and a microſcopical experiment. The firſt is no other than a ſimple repreſentation of an object, in the circumſtances in which we obſerve it; the ſecond is the analyſis of the repreſentation of this object, by which we are aſſured that an object of ſuch a nature, and not of any other, does actually exiſt. In the firſt caſe, we ſimply feel an impreſſion of the light, or view at moſt nothing more than an image; in the ſecond, we judge of the nature of an object by the image it preſents to us. Every one is capable of diſtinguiſhing with the microſcope; but few know how to judge of the things they ſee. Extenſive knowledge, and the greateſt ſagacity, are requiſite to an obſerver, to conceive the experiments that neceſſarily conduct to a true acquaintance with a real object.

There is a ſpecies of errour into which even the moſt practiſed obſervers may eaſily fall. All our opinions on bodies are no more than ſimple compariſons; that is to ſay, we judge that ſuch a body, ſeen with the microſcope, is of ſuch a nature, and not of any other, becauſe it preſents itſelf to us under the ſame forms or appearances, under which we are accuſtomed to ſee ſome object, when we examine it with a microſcope, that is already known.

It is morally impoſſible that two images ſhould be every way alike, and that the external objects

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they reprefent fhould be every way different. An obferver of penetration, if two objects are not fimilar, will at length find fome difference in the appearance of them, and will avoid an errour. But how much induftry and attention muft he not neceffarily employ ? This however is not the moft dangerous fhoal, although very enlightened obfervers fometimes run aground upon it. It fometimes happens that we fee a certain object with a microfcope, which feems to agree in every refpect with the bodies we are beft acquainted with, particularly when the ftructure of thefe bodies is not very common. It then feems impoffible to us that an image can have fo fingular and perfect an agreement with objects that we know, and that the object which furnifhes it can be notwithftanding of a perfectly different nature ; it is not impoffible however but that this may happen. When, afterwards, the images that are reprefented are irregular, and thefe irregularities are fo many confequences refulting from the object which we figure to ourfelves to exift, we have not a moment's doubt, and yet may ftill be deceived.

To prevent the fpringing up of thefe errours, it is abfolutely neceffary to analyfe the obfervation itfelf ; that is to fay, to make a microfcopical experiment capable of affuring us, that the object is in reality fuch as the microfcope has figured it to us. This is the moft difficult part of the undertaking, and this is what makes a difference betwixt obfer-

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vation and obſervation, betwixt obſerver and obſerver.

A ſimple bare obſervation does not admit a full confidence, even although it be made by a celebrated obſerver, becauſe it tacitly ſuppoſes that there is a neceſſary and excluſive agreement betwixt the image repreſented by the microſcope, and the real exterior object; which is not always the caſe. The image repreſented by the microſcope may agree with ſeveral objects at the ſame time, and it belongs to the induſtrious obſerver to determine which is the real exteriour object it repreſents and correſponds with. It is then not ſufficient to ſay, I have ſeen ſo and ſo. The circumſtances muſt be determined, and the obſervation varied a thouſand different ways; it muſt in ſhort be analyſed, and the experiments rendered deciſive.

Of all microſcopical obſervations, I know of no one that can eaſier lead the moſt conſummate and penetrating obſerver into an errour, than the external ſtructure of the nerves, in all of which it ſeems impoſſible for us not to diſtinguiſh, and that very conſtantly, a ſpiral form of the utmoſt regularity. The more we obſerve them, whether they be large or ſmall, ſimple or compoſed; whether we examine their principal trunks, or their leaſt viſible branches; we remain more and more perſuaded of their having a conſtant ſpiral ſtructure. Their very irregularities ſerve as proofs that they are all of this form, rather than they tend to undeceive us.

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They not only conciliate themſelves to this ſtructure, but beome ſo many luminous conſequences of it.

The ſtructure of the nerves I have examined, tends greatly to a doubt, even of thoſe obſervations that appear the moſt conſtant and certain, and ſhould at once inſpire us with moderation and miſtruſt. A haughty and confident tone only becomes an ordinary obſerver, quite ignorant of the innumerable circumſtances which may deceive him, and is frequently the character of a viſionary, who is more eloquent than judicious; of a learned man who loves rather to divine nature in his ſtudy, than to conſult her where ſhe is; or rather, of one who is fond of ſubſtituting dreams and viſions, to truths and matters of fact. A ſingle experiment, one lonely obſervation, has often reduced to vapour, whole libraries of ſuch philoſophical romances; and to the ſhame of our age be it ſaid, they are ſtill multiplied, and perſons are found who are fond of loſing their time in reading them.

No part of what I have juſt now obſerved can be applied to the celebrated Dr. Monro of Edinburgh. He has not ſought to divine the nature of the nerves, but has examined into it; neither has he invented vain hypotheſes, but has conſulted nature herſelf. If he has been notwithſtanding miſtaken, it can only be obſerved, that errour is more acceſſible than truth. Truth is ſimple, errour is infinite; or rather, if truth expreſſes itſelf by unity, errour is capable of expreſſing itſelf by infinity, or

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at leaſt in ſo extenſive a way as nearly to approach it. It is certain, in relation to us, that errour aſſumes all the appearances of truth, and may ſeduce us a thouſand different ways. To be ſatisfied of this, it will be ſufficient to read the hiſtory of human errours. The greateſt philoſophers have not been exempt from them, and the moſt enlightened nations have fallen into errours that have continued as long as themſelves.

The efforts then that man makes to come at the truth, are a kind of game of hazard, in which the probability of falling into an errour is very great, and that of attaining at a truth very ſmall. Thoſe who arrive the lateſt are leaſt ſubject to err, ſince they profit by the errours of others, by finding the number of caſes that lead to them diminiſhed. Hence ariſes the impoſſibility of a work, complete, and original in all its parts, being produced by one man, in a ſingle age.

I hope the enlightened reader will pardon me this ſmall digreſſion on microſcopical errours, and that he will look upon it as a kind of apology for my work itſelf, which, whatever care I have taken to beſtow the attention of which I am capable, and which my circumſtances have permitted me, on my obſervations, I do not think free from human imperfections. And where is the obſerver who can ever aſſure himſelf of having ſeen every thing, and if I may ſo expreſs myſelf, of having exhauſted nature, in the firſt trials he may have made, on ſo many ſmall and diſtinct parts of the animal body!

After a century of obſervations made on the blood by ſo many good obſervers, from Lewenhoeck down to Father della Torre, we ſtill ſeem ignorant of the figure, compoſition, ſize, and ſtructure of the corpuſcles that give a red colour to this fluid: this will be ſeen in my *microſcopical obſervations*, when I determine, imperfect as they are, to make them publick. It will now be ſufficient to excite the attention of naturaliſts on theſe ſubjects, and to give a neceſſary impulſe to the moſt ſkilful obſervers, to the end that they may beſtow their induſtry on a ſubject ſo important to the underſtanding the mechaniſm of the animal machine. When I ſhall myſelf return to theſe matters, with a mind tranquillized and refreſhed, I may add many important things, that may have eſcaped me the firſt time; and may correct others which I now think have been well obſerved. The true merit of a naturaliſt is in proportion to the number of diſcoveries he has made, and to the difficulty and importance of them. Man is liable to errours, and they ſhould therefore be pardoned him. They muſt be conſidered, to adopt the language of geometricians, as *quantitates evaneſcentes*, in compariſon to the diſcoveries; when theſe laſt are many, uſeful, and original. *Dices enim*, (ſays the great Haller) *cum fide autorem eſſe, cum quo naturam ſæpius conſentire videbis, &c.* But when theſe diſcoveries are not found in a book, errour entirely diſcredits the work, and its authour. The leaſt blameable is the man who copies truths already publiſhed,

published, without adding errours to them; but his name will not be handed to posterity, because it will be indebted to him for nothing new, which may serve to recall the recollection of him.

Nothing but ignorance and envy can possibly confound the merits of these two classes of writers, and unjustly attribute to one, what the other alone is deserving of!

Observations on the Hairs, Epidermis, Nails, Bones, and Fat.

The learned Edinburgh professor, after having examined the parts of animals that are most composed and most interesting, those on which we have remarked above, extends his observations to many other parts, less important if you please, but which on that account are not better known: such as the nails, hairs, epidermis, &c.

He finds nervous spires likewise in these parts, or pretends rather that they are composed of nervous cylinders, although he agrees besides, that their nervous substance is not sensible; that is to say, that the nerves woven in them are not organs of sensation, as they are in general in all the other parts of the animal body. The opinion of Dr. Monro is by no means absurd, although it may be false on the side of observation; because it is certain that many of the parts which are sensible, and provided

provided with nerves, harden, oſſify, and loſe their ſenſibility. But does obſervation clearly demonſtrate that all theſe parts have been compoſed of nerves?

Dr. Monro aſſures us that theſe organical parts are woven with cylinders entirely ſimilar to thoſe of which he believes the nerves to be compoſed. This reſemblance can only have a relation to the ſize and reſpective figures of the part, ſo that the conſequence he wiſhes to deduce from it, is neither a demonſtrated, nor an eſtabliſhed, truth. But though it ſhould even be proved that all theſe parts are compoſed of winding cylinders, in every reſpect like thoſe that ſurround the primitive nervous cylinder, it does not on that account follow, that they muſt be compoſed of nerves, nor that they muſt neceſſarily accompany the nerves in theſe parts; ſince we have ſeen above that the cylinders which ſerpentine and run along the nerves, although they form the coats of nerves, are not ſo in themſelves. The cellular ſubſtance in the body is quite woven with theſe cylinders, and is nevertheleſs not compoſed of nerves.

But this does not render the Doctor's obſervation unworthy attention. If all the parts in the body had the ſame ſtructure, if they were all compoſed of winding cylinders, and if theſe cylinders were in all of them of the ſame ſize, the knowing that there is a general primitive texture common to all the ſolid parts of the body, would however be an excellent diſcovery.

Let me examine then the reality of this particular, and in the course of my obſervations, pay as ſcrupulous an attention as I can to theſe ſmall bodies, which can ſcarcely be diſtinguiſhed with the ſtrongeſt glaſſes.

My firſt obſervations relate to hairs. I ſhall only mention a few of the principal of them; as many as are neceſſary to give a general idea. I took a hair, and cleanſed it ſeveral times by paſſing it ſlightly acroſs a piece of fine linen moiſtened with water; I then examined it with lens' of different ſtrengths, employing ſome that magnified 400 times, and ſo upwards to 700, and have invariably ſeen the ſame ſtructure, and the ſame compoſition. Fig. 1. Plate I. repreſents an end of an hair, in which, towards its axis at *a*, *a*, I obſerved a dark ſpot, which was interrupted towards the middle of its length. All the reſt of the hair was of the colour of tranſparent amber. It appeared to be woven, and formed, or covered, with ſmall interrupted cylinders, winding in the manner of inteſtines. Among theſe cylinders, there were here and there very ſmall globules, the diameters of which did not exceed thoſe of the cylinders themſelves, and theſe globules were in ſeveral places ſituated betwixt the intervals of them. The figure mentioned above repreſents all this very clearly.

I bruiſed the hair *m*, *m*, towards one of its extremities, and it ſeemed to me to be formed of ſeveral irregular rugged trunks, compoſed of groups, or bundles, of very ſmall winding cylinders. Fig.

2. reprefents one of thefe trunks, which I bruifed more than I had done before. I found it as it is feen in the figure, that is to fay, formed of winding cylinders, with many globules or round bodies, difperfed over them.

Fig. 3. reprefents a fmall portion of the hair of Fig. 2. feparated with the point of a needle; in it are feen feveral globules detached from the winding cylinders.

Fig. 4. fhows another fmall portion of the hair of Fig. 2. which is pretty much like the other. However when I looked at it after I had wetted it well with water, it was become like an irregular tranfparent pellicle, in which there fcarcely remained any traces of globules or winding cylinders; but after I had left it to dry, it foon recovered its firft fhape, appearing as it does in Fig. 4.

On the Tranfpiration.

The learned Father della Torre, who has bufied himfelf fo much in obferving the minuteft parts of the animal body with very ftrong lens', affures us of his having difcovered that we tranfpire through the epidermis, an immenfe quantity of fmall, tranfparent, differently fhaped lamina. Thefe lamina heaped fome over the others, compofe the fcales of the cuticule, which he fays is interwoven with lymphatick veffels. Laftly, he adds, that thefe theſe ſmall laminæ are even ſeen with the naked eye, in the viſcous humour that covers the ſkin of eels.

In imitation of Father della Torre, I waſhed one of my fingers well in ſeveral waters, and having repeatedly dried it, applied it to a glaſs, keeping it there for ſome time, till the glaſs became ſoiled with it. On making uſe of a ſtrong lens, which magnified upwards of 700 times, I could diſtinguiſh nothing beſides very ſmall tranſparent globules, which neither dried nor diſappeared, as aqueous vapours do, but continued the ſame on the glaſs. Although there were ſeveral that touched each other, they did not unite their ſubſtances, to form larger ones. They were all of the ſame ſize and equally round, as is ſeen in Fig. 6. which induces me to believe that they are not a pure aqueous vapour, but rather a groſs oily ſubſtance. They are about four times ſmaller in diameter than the red globules of blood. Fig. 7. repreſents the ſize of a globule of blood, ſeen with the ſame lens as the globules of Fig. 6.

I repeated theſe experiments a great many times, on the tranſpiration of the epidermis of many other parts, and could only obſerve the globules of which I have ſpoken.

On the Gluten of Eels.

Being deſirous of examining the gluten of the ſkin of eels, I had ſeveral brought me of different ſizes,

 and

and found, after taking the gluten in a very ſmall quantity, and diluting it a little in water, that it was formed of uniform irregular bladders, filled with very minute ſpheriodal corpuſcles, as they are deſcribed by Fig. 8.

I ſuffered them to dry on the glaſs, and they then appeared as they are ſeen in Fig. 9, that is to ſay, more irregular than before, with large tranſparent edges all round them, with irregular corpuſcles ſcattered in different parts within.

I broke two or three of theſe bladders or veſicles, and a great number of very ſmall corpuſcles poured from them, as is ſeen in Fig. 11.

The veſicle *a*, of Fig. 10, repreſents one of the veſicles of Fig. 9, in which an oviform body is obſerved, with a ſpot in its middle. At its ſide is placed a globule of blood, *c*, by way of comparing their relative ſizes.

On the Epidermis.

After having repeatedly waſhed one of my fingers with great care, I detached from the epidermis ſeveral very fine laminæ, which had ſcarcely a ſenſible thickneſs, with a razor. Fig. 12, of Plate VIII. repreſents one of theſe fragments examined with a very ſtrong lens. It appeared to be a web of winding cylinders which approached each other, and retreated, with great order and regularity: very ſmall globules were here and there diſtinguiſhed in it. I now covered

covered one of theſe laminæ with water, in which ſtate it appeared more tranſparent, and the cylinders and globules were ſeen more diſtinctly than before. I employed glaſſes which magnified 700 times in diameter, but could perceive nothing more. Neither hole nor poroſity could be diſtinguiſhed with the microſcope, ſo that I can venture to ſay, that thoſe who believe they have ſeen pores in the epidermis, are abſolutely deceived. Lewenhoeck was the firſt to lead other obſervers into this errour. Not that I wiſh to aſſert that there really are none, I only ſay that they are not obſerved with the microſcope. It is very probable that the lymphatick veſſels which Father della Torre ſays he obſerved in the epidermis, in the form of net-work, are no other than my winding cylinders; and I think them badly deſcribed in Fig. 7, of Plate XII. of his work, and that they do not form a net-work, as he wiſhes to believe.

On the Nails.

I detached a very fine lamina from one of my nails, with a razor, and on examining it with very ſtrong lens', found it to be formed of the uſual winding cylinders. They appeared here however a little cloſer than uſual, and perhaps ſomewhat ſmaller. I found the uſual globules ſcattered amongſt them. Fig. 14. repreſents a ſmall portion of a nail, which I examined. I then covered another portion with water, and

obſerved

obſerved it in this ſtate. It ſeemed to me that the winding cylinders were diminiſhed in number, and were in ſome meaſure ſtraightened. Every thing appeared more tranſparent and homogeneous, as is repreſented by Fig. 15.

On the Bones and Teeth.

The very bones, and the enamel of the teeth itſelf, are compoſed of the uſual winding cylinders. I have not thought it neceſſary to deſcribe them particularly by figures. The only difference I obſerved was, that the winding cylinders which compoſe the enamel of the teeth, are ſhorter and better united together.

On the Fat.

I wiſhed likewiſe to examine the fat, and obſerved that of ſeveral animals in its natural ſtate. I found betwixt the laminæ of the cellular membrane an infinite number of ſmall bags or bladders, which differed in ſize in different animals. Theſe veſicles were filled with fat, or with an oily humour, either more or leſs concrete, or quite liquid. The veſicles were heaped ſome over the others, and covered and ſurrounded on all ſides with winding cylinders, as is ſeen in Fig. 19, of Plate VIII. By the means of warm water, and a few pricks of a needle, I contrived

trived to ſeparate from theſe veſicles ſeveral of the winding cylinders, and I then perceived a bladder formed by a very ſubtile, tranſparent, homogeneous, humour, filled with fat, as at Fig. 20. I could not obſerve any thing faſtened to it, nor any excretory or ſecretory veſſels, whatever attention I paid to it. I compreſſed ſome of theſe veſicles belonging to fiſhes, filled with oil, and obſerved in theſe caſes, that the oily ſubſtance tranſudes on all ſides, and regularly, through the ſides of the veſicle.

The fat of different animals, as well with warm blood as with cold, which I examined, appears to me to be ſeated in round bags or bladders, larger or ſmaller, and more or leſs regularly ſhaped, ſituated in the cavities which the laminæ of the cellular membrane leave betwixt them. I will not deny but that a quantity of the fat, not encloſed in the bags, but looſe, may be found betwixt theſe laminæ. I only mention what I have obſerved repeatedly in ſeveral animals. This obſervation has frequently ſurprized me, becauſe I knew it to be quite contrary to the received opinion. Thus I cannot determine whether the veſicles of the fat are naturally deſtitute of particular conduits intended for receiving it, and tranſmitting it elſewhere, according to particular circumſtances and exigencies; I only remark, that although I have ſought them very attentively, I have not yet been able to ſee them. If it be true that theſe conduits do not exiſt, the veſicles muſt be ſuppoſed to abſorb and reject this oily matter, by means of poroſities in the pellicles. This ſeems confirmed

by

by the experiment on tranſudation I have related above.

On Ivory.

I ſeparated a fine ſcale of Ivory, which had been previouſly well poliſhed, with a ſharp knife. I obſerved it in water in a refracted light, and in the ſame light I viewed it dry. In the laſt inſtance it ſeemed to me far leſs tranſparent than in the water, but equally organized. Fig. 21, of Plate VIII. repreſents this ſcale or lamina, in which the uſual winding threads are diſtinguiſhed.

On Sponges.

Curioſity led me to examine with ſome attention, the ſubſtance of which ſponges are formed. They are believed to be the work of animals. Fig. 1, of Plate X. repreſents a branch of this ſubſtance. I viewed it in an obſcure, and in a refracted light. It ſeemed likewiſe provided with the uſual winding threads, and was hollow in the middle.

This is all I have been hitherto able to obſerve of the ſtructure of the animal body. I confeſs that a great deal remains to be done, and am not altogether ſatisfied with the obſervations I have made. I flatter myſelf that at a future time I may be able to clear up up many doubts that ſtill remain. The firſt advances are however made, and theſe are always the moſt painful; when the paſſage is once cleared, it is no longer ſo difficult to perceive the courſe we ought to take, and to avoid the precipices that may lie in our way.

It ſeems to me very likely that the primitive cylinders are abſolutely no other than what Dr. Monro calls nerves, and which he believes to be truly ſo. He finds them indeed of $\frac{1}{9000}$ of an inch in thickneſs, whilſt they appeared to me to be of $\frac{1}{13000}$. But as this relates to a very minute body, it is not impoſſible but that the ſame object may be judged $\frac{1}{4}$ larger or ſmaller by two different obſervers. Father della Torre, and Jacques Jurin, differ from each other, as twenty-ſeven to one, in determining the ſize of the globules of blood.

The miſtake of the celebrated profeſſor of Edinburgh ſeems to me to conſiſt in his having taken theſe winding cylinders for the nerves themſelves, whilſt they are only the coats or ſheaths of them. They are common to the muſcles, tendons, and viſcera; and laſtly, to the cellular membrane, with which all the organical parts of animals are woven or enveloped. They are diſtinguiſhed in the epidermis, in hairs, in the nails, in the bones, and in the veſicles of the fat; they are almoſt uniform every where, both in their progreſs and ſize, ſo that I find no difficulty in believing that they are of the ſame nature and quality in all the parts of a living body, and that they tend to the ſame ends, and have the ſame uſes; it muſt however be obſerved, that they may harden more or leſs, according to accidental circumſtances, above all in the different organs, and different ſtates, of the animal machine.

On Vegetable Subſtances.

Dr. Monro is not content with examining all the parts of the body, but carries his reſearches to vegetable ſubſtances themſelves. He finds them to be formed of winding cylinders, exactly ſimilar to thoſe he obſerved in animals, and of which he ſuppoſed the nerves to be compoſed.

I ſhall relate in a few words ſome of the very many obſervations I made on plants; and think they will be ſufficient to determine us in what we ought to credit on the ſubject.

