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Contributors

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DISSERTATION

ON THE

Sensible and Irritable Parts

O F

ANIMALS.

By M. A. HALLER, M. D.

President of the Royal Society of Sciences at
Gottingen: Member of the Royal Academy of
Sciences at Paris: &c.

Translated from the LATIN.

With a PREFACE by M. TISSOT, M. D.

LONDON,

Printed for J. NOURSE at the *Lamb* opposite
Katherine-street in the *Strand*.

MDCCLV.

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LONDON,

Printed for J. Murray at the Lane, opposite
Kew's Head in the Strand.

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DR. TISSOT'S
P R E F A C E

TO THE

Lausanne Edition of this Treatise.

AS long as philosophy was confined to hypotheses founded only upon imaginary facts, this science might be considered as a downright Proteus, putting on new forms almost every day, because one fanciful opinion has always a right to supplant another. Hence it happened, that nature remained absolutely unknown, and the best philosopher was only a man of a tenacious memory, who had stuffed it with chimeras of all those who went before him, and either gave the preference to some particular one, or rejected them altogether to make room for his own. During the last century, some happy geniuses, the chief of whom we may reckon the Lord Chancellor BACON, discovered the fallacy of this sort of philosophy. They found it absolutely necessary

cessary to reject all that chimerical nonsense
 which passed by the name of philosophy;
 and they were sensible at the same time, that
 this science was not to be created, but studied.
 That they must observe the phænomena, which
 is natural history, and experimental philoso-
 phy; and enquire into the causes of them;
 which is rational philosophy, and, properly
 understood, is no more than observation, but
 very accurate and extended. This compre-
 hends at once a great number of phænomena,
 observes what they have in common, and how
 they are connected; and not contented with
 the phænomena themselves, it endeavours to
 penetrate into the causes of them, to unravel the
 properties of the matter which produces them,
 to discover the principal ones which serve as
 causes to a great number of others, and which
 may properly enough be called *the keys of na-
 ture*; for the knowledge of them actually
 solves a number of facts, of which we did not
 discover the reason before; and a fact, of which
 we are acquainted with the cause, is a great
 deal more interesting and useful than another.
 It appears very obvious, that this rational phi-
 losophy cannot make such rapid progress as na-
 tural history; in the mean while it has ad-
 vanced from time to time. The properties of
 the air, the circulation of the blood, and elec-
 tricity,

tricity, which were discovered in less than the space of a century, have thrown more light upon philosophy than it had received for two thousand years before, and they have successfully attracted the attention of all the philosophers in Europe. The great discovery of the present age is IRRITABILITY, described in the following treatise, which I shall not pretend to say any thing in praise of, seeing its celebrated author, for these twenty years past, has favoured the public with so many excellent performances, and now this subject is become the principal topic of all those who devote themselves to the important study of the animal œconomy.

This discovery has met with opposition from different motives. Indolence, to avoid the trouble of an examination; vanity, to shun the imputation of ignorance; and envy, to deprive the discoverer of his due praise, have denied its existence; and after it was confirmed by so great a number of facts, that it was impossible for the strongest prepossession to call the truth of it in question, they have attempted to prove that it was discovered before, under other properties which have been known a long time. But this last resort was very soon given up. *Irritability* is a property entirely different from all those which were known before in the

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body;

dy * ; and being essential to all animals, perhaps likewise to all plants, will henceforth justly be reckoned amongst the principal qualities of organized bodies.

It must appear very surprising, and at the same time not a little mortifying to mankind, that a property which, as Dr. ZIMMERMAN says, constitutes perhaps the very basis of life, and which chance must have made us sensible of a thousand times, should have escaped the eyes of all who imagined themselves to be observers, and some of whom were actually such. Perhaps it might not be impossible to assign the reason of this phenomenon, if the time for it was arrived; but it is sufficient to say, that it resembles several others of the same kind. Attraction, the weight and elasticity of the air, shewed themselves to the senses every day; but it required a TORICELLI and a NEWTON to illustrate them. Why may it not be the same case with Irritability? and may it not be justly said, that discoveries of this nature do so much the more honour to him that makes them, that he lives in a more enlightened age? When we are unacquainted with a thing we examine it closely, and in an ob-