When in London, I examined the famous moving plant called *hedyſarum movens*, and ſought the interior mechaniſm of that regular perpetual motion which diſtinguiſhes it from all other plants, and brings its nature ſo near to that of animals. Although I found nothing ſatisfactory as to the object of my curioſity, I did not meet with any difficulty in diſcovering canals or cylinders, and that principally in the ſtalks of the leaves, which ſeemed to be compoſed of ſpires or bands, as they are deſcribed in Fig. 13, of Plate X. But I ſoon

ſoon perceived theſe to be veſſels formed of a ſingle thread, which turned round a common centre, and, cloſing on all ſides, formed the coat of the large veſſel itſelf. I ſucceeded in unfolding great lengths of theſe, and then knew them to be the pipes (trachées) of plants, known before my time. The ſpires or bands are here real, inſtead of which they are in the nerves only apparent, although in both they appear perfectly alike: ſo true is it that analogy is very ſubject to lead us into errours.

Fig. 14. of Plate X. repreſents the pipe partly unfolded. The threads of which it is formed are tranſparent in their length, and their edges are obſcure in ſuch a way, that by meeting each other they form dark bands.

There is however in plants, another ſtructure of parts; a more general organization. This organization ſeems to compoſe the greater proportion of their maſs, and is perfectly analogous to the winding cylinders we have obſerved in ſo many parts of the animal machine.

Fig. 15. of Plate X. repreſents a ſmall fragment of a roſe leaf, partly ſtripped and torn, with the point of a knife. The uſual globules and winding threads are obſerved in it.

Fig. 2. of Plate X. repreſents a very ſmall bit of elaſtick reſin, in which are ſeen the winding threads.

Fig. 22. of Plate VIII. repreſents a cotton thread obſerved dry.

The

The ſurface of the cotton thread ſeems interſperſed as uſual with ſmall cylinders. Fig. 23. of the ſame plate ſhows two of theſe cotton threads put in water: there are fewer of theſe winding cylinders on their ſurface, they are leſs regular, and ſeem to penetrate into the inner part of the threads.

Fig. 12. of Plate X. is a fragment of amber, which, like all other bodies, ſeems covered with winding cylinders.

On Foſſils.

Dr. Monro's obſervations on foſſils are very ſingular, and have the air of a true paradox. He believes them to be all formed of winding cylinders. Earths, ſalts, metals, he finds them all made of cylinders, and even diſcovers them in gold coin; in guineas. He brings nothing but obſervations to ſupport his opinion, and obſervations ought to be combated or confirmed by obſervations, and not by words.

I examined ſeveral of theſe ſubſtances, and ſhall only mention a few of them, intending to treat of them more at length in my *microſcopical obſervations*, when I ſhall give my ſentiments on this obſcure matter, which I preſume will long divide the opinions of obſervers. Nothing leſs than the ſeeing the ſmalleſt conſtituent parts of bodies is neceſſary to know whether there is an organical, ſimple, ſole, primitive ſtructure, common to animals, to vegetables, and to foſſils. Here ſimple obſervation is not ſufficient, and it does not avail much to be ſkilled in the microſcope. The apparent object does not diſtinguiſh itſelf from the real one. Not to be deceived, obſervations muſt be analyzed, and experiments made; but the road in both is long and painful, and we do not always ſee how we are to fall into it.

I began my obſervations by examining earths and marbles. Fig. 7, of Plate IX. repreſents ſeveral grains of calcined magneſia, which I obſerved both wet and dry in a refracted light. They were in a greater or leſs degree of a cylindrical ſhape, and rounded at their extremities. The uſual winding threads were likewiſe very diſtinctly obſerved in them. When in water every thing was very tranſparent.

Fig. 9, of Plate IX. repreſents two atoms of white marble obſerved dry. The uſual winding threads were likewiſe ſeen here.

Fig. 10, of Plate IX. repreſents an atom of heavy ſpar, which I obſerved in a refracted light. Nothing was diſcernible beſides the winding threads, which were very regular.

Fig. 11, of Plate IX. repreſents an atom of phoſphorick ſpar, which diſplays in every part of it the uſual winding threads.

Fig. 3, of Plate X. repreſents an atom of common ſalt. The winding threads are found in it, but not ſo abundantly as elſewhere.

On Gold.

From earths I proceeded to metals. I drew out with the wire-drawing iron, a cylinder of very pure gold, and after having ſcraped and poliſhed it well, I obſerved it breadthwiſe. It appeared quite covered with winding threads, as is deſcribed in Fig. 1, of Plate IX. Fig. 2, of the ſame Plate, is the ſame cylinder obſerved longitudinally. I examined it both ways in a reflected light.

Fig. 3, of Plate IX. is a ſmall piece of gold leaf. It appeared to be woven with ſimple winding threads. I viewed it both wet and dry.

From gold I proceeded to the examination of ſilver. The four ſmall objects of Fig. 4. Plate IX. are atoms of very pure ſilver filings. They very much reſembled hairs. In a reflected light the uſual winding threads were ſeen in them; but in a refracted light all was obſcure, except at the very extremities, where the winding threads likewiſe ſhowed themſelves.

I examined a very thin plate of lead in a reflected light, and obſerved the uſual winding threads, as they are ſeen in Fig. 10, of Plate X.

Braſs, likewiſe, in a reflected light, diſcovered theſe winding threads, as they are repreſented by Fig. 11, of Plate X.

Tin, obſerved in a reflected light, likewiſe ſhowed theſe

theſe winding threads. Fig. 7, of Plate X. is a ſmall fragment of tin obſerved in this way.

Antimony, as is ſeen in Fig. 8, of Plate X. has theſe winding threads. It was obſerved dry, and in a refracted light.

Fig. 9, of Plate X. repreſents an atom of cobalt, in which are obſerved the winding threads.

Fig. 6, of Plate IX. repreſents an atom of zinc, obſerved dry, and in a reflected light; it has winding threads as uſual.

Fig. 8, repreſents an atom of biſmuth obſerved dry in a reflected light; the uſual threads are diſtinguiſhed in it.

Fig. 12, ſhows an atom of nikel, which, obſerved wet, had the winding threads.

I paſs over in ſilence a great many other obſervations on foſſils, which all agree with thoſe we have hitherto ſeen, ſo that it ſeems a truth eſtabliſhed by obſervation, that all bodies, viewed with very ſtrong lens', diſplay the ſame form or appearance.

This is not the place to determine what is real, and what is only apparent. Simple obſervations are not ſufficient to decide with certainty. There is need of a very nice analyſis of all the circumſtances; it is neceſſary to prepare the ſubſtances for obſervation; in a word, experiments are wanted. I wiſh to avoid the giving my opinion on this matter. I cannot do it with the brevity that is requiſite, and it will be the ſubject of another work. In the mean time I ſhall be very happy to know the different opinions of obſervers on this ſubject. The moſt ſkilful will be the laſt to determine; thoſe who are pretty much ſo will not find any great difficulty; and the novices, and all thoſe who do not obſerve at all, will decide at once.

To retard a little at leaſt, the judgements of all of them, I think it proper to ſubjoin as briefly as I can two important particulars. I let fall into a baſin of cold water ſome drops of melted ſilver. I afterwards examined ſeveral of the ſmalleſt of them, and inſtead of finding the uſual winding threads, they appeared to me to be formed of ſmall ſhining grains, emboſſed at ſeveral parts of them. Fig. 4. Plate X. repreſents a corpuſcle of ſilver in which ſome of the winding threads are however ſeen ſcattered. The other parts ſeemed formed of unequal globules. I viewed it in a reflected light.

Fig. 5. of Plate X. repreſents another ſmall bit of ſilver in which the very ſmall ſhining corpuſcles are ſeen, without any winding threads. I examined it in a reflected light.

Fig. 6. of Plate X. is another ſmall portion of ſilver, examined in a refracted light. It ſeemed formed of points, pyramids, and very ſmall diamonds, and I could obſerve no winding threads in it. Theſe different ſtructures in the ſame body engaged me to try a few freſh experiments.

I ſcratched, with the point of a needle, a ſmall bit of talc, which was tranſparent and homogeneous throughout. I examined it with the lens, and it appeared to me in the way it is deſcribed in Fig. 16. of of Plate VIII. There was a ridge in the middle of it, with winding threads, and globules. The threads did not vary much from thoſe I had obſerved before. I wet it with water, and poliſhed it with linen, but it ſtill remained in the ſame ſtate.

Upon this I ſuſpected, that probably the ſimple contact of very minute round bodies was capable of cauſing this appearance of winding threads. In conſequence of this ſuſpicion, I examined dry hair-powder, but could obſerve nothing in ſupport of it. Although the globules touched each other in ſeveral parts, they were very diſtinct from each other, as is repreſented in Fig. 18. but ſcarcely had I wet them a little, when I obſerved in ſome places, continued, longitudinal, homogeneous, bodies, tranſparent throughout, as appears in Fig. 17. of Plate VIII. It is true that theſe winding threads were very few in number (a), when compared with the globules which ſurrounded them, and that they were ſcattered here and there without that regularity and paralelliſm which is ſo conſtantly obſerved in other bodies. But we learn at the ſame time by this obſervation, and by this experiment, that water is capable of ſo inſinuating itſelf and ſettling betwixt the globules, that in certain inſtances it imitates an homogeneous, tranſparent, uninterrupted, thread, or cylinder.

I repeat, that good obſervers will be very ſlow in pronouncing any thing in a decided way; but it is

(a) More of theſe threads are expreſſed in the engraving of the above figure, than what I really obſerved.

proper at the ſame time that obſervations ſhould be made and varied in all poſſible manners. Obſervations alone may ſupply us with certain intelligences, after they have been well analyzed, and all the circumſtances of them well known.

A Letter addreſſed to Mr. ADOLPHUS MURRAY, *a celebrated Profeſſor of Anatomy at Upſal, in the Year* 1778.

I ENCLOSE you three deſigns marked 1, 2, 3, (*a*), which I thought ſufficient to bring to your recollection the idea of the new canal I have diſcovered in the eye, and which I had the pleaſure to ſhow you when you paſſed through Florence. 'Twas on this occaſion that I had the ſatisfaction of knowing you, and of being favoured with your agreeable friendſhip.

To ſo enlightened an anatomiſt as yourſelf, it will be ſufficient to ſimply point out the principal parts; every thing elſe would be quite ſuperfluous.

The three figures 8, 9, 10, (Plate VII.) expreſs the three principal ſections in the eye of an ox,

(*a*) Theſe are the three numbers annexed to the drawing ſent to Mr. Murray; but to preſerve the order of the plates in this work, I have employed in their place, Figures 8, 9, 10.

 which

which I ſhowed you when you was at Florence. I ſhall not mention to you the other ſections, ſince I conſider theſe three as the moſt eſſential and neceſſary to ſatisfy your enquiries.

Fig. 8. as you ſee, repreſents almoſt half the eye, obſerved in its inner part. Letter *n*, points out the tunica ſclerotis; *m*, the ciliary body, likewiſe called ligamentum ciliare; *e*, the proceſſus ciliares; *c*, the uvea; *a*, the pupil. You ſee by the deſign, that my canal of the eye anſwers to the circular band indicated by *m*, which forms the corpus ciliaris.

Fig. 9. is the half of the preceding figure. Letter *n* indicates the tunica ſclerotis; *e* the proceſſus ciliares; *c* the uvea; *a* the pupil. Letter *m*, at the the right of the ſame figure, expreſſes the corpus ciliaris divided; the opening points out the hollow of the body, or canal rather, I have diſcovered in the eye.

The three letters, *r*, *m*, *o*, not only ſhow the canal, but likewiſe the upper ſide of it divided in two. Letter *m*, is the body of this new canal opened, and *r*, *o*, are the two extremities or lips of the inciſion made in the upper part of its coat.

You muſt perceive hence, that this new canal is formed of the ligamentum ciliare, which is wrapped in its ſubſtance; you will underſtand this ſtill better by throwing your eye over Fig. 10. which is the other half of Fig. 9.

Letter *a*, in this figure, ſhows the tunica ſclerotis ſtripped of the tunica choroides. Letter *c*, indicates

cates the ridge in which the ciliary ligament is faſtened by means of the cellular filaments. Letter *r*, points out the tranſparent cornea. The three letters, *e*, *o*, *s*, belong to a membranous ſubſtance formed by the meeting of the tunica choroides, *e*, the ligamentum ciliare *o*, and the uvea *s*. Letter *o*, ſhows that part of the ligament which is inſerted into the ridge *c*, of the tunica ſclerotis.

A very ſmall crevice is diſcovered in the ſide of this canal at *o*; this part, or this ſide, of the canal is white and cellular, and is faſtened very ſtrongly to the tunica ſclerotis through the whole of the circular ridge *m*, of Fig. 8.

I have paſſed water, quickſilver, &c. from one ſide to the other of this new canal, without the ſmalleſt laceration being cauſed by the paſſage of theſe fluids. Its coat within is very ſmoth and equal. The part *o*, can very eaſily be detached from the tunica ſclerotis *e*, even by forcing it a little with a ſmall ſcale of ivory; and the detached membrane, without being at all torn, is then ſeen to form the new canal as it is obſerved at *o*.

I ſend you the drawing of this new canal of the eye, not becauſe I wiſh you, as you tell me you are deſirous of doing, to publiſh it in the acts of the academy at Upſal, but ſimply becauſe you aſk it of me. It is enough for me that I am perſuaded of my great eſteem for you, and of the pleaſure I receive from the correſpondence of a man of your merit. Do what you will with it, for I am totally indifferent. You muſt certainly have perceived when you was here

here how little I valued this diſcovery, now grown out of date with me; I ſay *diſcovery*, ſince you will have it called ſo.

I ſhall however ſay nothing to you on the uſe of this new canal, and of the tranſparent liquor with which it is found moiſtened. I have at preſent no obſervation ſufficiently ſure, nor experiment ſufficiently deciſive, to render me certain thereupon. I will not advance imaginary hypotheſes, nor ſimple probabilities. I abandon and ſubmit this difficult ſubject to your genius, leaving to you the glory of clearing it up.

SUPPLEMENT.

ADVERTISEMENT

OF THE

FRENCH EDITOR.

THE impreſſion of the two volumes of this work was quite finiſhed, when I heard that our authour, always indefatigable, and always miſtruſting his own labours, had, in the ſhort intervals of leiſure, which his painful and multiplied occupations afforded him, made a great number of experiments relative to the different matters he has treated of in the preſent work. He has very freely conſented to the deſire I teſtified to him of making them publick, by inſerting them here under the form of a ſupplement. He has likewiſe communicated to me an extract, or rather, the dernier conſequences, and

and moſt general concluſions, of a treatiſe on *opium* he has juſt finiſhed, and which, if it were publiſhed entire, would alone form a work apart. It is a real pleaſure to me to be able to enrich this edition with ſo many new and intereſting truths, which together concur to render it more complete, and to give to ſo extenſive a ſubject-matter, a perfection which would be in vain ſought in the works of the moſt renowned naturaliſts of the two laſt centuries.

The learned and impartial reader will readily agree with me, that this work cannot fail of forming an epoch in experimental philoſophy; and I care but little what the ignorant, and above all the envious, the number of whom is however but too conſiderable, may think of it. I ſhall reply to theſe, with the Latin poet, *Odi prophanum vulgus et arceo.*

Although I have inſerted this ſupplement at the end of the ſecond volume, the reader is begged to read it before the article that treats of "the toxico-"dendron, and ſome other vegetable ſubſtances;" or rather, to read the different parts of it jointly with the chapters to which they relate. The authour has contrived it in this form, for the ſake of brevity.

He has diſcovered an important truth, by rendering himſelf certain that the venom of the viper is a poiſon, when ſimply ſwallowed by animals; and that, contrary to the opinions of the beſt authours till this day, it kills in this way very ſuddenly.

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The oil of the cherry-laurel, and the rectified ſpirit of this plant, which when injected into the veins of animals, inſtantly kill them, furniſh another truth which removes the myſtery and embarraſſment as to the action of this poiſon, that had obliged our authour to make an exception to the common laws he had eſtabliſhed, of the other poiſons.

But what deſerves the greateſt applauſe is, his having found, when there did not ſeem the ſmalleſt hope or probability of it, having found, I ſay, a matter, which when united with the venom of the viper, renders it innocent; a matter, which may now be regarded as the true ſpecifick againſt this formidable poiſon. This important and unexpected diſcovery, which we owe entirely to the indefatigable genius of our authour, has been managed with an extent and depth of inveſtigation that are proper to him; and with that delicate analyſis of experiments, which forces nature to unveil her moſt hidden ſecrets. Poſterity will judge of the remedy, and of the merit of this diſcovery. In the mean time, I exhort naturaliſts to follow his ſteps in the road now open to their reſearches; to multiply experiments on the largeſt kinds of animals, according to the wiſh of the authour of this new remedy himſelf; and to determine the circumſtances with the niceſt exactneſs, in which it may be moſt uſeful and moſt certain. The remedy ſeems ſure if given in time, and the ſpecifick is diſcovered; it remains to eſtabliſh the mode of applying it with the greateſt ſucceſs, and the leaſt pain and inconvenience.

The authour ends his ſupplement by ſetting in the cleareſt light, the action of opium applied to the different parts of a living body; he proves demonſtratively, that the proper vehicle for opium is no other than the blood; that opium acts inſtantly on the blood; and that in whatever way it is applied to the nerves, it produces no changes in them: three experimental truths of the greateſt importance, and which oblige the reaſoning philoſopher to form a new theory on this ſubject, ſince they muſt cauſe almoſt all that has been ſaid on opium, by the majority of other writers, to be regarded as errours and ſimple imaginations.

SUPPLE-

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THE experiments I had made on the ſpirit of the cherry-laurel, which, notwithſtanding I had found it innocent when applied to the eyes of quadrupeds, ſuch as guineapigs, rabbits, &c. (in the quantity however in which I had tried it) was capable of killing pigeons in a few minutes, when applied to their eyes: theſe experiments, I ſay, made me ſuſpect that the oil itſelf of the cherry-laurel would be a poiſon to theſe animals, that are ſo ſenſible to the ſlighteſt impreſſions. Here follow the experiments I made on the ſubject.

I let fall into each of the eyes of a young pigeon three drops of the oil of the cherry-laurel; in the ſpace of a minute it became ſlightly convulſed; in two minutes the convulſions were become violent and general, and it then fell on its breaſt without the power of ſupporting itſelf. In two minutes more it died. Neither of its eyes nor eyelids were ſenſibly enflamed; there were circular red veſſels however in the uvea, at ſome diſtance from the pupil

pupil. Two other pigeons treated like this one, died, one in five minutes, the other in lefs than feven. It is certain then that the oil of the cherry-laurel, as well as the fpirit, is a violent poifon when applied to the eyes of pigeons.

Thefe experiments induced me to think that the eyes of pigeons were formed in fuch a way, or were delicate and fenfible to fuch a degree, that the venom of the viper, which I had found innocent when applied to the eyes of other animals, would make violent impreffions on them. Although neither of the pigeons died to which I applied this poifon, my fufpicion was not altogether without ground; for having repeatedly covered the eyes of two of them with the venom, I foon found their eyelids to fwell confiderably, infomuch that the eyes themfelves were fcarcely perceptible, and feemed as if funk in a hollow. In feven minutes their eyes were no longer vifible, and the pigeons were feveral hours before they could open their eyelids. The uvea and tranfparent cornea were not inflamed, but the inner part of the eyelids was very much fo. The venom of the viper then is not altogether innocent, when fimply applied to the eyes of certain animals; although in fome others it is not hurtful when applied in the fame quantity. I am however perfuaded that if it were applied for a long time to the eyes of thefe laft, its effects would be perceived; and that it might even occafion death, or fome violent derangements at leaft.

Thefe

Theſe experiments on the eyes of pigeons, which become inflamed when the venom of the viper is applied in abundance to them; and thoſe I had made on the *ticunas*, which does not occaſion death when ſwallowed, unleſs it be in a large quantity; confirmed me ſtill more in my ſentiment (*a*), that the viper's venom itſelf might kill animals when ſwallowed plentifully. Chance having thrown a good number of very large vipers in my way, and thoſe in full vigour, I was determined not to loſe the opportunity of eſtabliſhing for poſterity, ſo important a point of natural hiſtory. As it is not the place here to enter into a detail on this ſubject, I ſhall for the preſent content myſelf with relating, in a few words, an experiment I made on a pigeon, young it is true, but very lively and in ſound health.

I cut off the heads of eight vipers, and received the venom expreſſed from them, in a tea-ſpoon; it was almoſt full, and might contain thirty drops or more. I introduced the whole of this into the œſophagus of a pigeon that had faſted for eight hours. In leſs than a minute it became very weak, two minutes after it began to reel, and at length fell on its ſide in violent convulſions, dying in leſs than ſix minutes. The beak, œſophagus, and crop, to the very gullet, were inflamed and livid, and the blood was blacker than uſual; theſe parts were ſo diſcoloured, that they ſeemed to tend to gangrene and ſphacelus.

(*a*) Page 105 of this volume.

It can no longer then be doubted but that the venom of the viper, contrary to what Redi and many other famous obſervers after him have written, is a violent poiſon. This venom, like the *ticunas* and ſeveral other poiſons, when taken in a ſmall quantity, either does not produce, or does not ſeem to produce, any effect; although it is notwithſtanding true that it kills very ſuddenly, and that in very ſmall quantities, when introduced into animals by the medium of wounds, and of the blood. It is certain that the famous Jacques, the viper-catcher Francois Redi ſpeaks of *(a)* venturouſly offered to ſwallow whole ſpoonfuls of it; but we do not read in any part of the works of that celebrated writer, that the good Jacques had afterwards fulfilled his promiſe, and he was certainly fortunate in not having done ſo. The greateſt proof Redi gives of the courage or temerity of this man, is his having ſwallowed the venom of three vipers in half a glaſs of wine; that is to ſay, that he ſwallowed a few drops of it, perhaps three or four only, conſidering the imperfect method he muſt have employed to extract it. I am perſuaded that the venom of even a more conſiderable number of vipers, mixed with ſo large a proportion of wine, would not be dangerous to the perſon who ſhould ſwallow it; but on the other hand I am of opinion, that a whole ſpoonful of this poiſon, unmixed with other ſubſtances, would be very capable of killing a man himſelf. The expe-

(*a*) Obſervations on Vipers, publiſhed in Italian, at Florence, 1664, Page 17.

riment

riment Redi himſelf made, proves ſtill leſs than the other. He ſqueezed the venom of four vipers into a glaſs of water, and gave it to a kid without any ill conſequences. The water was in a much larger quantity than the wine I have mentioned, and conſequently the venom ought to have been far leſs active, ſince it was more diluted and more divided. But, as Redi pretends, it does not follow from all this that the venom of the viper, introduced in abundance into the ſtomach, would be neither mortal nor hurtful. It is both hurtful and mortal when taken in a large quantity. This errour is indeed common to the ancient philoſophers, who believed that the venom of ſnakes was only a poiſon when introduced into wounds: *Non guſtu, ſed in vulnere, nocent*, ſays Celſus; and Lucan before him, puts into the mouth of Cato. *Morſu virus habent, & fatum dente minantur; pocula morte carent.*

The venom of the viper, as is ſaid in the firſt volume, although ſeparated from the animal, retains its hurtful qualities for ſeveral months; but a well determined experiment has latterly induced me to believe, that its faculty of killing, if indeed it laſt ſo long, does not continue beyond the ninth month. This is the experiment.

I wetted four pieces of thick brown paper, each of them with about twenty drops of venom, and encloſed them ſeparately in glaſſes. At the end of nine months, I forced theſe papers into the wounds of the legs of four young pigeons. Neither of the animals died, nor gave any ſymptoms of the diseafe of the venom. The venom thus preferved then had loft the faculty of killing even pigeons, that are fo readily deftroyed by it when frefh.

On the Lunar Cauftick.

Although, as has been feen in the firft volume, I had fatisfied myfelf that the fluid volatile alkali is not a fpecifick againft the bite of the viper, and that it does not deprive the venom of this animal of its noxious quality when mixed with it, I had however the curiofity to examine likewife the lunar cauftick, and began my refearches by mixing the venom of the viper with this cauftick fubftance, to fee whether it would ftill preferve its original venomous quality, as it preferves it when it is united with the fluid volatile alkali. I made my experiments by mixing equal quantities of the venom and lunar cauftick, adding a few drops of water to render the pafte, which I applied to the wounds of animals, fomewhat more liquid. And as fmall birds are eafieft killed by the venom, I wifhed to try on fuch the effects of this pafte, and procured fparrows and pigeons for that purpofe.

Experiments on Birds.

I wounded the legs of five birds with the venomous teeth of vipers, making fcarifications immediately, and applying the above paſte. Neither of them died, nor ſeemed affected with the diſeaſe of the venom; and there was no gangrene in either of their legs, although the muſcles were much burnt by the cauſtick.

I repeated this experiment on five other birds, and in addition, waſhed the parts with water, after having applied the paſte. Here again neither of them died, and I found that the muſcles of the legs were leſs corroded and burnt by the cauſtick.

As theſe animals are ſo ſmall that an atom of venom ſuffices to kill them, it appears that it can be no longer doubted for a moment, but that the lunar cauſtick renders the venom of the viper innocent when it is ſcarcely mixed with it; for I applied the paſte to the wounded muſcles, the moment I had blended them together. I wiſhed however to try it on ten other birds; and to my great ſurpriſe, they all recovered very readily. Still I could not determine as to the unexpected novelty of theſe conſequences; and fearing that accidental circumſtances might have prevented the action of the venom, I reſolved to make freſh experiments on the ſame animals. I wounded the legs of ſix others, multiplying the inciſions to introduce a good deal of the venom, of which I am ſure the quantity of paſte I employed in each muſt have contained a drop at leaſt. In theſe experiments two of the birds actually died, one in the ſpace of ſix hours, the other in twenty-eight. On the morrow I repeated this experiment, with the ſame circumſtances, on ten other birds; one only of them died, and that at the end of twelve hours. Fearing that the wounds alone might have brought on death, particularly as they were irritated by the cauſtick, I tried ten birds, on the legs of which I made wounds as uſual, and applied the cauſtick by itſelf. One of them died at the end of eight hours. So that it ſeems at leaſt very probable, if not very certain, that the three birds before mentioned, died likewiſe of their wounds, and not of the effects of the venom. I wounded ten other birds in ſeveral places, in the pectoral muſcles, to which in a ſhort ſpace I applied the paſte. Not one of them died.

Experiments on young Pigeons.

The pigeon, next to ſmall birds, particularly if very young, is the animal killed with the ſmalleſt quantity of venom. I choſe four of theſe for a trial, and operated on all of them in the ſame way. I made ſeveral tranſverſe wounds with ſciſſars, in the muſcles of their legs, and introduced the venomous paſte abundantly into the wounds, which although pretty deep, did not bleed conſiderably. Neither of theſe pigeons either died, or ſeemed to have the diſeaſe cauſed by the venom of the viper. The next day I repeated this experiment on twelve pigeons, the legs of which I wounded in ſeveral places,

places, and again applied the paſte; neither of them died. I varied the application of the venomous paſte, which I ſometimes forced into the wounds with ſmall bits of wood, ſometimes with pieces of ſtout thread, ſmeared with it. Neither of them died in theſe trials. I proceeded to the muſcles of the breaſt, which I wounded in different ways, and diverſified the application of the paſte: but it was in vain that I multiplied my experiments; neither of the pigeons died.

It cannot now be doubted but that the lunar cauſtick, when mixed with the venom of the viper, renders it innocent; and thus every thing concurs to make us regard it as the true and only ſpecifick againſt this poiſon. I can now flatter myſelf with having at length diſcovered a certain remedy againſt the bite of the viper; a remedy that ſo many people have hitherto ſought in vain. But does the viper's venom loſe its deadly qualities, when mixed with the lunar cauſtick, becauſe its nature is changed, or rather, becauſe being united with this ſtrong cauſtick, in the way with acids when they are ſaturated by alkalies, or by abſorbent earths, it can no longer exerciſe its original qualities? May we not ſuſpect that the lunar cauſtick, by criſping the blood-veſſels, prevents the poiſon from inſinuating itſelf in this way into the blood? This laſt ſuſpicion does not ſeem ſupportable, ſince the mineral acids, which likewiſe criſp the veſſels, are however not capable of rendering the venom innocent, and ſince the fluid volatile alkali itſelf has no ſuch property: this muſt appear ſtrange to us when we conſider the great agreement there is betwixt the fluid alkali, and the lunar cauſtick.

I muſt confeſs that I felt a real ſatisfaction, when I flattered myſelf that my labours had been crowned with ſuch great ſucceſs; and the knowing that the venom of the viper does not loſe its hurtful qualities when united to other very active ſubſtances, ſuch as the mineral acids, concurred to inſpire me with confidence. But I well recollected the errour I had fallen into in France, when I thought I had found a certain remedy againſt the bite of the viper, becauſe I was able to recover ſmall birds and pigeons that had been bit. Simple analogical proofs had no longer any weight with me, and in the preſent caſe only ſerved to make me recur to immediate and irreſiſtible experiment, which alone ought to be conſulted in philoſophical reſearches. This is the only purpoſe to which a prudent philoſopher, if he does not wiſh to be deceived or led into errour, ought to apply theſe proofs; and 'tis to this application of them that the penetrating naturaliſt owes his moſt important diſcoveries.

Experiments on Birds.

I wounded the muſcles of the legs of four ſmall birds with venomous teeth, and after having made ſlight ſcarifications, I applied the lunar cauſtick, waſhing

washing the wounds soon after. Neither of them died, nor had the disease of the venom.

I wounded four other birds like the preceding ones, in the legs, with venomous teeth, and afterwards washed and scarified the wounds, but did not apply any remedy. They all died, one in a single minute, one in four minutes, one in seven, and the fourth in eight.

I wounded the muscles of the legs of four others with scissars, and applied the venom to them. Immediately after, I scarified the wounds, applied to them the remedy, and washed them. They all four recovered.

I treated four other birds in the same way with the last, and neither of them died.

I thought it proper to repeat the same experiment on ten others. I wounded their legs, applied the venom, scarified, and medicamented, and all ten of them recovered.

I must not however conceal that of five other birds I had wounded in the legs with venomous teeth, three died, although they had been scarified and dressed with the lunar caustick in the way described above. Two of them died at the end of three hours, the third on the twentieth.

Two other birds died in the same way, out of four, of which the pectoral muscles had been wounded with venomous teeth, and had been dressed in the usual way, after the scarifications had been made. One died in three minutes, the other in three hours.

Another

Another time I wounded the pectoral muscles of three birds with a lancet, and applied the venom. I dressed the wounds with the lunar caustick, and the three birds all died, one in half an hour, a second in eight hours, and a third in nine.

I feared that the wounds made in the pectoral muscles, joined with the caustick, were alone sufficient to occasion death; and therefore wounded these muscles in three other birds, and applied the lunar caustick to them: neither of the birds died.

We may I think conclude from the experiments related above, that the lunar caustick, when applied in the way I have described, destroys the effects of the viper's venom, in birds. If it however does not prevent some of them dying of this venom, it may be believed, either that the remedy has been applied too late, or, which is more probable, that it cannot always reach the envenomed parts by introducing itself as far as where the venom has penetrated. Every one must perceive that in these cases it cannot have corrected the deadly qualities of the venom, as it certainly does correct them, in the way we have seen, when they are united together.

Be that as it may, the lunar caustick being capable of recovering birds, is not a sufficient assurance to us that it would recover other creatures in the same way: and even though this caustick should be the true antidote against the venom of the viper, as it incontestibly appears to be, it does not follow

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of neceſſity, that it ought to cure the larger kind of animals. Circumſtances may vary, the ſcarifications may be more dangerous, and the application of the ſpecifick to the envenomed parts, either more difficult, or leſs certain.

Experiments on Pigeons.

I wounded the muſcles of the legs of four pigeons with venomous teeth, made the uſual ſcarifications, applied the lunar cauſtick, and covered the wounds with linen. Two of the pigeons died in a few hours, the other two ſurvived. I waſhed the wound of one of the two firſt, after I had dreſſed it, but did not waſh that of the other; and I obſerved the ſame treatment to the two that ſurvived.

I repeated theſe experiments on four other pigeons, to which I applied the venom taken from the animal, and did not waſh either of their wounds. Neither of them died, nor had any appearance of the diſeaſe of the venom.

I made the ſame experiment on ſix others, and applied the venom to the muſcles, after having wounded them. They all ſurvived.

I returned to my firſt experiments, fearing that the remedy had not reached all the parts where the venom had penetrated, in ſpite of my having made large and deep ſcarifications. I operated on the legs of of feven pigeons, three of which died in lefs than an hour, the other four had no complaint.

I paffed on to the pectoral mufcles. I wounded thofe of four pigeons in feveral places, and applied the lunar cauftick. They all four recovered.

On repeating the fame experiment on four other pigeons, neither of them died, nor became ill. I got ready twelve other pigeons as above, and applied, firft the venom, and immediately after the lunar cauftick, to the wounds in their pectoral mufcles. They all twelve recovered.

I wounded the pectoral mufcles of four others with venomous teeth, and immediately fcarified the wounds, and applied the cauftick. Two of them died in lefs than an hour.

It cannot be doubted after this, but that the deaths of the animals to which the lunar cauftick was applied, were occafioned by its not being invariably able to reach all the parts envenomed; and not becaufe it is no true fpecifick againft this poifon.

I muft again confefs ingenuoufly, that having one day applied the venom to wounds made in the mufcles of the legs of two pigeons, one of them died at the end of four hours, notwithftanding I had treated it with the cauftick. Another time I wounded the mufcles of the legs of two pigeons, with venomous teeth, and one of them died in eighteen hours. But unlefs I am deceived, all thefe cafes only tend to prove ftill more, either that the remedy does not always come in time, or does not always unite with itſelf the venom : this ſeems to be ſufficiently demonſtrated by the time that theſe animals continue to live, whilſt they in general die in a very ſhort ſpace, when the remedy is not applied.

I was deſirous of making a trial on ten other pigeons, and having wounded their legs with venomous teeth, I ſcarified them, and applied the cauſtick a little after. Five recovered without having had any complaint, a ſixth died in my hands, and the other four died, one in three, one it ſixteen, one in eighteen, and one in nineteen hours.

I think it quite ſuperfluous to give a detail of ſeveral experiments I made on the muſcles of the legs and breaſts of fowls. I wounded them in ſeveral places, applied the venom abundantly, and treated them in the uſual way with the cauſtick. Neither of them died. This may readily be accounted for, ſince pigeons, which are much more tender, and which ſink more readily under the effects of the poiſon, have in the ſame circumſtances eſcaped death.

Experiments on Quadrupeds.

I wounded two guineapigs, each of them repeatedly in the muſcles of one of its legs, with venomous teeth, and after having made the ſcarifications, I dreſſed the wounds with the lunar cauſtick : one recovered, the other died at the end of five hours.

I treated

I treated four other guineapigs in the ſame way; and only one of them died, and that at the end of five hours.

I wiſhed to try whether on applying the venom drawn from the animal, to the wounded muſcles, the remedy would be more certain. I wounded the muſcles of the legs of ſix very ſmall rabbits in ſeveral places, and applied the remedy; neither of them died. I treated the pectoral muſcles of ſix other rabbits in the ſame way, and all of them recovered.

I proceeded to ſmall guineapigs, on ſix of which I tried this remedy. To three of them I applied the venom to the muſcles of the legs, to the other three to thoſe of the breaſt, each of which I had previouſly wounded: I then applied the cauſtick. Neither of the guineapigs died.

I returned to the venomous teeth, with which I wounded eight very ſmall rabbits in the legs. A little after I ſcarified and medicamented the parts. Two of the rabbits died; the other ſix recovered.

It can no longer be doubted but that the lunar cauſtick is a true ſpecifick againſt the venom of the viper. But the mode of applying it to the envenomed parts is not certain, and it is natural to conceive that the difficulty will become much greater, when the application of it is attempted on the immediate bite of the viper; particularly if the creature may have bit ſeveral times, and if the traces of the bites can ſcarcely be diſtinguiſhed. In theſe

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cafes there will be always fome uncertainty, and if the remedy fail in correcting the venom, too extenfive and multiplied fcarifications may be extremely hurtful.

Bites of the Viper treated with the Lunar Cauftick.

The latter part of my experiments is the more important, in having for its object the fecuring us againft the bite of the viper. My experiments are too few in number, and too little varied, either to allow the drawing from them all the practical utility that may be hoped, or to render the method I have propofed perfect. Owing to the feafon, I have experienced a fcarcity of vipers; and the circumftances in which I have found myfelf, and the obligations I have had to fulfill, have prevented my applying myfelf more attentively, and in the way I fhould have wifhed, to this fubject. I fhall for the prefent publifh the refult of fuch experiments as I have been able to make, intending to return at a more convenient opportunity, to an enquiry that has the good of my fellow creatures for its object. In the mean time, I hope that philofophers and naturalifts will pay every attention to this branch of medicine, and will fpare no pains to render it more certain and ufeful.

I had a middle fized rabbit bit five times fuceffively in the leg, by a large viper, and, after making fcarifications, ap plied the cauftick, and wafhed and bound

bound the wounds. The rabbit died at the end of twelve hours.

I had another rabbit bit ſeveral times in the leg by a viper. It died in the ſpace of an hour, although it was treated like the preceding one.

I had two guineapigs bit in the legs by a viper, each one three times, and after making the ſcarifications, I applied the cauſtick. Both of them died in a few minutes.

I repeated this experiment with the ſame circumſtances, on a large guineapig, which died in the ſpace of twenty-four hours.

Theſe five unexpected deaths convinced me how eaſy it is to be deceived, even in matters of obſervation and experiment, and how little truſt is to be repoſed in analogy. The minuteſt circumſtance ſuffices to render what in itſelf would be very uſeful, both uſeleſs and hurtful. Every one may perceive, that in the preſent caſe the whole difficulty lies in making the cauſtick penetrate into all the parts to which the venom has found its way. But how can this difficulty ever be ſurmounted? The holes made by the teeth of a viper are very ſmall, and often inviſible. They run in different directions within the ſkin, and have different depths, according to a thouſand varied circumſtances. The ſwelling or inflammation that ſucceeds, augments the difficulty ſtill more, ſo that the ſcarifications are made almoſt at hazard.

I muſt not howeve omit that I have cured five other larger rabbits by this method, after they had been repeat-

repeatedly bit by vipers ; as alſo ſeveral guineapigs that I had had bit in the ſame way ; all of which would probably have died, if they had not been treated with this new remedy. But I recovered a much greater number of theſe animals, when they had been bit a ſingle time only, although even in this inſtance ſome of them died ; and this was undoubtedly occaſioned in the way related above ; that is to ſay, not by the inefficacy of the medicament, but becauſe it cannot always reach the parts where the poiſon has penetrated and lodged. There are likewiſe other circumſtances which elude the new method I have propoſed, and theſe are when the diſeaſe, by accidental circumſtances, is more internal than external ; that is to ſay, when the venom is ſuddenly introduced in a large quantity into the blood of the animal, by the means of ſome veſſel that the teeth may have penetrated. And I do not think it impoſſible but that the bite of the viper may kill even inſtantaneouſly, provided it ſhould ever happen (which is not abſolutely impoſſible) that the teeth ſhould pierce a large venous veſſel in ſuch a way, that a quantity of the venom would be inſtantly carried to the heart. In this caſe, which differs little or not at all from the artificial injection of the venom, the diſeaſe may be incurable, and obviate all remedy.

The lunar cauſtick, I repeat it, renders the venom of the viper innocent, and is its true ſpecifical remedy ; but much remains to be done, to apply it with the greateſt advantage in the bite of this animal. It would perhaps be ufeful to fwallow it diluted with water, even in pretty ftrong dofes. If the venom of the viper derange the blood, and be fatal when it is introduced into the torrent of the circulation of humours, the lunar cauftick, taken internally in a liquid form, may weaken its noxious qualities, and correct it in the veffels themfelves, to fuch a degree as to deftroy, or diminifh, the internal difeafe that this venom produces.

After my having difcovered that the lunar cauftick renders the venom of the viper innocent, it is natural to conceive that I ought to make fome trials on the *lapis infernalis*; I have indeed made feveral.

I found that a pafte formed of this ftone, and of the venom of the viper, might be applied with impunity to the wounded mufcles of birds; on choofing ten of them for thefe experiments, not one died. But of three which I envenomed with the teeth, and dreffed with the *lapis infernalis* fcraped to a powder, two died, one at the moment of the application, the other at the end of two hours. I had four pigeons bit in the legs by vipers, and treated them with the fame cauftick. One died in my hands immediately after I had applied it, another in the fpace of an hour, and the two others recovered.

Notwithftanding that the feafon was becoming unfavourable, and that I had no longer a hope of finding any vipers, I met with thirty-four of them by accident, in an excellent ftate and very vigorous. The firft purpofe to which I applied them was that of verifying my new remedy, and of ſeeing at the ſame time whether a ſolution in water of the lunar cauſtick given internally, would be at all efficacious to animals bitten by the viper.

I deſtined four very ſmall guineapigs for this experiment, and made them drink a teaſpoonful of the above ſolution: it was rather weak, but ſtill diſagreeable to the taſte. I wounded the femoral muſcles of three of them with venomous teeth, made immediate ſcarifications, and applied the lunar cauſtick as uſual; neither of them died.

I made another ſmall guineapig ſwallow two teaſpoonfuls of the above ſolution, and it died in my hands. I conclude from this that the quantity I employed was too great. I afterwards gave a ſingle teaſpoonful, as in the firſt experiment, to other four ſmall guineapigs, and had them immediately bit by as many vipers, making ſcarifications inſtantly after. They all four died. One when ſcarce bitten, another in an hour, a third in three hours, and the laſt in five. The reſult of this experiment ſhows that the bite of the viper is far more dangerous, than wounds that may be made artificially with its teeth, although filled with venom. One reaſon perhaps is, the difficulty of conveying the remedy nicely to all the parts where the teeth of the viper penetrate when it bites at its will. I likewiſe imagined at the time that the ſmallneſs of the animals I made choice of might partly have cauſed this, and determined in conſequence to make trials on larger and ſtronger ones, better able to reſiſt the effects of the poiſon; particularly the internal malady, which is communicated much quicker in ſmall animals. I had ſix fowls bit in the thigh by as many vipers. Five of them ſwallowed three teaſpoonfuls each of the ſolution of the cauſtick, the other did not ſwallow any. I applied the lunar cauſtick in the ſame way to each of their wounds; the laſt died, the other five all recovered.

I had ſix rabbits of a middling ſize bit in the thigh by as many vipers. I immediately applied the lunar cauſtick to their wounds, and made them all drink the ſolution of it. Four of them recovered; the other two died, one in three hours, the other in eight.

I repeated this experiment on ſix other rabbits, ſomewhat larger than the above, and neither of them died. On having four others bit, and treated exactly in the ſame way, they all likewiſe recovered.