* Vis ab omni aliâ hætenus cognitâ proprietate corporum diversa & nova est: neque enim a pondere, neque ab attractione, neque ab elatere pendet. *Prim. Linæ Physiol.* § 408.

ject which no body has examined before, we expect to meet with something new; but in an age like the present, and upon a subject so much searched into as the human body, we do not flatter ourselves with discovering essential properties; all that we can naturally hope for, is only to push farther those discoveries already made, which require no more than dexterity and patience to bring them to perfection. Wherefore to discover such a property as Irritability, and to examine it after it is discovered, requires a penetrating accurate genius, a lively sense of the want of such a discovery, and a sagacity of *smelling it out beforehand* so to speak. Such a person ought to know perfectly all that was known before, and represent it to himself in the strongest light, in order to be convinced that he does not see falsely, or observe what others have taken notice of before, but have not judged worthy of their consideration. He ought to have a particular turn to discovering the truth, and a strong propensity to lay hold of it, not to suffer this first glimpse to escape him, which to common eyes would appear only an *ignis fatuus* not worth attending to, but shines like a bright meteor to those whom nature has destined to observe it; which method of discovering is in a manner to create. But may Dr. HALLER

be said in this sense really to have created Irritability? You will find at the end of the following treatise a short history of this quality; in which he informs us, that several men of learning have given him the honour of the discovery; but far from subscribing to their testimony, his modesty, which is always a mark of superior talents, prompts him to name some authors, in whose works he alledges, it was hinted at before. But if you only take the trouble to read them, you will easily see, that what is contained in them proves only that they imagined a concealed cause, to which they attributed those phænomena which they could not account for; but by no means that they were acquainted with Irritability. HIPPOCRATES long ago searched after this cause, and pointed it out by the name of *έννοημον*; and BAGLIVI, who owns himself that his system was founded upon the ideas of HIPPOCRATES, imagined a force in the solids which he did not distinguish from other known forces, which he evidently appears to confound with elasticity, and places it in parts where it is not to be found. GLISSON is the first, says Dr. HALLER, who made use of the word *Irritability*, but in reality GLISSON saw no more than the common butchers see every day, *viz.* palpitations of the flesh after death, which are renewed

renewed upon touching. The tone of the fibres, so often repeated by the followers of STHAL, is no more than elasticity; and the principle of motion of Dr. GORTER, is only elasticity joined to sensibility. Nay, this able physician has made so few experiments upon the subject, that he attributes the cause of fevers* to the irritation of the arteries, which are neither irritable nor sensible. This however was all that was wrote upon the subject, when Dr. HALLER, in 1739, published his first notions of Irritability, which he continued to explain in the following years. Some years after Dr. WINTER, in an academical oration, and likewise in a thesis published by one of his pupils, founded a system upon Irritability, such as he imagined it to be, and not at all such as it really is. In short, it appears from thence, that far from being the inventor of it, it may justly be said, that he had not a true idea of it. His system is the same with that of BAGLIVI, and so little corrected, that he has fallen into the very same error with him, taking for the principle of all our motions the *dura mater*, which is void of all sensation. What dependence can we have upon systems purely imaginary, and the futility of which is

* Compendium Tr. 52. § 9.

proved by one single experiment? But at the same time Dr. WINTER is very much to be commended, for having publicly acknowledged that he was in a mistake, though certainly such an acknowledgment must be less mortifying to a man of his superior reputation, than to those authors of low rank, who are only known by publishing false doctrines, and soon relapse together with their works into oblivion. Dr. KAAU BOERHAAVE in his ingenious treatise *de Impetum faciente*, published only in 1745, makes observations on the *ἐνστικτον* of HIPPOCRATES, but that he has not placed it in Irritability, appears from the manner in which he characterises it § 145. He says it is a force which belongs neither to the body nor the soul, which takes place the moment these two are united, and ceases the very moment of their separation. But this character is very opposite to that of Irritability; chimera and reality are not to be confounded together. If he has mentioned this property in his other works which are since published, it is easy to see from what source he has derived it, and we ought to conclude, that Dr. HALLER is really the person who has discovered and explained the doctrine of Irritability. The confused hints which are to be met with elsewhere, ought no more to deprive him of the honour of this discovery, than the
 sympa-