The number of theſe experiments is ſtill too circumſcribed to render us certain that the lunar cauſtick is a never failing remedy againſt the bite of the viper; and this is owing to the difficulty of conveying it to all the parts into which the venom has inſinuated itſelf: three or four hundred experiments would ſcarcely ſuffice to fully clear up this important matter; I however have no doubt of the efficacy of this remedy, and can affirm that the lunar cauſtick is the true ſpecifical remedy againſt this dreadful poiſon.

On

On the Poiſon called Ticunas.

The ſingular and unexpected effects of the lunar cauſtick, which, when united with the venom of the viper, renders it innocent, made me ſuſpect that the effect might be the ſame if it were united with the *ticunas*. I mixed them then in equal quantities, and formed a ſomewhat ſoft paſte. I now made ſeveral wounds in the muſcles of a pigeon's leg, and applied this paſte to them. The pigeon died in leſs than two minutes. I repeated this experiment with the ſame circumſtances on another pigeon, which died in leſs than three minutes. I again repeated it on two others, one of which died in leſs than two minutes, and the other ſoon after the end of the third. The lunar cauſtick then does not correct the deadly quality of the *ticunas*, and conſequently is not its ſpecifical remedy. The paſte formed of it does not even retard the death of animals to which it is applied; ſince two pigeons, to the legs of which I had ſimply applied the *ticunas*, died in three minutes, and not ſooner. I paſs over in ſilence many other analogous conſequences which I obtained on making like experiments on guineapigs and ſmall rabbits.

Having procured ſeveral ſnakes, ſimilar to thoſe the examination of which is mentioned in page 139 of this volume, for the occaſion, I had the curioſity to ſee whether the poiſoning them with the *ticunas*, in the muſcles of the tail, would be attended with the conſequences I had before obſerved; and whether theſe creatures would continue in an apparently lifeleſs ſtate, for as many hours as they did on the former trials. I introduced then into the muſcles of the tail of one of theſe ſnakes, an American arrow which I had previouſly ſteeped in the *ticunas*, liquified by the heat of boiling water. I made a conſiderable wound along the vertebræ, that the poiſon might penetrate well into the muſcles. At the end of an hour the ſnake ſcarcely ſtirred, and in an hour more ſeemed dead, and quite deprived of motion and irritability. In this apparent ſtate of death, I examined the motion of the heart attentively through the ſkin, and could perceive a feeble and ſlow contraction of this muſcle. This contraction continued for twenty-ſeven hours, conſtantly diminiſhing in force, and in this interval every one would have looked on the creature as dead, for ſetting aſide the motion of the heart, it was quite ſtill and free from irritability. At the end of twenty-ſeven hours the motion of the heart gradually became quicker and more conſiderable; and I then thought I perceived, on giving the creature a violent blow, a ſmall undulating motion. In forty hours, I obſerved a motion, ſometimes of one extremity of the body, ſometimes of the other, but very feeble. In ten hours more this motion, and the return as it were to life, were manifeſt and certain, notwithſtanding the ſnake could neither

ther crawl nor hold up its head. I left it in this ſtate during the night, and the next morning found it very lively, in good health, and able to crawl; but ſix hours after, it died.

The event was exactly the ſame on treating two other ſnakes in the ſame way. Another much ſmaller one died in leſs than two hours; its heart continuing to move three hours after its loſing all apparent ſigns of life.

It cannot then be doubted but that the *ticunas* is a mortal poiſon, even to animals with cold blood, although it be true that it is far leſs ſo to them, than to thoſe that have the blood warm. But that which well merits our attention, is the apparent ſuſpenſion of life, and of the voluntary motions of all the animal's muſcles, except that of the heart, the irritability of which is indeed diminiſhed, but not totally deſtroyed.

We cannot afterwards but admire, that a long continuance of this iſolated motion of the heart, is capable of reſtoring by degrees to the animal, that life, and that motion of all its other organs, which it had entirely loſt. Without the action of this muſcle, all muſt irrevocably have ceaſed.

On the dangerous Conſequences of the Oil of the Cherry Laurel.

The following experiments on the oil of the cherry-laurel, will not only ſerve to complete thoſe

I have already made on this ſubject, but to ſhow in a clearer way, that this oil, whether given internally or applied to the wounds of animals, is one of the moſt terrible and deadly poiſons known. This important truth will, I hope, deſtroy once for all, the abuſe that has crept into ſeveral places in Italy, of ſelling the oil of the cherry-laurel publickly in the ſhops, to all thoſe who aſk for it. It is eaſy to ſee how dangerous this cuſtom may be to ſociety; and this danger is encreaſed again by the method adopted in ſelling it. It is uſually maſked under the title of *Eſſence of bitter Almonds*, and is found under this name in the printed liſts of diſtillers, who ſell it with other eſſences, oils, and liquors, that may be drank with the greateſt impunity. Nay, what is more, this dangerous poiſon enters into the compoſition of a liquor for the uſe of the publick, which is made and ſold without any ceremony; and to prevent all ſuſpicion of the true nature of this poiſonous mixture, it is vended under the title of *Liquor of bitter Almonds*, or *of Peach-flowers*, and is even put into milk, and into ragouts. It is true that a ſmall quantity of this poiſon is employed in them, and that theſe liquors are not drank like wine and water; but a poiſon is always a poiſon; and we do not beſides know but it may be hurtful when uſed a long time, although in very ſmall quantities; and whether it may not give a diſpoſition to certain diſeaſes. I have heard ſome perſons ſay, that when ſwallowed it muſt be an excellent

cordial,

cordial, which its truly agreeable and aromatick ſmell may eaſily induce them to believe.

Tuſcany owes to a philoſophick ſovereign, the knowledge of this pretended *oil of bitter almonds*, and the advantage of being ſecured from the abuſe that may be made of it : ſo true it is that philoſophy is uſeful even to ſovereigns, and that, according to the wiſh of one of the ancients, they ought all to be philoſophers, or at leaſt to know how to philoſophize on occaſion !

The Oil of the Cherry-Laurel is a Poiſon to Vipers.

Having had an opportunity of providing a quantity of the oil of the cherry-laurel, I wiſhed to ſee what effects it would have on vipers, and accordingly made a large one ſwallow about ten drops. In leſs than two minutes it could ſcarcely drag itſelf along, and at the end of ſeven ſeemed quite dead. In two minutes more it was perfectly inſenſible to the ſtimulus of a needle. However on obſerving attentively, its heart was ſeen to move, and to riſe and ſink alternately : it continued this motion, which regularly diminiſhed, for three hours. In theſe animals we may judge extremely well of the total repoſe of this muſcle, without opening the thorax ; an obſervation that in many caſes may be highly important. We may likewiſe obſerve this motion of the

the heart externally in other animals with cold blood, even in frogs, but with greater difficulty.

I have found in general that the oil of the cherry-laurel is a very ſtrong poiſon, even to vipers, the ſuddenneſs of its effects on which, is proportioned to the quantity ſwallowed. I have ſeen theſe creatures die in a very few minutes on giving them thirty or forty drops, in which caſes they have ſickened and become motionleſs inſtantly; and I have found it mortal even when given to them in the ſmall quantity of a drop, or at moſt of two. It is true that in theſe laſt inſtances, the diſeaſe diſcovers itſelf very late, and that the viper lives many hours after. 'Tis obſerved that the muſcular irritability is in general very ſoon deſtroyed, notwithſtanding which the heart continues to move for a very long time, even after the animal ceaſes to give any ſign of life, or ſenſation. The heart, I do not here ſpeak of the inteſtines, is an exception to the general rule of the other muſcles, and this intereſting point of animal phyſicks deſerves ſtill more the attention of philoſophers, as having been hitherto totally neglected.

The Oil of the Cherry-Laurel is a Poiſon to Snakes.

I made a ſnake ſwallow five drops of the oil of the cherry-laurel, which it had ſcarcely taken, when I obſerved it to crawl with difficulty; in leſs than two minutes

nutes it appeared to be almoſt dead, ſince it only preſerved a ſmall motion of its tail, which ceaſed a little after. I ſtimulated it in different parts of its body without any effect. On opening the thorax, I found the heart and auricles motionleſs; but when I pricked them with a needle, they began to move, and this motion continued ſeveral hours. At length I ſeparated the heart from the thorax, and it inſtantly ceaſed to move. However on touching it repeatedly with the point of a needle, it each time contracted itſelf once, and continued to do ſo for ſeveral hours. It never had a ſpontaneous motion, and never contracted itſelf more than once on being pricked with the needle.

The Oil of the Cherry-Laurel is a Poiſon to Snakes, when applied to their Muſcles.

I made a wound of about an inch in length, in the muſcles of the tail of a young ſnake, and applied to it about forty drops of the oil of the cherry-laurel. It died in leſs than ten minutes, without having been convulſed, and without the ſmalleſt ſign of irritability remaining in its whole body.

I laid bare a great length of the muſcles of the tail of an ordinary ſized ſnake, and wounded them in ſeveral places. I applied an abundance of the oil of the cherry-laurel to the wounded parts throughout, and a moment after repeated this again.

In

In leſs than a minute the ſnake moved languidly and with difficulty, and the motion of the different parts was feeble, in proportion as they approached the tail. In an hour after, however, it had in a great meaſure recovered its former vivacity. I then applied freſh oil to the ſame wounds, and in leſs than a minute it could ſcarcely ſtir itſelf, remaining twiſted in the form of a zigzag; however, in half an hour it became as lively as uſual.

I applied the oil of the cherry-laurel twice, to the muſcles of the tail of another ſnake, which although it had appeared dead, and remained in that ſtate for ſeveral hours, both times recovered itſelf. However, after reviving the ſecond time, and becoming very lively, it died of itſelf in a few hours.

This oil then, when it is applied to the muſcles of ſnakes, inconteſtibly produces violent derangements in a very little time, but is not, however, capable of killing them when they are very large; neither does it, in the circumſtances at leaſt in which I have obſerved it, kill the ſmaller ones very ſuddenly, notwithſtanding it is otherwiſe certain, that ſnakes without diſtinction die readily, when made to ſwallow it, even in a very ſmall doſe.

It is a Poiſon to Vipers, when ſimply applied to their Muſcles.

I wiſhed to ſee whether the oil of the cherry-laurel would be mortal, when applied to the wounds of of vipers purpofely made, and whether it would be lefs fo in thefe cafes than in thofe I have related, of their fwallowing it. It has been found by a long fucceffion of experiments I have made for that purpofe, that both the fpirit and oil of this plant produce violent derangements when applied to the mufcles of vipers, but not to fuch a degree as when they are taken internally. In thefe cafes the heart continues to move in the fame way that it does in the other animals with cold blood, whilft all the other parts remain motionlefs and infenfible to the moft active ftimulants. In like manner I have obferved, that on introducing a few drops of the oil into the natural aperture that is found towards the tail of the viper, the creature died in the fame way as in the cafes related above, that is to fay, with a lofs of mufcular irritability, but with a continuance of the motions of the heart.

I bathed the mufcles of a vipers tail, ftripped of the fkin for a great length and wounded in feveral places, with the oil of the cherry-laurel; an inftant after, it had loft the motion of the part of the body towards the tail; it twifted and wrapped itfelf up, fwelled confiderably, and was violently convulfed.—I likewife bathed the mufcles of another viper towards its tail, with this oil, and in twenty feconds it could fcarcely move itfelf. It was contracted and wound up, was fwelled to twice its fize, and numbed. It died in lefs than three hours.

It acts likewise as a Poison, when applied to the Muscles of Pigeons.

It will be sufficient to relate here some of the experiments I made on the muscles of pigeons, to show in what way the oil of the cherry-laurel acts in these cases.

I stripped the whole of the skin from the leg of a young pigeon, and wounded the muscles in several places, without dividing any perceptible blood-vessel. I then applied about twenty drops of the oil to the part, which was more than a square inch in superficies. In six minutes the pigeon was no longer well able to support itself on its feet. In three more it was seized with convulsions, and at length fell on its breast. In six minutes more it had lost its strength, but still breathed, and kept open its eyes. After six other minutes it began to stir a little, and remained quiet for the space of twenty more. It at length recovered, and became as well as ever.

I laid bare the breast of a very young pigeon, wounded the pectoral muscles in several places, and applied to them about twenty drops of oil. Five minutes after, the pigeon became very feeble, and almost incapable of supporting itself; it however survived, and had no other symptoms. The wound was very large, and well covered with oil.

I re-

I repeated this experiment on another pigeon, applying twenty drops of the oil, as above. In the ſpace of three minutes it could no longer ſupport itſelf, and died in five more.

Two other pigeons, ſomewhat larger, recovered, although treated preciſely in the ſame way with the laſt.

I returned to my experiments, on the legs. I laid the leg of a middling ſized pigeon entirely bare, and wounded the muſcles in ſeveral places. I then ſucceſſively applied to the wounds more than thirty drops of the oil: in thirty minutes it could no longer ſtand, but ſoon revived, and recovered perfectly.

This experiment made on two other pigeons, was attended with the ſame ſucceſs; neither of them died, but on the other hand ſoon recovered from the feebleneſs that ſeized them: however having repeated it on two much younger ones, they both died in violent convulſions in leſs than three minutes, notwithſtanding I had not laid their legs ſo bare as uſual, and had applied a leſs quantity of the oil.

From all theſe experiments, it appears that the oil of the cherry-laurel is in reality a poiſon to animals, when applied in an immediate way to the muſcles, by the means of wounds; but that it is in this way far leſs deadly, than when ſwallowed.

It.

It is a Poiſon when applied to the Eyes of Pigeons.

I omit the mention of ſeveral experiments I made on the eyes of pigeons; it will be ſufficient to know that the oil of the cherry-laurel is a violent poiſon when applied to theſe organs, and that it cauſes a ſpeedy death to theſe animals, in the ſame way with the ſpirit itſelf of the plant.

Applied to the Heart, it renders it motionleſs.

The faculty poſſeſſed by the oil of the cherry-laurel of deſtroying the irritability of fleſhy fibres, induced me to try whether, on being immediately applied to the heart, it would render it motionleſs, even to external ſtimuli. I therefore let fall a few drops on the hearts of ſeveral frogs, the motions of which ſoon ceaſed, and could not be reſtored by the pricks of a needle. The ſpirit of the cherry-laurel produces the ſame effect, but leſs ſuddenly and more imperfectly than the oil.

It

It kills, when applied to the Brain.

I was afterwards defirous of feeing whether this oil would be mortal when applied to the brain of frogs, and a few minutes after the application of it obferved that the creatures could fcarcely crawl: in lefs than fix minutes they died. The heart however continued to move. The brain being ftimulated, no contraction of any part enfued, but on forcing a pin longitudinally into the fpinal marrow, the feet fprung up violently. This would lead one to believe that the nervous fubftance, when put in contact with the oil of the cherry-laurel, lofes its power of contracting the mufcles; but that this poifon notwithftanding, is only capable of removing it in the nerves or nervous productions that it immediately touches.

It deprives the Nerves with which it is put in contact, of the faculty of contracting the Mufcles.

To affure myfelf of this I applied it to the crural nerves of frogs, and in lefs than two minutes obferved that one of thefe creatures on which I tried it, had loft the faculty of contracting its feet, and that when I ftimulated thefe nerves, at the part where the oil had touched, with a needle, they no longer put the mufcles in action. But every time I ftimulated them towards the legs, where the oil had not reached, the feet contracted themfelves forcibly. The nerves then are not the organ or inftrument by which the oil of the cherry-laurel communicates its deleterious qualities to the other parts of the machine, and are themfelves only fufceptible of them at the precife part to which the oil of the cherry-laurel has been immediately applied. The fpirit of the cherry-laurel, when applied to the nerves, produces effects analogous to thofe of the oil, but not fo violent. It is very probable that this action of both oil and fpirit is fimply mechanical, and that they alike operate in the way with fubftances that corrode and crifp.

The Oil of the Cherry-Laurel, adminiftered to Leeches, deadens the Part it immediately touches.

I injected the oil of the cherry-laurel into the bellies of feveral leeches, which immediately deadened, and were no longer fufceptible to external ftimuli. The fame thing happened when I tried the fpirit. I injected fome of them for almoft half their length, preventing the oil from paffing further by a ligature, and faw with furprize that the injected half was deadened, and no longer fenfible to any ftimulus, whilft the other part was alive, and continued in that ftate for many hours. This very

very ſingular phenomenon is not found either in ſnakes or vipers, every part of which becomes inſenſible almoſt at the ſame time; and this difference may be principally cauſed by the diverſity of motion in theſe animals, when compared with the others.

I bathed the part of a leech towards the head, with the oil of the cherry-laurel. In leſs than three minutes there was no motion in this part, whilſt the other half was ſtill living and in full action ſix hours after.

I bathed the part of another leech towards the tail, and in leſs than two minutes found it motionleſs; the other part continued to move for ſix hours.

I rubbed a part of a ſnake's tail that I had cut, with the oil of the cherry-laurel; in leſs than half an hour it had loſt all motion.

The Oil of the Cherry-Laurel is deſtructive to Animals, when injected by the Jugular.

The multiplied examples I have related above, of the deleterious powers of the oil of the cherry-laurel, induced me to think (notwithſtanding I had two years before in London, injected the ſpirit of this plant into the jugular veins of rabbits without its cauſing their death) that it might kill animals, when injected into the blood. I preſumed that the oil being much more pungent and burning than the ſpirit, would act on the blood with far greater energy. In conſequence of this I made ſeveral experiments.

I injected into the jugular vein of a large rabbit, ten drops of the oil of the cherry-laurel, with which I had mixed five or ſix drops of water. At the moment the liquor paſſed through the ſyringe into the vein, the animal fell into convulſions and died. I opened the thorax, and found the blood blacker than it is in its natural ſtate. The left ventricle and left auricle of the heart were almoſt empty, and the little blood that remained in them coagulated. The right ventricle and auricle were ſwelled, and filled with coagulated blood. There was no motion, neither could any be excited by ſtimulations. The lungs were covered throughout with large dark ſpots, and all their veſſels filled with coagulated blood, ſome of which was likewiſe extravaſated in ſeveral places. From the part of the liquor that remained in the ſmall ſyphon, I judge that ſcarcely ſeven drops had entered the vein.

I tried the oil on another rabbit in a leſs quantity, only injecting five drops with as many of water. The rabbit became convulſed and died inſtantly. I likewiſe opened the thorax of this one, and found the heart and auricles in motion. The right ventricle and auricle were ſwelled, and the oppoſite cavities contained a little blood. In a ſhort ſpace the heart ceaſed to move, and I found the blood in the right auricle and ventricle ſomewhat viſcous

and

and black. In the oppoſite cavities lay a little blood of a red colour. The lungs were ſtained with blood all over, but not ſo much ſo as in the former caſe, and the blood in the veſſels ſeemed to be ſtagnant.

I do not think that more than three drops of the oil entered the jugular vein, and yet we ſee that the animal died inſtantly. It cannot here be doubted but that the cauſe of the death lay in the lungs, and in the blood that was found ſtagnant in their veſſels. It is ſuperfluous to remark that the ſuddenneſs of this event, and the certain marks of a general coagulation of blood in the lungs, directly exclude the pretended action on the nerves, and furniſh a certain proof to the contrary.

I have ſince found that if the oil be injected in a much ſmaller quantity, either death does not enſue, or happens much later; in this caſe there are very violent convulſions, which are certainly produced by the anxiety in the animal, cauſed by the blood's becoming by degrees ſtagnant in the veſſels.

The Spirit likewiſe of the Cherry-Laurel, kills when it is injected into the Veſſels.

On making theſe latter experiments it was natural to ſuſpect that the ſpirit of the cherry-laurel, when injected into the veſſels, would likewiſe bring on death, and that the experiments I made in London were not concluſive, becauſe they were too few in number, or perhaps likewife, becaufe I employed a weak fpirit. Be that as it may, I wifhed to affure myfelf afrefh by experiment, and I do not blufh to oppofe my own trials to new ones, more decifive, more precife, and more numerous.

I prepared then a fpirit of cherry-laurel three times diftilled, and having put about fifty drops into the fyphon, I injected them into the jugular vein of a rabbit. A fhort time after the injection, perhaps in lefs than forty feconds, the creature died in convulfions, which were however not violent. Having opened the thorax, I found the lungs quite fpotted; the fpots were very fmall, and refembled fo many dots of a darkifh red colour. The blood in the pulmonary veffels appeared quite vifcous and ftagnant, and I found it in the fame ftate in the heart.

This experiment, repeated on three other rabbits, varied but little in the refult; one of them died at the moment of the injection.

The fpirit I employed was highly active, and killed the animals very fuddenly to which I gave it in an exceedingly fmall dofe. I fhall not enter into a detail of any further experiments, becaufe thofe I have juft defcribed are fufficient for my purpofe; however I met with a cafe of a middling fized guineapig that particularly deferves to be noticed. I made it fwallow a teafpoonful of the fpirit of the cherry-laurel of the third diftillation, which had fcarcely reached its ftomach, when it fell as if dead, and remained in this ftate for fix minutes: it then fuddenly

denly ſprang up and began to run, although with ſome difficulty. In a few minutes it appeared as ſtrong and lively as it was before it had ſwallowed the liquor; I however found it dead two hours after.

It is then beyond a doubt that the ſpirit of the cherry-laurel itſelf, given in ſufficient doſes, and rendered more active by re-diſtillations, is a violent poiſon when introduced into the blood by the jugular vein, and that it kills inſtantly; ſo that it is no longer an exception to the law I have eſtabliſhed of the other poiſons, which when introduced immediately into the blood, without touching either the nerves or the wounded ſolids, kill in an immediate way, and in a few inſtants after bringing on convulſions. It is not only abſurd to have recourſe to the nerves to explain the action of the poiſon in theſe caſes, but this imaginary hypotheſis is quite ſuperfluous, ſince its violent effects on the blood are ſo very evident.