sympathies of ARISTOTLE, or the obscure and universally extended force of BACON Lord VERULAM, ought to deprive Sir ISAAC NEWTON of that of having first known the force of attraction; and as this property will be transmitted to the latest posterity under his name, so Irritability will be distinguished for the future by the epithet of HALLERIAN. Those obscure and false ideas which are to be met with in some works, have been so far from facilitating Dr. HALLER's discovery, that they should rather have tended to obstruct it. In the arts an imperfect sketch, or even a faulty one is of use, as it leads to something better, and by degrees to perfection. We owe to the rudest draughts those very machines which at present attract our admiration; and the first hut may be considered as having been the model of the most lofty edifices. But the case is by no means the same in the sciences; a faulty system, especially if it is founded on fantastical ideas, imposed upon us instead of accurate experiments, leads us astray, renders more difficult, retards, and frequently absolutely obstructs the progress which we might otherwise be able to make; and we ought to reckon ourselves much more obliged to those, who, in order to come at the truth, are obliged to eradicate a thousand errors, which are planted like so many obstacles

cles in the way, than to those who find out a smooth road, though it has not been beaten before, and this obligation we lay under to Dr. HALLER; because in order to discover the true principle of motion in man, he has been obliged to make his way through a whole heap of imaginary systems.

The whole animal œconomy rolling upon this principle, it is easy to imagine what a change this discovery must produce, in explaining of facts. To England we owe philosophy, and to Switzerland we shall owe physiology, the immovable basis of which will be the treatise on Irritability. We may see by this specimen of our author, the happy use which he has already made of that property.

A great number of facts upon which Irritability is establish'd, were formerly publish'd by Messieurs ZIMMERMAN, OEDER, CASTEL, ZINN, SPROEGEL and WALSTORF, pupils of Dr. HALLER, witnesses of those experiments, encouraged by his precepts, and animated by his example to make new ones themselves. Their works are known, and have been well received by the public, as they justly deserv'd; but in all of them there wanted that last degree of precision, which is never found in the first essays upon a subject intirely new, which is only the production of a masterly hand, and characterises the following

following treatise, inserted in Latin in the second volume of the *Transactions of the Royal Society of Gottingen*. In this treatise we find a very careful distinction between Irritability and Sensibility; experiments made with an exactness, of which those who are incapable of the same, are neither sensible of the difficulty nor the use; determining the parts which are susceptible of the one and not of the other, those which possess neither the one nor the other, and those in which both are united together. As I imagined that a table, to represent at one view the result of all these experiments, would be of some service, I have inserted one at the end of this preface.

Persons of inferior capacity, to whom nature has left no other means of associating their names with those of great men, but by vilifying their works, and who deny the usefulness of theory in practice, because they do not comprehend it, will perhaps ask what is the use of all these discoveries, and if the art of healing will receive any new degree of perfection from them. Such persons do not conceive the use of theory in practice, for want of that thorough knowledge of both, which is necessary to discover the connexion between them, and that extent of genius, which, taking in several objects at the same time, and uniting them together

ther in one point of view, discovers the connexion that there is between them, and the necessary relation which obtains in all the sciences, between that of knowing the human body and the art of healing. Consult those celebrated physicians whom all Europe look upon as the first of the present age (I might have said as eminent as ever appeared) *viz.* VAN SWIETEN, WERLHOF, TRONCHIN, ELLER, SWENKE and DE HAEN, they will all tell you, that they owe the extraordinary and continued success, which has raised their reputation to so high a pitch, *to this enlightning theory*, as M. DE LA METTRIE says, speaking of that of the great BOERHAAVE, *which alone would be sufficient to persons of the least experience, and direct them safe in their practice, whilst without it the most consummate practitioner must always continue reduced to guess and conjecture.* One may say, that great physicians have a different practice from those of the ordinary sort. The former have particular methods of treating their patients, which the latter cannot comprehend the reason of, because they depend upon a skilful application of general principles, which these are either entirely ignorant of, or else are not capable of making a proper use of. They follow slavishly the same beaten method, though it has frequently been found

found hurtful, or at least useless; and as they are incapable of leaving it, all that we can expect of them is, that they should succeed in cases where that method happens to be proper. You must not require any thing further of them, it is very well if, in order to conceal their ignorance, they do not run down those prescriptions, which are above their comprehension as to their use, and over which should be inscribed the following motto,

Odi prophanum vulgus.

If the dependence of pathology on physiology was better known, there would be no need for displaying the great influence which this new discovery must have upon the art of physic; but unfortunately we want a work intitled, *The application of theory to practice*. This has determined me to venture some thoughts on the practical uses of Irritability, which perhaps may serve to incite the curiosity of the reader to further discoveries*.

* We have already two theses in which the cure of Irritability in practice has been treated of, the one is that of MANITIUS *de idiosyncrasia ex diversa solidorum corporis humani irritabilitate optimè dijudicanda*; and the other of Dr. DE LA MOTTE of Paris, *an omnis morbus ex irritabilitate aucta aut imminuta*; but these two works have left the subject still new.

The manner in which opium acts, which has given rise to so many systems equally opposite and chimerical, and has occasioned such a number of disputes, which could never before be decided, is determined at last, since the discovery of Irritability. It is neither by attenuating nor by thickening the humours, by exalting or absorbing the sulphureous particles, by restraining the furious *archeus*, nor by binding the nervous fluid, that opium occasions sleep; but by diminishing the Irritability of all the parts, excepting that of the heart, which is only a very little, and most frequently not at all weakened by this medicine. The action of the muscles intirely ceases; the senses are buried in a profound sleep; the heart only and the lungs, the one because its Irritability is not at all altered, the other because its action is independent on Irritability; I say, these organs alone continue their motions the same as before. Those viscera which are in the same case with the lungs continue their functions, while the actions of the stomach and intestines are diminished, and hence may be deduced, in what cases opium must be serviceable in stopping too violent evacuations, *viz.* when they depend upon too great an Irritability of the intestines; and consequently if this is too weak, narcotics must be hurtful.

This great principle serves as a basis to the whole practice of this remedy, and the manner of its acting accounts for all the symptoms which it produces. But it would be too tedious to enter into this detail, which every one may easily follow.

We frequently see persons in whom the most trifling moving cause, occasions much more considerable commotions, than it produces in those of a stronger habit. They cannot bear the slightest unusual impression; the least alarm, from whatever cause, will occasion extraordinary symptoms in them, which, according to their appearances and the part where the first cause of the complaint is imagined to reside, are known by the name of vapours, or hysterics, or other extraordinary complaints*, the immediate cause of which has always been attributed to an excessive mobility of the animal spirits, but the true one is too great an Irritability of the parts. This principle, combined with sensibility, accounts for the most extraordinary phænomena attending those

* The celebrated Dr. GORTER, to whom the practice of physic is so much obliged, is the first who has treated expressly of *mobility*, a disease so frequent and so little known. The description which he gives of it is very exact, and I would advise every physician to be acquainted with what he says of it in his *Compendium*, and his *Systema Præceos*.

disorders, and at the same time directs us to the proper cure of them. In short, seeing Irritability depends upon the mucus, and the different degrees of it are proportioned to the consistence of this singular body, so that the former is greater in proportion as the latter is less*, in order to remove the excess, the mucus must be restored to its necessary consistence. Those remedies therefore which are called tonics are the only ones to be made use of for this purpose; bleeding, purging, salts, mineral waters (at least the most part of them), and watery medicines, ought to be forbid, and in place of them should be substituted a proper regimen, exercise, friction, ligatures, gentle astringents, aromatic wines, &c. And indeed practice having so often confirmed the usefulness of this method, may we not hence justly conclude that system which explains them to be true, and which Dr. HALLER only proposed by way of conjecture? Age, which gives a greater consistence to the mucus, diminishes this excessive mobility; and hence we see instances every day of hysteric women getting rid of these complaints, or at least becoming a great deal less subject to them, after a certain age. There is likewise a point beyond which the

* Dr. ZIMMERMAN p. 8.

consistence of the mucus becomes bad by growing too thick, because hereby the Irritability is rendered too weak to allow the motions to be performed by their ordinary causes; and this thickness being the inevitable consequence of old age, hence this state is necessarily followed by death, which is nothing else than a cessation of all motion; for in old age Irritability gradually ceases, and without Irritability there is an end of motion, and without motion no more life. Nature produces in the tendons the effect of old age, and though they are composed of muscular fibres, and are really a continuation of the muscles, their too great compactness prevents their being irritable. This very phænomenon, if it was thoroughly examined, might perhaps serve to inform us in what the Irritability of the mucus consists; the explications which I have just hinted at supply us with others for a great number of phænomena, and lead us to the true rules of practice in a great many cases, where till now they were extremely false.