The Lunar Cauſtick does not render the Oil of the Cherry-Laurel innocent.

I wiſhed to know whether the lunar cauſtick, blended with the oil of the cherry-laurel into the form of a paſte, would not ſerve to correct it.

I made ſeveral ſmall wounds in the pectoral muſcles of a pigeon, and applied the paſte to them. In leſs than a minute it was ſeized with convulſions, and died an inſtant after.

 I re-

I repeated this experiment on another pigeon, which in ſix minutes fell into violent convulſions, and died in a very ſhort ſpace.

I made a comparative experiment, to ſee what effect the lunar cauſtick alone would have, applied to the pectoral muſcles of a pigeon. It was a little diſordered by it, but ſoon recovered itſelf perfectly, without having been at all convulſed.

I applied the paſte I have ſpoken of, to four other pigeons prepared in the uſual way. They all died convulſed in leſs than five minutes.

It is evident then that the cauſtick alkali does not correct the deleterious qualities of the oil of the cherry-laurel; neither does it correct thoſe of the ſpirit, as I have found by ſeveral experiments, the relation of which I here omit.

On Opium.

I was a long time deſirous of rendering myſelf acquainted by experiment, with the effects of opium applied to the living body. The little uniformity found in the authours who have treated on the properties of this ſubſtance, was a ſtrong incentive to my applying myſelf ſeriouſly to ſo intereſting a ſubject. The experiments I had tried of the immediate application of opium to the nerves, of which mention has been made in the ſecond volume of this work, were too few in number, and too little varied,

varied, to enable me to ſpeak with aſſurance, and without the riſk of having myſelf been miſled by them, on this matter. A little leiſure when I leaſt expected it, at length enabled me to make a great number of experiments on opium, of which I ſhall at preſent content myſelf with giving the moſt general conſequences, with a ſmall detail of neceſſary particulars, to enable my readers to form a clear judgment on the ſubject.

I had announced ſeveral years ago, that opium, diſſolved in ſpirit of wine, and applied to the crural nerves of frogs, deprives them of the faculty of contracting the muſcles; and that this effect ought ſolely to be attributed, not to the opium, but to the ſpirit in which it was diſſolved; ſince experiment had demonſtrated that opium, ſimply diſſolved in water, did not in any way derange the nerves to which it was applied. The famous Haller availed himſelf of the experiments and concluſions I have mentioned, in ſeveral parts of his publications againſt the Engliſh phyſician Robert Whytt, who every where ſupported the immediate action of opium on the nerves themſelves.

The various experiments which other naturaliſts have made ſince me, and which are not very conformable to mine, and the different hypotheſes which authours have latterly ſupported on the action of opium, have obliged me to repeat ſeveral of the experiments I made a long time ago, and to give them a greater extenſion and certainty.

I thought

I thought it proper to begin with animals that have warm blood, and to apply the opium to the different organs and parts of the living body. And as ſpirituous ſubſtances are one of the beſt diſſolvents of opium, I wiſhed in the firſt place to examine all the effects of a ſolution of it in ſpirit of wine. I employed an ounce of opium and three of the ſpirit well united together, and heated *in balneo mariæ*. I likewiſe made a ſolution of opium in water without a drop of ſpirit of wine, employing the ſame proportion of water that I had done of the ſpirit, mixing it well with the opium in a mortar, and keeping it ſeveral minutes in a jar *in balneo mariæ*; I then added freſh water according to the exigencies of the different experiments.

Reſult of the Experiments.

Guineapigs which I made ſwallow a ſpoonful of ſpirit of wine, inſtantly became motionleſs, and died in leſs than 20 minutes. Thoſe that ſwallowed the ſpirituous ſolution of opium, became motionleſs in a few minutes, and died in leſs than twenty-ſeven.

Thoſe into the bellies of which the ſpirituous ſolution was injected, became motionleſs in an inſtant, and all without exception died in leſs than half an hour.

Thoſe beneath the ſkin of which I injected the ſame preparation of opium, died in leſs than half an an hour; and I had ſcarcely made the injection, when they could no longer ſtir their hinder feet.

Thoſe into the anus of which I injected it, at the end of half an hour could no longer ſupport themſelves, and died in an hour.

Thoſe that drank the ſpirituous ſolution died in three hours; they had ſcarcely ſwallowed it when they became inſenſible and motionleſs.

Thoſe into the bellies of which the aqueous ſolution was injected, died in leſs than two hours; they loſt the greater part of their motion in leſs than half an hour, and were violently convulſed.

Thoſe, beneath the ſkin of which the ſame ſolution was injected, died in three hours. In the ſpace of half an hour they had loſt all motion, particularly that of their hinder feet.

Thoſe that I made ſwallow this aqueous ſolution, were very ſoon incapable of moving; but only two died, of ten on which I tried the experiment.

Thoſe into the anus of which the aqueous ſolution was injected, died in leſs than three hours; in the ſpace of half an hour they could no longer ſupport themſelves on their feet.

It is then a fact, confirmed by all my experiments, that opium, when ſimply diſſolved in water, kills animals with warm blood; although it is beſides true, that when diſſolved in ſpirit of wine, its effects are much quicker and more violent; but they then proceed, in a great part however, from the ſpirit itſelf, ſince we have ſeen that this ſpirit is alone capable of producing all theſe effects, and that

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it even produces more violent ones, and that more expeditiously.

My experiments on opium, either dissolved in spirit of wine, or simply in water, have been hitherto made on animals with warm blood. I was desirous of repeating them, varying them, and rendering them general, on those that have the blood cold, and presumed that I should obtain consequences equally new and important, since I was about to operate on creatures much more irritable than the others, and in which life is of longer duration, and more tenacious.

I chose turtles and frogs, in preference to several other animals, and likewise made some trials on leeches; a very singular creature, and one which differs, as well in its organs as in its vital functions, from all other known animals.

Result of several Experiments made on Leeches.

Leeches immersed in spirit of wine, died in two or three minutes.

Leeches immersed in a solution of opium in spirit of wine, died nearly in the same space of time.

Leeches immersed in a solution of opium in water, died pretty nearly in the same space of time.

I plunged half the body of a leech into spirit of wine, and found in a little time that this part had lost all motion, whilst the other half continued in action.

action. The experiment ſucceeded in the ſame way, whether the part of the leech towards the head was plunged, or that towards the tail.

The ſame conſequences enſued on plunging a leech into the ſolution of opium in ſpirit of wine, or into the ſolution of it in water; and I looked upon it as ſomething very extraordinary, that one half of the creature ſhould become dead, whilſt the other half continued in the ſtate of not having undergone any change, or ſuffered any injury. As to the action of opium on theſe animals, it appears certain that it ſhould be conſidered as that of a very violent poiſon.

Experiments on Turtles, made to ſwallow different Subſtances.

A turtle which ſwallowed ſpirit of wine, died in leſs than 20 minutes.

Another turtle which ſwallowed the ſolution of opium in ſpirit of wine, died in the ſpace of an hour.

Another, after having ſwallowed the aqueous ſolution, preſerved its vivacity for four hours; it died at the end of ten.

I repeated theſe three experiments with the ſame circumſtances on ſix other turtles, and the conſequences were perfectly analogous to the preceeding ones.

It is easily seen that opium, although dissolved in water, acts violently on these animals, so as even to kill them; but that its action is but trifling, when compared with that of spirit of wine.

Injections made into the Anus of Turtles.

I injected three turtles of the same size, at the anus, by means of a small glass syringe. On the first of them I tried spirit of wine, and in a few minutes it could scarcely stir. At the end of an hour, it was quite dead.

I injected the second with an equal quantity of a strong solution of opium in spirit of wine, and in half an hour it scarcely seemed alive or able to stir: it died at the end of the seventh hour, but the motions of the heart continued for an hour after.

I injected the third with the same quantity of the aqueous solution: it was very lively at the end of the sixth hour, and survized the sixteenth.

I have however observed in general, that turtles do not die when injected at the anus with the solution of opium in water. Those I injected with the spirituous solution, all died in less than three hours: the injection was scarcely made when they lost their strength and vivacity, and in half an hour they almost ceased to give any signs of life.

The action of the aqueous solution of opium, when introduced by the anus, is clearly proved; but it is very

very ſlow and weak, when compared with that of the ſpirit of wine.

Injection made beneath the Skin of Turtles.

I made an opening with a lancet in the ſkin betwixt the legs and belly of a turtle, and injected within it ſpirit of wine. In a few ſeconds the turtle became motionleſs, and died in leſs than an hour.

I injected another turtle in the like way, with an equal quantity of the ſpirituous ſolution of opium. In ſeven minutes it became without motion, and died at the end of four hours.

I injected a third with the aqueous ſolution of opium. It continued lively for two hours, but died at the end of the eighth.

The reſult of a repetition of the ſame experiments on nine other turtles, was perfectly analogous to that of the foregoing; ſo that there can be no longer any doubt of the action of an aqueous ſolution of opium, injected beneath the ſkin of turtles.

Turtles in which the Heart was laid bare.

I wiſhed to ſee what changes would be wrought in the heart of a turtle, on applying to it opium, and ſpirit of wine.

 I ſtrip-

I ſtripped the pericardium from the heart of a turtle, and applied to it ſpirit of wine ſeveral times ſucceſſively. In twenty minutes it had loſt all motion, notwithſtanding the animal continued alive. It died however in leſs than an hour, when no part of its body was any longer irritable.

I applied the ſpirituous ſolution of opium to the heart of another turtle, prepared as above. In half an hour it was become motionleſs, even to ſtimulation. The animal died at the end of three hours.

I applied the aqueous ſolution of opium to the heart of another turtle, and it continued its motion perfectly well for two hours: it ſtill ſtirred a little at the end of the ſixth. The turtle lived eight hours.

I applied an aqueous ſolution of the cortex to the heart of a fourth turtle; it had ſtill retained a little motion at the end of the ſixth hour. The turtle died on the eighth.

I laid bare the heart of another turtle, and ſprinkled it ſucceſſively with ſeveral drops of ſpirit of wine. The two auricles inſtantly ceaſed to move, and the heart in leſs than two minutes had no longer any motion, even when ſtimulated. The turtle lived a long time in this ſtate.

I opened the thorax of three turtles, on the heart of one of which I poured the aqueous ſolution of opium, on that of another the ſpirituous ſolution, and on that of the third the liquid laudanum of Sydenham. That on which the ſpirit of wine was poured continued to move ſeveral hours longer than

than the other two, which ceafed their motion as if it had been by concert. The contractions of the auricles of the heart on which I had poured the laudanum, were for fome time two by two, they then became three by three, foon after which the animal was quite dead. The heart continued a longer time contracted, in proportion as the intervals betwixt its contractions were of greater duration; a new and fingular phenomenon, which cannot be readily explained by the common theories.

Thefe experiments are not fufficient to determine that a folution of opium in water has no action on the heart; particularly as, when it is applied during the time this vifcus remains in the thorax, there are veffels and blood that may ferve to introduce it into the torrent of the circulation, and carry it to the other vifcera, by which a derangement of the animal economy may be brought on; fo that we cannot in this cafe attribute what may perhaps be the effect of a change in the blood, or of fome other unknown caufe, to its immediate application to the heart: to render the experiment decifive and unequivocal, it muft be made in fuch a way, that all the other parts being excluded, the heart may alone feel the action of the opium. It ftruck me then to purfue the following method:

Turtles, the Hearts of which were detached from the Thorax.

I removed the heart of a turtle from the thorax, and covered it with ſpirit of wine. Its motion ceaſed in a few minutes.

I put the ſpirituous ſolution of opium on the heart of another turtle. In the ſpace of a quarter of an hour there was ſcarcely any contraction of it, and in twenty-ſix minutes it no longer ſtirred, even when ſtimulated.

I plunged the heart of another turtle into the aqueous ſolution. It ſtill continued to move, but not forcibly, at the end of half an hour. In two hours all was at reſt.

I plunged another heart into ſimple water, and it ſtill retained a little motion at the end of the third hour.

Another plunged into the aqueous ſolution of the cortex, ceaſed to move at the end of two hours.

I made three other experiments on the heart, ſeparated from the thorax, and plunged into the ſpirituous ſolution of opium, and could not perceive any ſenſible difference in the diminution and loſs of its motion, when compared with that of three others, two of which I had immerſed in the ſolution of the bark, and the third in water.

It seems then very probable, if not very certain, at least from the few experiments I have related, that a solution of opium in water has no immediate action on the hearts of turtles: a new and important truth, contrary to the opinions of the greatest naturalists of this age, and which should induce others that have more leisure than myself, to investigate this important matter, by multiplying their experiments, varying them many different ways, and guarding themselves as much as possible against all accidental circumstances. Care must be taken that the opium may not form a glutinous fluid, and that it may not dry speedily when applied to the part. I avoided this inconvenience by wetting the part from time to time with water.

Frogs that have been made to swallow Opium.

I made a frog swallow about forty drops of spirit of wine. In forty minutes after I found it dead.

I made a second swallow forty drops of the spirituous solution of opium. At the end of forty minutes it was dead.

I made a third swallow the same quantity of the aqueous solution, twenty-five minutes after which it scarcely stirred. It was then lying on its back with its legs stretched out. It died in somewhat less than three quarters of an hour.

On repeating these experiments on twelve other frogs, I found a very sensible difference, but not such as to enable me to say to a certainty, that the aqueous solution of opium itself, kills frogs in a little space, and that it causes convulsions and a drawing back of the muscular parts.

Frogs injected beneath the Skin.

I injected a frog beneath the skin with spirit of wine. It died a minute after.

I injected a second with the spirituous solution of opium, and in a short time it could not support itself on its feet. It however was able to crawl a little at the end of thirty-five minutes, and died on the fortieth.

I injected a third with the aqueous solution of opium. In the space of ten minutes it scarcely stirred, and had its legs stiff and extended. It died in forty minutes.

I repeated the same experiment on several other frogs, and the consequences were pretty much the same. It is certain then that the aqueous solution of opium kills animals, when injected beneath the skin.

The Hearts of Frogs laid bare, but ſtill remaining in the Thorax.

I covered the hearts of three frogs with the aqueous ſolution of opium, and a fourth with water alone, by way of a comparative experiment. I could perceive no great difference in the ceſſation of the motion of the four hearts.

In ſix other frogs I found that the motion had ceaſed ſomewhat ſooner in the hearts to which I had applied the opium ; but having again repeated the experiment on ſix others, I found on the contrary that the motion of thoſe hearts to which I had applied the opium, ceaſed much ſooner than thoſe to which I had applied the ſolution of the cortex ; ſo that I cannot conclude from theſe experiments, that opium, in frogs however, diminiſhes the irritability and motion of the heart.

To enable me to eſtabliſh ſomething more certain, I made the following experiments.

Hearts of Frogs ſeparated from the Thorax.

I put the heart of a frog into ſpirit of wine. Its motion ceaſed in two ſeconds.

I put a ſecond heart into the ſpirituous ſolution of opium. It ceaſed to move in twenty ſeconds.

I put a third into pure water. It continued in motion for forty minutes.

I put the hearts of three frogs into pure water. The motion of one of them ceafed in twenty-one minutes; but it fpontaneoufly recovered its ofcillations, and that repeatedly.

The fecond loft its motion in ten minutes, but recovered it of itfelf.

The third ceafed to move at the end of fifty minutes.

I repeated thefe experiments on upwards of fifty hearts feparated from the thorax. I endeavoured to obferve the fame circumftances in all of them. I put feveral of them into the folution of the cortex, others into pure water, and others again into the aqueous folution of opium. The confequences have been very diverfified, and very inconftant; I could not however conclude that opium has a real action on the hearts of thefe animals, when applied in the above manner. Such at leaft is my prefent opinion, and it is my defign to make at a future period a greater number of experiments.

It now remained for me to make a new kind of experiments, which are perhaps the moft important, and the object of which is to examine whether opium acts on the nerves.

Frogs, the Brain of which was laid bare.

I laid bare the brain and ſpinal marrow of a frog, and applied to them ſpirit of wine. In ten minutes the frog could ſcarcely move, and was dead in thirty-five.

I applied pure water to the brain of another frog prepared in the way with the preceding one. It was ſtill lively twenty-four hours after.

I applied the aqueous ſolution of opium to the brain of a third, which at the end of twenty minutes moved with difficulty.

I applied to another the ſpirituous ſolution of opium, and thirty minutes after, it was ſtill able to move a little.

I applied the aqueous ſolution to another, which in forty minutes was drawn together, and ſtirred but little. It died at the end of the fifty-ſeventh.

I applied the ſame ſolution to another, and thirty minutes after found it contracted, with the body drawn backwards, and the hinder feet ſtretched out and lengthened.

I applied ſpirit of wine to another, which died at the end of ten minutes. The heart however continued ſtill in motion.

Another treated in the ſame way died in twenty-ſeven minutes, and another again in forty-five; both of them after a few minutes moved with difficulty.

On repeating this experiment on another frog, it died in fifty-four minutes; at the end of the ſeventh it fell into convulſions, and could neither walk nor ſupport itſelf.

Another treated in the ſame way was ſeized with very ſtrong convulſions, and after four minutes was no longer capable of walking.

The crural Nerves of Frogs laid bare.

I opened the belly of a frog, laid bare the crural nerves, and applied the ſpirit of wine to thoſe on the right ſide. At the end of four minutes I repeatedly ſtimulated the right foot, which remained conſtantly motionleſs.

To thoſe of another I applied the ſpirituous ſolution of opium. After eight minutes I found that the right foot could no longer ſupport itſelf, in what way ſoever I ſtimulated it; but ſcarcely did I ſtimulate the nerves of the left ſide, when the left foot contracted itſelf perfectly well.

I applied to the crural nerves of the right ſide, in a third frog, the aqueous ſolution of opium. At the end of two hours, the right foot ſtirred on ſtimulating theſe nerves, but not ſo well as the left foot did, on ſtimulating the nerves of that ſide.

I applied the aqueous ſolution of opium to the crural nerves of the right ſide, in three frogs, and ſimply applied water to the correſponding ones of the left; the motions as well of the muſcles of the right ſide, as thoſe of the left, ceaſed indifferently, *The crural Nerves of Frogs, to which a partial Application was made.*

I applied ſpirit of wine to one ſide only of the crural nerves. At the end of nine minutes, they could no longer contract the feet when ſtimulated on that ſide; but ſtimulations on the other ſide had a contrary effect.

I again applied the ſpirit of wine to one of the crural nerves only. At the end of four minutes they no longer contracted the feet, which moved however when ſtimulated at the part where the ſpirit of wine had not touched. On touching the other nerves, the muſcles every where contracted themſelves very well; a proof that the action of this fluid does not extend to beyond the parts that are touched by it.

I got ready a frog for a comparative experiment, to the nerves of which I applied nothing; at the end of forty minutes it ſtill contracted its feet.

The foot of another frog, four minutes after I had applied ſpirit of wine, no longer contracted itſelf, unleſs when the part of the nerves towards the legs and thighs, where the ſpirit had not reached, was ſtimulated.

The

The Crural Nerves of Frogs detached from the Vertebræ.

I cut the crural nerves at their going out of the vertebræ, and applied ſpirit of wine to thoſe of the right ſide. At the end of two minutes the right foot, although ſtimulated, and its nerves prick-ed, no longer contracted itſelf; whilſt the nerves of the oppoſite ſide were ſcarcely touched, when the left foot contracted itſelf forcibly.

I applied the aqueous ſolution of opium to the right ſide of another frog prepared in the ſame way. At the end of thirty minutes I found on irritating the nerves, that the correſponding foot moved, but not ſo forcibly as the left foot on ir-ritating the nerves of that ſide.

I plunged the crural nerves of another on one ſide into the above ſolution of opium, and thoſe on the other ſide into pure water, and found each of them, when irritated, to contract the feet alike, even at the end of fifteen minutes.

Theſe experiments are not at all deciſive; they may however furniſh matter to a variety of re-flections. Notwithſtanding it appears indubitably that opium, even when ſimply diſſolved in water, whether it is introduced into the ſtomach by the œſophagus, or into the inteſtines by the anus; whether it is injected beneath the ſkin, or into the abdomen;

abdomen; whether it is applied to the brain, or to the medulla oblongata; acts on the animal body: yet a doubt remains whether its action and energy are wrought on the nerves, or whether it needs the vehicle of the blood and circulation, and the motion of the humours, to give it activity. We have seen that the venom of the viper acts in no other way than by the medium of the blood; and the two vegetable poisons of the *ticunas* and cherry-laurel appear to act in the same manner. It is certain that all poisons, as well as opium, kill when swallowed; but this does not prove that their action is wrought immediately on the nerves, and that they do not employ the medium of the blood. There are many passages open in the mouth, in the œsophagus, in the stomach, and in the intestines, through which the most nimble and active particles of these poisons may introduce themselves readily into the blood. Thus the difficulty which arises from the mortal effects of opium when taken internally, does not prove that it acts immediately on the nerves; and we have besides demonstrated that the three poisons, the venom of the viper, the *ticunas*, and the cherry-laurel, have no immediate action on the nerves themselves.