The causes and cure of convulsive disorders, so intimately connected with hysterics, receive a new light from this discovery; specifics are hereby rejected as useless, and there are only two indications to be answered, *viz.* to remove the stimulus and diminish the Irritability.

tability. But evacuating medicines always encrease this, and therefore can only be of service in cases where they are capable of removing the stimulus.

The action of those purgatives which we are better acquainted with, assist us in determining the use and choice of them. Diseases of the bowels, the cure of which is sometimes so tedious and difficult, that able physicians have looked upon them as incurable after they are become inveterate, will be removed with more ease, because their cause being known we are thereby led to find out their remedies. Chance has discovered, that persons who have been drowned have sometimes been brought to life again by air being blown into their anus. Reason teaches us that this effect must be produced by exciting the Irritability of the intestines, which renews that of the vital organs; and hence it is concluded, that an irritant as harmless but more powerful than air, such as cold water, will produce more certainly the same effect. It is easy to conceive how remedies may act when all sensation is lost, after we know that the organs of motion and sensation are not the same. We may see in Dr. ZIMMERMAN's essay* the manner in which he explains that phænomenon, which before

* § 39.

was not to be accounted for, *viz.* why some paralytics preserve the sense of feeling, whilst others who are called *paresie*, lose the sense of feeling and preserve that of motion. Palpitations are now easily to be explained, and for the honour of all the pathologists who have searched into the cause of them, it were much to be wished that Irritability had been sooner discovered. By dispossessing several parts of the melancholy right which was ascribed to them, of being the seat of pain, and by marking out those which are really so, Dr. HALLER teaches us to which we are to make our applications, and thereby renders perfect the art of physic in one very important part, *viz.* that of quieting the symptoms.

The theory of the constitutions explained by Irritability, in a work which Dr. ZIMMERMAN is preparing upon this subject, will throw a new light upon the whole practice and basis of morality. The influence of our body upon our ideas is so sensible, that every one is acquainted with it. We see it verified every day, that a small quantity, more or less, of food, a few drops of certain liquors, or some grains of nightshade, intirely change our manner of seeing things, and consequently of judging of them. Our ideas of beauty and goodness, of good and evil, or of vice and virtue, and our

actions consequent upon these ideas, vary according as our blood circulates with more or less rapidity, or is more or less thick. It is therefore certain that our manner of living changes our manner of thinking, that the operations of the mind while it is united with the body, may be varied by the use of air, diet, old age, sleep, motion, rest, and medicines. In consequence of this there must be a medicine for the mind, which has been thought of in all ages; in all ages it has been wished that this subject was treated of, that the true principles of it were enquired into, and certain practical precepts of it laid down. But this work has never been brought to perfection hitherto; all that we have, even the most modern, upon this subject, proves the difficulty of the enterprize and the boldness of the undertakers, much more than their capacity. In a work of this kind such universal knowledge is required, that it is by no means surprizing it has hitherto been wanted; but this essential vacancy in the libraries of moralists and physicians, will be filled up with dignity by Dr. ZIMMERMAN's treatise, which obligation we shall owe to Irritability.

There will be no further occasion to have recourse to imaginary suppositions, in order to explain the phænomena of the apoplexy. If
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the heart and other organs of the circulation continue their motions, when all the animal motions are suspended, 'tis owing to the same reason that explains the action of opium; viz. because there is a stimulus which determines the motion of the heart, independent of all sensation and of every other motion. But the apoplexy is only a profound sleep, depending upon the very same causes with sleep, and therefore is to be accounted for in the same manner*.

The theory of fevers, and that of inflammations, in a word, of all the diseases which depend upon an increase of the circulation, will henceforward be fixed; seeing the cause of the circulation being known, we are thereby conducted to the knowledge of those things which can either increase or weaken it. The blood being rendered more acrid, of course becomes more irritating, hence acrimony is capable of producing a fever; and the different species of acrimony, the order in which they are produced, or evacuated, will form the different species of fevers. There still remain discoveries to be made upon Irritability, especially with relation to the force of different stimulants, which depends perhaps upon several causes;

* See the *Primæ Linæ Physiologicæ*, N^o 568, 576, and 400.

and the more that the nature of it comes to be understood, the more easy will it be to give a reason for all the motions depending upon it.

Several accidents in surgery, which were only troublesome because we were mistaken about the cause of them, will henceforth cease to be so, seeing their cause being better known leads us to the proper method of treating them, and the method of treating them being known, assures us of a cure *. The uncertainty which surgeons apprehended of the possibility of several important operations, and which men eminent in the profession, ventured upon only as desperate, while others durst not at all meddle with them, has occasioned the death of a number of persons, in cases where the patients will be saved for the future, because these new experiments assure us of the safety of those operations.

The examples which I have already related, will be sufficient, I hope, to convince persons of the greatest obstinacy, of the real advantages which the discovery of Irritability must procure to us. I shall finish with some general reflections upon the objections which may be made or have already been made to it,

* See Dr. ZIMMERMAN p. 14, 15 and 16, and Dr. CASTELL § 42, 43, 44 and 45.

1. It is by no means an ideal system which Dr. HALLER advances in his treatise, it is not an assemblage of analogical conclusions upon four or five experiments hastily made, and frequently in so bad a manner, that the chief care of the author is to reconcile the results of them; but it is a chain of facts, which have been confirmed by a number of experiments made with the greatest exactness, and repeated very frequently during the course of six years, before the publication of this treatise, and even since that till the present time; the consequences of which have always been uniform, and have all concurred to confirm the same truth. It is not therefore by a few arguments that Irritability is to be attacked; it is not by trivial objections, founded upon chimerical consequences, which an over-heated imagination may draw from them; neither is it by some observations, or some experiments made at random. Whoever wants to deny the facts which Dr. HALLER advances, or rather that his experiments have been properly made, ought to be as well provided as he with a number of experiments equally well attested. But it is not to be suspected that Irritability will ever be attacked in this manner; to believe this would be wronging of nature, which is invariable in her laws, as those who know them by inquir-

ing into them will always find her. When observations upon the same subject do not resemble each other, it is always either because one of the observers has not perceived the different circumstances which ought necessarily to vary them; or because, as indeed is too often the case, he decides the result of the experiment before he has made it, and only makes it in order to confirm the other; so that he sees what he was resolved beforehand to see. Some physicians treat the book of nature as the divines have treated the bible; they do not consult it in order to know what it contains, but to find in it something to authorize their ideas. They do not apply to nature for information, but they feign oracles, and deliver them boldly as her decisions. Thus books are multiplied, and confusions in proportion, because the false must be lopped off before the truth can be laid hold of; and I should not be much surprized, if a person who knew nothing of the universe but by the works of ill qualified or systematic authors, (who are by far the most numerous) should believe it to be the work of chance, so little uniformity and harmony would they find there.

2. The experiments relating to Irritability having been made upon brutes, can the truth of the result of them be affirmed with regard to man? It is easy to see that this objection is

is the effect of that low jealousy, which persecutes talents and merit, or rather mankind in general, by endeavouring to discourage those great men who enlighten it; provided they were capable of being hurt by those weapons, which, like slender arrows drawn by the weak arms of children, can only rise high enough to fall back upon their own heads. But we must not leave even this gloomy consolation to envy; while we despise the insect which teases us, and which we do not distinguish from numbers of the same kind, we endeavour to guard ourselves from its sting, the effect of which is so much the more sensible, the handsomer the face is against which it is darted. Dr. HALLER's treatise consists of two parts, the first of which treats of sensibility, and the experiments which he relates, contrary to what has hitherto been generally believed, are such as we had the greatest reason to suspect to be inapplicable to man. But the human body has really been the subject of most of these experiments, and hence all those doubts necessarily cease. He mentions some authors who had observed before him the insensibility of the tendons, and he proves that insensibility, by a fact which he was witness to himself. In the supplement he quotes the celebrated Dr. ELLER, whose authority cannot be suspected, as a witness of the insensibility