To be enabled to make some very probable assertion on this difficult matter, an experiment must be imagined in which opium may act freely against the nerves, without the smallest introduction of it into the blood, or rather, without its touching the blood-vessels. Such an experiment, considering the

the dexterity and preciſion it requires, is not one of the eaſieſt to make, and can be only well tried on very ſmall animals, and on a very few of the nerves. I could think of nothing that would anſwer my purpoſe better than the crural nerves of frogs. To obtain certain conſequences, and ſuch as do not proceed from deceitful and variable experiments, it was neceſſary to make a great many trials, to exclude all the preparations that accidental circumſtances might have rendered imperfect, to compare the different conſequences with each other, and to weigh them in each caſe, with thoſe of the experiments intended to ſerve as comparative ones.

The following is the plan I have purſued in making theſe experiments. As the number of them already exceeds 300, I ſhall regard the inferences I have drawn from them as certain, till ſome one ſhall be able to demonſtrate to me the contrary.

I open the belly of a frog, and lay bare the crural nerves, by means of ſmall pincers and ſciſſars, in ſuch a way that they are entirely freed from every other part. I then cut the vertebræ and body of the animal in two, preciſely at the place from whence theſe nerves go out, which, by gently agitating the neighbouring parts, fall betwixt the thighs of the animal. In this ſtate, I remove the thigh bones, and totally clear the nerves for the length of eight or ten lines, and in very large frogs of even more. I let fall the nerves of each thigh into a ſmall hollow glaſs, which receives them in

ſuch

ſuch a way, that I can fill each glaſs with a fluid of any kind without its touching the adjacent muſcles; the nerves are not only freed from each other, but likewiſe from all contact with the thighs. I uſually put into one of the glaſſes, ſuch a proportion of whatever I wiſh to try on the nerves, as to cover the greater part of them with it, without its being poſſible for any of the liquor to find its way to the thighs, and mix with the blood. I take the precaution not to leave any veſſel faſtened to the nerves, and to put a little water into the other glaſs, to keep the contents of it wet like thoſe of the firſt. In this way I can make a compariſon betwixt the nerves that are envenomed, and thoſe that are not, compute the time that they continue to contract the muſcles, and judge of the vivacity of the motions.

I deſtined 300 frogs for theſe experiments, and divided them into ten claſſes, according to the different intervals of time that I kept them under trial. Thus I kept the crural nerves of the firſt claſs, which as well as all the others was compoſed of thirty frogs, in contact for ten minutes, with an aqueous ſolution of opium on one ſide, and with water on the other. Thoſe of the ſecond remained in the ſame ſtate for twenty minutes, and ſo on to an hundred, after which time the nerves were no longer capable of contracting their muſcles. It is true, that in ſome other experiments I have found that 100 minutes were not ſufficient for the nerves to entirely loſe the power of contracting the muſcles; but theſe

different

different consequences depend upon a thousand particular circumstances, and do not invalidate the law of effects that I have observed in this series of 300 experiments.

This is the result of the trials I have made. At the end of the first ten minutes, I stimulated the medicated nerves; I shall distinguish in this way those to which I applied the opium, and those which were not medicated, and found that the two feet, right as well as left, contracted with the same force and vivacity.

At the end of twenty minutes, I tried the stimulation on the second class of frogs, and could perceive no sensible difference betwixt the motions of the two feet, which were almost as lively as those in the first experiment.

At the end of thirty minutes the motions of the two feet were feebler, but alike in both.

At the end of forty minutes, the feet scarcely contracted; but their distinct muscles were clearly seen to contract, when the crural nerves were stimulated; and the motions of these muscles were equally lively in each foot.

At the end of fifty minutes, the muscles were distinctly seen to move, but not so much as before. The motions were however equal in the muscles of both feet.

At the end of sixty minutes the motions were very small, but alike on both sides.

At

At the end of feventy minutes a great deal of attention was neceffary to diftinguifh them to a certainty, but I could find no difference betwixt the motion of the mufcles of the right foot, and that of thofe of the left.

At the end of eighty minutes there was no longer any motion to be obferved in feveral of the frogs, in whatever way I ftimulated either their crural nerves that were medicated, or thofe that were not fo. But in the others of this clafs, I could not find that the medicated nerves were lefs ready to contract the mufcles, than the others.

At the end of ninety minutes, very few of the frogs retained any motion, on their crural nerves being ftimulated; and in the thirty that formed this clafs of the experiments, I could not find that the opium had wrought any greater change on the nerves, than the fimple water.

At the end of 100 minutes the mufcles of the legs were immoveable, in whatever way they were ftimulated on either fide.

I can conceive nothing more decifive and more certain than the feries of experiments I have juft related, from which it feems of neceffity to follow, that the circulation of blood and humours in the machine is the vehicle for opium, and that without this circulation it would have no action on the living body.

The aqueous Solution of Opium injected into the Blood-vessels of Rabbits.

It now remains to be ſeen whether opium, when injected into the veſſels, cauſes death, and whether it produces the ſame derangements in the animal economy, when introduced into the circulation of the blood, as it does when ſwallowed, or injected into the different organs and viſcera.

I injected about eighteen drops of the aqueous ſolution of opium into the jugular vein of a large rabbit. It was ſcarcely injected when the creature could no longer ſupport itſelf, or walk, and its legs were bent and drawn aſide. It recovered in a few hours. I conceive that ſcarcely eight drops of the ſolution entered the vein.

I repeated this experiment on a ſecond rabbit, the legs of which, as in the firſt inſtance, were bent and drawn aſide. At the end of two minutes, it fell on its breaſt, moving itſelf a little now and then. After half an hour it ran about freely, and became perfectly well.

The injection tried on another rabbit did not ſucceed, ſince the opium, inſtead of entering the jugular vein, paſſed wholly into the cellular membrane. This rabbit had no ailment.

I en-

I injected into the jugular vein of another rabbit a tea-ſpoonful of the ſame aqueous ſolution, and it died inſtantly.

I repeated this experiment on another rabbit, with the ſame quantity of the ſolution, that is to ſay about forty drops; the creature died at the moment of injection.

I repeated this experiment on another rabbit, with the ſame doſe of the ſolution, a great part of which, however, flowed back during the injection. The animal could no longer walk, nor ſupport itſelf on its feet, which were ſtretched out. It died at the end of two hours.

I conceive it to be altogether ſuperfluous to relate, for the preſent however, a greater number of experiments on opium, injected into the jugular vein, and introduced into the circulation, without its touching any of the wounded ſolids. When once it is received into the veſſels, I do not ſee how it can communicate itſelf in an immediate way to any of the nerves, ſince anatomy aſſures us, that the inner membrane of the blood-veſſels is not furniſhed with what can properly be called nerves; and although this ſhould even be the caſe, opium, on touching a nerve, does not act on it in any way, nor produce any derangement in the animal economy, in whatever way it is applied to a nerve, whether that nerve is entire, or cut; whether it is covered with its proper coats or ſheaths, or its medullary pulp itſelf is put in contact with the poiſon. In all these cases, without any exception, opium has been found innocent.

Thus then, opium injected into the veins, produces heaviness, convulsions, and, at length, as has been seen, death itself. Wine produces pretty nearly the same effects. Spirit of wine diluted with water, likewise produces heaviness and convulsions, and if genuine and rectified, kills in an instant. The blood is then found congealed in the vena cava, in the auricles, in the right ventricle, and in the lungs; certain effects, and sure causes of death, without our being driven to any recurrence to the nerves.

Emeticks and purgatives when injected, bring on vomiting and stools, as if they had been taken by the mouth; a proof that their action conveys itself unaltered to the stomach and intestines, without the concurrence of the nerves, and in the same way as if these substances had been simply swallowed. And why not say as much of opium, when it is swallowed too? If there is no recurrence to the nerves in the cases of emeticks and catharticks, and there can be no reasonable recurrence to them, why should the nerves be employed to explain the action of opium, whilst this substance, when applied immediately to the naked nerve, does not act on it in any manner, and produces neither change nor derangement in it? I do not however think, that any one will recur to the nerves, in the cases in which opium, injected into the jugular vein, kills, as has been seen, instantly.

I do not pretend to exclude by my reiterated experiments, any other than the immediate action of opium on the nerves; and my aim is to prove, at the same time, the immediate action of opium on the blood, independent of the nerves, without perplexing myself with the imaginary hypotheses that neurologists may invent to support old errours and prejudices, and to make them agree with the principles I have just established. Real physicians have now a basis of certain experiments on which they may for the future found their theories on opium, which has been the subject of so much discussion, and which is still so little known; and I flatter myself that they will resolve to set aside the hypotheses and received opinions they have imbibed in the schools, and reflect maturely on the circumstances I have just related. I know the power of a prejudice in favour of old erroneous doctrines, and how great the resistance is even to the most certain and luminous experiments. The man who is at length convinced of the truth of facts, which are always irresistible, will not listen to the most direct consequences. Prejudice has certainly a great share in this repugnance; but it is, above all, our self-love that dreads to adopt new truths, because they carry with them a tacit avowal of our ignorance; hence arises the difficulty of bringing those who are advanced in years, and men of learning who have already acquired a reputation, to admit of new discoveries.

Let not the ſuddenneſs of the effects of opium, and the inſenſible diminution of its weight, be brought in favour of the nerves, againſt the blood: ſince it has been ſeen that the venom of the viper, the Ticunas, and the Cherry-laurel, when injected into the jugular vein, act inſtantly, ſo as to occaſion death, even when employed in very ſmall quantaties; and ſince it has been found by experience that the action or effect of theſe poiſons is wrought on the blood, and not on the nerves. Oil of vitriol kills, when injected into the blood, even in a very ſmall quantity, and no one, I apprehend, will ſay that this liquor acts on the nerves, and not on the blood. Common oil, and many other innocent ſubſtances, when injected in the ſame way into the blood, kill even very ſuddenly, and produce very ſtrong convulſions. Every one muſt perceive, that all the derangement theſe bodies can bring about in the animal economy, is ſimply mechanical, and dependant on the ſtoppage or diminution of the circulation in the different viſcera, and not on an affection of the nerves. We muſt not even be aſtoniſhed at obſerving very great derangements produced by very ſmall quantities of matter, ſince the active part of bodies, and particularly of medicaments, is abſolutely confined to very ſmall maſſes, I can almoſt ſay, to atoms. And I cannot conceive how any force can act on the nerves, and occaſion the greateſt diſorders in them, and can yet in no way act on the blood, whilſt we ſee that the $\frac{1}{1000}$ part of a grain of the venom of a viper is ſufficient to kill a bird, when

when mixed with its blood; and perhaps that which renders this animal gum poiſonous, may ſtill again be no more than the $\frac{1}{1000}$ part of this fraction of a grain.

Dr. Robert Whytt ſtarts, as an argument againſt the blood in favour of the nerves, that when the heart of a frog is removed, the opium it is made to ſwallow acts equally againſt ſenſation and motion; but that when the head is cut off, and the ſpinal marrow deſtroyed, the opium operates ſlower and leſs violently. The firſt part of this reaſoning is, as has been ſeen, altogether falſe, and the other part, even if it were true, would prove nothing; becauſe when the brain and ſpinal marrow are deſtroyed, the animal economy may be ſo changed, that the opium can no longer act, as it previouſly did, in a ſtate of health. Indeed purgatives, emeticks, and poiſons in general, only act on living animals. But in the preſent caſe, Whytt's experiment is not conformable to mine, which I have however repeated ſeveral times with the utmoſt attention. Here again, a few experiments can decide nothing, conſidering the great diverſity that attends the event of them.

To render the experiment more ſimple, and ſubject to fewer difficulties, I did not cut off the head of the frogs, but made a ſmall opening in the cranium, through which, with a large pin, I deſtroyed the whole of the brain and ſpinal marrow. In this way I avoided the great loſs of blood that the animal ſuſtains on its head being cut of, and facilitated the compariſon with the frogs that had been made to ſwallow opium, without the brain and ſpinal marrow. I noted the duration of the heart's motion, and from time to time ſtimulated the crural nerves of all of them. I can certify, that having prepared forty-eight frogs, twenty-four in one way, and twenty-four in the other, I could not perceive the opium to act later, or more feebly, in one caſe than in the other.

I however deduce from theſe conſequences two very important corollaries; the firſt of which is, that the motion of the heart neither depends on the nerves, nor on that chain of ſenſations that conſtitutes animal life. The ſecond is, that the action of opium is independent of the nervous ſyſtem.

I find in ſome authours a ſtrong argument againſt the blood, in favour of the nerves, in the caſe where opium is injected into the veſſels: it is, that the action of this ſubſtance is ſuddenly carried to the nervous extremities of the blood-veſſels themſelves, and from thence to all the other parts of the nervous ſyſtem. It cannot be denied but that fleſhy fibres are obſerved in the large blood-veſſels, whence it is certain, that there are likewiſe nerves in theſe parts, ſince there is no muſcle without a nerve. But theſe fleſhy fibres are only obſerved in the large trunks, and not elſewhere; and it would be abſurd to imagine a ſtructure that is belied by obſervation, with the only view of ſupporting an hypotheſis which is combated on ſo many ſides. It is certain that no nerves are ſeen to go towards the blood-

veſſels,

veſſels, to unite themſelves with them, ſince the greateſt anatomiſts have not been able to find any. On another hand, the ſenſibility of theſe veſſels is in no manner demonſtated, and I have ſucceeded in tying them many different ways, without the animal's giving any ſymptom of uneaſineſs. Indeed very great attention muſt be paid in making theſe experiments, which are exceedingly nice, that the veſſel at the place where it is tied is well cleared of all the neighbouring parts; that it is not tied at a part which may be accidentally croſſed by a nerve in its paſſage elſewhere; and that in tying it, neither the veſſel itſelf, nor the adjacent parts, is dragged. I muſt likewiſe recommend the not operating on very large veſſels, ſince I have ſometimes obſerved, that if a large torrent of blood is ſuddenly ſtopped, the animal appears to ſuffer from it. It is, in ſhort, a thing known to all the world, that the internal membrane of the veſſels is neither muſcular nor nervous, and therefore opium cannot act immediately on the nerves, from the ſole cauſe of its being put in contact with the internal coats of the veſſels.

I wiſhed to ſee whether opium, when ſwallowed, would diminiſh the force and velocity of the contractions of the heart, ſince it appears to have no action on this muſcle, that relates to the duration of its motions. I muſt confeſs, that I could eſtabliſh nothing certain on this point, notwithſtanding I directed upwards of one hundred experiments to this object alone. I found a great variety and inconſtance in thoſe on the frogs, on which I principally operated. I obſerved in general that opium given to animals with warm blood, in moderate doſes, increaſes the force and motions of the heart; but that if it is given in a great doſe, it ſeems at once to diminiſh the vigour of the animal, and the force itſelf of the heart: in this it reſembles many other ſubſtances that tend to deſtroy life, and to abate the vital powers. The action of opium is thus found to be quite comformable to the ſymptoms that we obſerve in man, when it is taken internally. The oſcillations of the heart, far from being diminiſhed, are moſt frequently increaſed; and the few contrary inſtances that may be found, do not in any way change the general law of the action of opium on animals.

I made twelve frogs ſwallow each about twenty drops of the aqueous ſolution of opium, and inſtantly ſeparated the heart from the thorax. I opened the thorax in twelve others, but did not remove the heart; all of theſe, as well as the others, had previouſly ſwallowed opium. I noted the time of its action on all the twenty-four, and found that the effects of the opium diſcovered themſelves much ſooner in the frogs, the heart of which remained in the thorax, than in thoſe from which I had removed it. The difference in time was more than one-half. By the effects of opium, I mean the faculty it poſſeſſes of rendering the limbs paralitick; that is to ſay, of depriving the animal of the power of exerciſing its muſcles. I do not here ſpeak of the heart itſelf, which continues to move for a very long ſpace of time, even after the animals are dead; nor of the nerves, which, on being ſtimulated, are yet capable of contracting the muſcles, although the creature can in no way move them of itſelf.

We muſt diſtinguiſh, then, the voluntary motions of the animal, from thoſe that are excited by an external ſtimulus acting on the nerves, the ſpinal marrow, and brain. The latter are not always wanted, when the former no longer exiſt; but in all caſes where the latter are not obſerved, the former are infallibly wanted.

There is another thing to be diſtinguiſhed in ſpeaking of the nerves, and of motion; this is the ſenſation, of which the nerves are the only organ in animals. In the courſe of my experiments I have frequently obſerved, that when the animal could no longer move any of its parts, if I ſtimulated its nerves with needles, or applied pincers to them, it gave very certain ſymptoms of ſenſation. It is beſides equally true, that the muſcles frequently contract on ſtimulating the nerves, notwithſtanding the creature has been a long time dead. So that the motion of the heart, and the power the nerves poſſeſs of contracting the muſcles when ſtimulated, decay much later than the voluntary motions and ſenſations.

I have likewiſe obſerved, that opium, on being applied immediately to a nerve, not only does not deprive it of the faculty of contracting the muſcles, but likewiſe does not deſtroy its natural ſenſibility;

and it has been ſeen, that its effects are quicker when animals have been made to ſwallow it without their heart having been taken out, than when they have been deprived of that muſcle. It ſeems to follow from all theſe particulars, that opium does not act immediately on the nerves, but has need of the circulation of the blood and humours, in exerciſing itſelf on animals.

I here conclude the principal conſequences of my reſearches into opium, and I wiſh I had been able to have entered into a circumſtantial detail of the experiments, in the way I have made them. I do not now regard the ſubject as exhauſted, and am as far from ſuch a belief, as I am from thinking that there is nothing to correct in my preſent work, and that nothing can be added to it. This very Supplement itſelf demonſtrates the contrary, and if I could in any way retard the publication any longer, I might probably be myſelf able to add many new particulars, to ſee many others in a clearer light, and perhaps to correct ſeveral of them. I ſhall therefore lend a willing ear to criticiſms, and to the objections that may be made againſt my work, and ſhall take a real pleaſure in correcting and rendering it perfect for a new edition, ſhould ſuch an one ever become neceſſary. But I proteſt at the ſame time, that I will anſwer none of thoſe pretended philoſophers, who oppoſe words to facts, ſophiſms and cavils to experiments, poſſibilities to obſervations, and prejudices and ſcholaſtick errours to natural, direct, and luminous, conſequences. Thus

I ſhall

I ſhall not think myſelf obliged to repeat my experiments, already repeated ſo many times, and to fancy myſelf in an errour, on account of a few iſolated experiments which ſome one may wiſh to oppoſe to me, ſimply becauſe they are not exactly comformable to mine. A ſimple glance thrown on my work itſelf, will ſhow how eaſy it is to be deceived in matters of experiment, even when many of them have already been uniform, and when one would the leaſt expect the poſſibility of being led aſtray. My experiments, on the ſide of truth, exceed the number of ſix thouſand, and the obſervations I have interſperſed through the work are at leaſt as numerous. I know very well that the queſtions I have propoſed and examined are likewiſe very numerous, and that there may be ſome few in the number, as I have obſerved on a former occaſion, that have not been diſcuſſed with as many experiments as were neceſſary. But in ſpite of all this, I firmly maintain, that a few experiments will not be ſufficient to deſtroy the great number I have made, and varied in ſo many ways, and that like contradictions will not be capable of making me change my manner of thinking.