sibility of the *dura mater*; and Dr. CASTELL relates other facts which prove the same thing *. It is true we have not the same number of experiments upon the Irritability of the human body as we have upon that of brutes, but still we have some; and supposing there were none at all, it would be equally just to conclude from the severest analogy that it exists. That pyrrhonism which denies all certainty, is not more foolish and dangerous, than that which admits only of geometrical demonstration. Inferences have their rules, and those propositions which we discover in following them exactly, have the same degree of force, as mathematical propositions the most strictly demonstrated. They are not to be contested, but from jealous ignorance, which is always inconsequential for want of principles. The most part of those physiological experiments, which during this last century have advanced physic to its present height, have been made upon brutes; and it is to these experiments that we owe the knowledge of the circulation, the mechanism of respiration, the rout of the chyle, and the history of generation. But there have never been any objections raised against their application to the human physiology; because

* Pag. 23, 24, 25 and 38.

we cannot be deceived as to the perfect uniformity of the mechanism in us and brutes, with regard to the vital and natural functions; for that is demonstrated by the exact resemblance of similar parts, and of the essential organical parts. The different make of the extremities, or the variety of the outward skin, does not prove the principles of their motions to be different. A crane that lifts a beam, or a block of marble, is always the same crane, and acts in the same manner in both cases. We may therefore conclude that Irritability in man is one of those truths which is undeniably demonstrated; and posterity, which alone stamps the merit of discoveries, by abstracting of persons, will promote this to that rank which its usefulness intitles it to. She will laugh when she observes, that after its opposers had failed in persuading us there was no such thing, they should endeavour to render the doctrine odious, by the consequences which they pretend naturally follow it. She will be diverted to see physicians following the example of religious sectaries and devotees, interesting the cause of God with theirs, and accusing of deism such as differ from them in opinion as to the pulsations of the arteries. A certain author, well known for the greatness of his talents and the bad use which he has made of them,

has

has mixed in the same work some ideas of Irritability, with those of materialism, and endeavoured to explain the sensations by this property; but Dr. HALLER, at the end of his treatise, has proved the futility of this system. However as this objection is inserted very carefully in a small dissertation of M. DELIUS, professor at ERLANG*, and he goes so far (so charitable is his doctrine) as to endeavour to prove syllogistically, that this new system tends to irreligion, therefore this proposition deserves to be examined.

1. It is generally allowed that the nerves are the organ, and the brain the receptacle of all our sensations, the sources of all our ideas; but the nerves and the brain are not at all irritable; Irritability therefore has nothing common with our sensations. 2. Supposing it was affirmed to be the principle of our sensations, as it appears to be that of the other motions, what dangerous conclusion could hence be deduced? What does it signify to those truths which depend on the nature of the soul, whether that be Irritability or some other property of matter? That analogy which I have proved before between men and brutes, (I mean all this time quadrupeds) clearly demonstrates

* Animadversiones in doctrinam de irritabilitate, tono, sensatione, & motu corporis humani.

that the principle of the sensations is the same in the one as it is in the others, and this principle not being the soul in brutes, can with as little propriety be called the soul in man. Sensation is produced the same way in the one as in the others; but in brutes, the result of this sensation is confined to a mechanical determination consequent upon it; while in man the soul perceives that sensation, this perception forms the idea, and the passage from the sensation to the idea is the essential character which distinguishes man from brutes. This difference, which so many divines deny, that they may enjoy the gloomy pleasure of depressing man below the brute creation, and of discovering less reason, wisdom, and conduct in him than in them; this difference, I say, has been lately shewn in as clear a light as possible, and that principle upon which deism founded one of its strongest arguments, has thereby been undermined. That industry, wisdom, foresight, knowledge, all these wonders, I should rather say all these monsters of reasoning, creatures of the imagination of the observers, and of that desire which they have of discovering every where the final causes of things, fruits of that vanity which attempts to explain every thing, and of that want of capacity which considers those things as trifling which only exist to give