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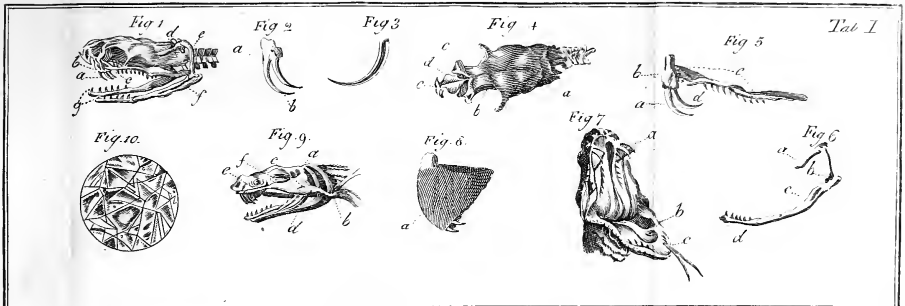
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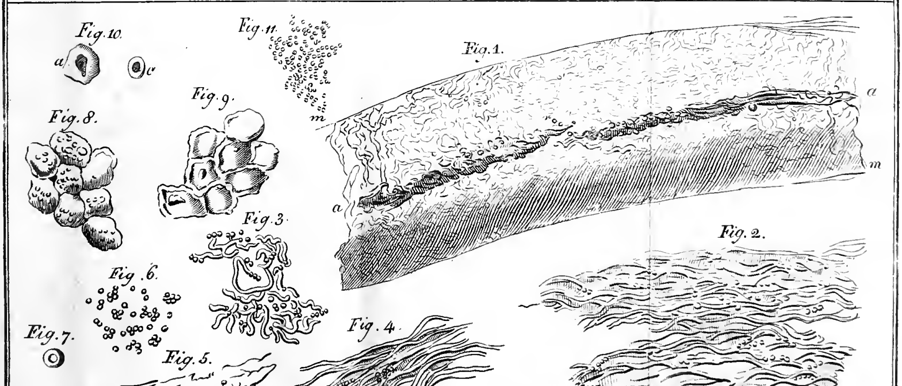
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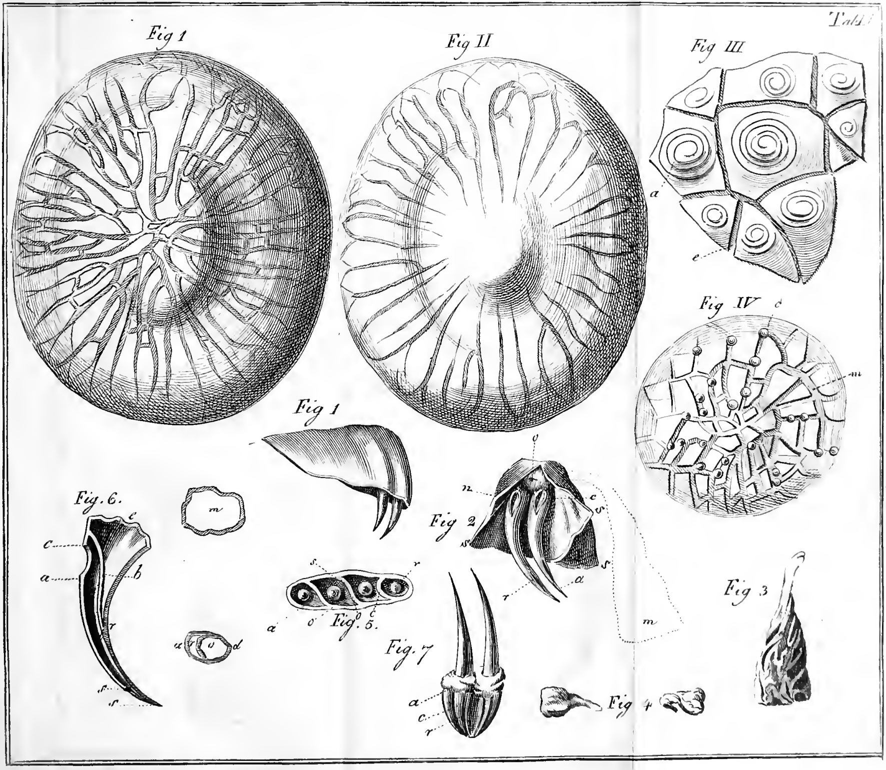

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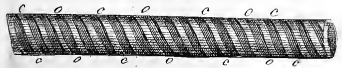

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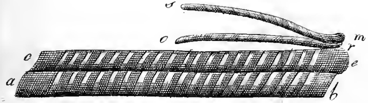

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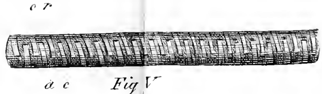

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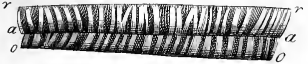

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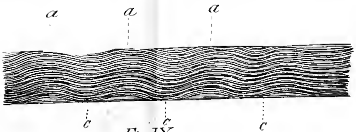

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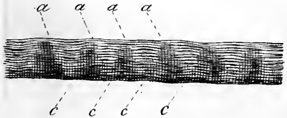

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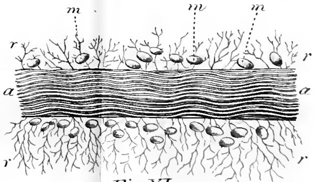

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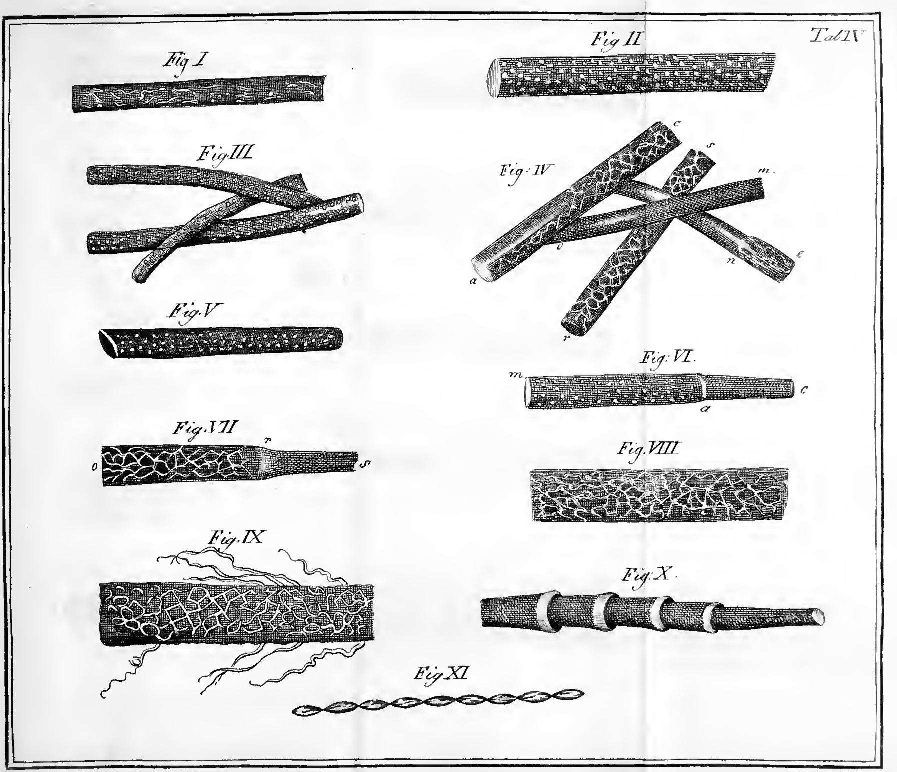
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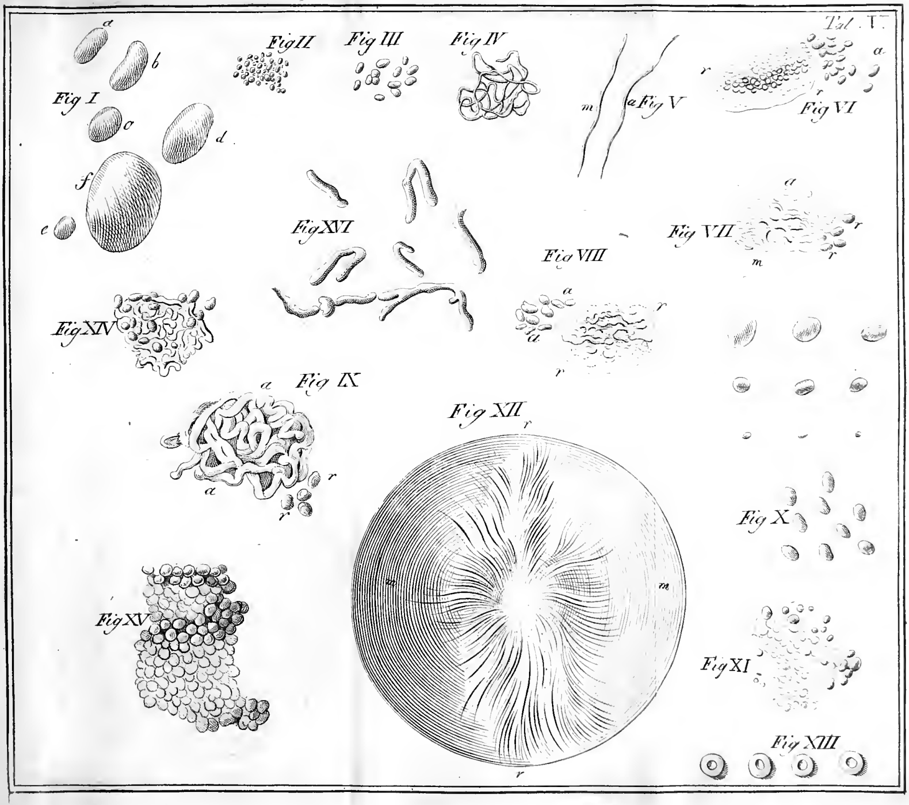
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Fig. I

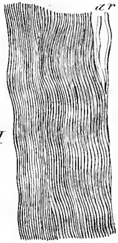

Fig. III

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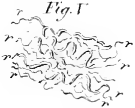

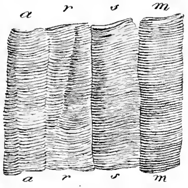

Fig. VI

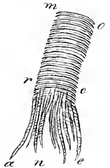

Fig. VII

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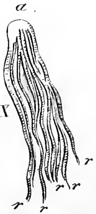

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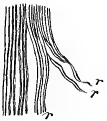

Fig. X

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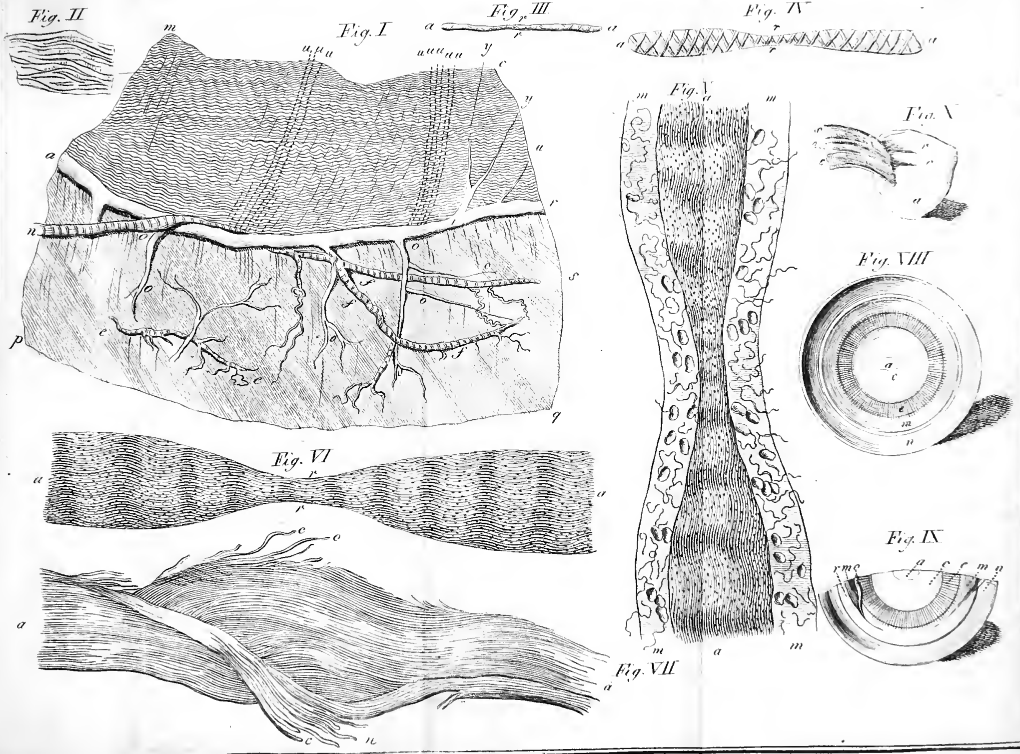
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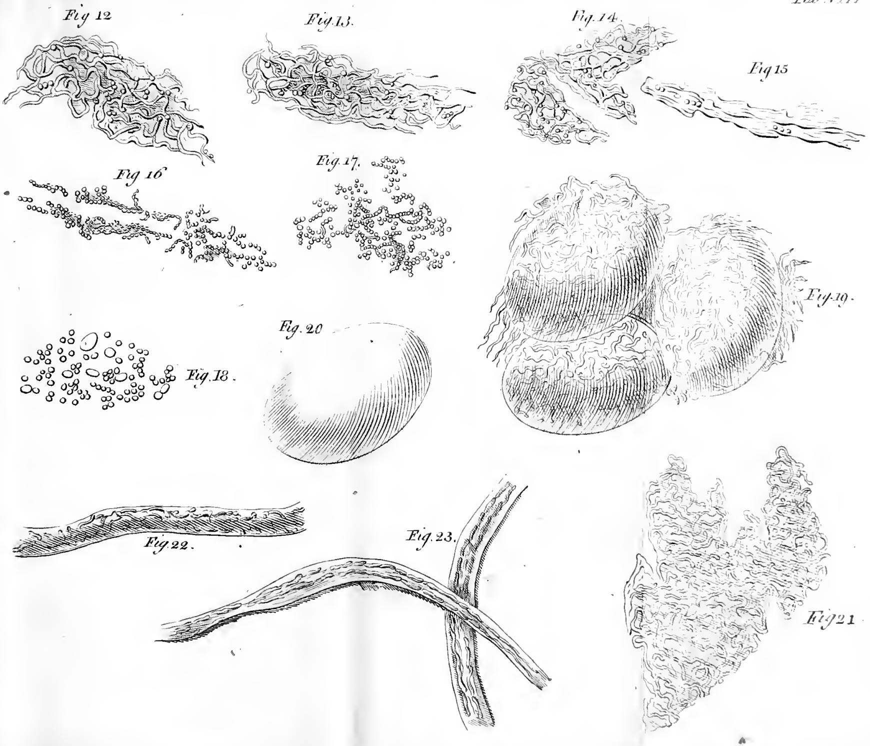
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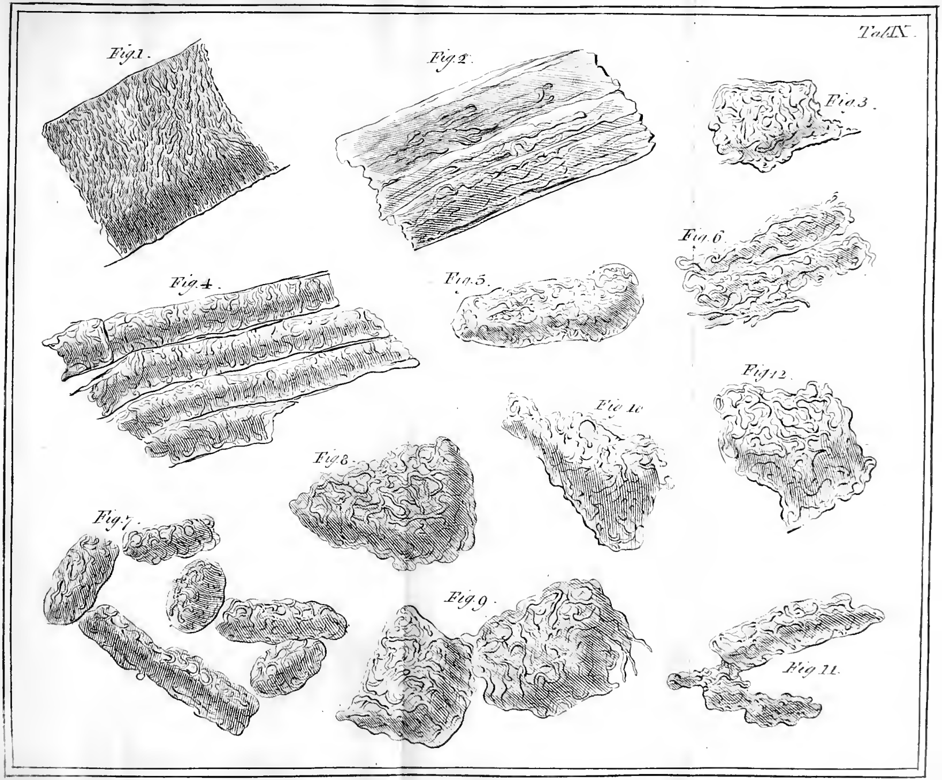
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Tab. X

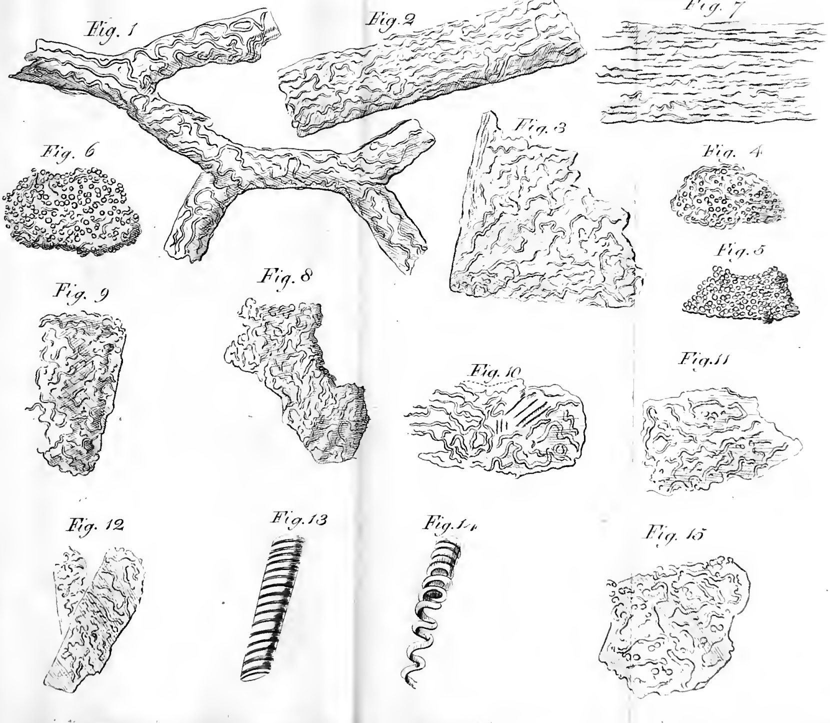
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Fig. 15

EXPLANATION

OF THE

PLATES.

Explanation of the first Ten Figures of Plate the First. Taken from Mead's Work on Poisons.

FIG. 1. offers the lateral view of the cranium and jaws of a viper. *a,* reprefents two venomous teeth on each fide, fixed in a folid bone, by the mechanifm that will be defcribed hereafter. Thefe folid bones at *b,* are articulated by ginglymus, as if they adhered to each of the proceffus zygomatici. By means of this articulation they have two motions; by the firft the teeth fhow themfelves, and prepare to bite; by the fecond they retire, and are carried within, bending themfelves towards the root of the tongue, fo as to approach the two jaws.

In Fig. 5. thefe teeth are feen on a larger fcale.

Thefe motions are produced by a gentle depreffion of the bone *c,* and in Fig. 5. *d,* which, faftened

5 to

to the bone *b*, above its articulation, forces it to unite with it, and to aſſiſt in theſe motions, by which it is either driven without or carried within; they are communicated to it, both by its connection with the lower jaw, and by virtue of the muſcles which belong to it, and which are deſtined to this purpoſe.

Fig. 1. *f*, ſhows the lower jaw, and *e*, *d*, the two *axes* or points of ſupport, by the means of which it performs the motions neceſſary to the devouring of its prey.

In Fig. 6. theſe two *axes*, or ſupporters, are ſeen at *a*, and *b*, as they ſerve to connect the lower jaw with the ſinciput and temporal bones.

To fully comprehend the mechaniſm the viper employs in ſwallowing its prey, it muſt be obſerved that the jaws, both upper and lower, *on the ſame ſide*, are capable of moving at the time the oppoſite ones remain fixed and motionleſs; ſo that both jaws on one ſide may be carried without, or brought within, whilſt thoſe on the oppoſite ſide either have contrary motions, or continue fixed and immoveable. Now theſe jaws are provided with ſmall teeth, Fig. 1. *g*, and Fig. 5. *c*, which adhere very ſtrongly to their ſuperficies, and which, from the purpoſe to which they are applied, may be called hooks. It is in conſequence of theſe alternate motions of retraction, that the prey is driven into the ſtomach.

The number of theſe hooks is more conſiderable in the upper than in the lower jaw.

Fig. 5. *c*, repreſents theſe teeth, or hooks, of the upper jaw; Fig. 6. *d*, thoſe of the lower.

Fig.

Fig. 4. ſhows the upper part of the head. The ſinciput at *a*, which in men is formed by the conjunction of the two parietal bones, is here formed of a ſingle one; whilſt the os frontis *b*, which is a ſingle bone in men, is formed in this animal of two bones united by a ſuture.

c, the anteriour entry of the orbit of the eye, hollowed in the frontal bone.

d, the bones of the noſe.

e, the maxillary bone, which in this animal is entire.

But before we leave the bones of this part, I muſt not forget to obſerve, that the venomous teeth differ from the others, not only on account of their ſize and motion, but in having very diſtinct qualities. In the firſt place, it muſt be remarked, that although two of them are found at each ſide, they are very rarely faſtened with equal ſtrength to the ſockets that contain them. Sometimes the exteriour tooth on each ſide is the looſeſt, and at other times this is the caſe with the interiour ones. At others again, the internal on one ſide, and external on the other, are the looſeſt. When the teeth jut out or project, that which is the beſt faſtened riſes more than the looſer one, which appears longer.

By weighing all theſe circumſtances, and ſome others that remain to be mentioned, we ſee that the viper, in biting, only employs one of theſe teeth on each ſide. Nature has made this diſpoſition, to the end that the action of one alone may ſuffice to convey into the animal the viper preys upon, all the venom prepared on one ſide, and which is ſhed with as much efficacy to the reptile itſelf, as if both teeth had acted equally.

The tooth of the viper, in wounding, deſcribes a ſegment of a circle, and this crooked ſhape, which makes it reſemble in a degree the claws of a bird of prey, Fig. 1. *a*, and Fig. 5. *a*, gives it a much greater ſtrength than it would otherwiſe have. But this ſhape prevents the tooth from diſengaging itſelf, whence it ſometimes happens that the animal preyed upon, in making efforts for its deliverance, plucks it out, particularly as the viper, perceiving itſelf dragged by a variety of motions, applies its tail to the earth, till it finds itſelf well fixed. If by this means it cannot preſerve its tooth, the latter is broke in the weakeſt part of its articulation. Nature, to remedy this evil, has contrived that the tooth which was before looſe, ſuddenly acquires a great degree of firmneſs, and that in the place of the one which fell, another, which is detached at pleaſure, immediately ſucceeds: a tooth broken or plucked out, is ſoon ſupplied by the ſmall rudiments of young teeth that are concealed in the ſocket betwixt the roots of the venomous teeth, and which, by degrees, at length become perfect.

In the rattle-ſnake I have remarked ſix teeth of this kind, growing on the ſame ſide. I ſhall not hazard any conjectures as to the cauſe of theſe auxiliary teeth falling into the empty ſocket; but from all that has been hitherto ſaid, there is great reaſon to believe that their true uſe has been aſſigned

to

to them, ſince the preſervation of theſe animals renders ſuch a ſupply indiſpenſably neceſſary.

The venomous teeth are hollow, from where they are fixed in the ſocket to the point. This hollow begins at the top of the orifice placed at the anteriour part of the tooth, Fig. 2. *a*, and ends at ſome diſtance, near the ſummit *b*. The reſt of the tooth is firm and ſolid, and cut like a tooth-pick.

Fig. 3. ſhows the hollow of this tooth cut in the middle.

The venom is prepared and ſupplied by a gland ſituated at each ſide of the cheeks. It is attached by a ligament (*a*, Fig. 9.) to the ſinciput, where it proceeds to faſten itſelf to the occiput and lower jaw, by means of another ligament *b*. A ſtrong white membrane which goes out of theſe ligaments, ſerves to bind it ſtill faſter, and to ſecure it from too great a compreſſion, to which it would otherwiſe be ſubject, either by too great an accumulation of the venomous fluid, or by too violent a preſſure made by the animal to expel this fluid. The excretory conduit *e*, is formed by a continuation of this ſame tunick. This conduit is carried from the gland to the cavity of the tooth, by means of the bag or ſheath (Fig. 7. 8.) which encloſes the teeth on each ſide.

f, Fig. 9. is a ſmall white gland, the proximity of which to the teeth cauſed is to be taken, for the ſecretory organ of the venom, though it ſeems to be nothing more than a lymphatick or ſalivary gland, and to be abſolutely wanted in the rattle-ſnake. All the

the muſcles that concur towards the act of biting, are ſituated in the viper in ſuch a way, that when they act they made a ſtrong compreſſion on the gland that contains the venom, and thus aſſiſt the ejaculation of the latter.

That which of all of them contributes the moſt to this ejaculation, is the muſcle *d*, Fig. 9. After riſing out of the lower jaw, it extends obliquely below the gland that contains the venom, till having paſſed betwixt the two ligaments, *a* and *b*, it is returned back upon the outer ſurface of the gland, and is ſtrongly faſtened to it in a direction parallel to its length, by the ligament *a*, which ſerves it as a tendon. This muſcle may likewiſe aſſiſt in cloſing the jaws. Its greateſt action however conſiſts in making a ſtrong compreſſion on the venomous gland it ſo exactly ſurrounds; and this compreſſion is made nearly in the way we employ in expreſſing the juice from an orange. The diſpoſition of this muſcle, (which extends over the whole ſurface of the gland, and takes the ſame direction as the excretory conduit) and the very end of this conduit (which appears tendinous, and terminates at the root of the teeth) gave riſe to the opinion of this muſcle ſerving for the retraction of the latter. But it is eaſy to be convinced of the contrary, by macerating a viper's head, the ſkin of which has been removed, in warm water, when the muſcle ſeparates eaſily, and ſhows the naked gland.

Fig. 7. preſents an entire viper's head. At *a* are ſeen the two venomous teeth on each ſide, encloſed

their proper ſheath. The different degrees of erection and diſtenſion are readily obſerved.

b ſhows the entrance of the trachea placed in ſuch a way, that it is as little as poſſible expoſed to compreſſion at the time of deglutition.

c ſhows the tongue of the viper, which ſerves it in ſucking up the dew. It is this perhaps that places the auxiliary teeth in the empty ſockets, as exigencies may require.

Fig. 8. ſhows the bag or ſheath that encloſes the two teeth. It is drawn large, that its fringed apertures *a*, may be the readier diſtinguiſhed.

Continuation of the Explanation of Plate the Firſt.

Fig. 1. *m m.* is a bit of hair, in which are ſeen, in the middle and internally, ſeveral brown ſpots; its whole ſurface ſeems to be covered with ſmall winding cylinders, in ſome meaſure parallel to each other.

Fig. 2. repreſents a ſmall portion of the ſame hair, which had been ſtrongly compreſſed on a glaſs port-object, with an iron plate.

Fig. 3. is a fragment of the above figure, in which very ſmall globules, detached from the winding cylinders, are diſtinguiſhed.

Fig. 4. is another fragment of Fig. 2. which, having been ſteeped in water, takes the appearance of a tranſparent and irregular pellicle as it appears in Fig. 5.

Fig. 6. ſhows the globules of tranſpiration.

Fig. 7. is a globule of blood, obſerved with the ſame glaſs that was employed in the obſervation of Fig. 6.

Fig. 8. is a maſs of the globules which form the gluten of the ſkin of eels. They appear like ſo many bladders filled with infinitely ſmall globules.

Fig. 9. is the ſame maſs of globules as in the preceding figure, but which have been a little dried. A ſmall body is ſeen interiourly, ſituated in each globule in a different part.

Fig. 10. repreſents one of theſe globules of Fig. 9. which contained in its middle part a ſmall body likewiſe ſpotted in the middle. *c.* which is one of the globules of blood, has been placed at the ſide of it, that their reſpective ſizes may be compared.

PLATE II.

Explanation of the Drawings marked with ſmall Figures.

Fig. 1. repreſents the two canine teeth of the viper.

Fig. 2. ſhows the bag or ſheath that covers them. *s s.* are the edges of this bag or ſheath which has been divided with ſciſſars. *n e.* are the two elliptical holes that are found at the baſis of the tooth. *r a.* are two equally elliptical clefts, which are ſituated

ated almoſt at the point of the ſame tooth. *m.* is the receptacle of venom: this receptacle opens in its upper part in a conduit, and this conduit proceeds to *o.* where it opens at the part in which the two teeth are faſtened in their ſockets.

Fig. 3. ſhows the ſame receptacle of venom, obſerved with a lens; its ſhape appears to be nearly triangular.

Fig. 4. is this ſame receptacle in its natural ſize.

Fig. 5. is a tranſverſe ſection of the above receptacle, which appears to be formed internally of ſeveral cavities filled with venom, and ſeparated by partitions, *s o c.*; the venom, as is indicated in the figure, flows out by drops.

Fig. 6. repreſents one of the canine teeth with all its cavities and apertures. *s s.* ſhows the elliptical cleft near the point of the tooth; and *c a.* the hole found at the baſis. *i i i.* are a cavity of the tooth, cloſed at *r*, and only open at *e*. The tranſverſe ſection of it ſeen at the ſide of the figure, and marked *m.* and the other ſmall drawing at the ſide of the figure, *a r o d.* expreſs another ſection of the ſame tooth, made in a direction with *a b.* of the figure itſelf.

Fig. 7. repreſents the gum in which the two canine teeth are fixed, and at their baſis are ſeen ſix ſmall teeth, which are not yet altogether formed, and which are deſtined to replace the canine teeth, when the viper chances to loſe them. *a c r.* are three of theſe ſmall teeth ſituated at the left ſide.

Explanation of the Drawings of this Plate marked with Numerals.

Fig. II. reprefents a drop of the venom of the viper, fuch as it appears when it begins to dry a little on the microfcope.

Fig. I. reprefents the fame drop when entirely dry.

Fig. III. is a heap of feveral fragments of dried venom. The letter *a.* fhows a fingular cleft turned in a fpiral form. The letter *c.* fhows one of thefe clefts, which feparates the fragments from each other.

Fig. IV. fhows a drop of venom taken from the mouth of the viper, and left to dry on a bit of glafs. At *o.* the fmall globules and knots are feen, which are nothing more than fmall air-bubbles. Letter *m.* fhows one of the clefts that feparates the fragments.

Explanation of Plate the Third.

Fig. I. reprefents a nerve feen with a lens that magnified fix times. The letters *c c c.* fhow the white bands, which are of the fame fize, and fituated at equal diftances. The letters *o o, o o, o o,* are the intervals, the colour of which is not fo pale; they are likewife of the fame fize, and equidiftant.

Fig.

Fig. III. is a nerve enlarged about eight times by a microſcope. Its bands are not ſo regular as thoſe of the former figure, and in ſeveral places ſeem to croſs each other.

Fig. II. is another nerve, the bands of which are more diſtinct, and approach each other in different parts with a degree of irregularity, but without croſſing.

Fig. VI. repreſents a nerve, ſeveral of the bands of which approach, and others croſs each other.

Fig. VII. is a nerve in which ſeveral of the bands croſs each other at different angles.

Fig. V. ſhows a double range of bands in a nerve examined with a lens that magnified ſix times; the bands of the two ranges, *a r*, *o c*, are equally large, and are all ſituated at equal diſtances: they enter into each other, as the band *o*. is ſeen to enter into the band *a*. and the band *c*. into the band *r*.

The nerve of Fig. VIII. is compoſed of two nerves: *r a*, *r a*, ſhow one of theſe nerves; and *a o*, *a o*, the other. The line *a a*. ſhows the union of the two nerves.

Fig. IV. ſhows a nerve ſeparated into four, *a b*, *c e*, *o r*, *s m*. In neither of theſe the bands either croſs each other, or meet.

Fig. XI. repreſents a nerve enlarged by a very ſtrong lens, and covered with its cellular membrane. *a a*. are the two extremities of the nerve. *m m*. are the oviform globules that are obſerved in the cellular membrane. *r r r r.* are the filaments of this membrane, floating in water.

The figures IX. X. represent the way in which these bands appear and disappear, as they are observed with a greater or less degree of light, and with lens of different strengths. *c c c c,* are the white bands of the nerve of figure X. and *a a a a.* the dark spots. In turning the reflecting glass of the microscope the bands disappear, and the winding fibres of Fig. IX. are seen in their place. The bands *c c c.* of Fig. X. become the convex winding fibres, *c c c.* of Fig. IX.; and the opake intervals *a a a a.* of Fig. X. take the appearance of the concave fibres *a a a a.* of Fig. IX.

Explanation of Plate the Fourth.

Fig. I. represents a primitive nervous cylinder which seems to have here and there on its coats several fragments of winding threads, and some round corpuscles in its interiour part.

Fig. II. represents another cylinder, which seems filled with very small globular corpuscles, immersed in a gelatinous transparent humour.

Fig. III. represents three primitive nervous cylinders.

Fig. IV. represents a heap of primitive nervous cylinders. *o m.* is one of these cylinders entirely stripped of its external membrane. The cylinder represented by *n c.* is bare, except its extremity *n e.* which

which is covered. The outer cylinder *a c.* is almoſt entirely covered with its membrane. The cylinder *r s.* is perfectly covered with its rugged membrane.

Fig. V. repreſents another of theſe primitive nervous cylinders.

Fig. VI. repreſents a primitive nervous cylinder, one half of which, *a c.* is formed of a tranſparent and uniform thread; and the other half, *m a.* is almoſt twice as large, leſs tranſparent, and irregular and rugged.

Fig. VII. repreſents a primitive nervous cylinder, in which *o r.* the thickeſt part, is covered with a cellular network, formed of very delicate filaments. The part *r s.* is ſtripped of this cellular net-work.

Fig. VIII. repreſents a primitive nervous cylinder covered with its exteriour coat. This is compoſed, as it appears to be, of very ſmall winding threads, which proceed for the length of the cylinder itſelf.

Fig. IX. repreſents a primitive nervous cylinder, covered with its external coat.

Figures X. and XI. repreſent two ſingular canals found in the ſubſtance of the brain.

PLATE V.

Fig. I. repreſents ſeveral oviform bodies, found in the cellular covering of nerves.

Fig. II. reprefents very fmall corpufcles found in the medullary fubftance of the nerves.

Fig. III. fhows the apparent fize of the globules of the blood of a rabbit, obferved with the fame glafs that was afterwards employed in Fig. IX.

Fig. IV. reprefents feveral winding cylinders of the cellular membrane of fat.

Fig. V. reprefents two threads, *m*, *a*, placed one at the fide of the other, that their relative fizes may be the better feen. The thread *m*. belongs to the cellular membrane of fat, and the thread *a*. to the external cellular membrane of a nerve. They are both perfectly cylindrical, and pretty equal in fize.

Fig. VI. *r r*. is a lamina cut from the cortical fubftance of the brain, and obferved with a very ftrong lens. *r a*. are fmall round corpufcles, which appear to be filled with a gelatinous humour.

Fig. VII. *m a*. fhows the windings and inteftiniform circumvolutions that are feen in the cortical fubftance of the brain. *r r*. are the above corpufcles.

Fig. VIII. *r r*. reprefents a thin lamina of the medullary fubftance of the brain, which, obferved with a microfcope, feems to be formed of an heap of inteftines *r r*.; at their fide, at *a a*. are feen feveral corpufcles detached from the cortical fubftance.

Fig. IX. reprefents a part of the retina where it is not radiated. It feems to be compofed of a very fine cellular membrane, interfperfed with fmall globules, *r r*.

Fig.

Fig. X. ſhows theſe globules of the retina of their apparent ſize, relatively to that of the globules of blood of Fig. XIII.

Fig. XI. is another ſmall portion of the retina, with the globules, and cellular membrane.

Fig. XII. repreſents the hollow of the eye, or the internal ſtructure of the retina, of a rabbit. At *r r.* are ſeen the nervous rays, which proceed from the centre, and go on two oppoſite ſides as far as the edges. The two oppoſite parts of the retina, *m m.* do not extend ſo far, and are not furniſhed with ſuch conſiderable rays. Theſe rays, or nervous fibrils, ſeem to be cut by knots or diaphragms placed at very ſmall diſtances. The engraver has not been ſo happy in the execution of this figure as in the others. It is not poſſible to meet with an engraver who can expreſs with the graving tool, all theſe little indeterminate ſtrokes, which characterize the truth of the object, and which are not neglected by the perſon who at once delineates the object, and obſerves it with the microſcope.

Fig. XIII. ſhows globules of blood obſerved with the lens employed in viewing the retina above. This is done that a compariſon may be formed of their relative ſizes.

Fig. XIV. repreſents a portion of the cellular net-work of the retina, which is ſimply a web of winding veſſels, to which the globules are attached.

Fig. XV. repreſents a ſhred of the retina, after it has been a little macerated. Several of the globules that compoſe it appear to be detached; the

print

print of them however remains, and the hollow in which they were ſunk.

Fig. XVI. ſhows ſeveral irregular bodies that were detached with the point of a needle from the medullary ſubſtance of Fig. IX.

PLATE VI.

Fig. I. repreſents a tendon magnified only ſix times.

Fig. II. repreſents another tendon, likewiſe obſerved with a very weak lens.

Fig. III. repreſents a primitive tendinous faſcia, which appears to be formed of ſeveral primitive and parallel tendinous threads. *a r.* are two of theſe threads detached from the others.

Fig. IV. repreſents another tendinous faſcia ſtripped of its cellular membrane, and compoſed of primitive threads, *r r r.*

Fig. V. is a ſmall portion of the cellular membrane of a tendinous faſcia, which appears to be formed of ſeveral winding cylinders. *r r r r.* are the ends of theſe cylinders.

Fig. VI. repreſents four primitive fleſhy faſciæ in contact with each other, and covered with their cellular membrane. The two marked *m m. s s.* have their ſmall furrows in a circular form ; in the other two, *a a. r r.* they are not ſo regular.

Fig.

Fig. VII. reprefents a primitive flefhy fafcia, partly covered with its cellular membrane.

a, *e*, are the primitive flefhy threads, feparated and bare.

Fig. VII. reprefents a primitive flefhy fafcia covered with its membrane.

Fig. IX. is the fame fafcia deprived of its membrane. Its threads are united at *a*, and are difperfed at the other end at *r*, *r*, *r*.

Fig. X. reprefents a fmall portion of the cellular membrane of the mufcles, formed of winding threads *r*, *r*, *m*, *m*.

PLATE VII.

Fig. I. reprefents a portion of the diaphragm of a rabbit. The letters *a*, *p*, *q*, *r*, fhow the flefhy part. *a*, *m*, *c*, *r*, the tendinous part. *n*, is the trunk of the nerve that goes to the diaphragm. *a*, *r*, are a vein. *f*, *f*, are branches of the nerve *n*. *a*, *r*, *y*, *y*, are branches of the vein. and *u*, *u*, *u*, *u*, are almoft imperceptible ramification of the vein *a*, *r*.

Fig. II. reprefents a very fmall portion of the tendinous part of the diaphragm, obferved with a very ftrong lens.

Fig. III. reprefents a nerve of the eighth pair of a rabbit, which had been divided twenty nine days before. It is drawn about twice as large as it naturally is. The letters *r*, *r*, fhow the part where it was reproduced.

Fig.

Fig. IV. is a repetition of Fig. III. magnified in a greater degree, that the ſpiral bands may be better ſeen.

The letters *n*, *n*, *n*, *n*, of theſe two figures, indicate a part of the reproduced nerve, where is ſeen a white annular ſpot.

Fig. V. repreſents the ſame nerve ſeen with a very ſtrong lens. *a*, *a*, is the body of the nerve. *m*, *m*, *m*, *m*, the cellular membrane that covers its ſides.

Fig. VI. is the ſame nerve ſtripped of its covering. The progreſs of the primitive nervous cylinders is ſeen in it, and the point of reunion or reproduction, where the diameter of the nerve, as well as that of the fibres, diminiſhes conſiderably, is marked *r*, *r*.

Fig. VII. is the ſame nerve, but a good deal torn with a needle, particularly at the part where it was reproduced, to determine the continuity of the primitive nervous cylinders. *a*, *a*, are the two ends of this nerve. *c*, *n*, *c*, *n*, ſeveral of the primitive cylinders that were torn.

Fig. VIII. repreſents the anteriour half of the bulb of the ox's eye, obſerved at the concave ſide.

The letter *n*, points out the tunica ſclerotis. *m*, the ciliary body, likewiſe called ligamentum ciliare. *e*, the proceſſus ciliares. *c*, the uvea, and *a*, the pupil.

Fig. IX. is the half of Fig. VIII. in which the ciliary body, as well as the new canal *o*, is partly detached from the reſt of the figure. *r*, is the tunica

nica ſclerotis ſtripped of the tunica choroides. *c*, the ſmall hollow in which the ciliary body, or ligament, is attached to the tranſparent cornea. The letters, *e*, *o*, *s*, indicate a membranous ſubſtance formed by the meeting of the tunica choroides *e*, the ciliary ligament *o*, and the uvea *s*. The letter *e*, ſhows the part of the ligament that is faſtened in the hollow *c*.

PLATE VIII.

Fig. 12. repreſents a very fine lamina of the epidermis. Very ſmall globules are obſerved in different parts of it.

Fig. 13. repreſents another lamina of the epidermis, covered with water. It is not different from the firſt.

Fig. 14. repreſents a ſmall bit of human nail obſerved dry.

Fig. 15. ſhows the ſame portion of nail, but ſteeped in water.

Fig. 16. ſhows the appearance of a ſmall hollow made with a pin in a lamina of talc; both edges are interſperſed with winding threads and globules.

Fig. 17. repreſents a pinch of hair-powder, or powdered ſtarch, wetted, and afterwards obſerved with the microſcope.

Fig. 18. ſhows the appearance of the above powder obſerved dry.

Fig.

Fig. 19. reprefents the veficles of fat as they are feen betwixt the laminæ of the adipofe membrane; they are filled either with an oily or fatty humour, according to the animals to which they belong, and are covered with winding cylinders.

Fig. 20. fhows one of the above veficles deprived of its winding cylinders.

Fig. 21. reprefents a lamina, or rather a fcraping, of ivory.

Fig. 22. reprefents a piece of cotton thread obferved dry.

Fig. 23. fhows the appearance of the fame thread, fteeped in water.

PLATE IX.

Fig. 1. reprefents a cylinder of very pure gold, obferved breadthwife.

Fig. 2. is the fame cylinder, obferved longitudinally.

Fig. 3. is a fmall bit of gold-leaf.

Fig. 4. fhows four atoms of very pure filver filings.

Fig. 6. fhows an atom of zinc, obferved dry.

Fig. 7. reprefents feveral grains of calcined magnefia, the furface of which is covered with winding cylinders.

Fig. 8. fhows an atom of bifmuth, obferved dry.

Fig. 9. reprefents two atoms of white marble.

Fig.

Fig. 10. is an atom of heavy ſpar.

Fig. 11. is a fragment of phoſphorick ſpar.

Fig. 12. is an atom of nikel.

PLATE X.

Fig. 1. repreſents one of the branches, or branched cylinders, of which ſponges are formed.

Fig. 2. repreſents a very ſmall fragment of elaſtick reſin.

Fig. 3. is a grain of common ſalt.

Fig. 4. is an atom of ſilver, in which the winding threads are ſeen interſperſed here and there as uſual.

Fig. 5. ſhows another ſmall bit of ſilver, in which, inſtead of the winding threads, ſmall ſhining grains are obſerved.

Fig. 6. is likewiſe another ſmall bit of ſilver, which appears to be formed of points and pyramids.

Fig. 7. repreſents a ſmall bit of tin, likewiſe furniſhed with the uſual winding threads.

Fig. 8. is a bit of antimony.

Fig. 9. is an atom of cobalt.

Fig. 10. is a very fine lamina of lead, covered with the uſual winding threads.

Fig. 11. is a bit of copper, which, like the other bodies, ſhows on its ſurface the winding threads.

Fig. 12. is a ſmall fragment of roſe-leaf, partly torn with the point of a knife.

Fig. 13. ſhows a pipe, formed by a ſpiral band, found in the ſtalks of the leaves of the hedyſarum movens.

Fig. 14. is the ſame pipe, partly ſtripped at its inferiour end.

Fig. 15. is a fragment of amber, which like all other bodies, appears to be covered with winding cylinders.