harmony to the whole; all these chimeras vanish, and if beings, intirely corporeal, perform their labours with more order than men, it is only because matter, conducted by the Creator, is better managed than that which is in the hands of the creature. The brutes, properly so called, are restrained by wise laws, which in them are invariably executed; whereas, on the contrary, the soul frequently rebels against these laws, in man. From all these facts the following syllogism results, very opposite to that of professor DELIUS at Erlang, *viz.* a property common to two beings is by no means the cause of their difference; but because Irritability is common to man and brutes, it is not therefore the cause of thought. It influences both the vital and natural motions, nay further, it may be allowed to influence the sensations, and all the animal motions depending upon them, without this doctrine being suspected of impiety, because we are certain that the cause of sensation is independent of thought. Perhaps the soul may absent itself from the body, or, to speak more justly, may take no notice of what passes, without the life being thereby affected in man. What employment could the soul have presiding in the wild man of *Arnobius*, if ever there was such a person? What employment

can

can it have in the fœtus, that mass indeed organised, but deprived of all sense, and buried in perpetual sleep? Does it shew any sign of its presence in a new born infant? One is lost in chimerical questions concerning the moment of the union of the soul and body, which is doubtless not single; for the body can live without the soul. This union only consists in the inspection which the soul has over the body, and does not take place but when this inspection is exercised, and the soul of consequence produces some motion in the body. During the first months of our life, this union is far from being continued, but it becomes more so by degrees, though perhaps during life it has its interruptions, which are probably the cause of these contrarieties, the reason of which has not hitherto been discovered.

We are but imperfectly yet acquainted with the phænomena of the load-stone, of attraction and electricity. Irritability comes next to open a new field of enquiries, a new source of solutions; and perhaps we are upon the point of discovering some other property, that will throw a light upon those obscure subjects, of which at present we see only the dawn.

The sensible parts.

THE brain, the nerves, viz. their *medulla*, and the following parts by means of the nerves.

The skin, the muscles, the internal membrane of the stomach, intestines and bladder, the ureters, the breasts, the *uterus*, the *vagina*, the *penis*, the tongue, the teeth, the eyes, and especially the *retina*, the *tunica choroides*, but less than the *retina*; the heart, though not so much as the other muscles.

The *viscera*, the lungs, liver, spleen and kidneys, as likewise the glands, have few nerves bestowed upon them, and consequently are not endowed with much sensation.

The irritable parts.

The heart, the muscles, the diaphragm, the *œsophagus*, the stomach and intestines, the lacteal vessels, the thoracic duct, the bladder, the glands and mucous sinuses in man, the *uterus* and genitals, which last are endowed with an Irritability peculiar to themselves.

The insensible parts.

THE cuticle, the cellular membrane, the fat, the tendons, the membranes which cover the viscera and articulations, the *mediastinum*, *omentum* and mesentery; the *dura* and *pia mater*, the ligaments, the *pericæstum* and *pericranium*, the bones, the marrow, the *cornea* and *iris*.

The arteries and veins are insensible, excepting those which are accompanied with nerves, such as the carotid, the lingual, the temporal, the pharyngeal, the labial, the thyroïdal, and the *aorta* near the heart.

The cornea, Havers's gland, and the coats of the nerves are likewise insensible.

The unirritable parts.

The nerves, the cuticle and the skin, the membranes, the arteries, the veins, the cellular membrane, the *urethra*, ureters, lungs, liver, spleen, *dartos*, and round ligaments of the *uterus*, the tendons, ligaments, *pericæstum*, *meninges* of the brain, and the *iris*.

The excretory ducts can scarcely be called irritable, at least they require a very strong irritation.

The parts which are both sensible and irritable.

All those which have nerves and muscular fibres; the muscles, the heart, the whole alimentary canal, the diaphragm, the bladder, the *uterus*, the *vagina* and genitals.

Insects are irritable and sensible all over.

TREATISE

ON THE

Sensible and Irritable Parts

OF

ANIMALS.

SOME months ago^a my pupil and friend Dr. ZIMMERMAN published an inaugural dissertation upon Irritability; part of the experiments relating to that subject I saw him perform, and I shall relate them here, in the same order as I find them, in my notes. Others of them I was not witness to, and therefore I shall quote these from his thesis. Since the year 1746, I have made a great many of the same kind myself, which he was either a witness

^a Dr. HALLER read this memoir in the academy of Goettingen, April 22, 1752, and the dissertation which he quotes was published in July 1751, under the following title, *Dissertatio Physiologica de Irritabilitate, auctore JOHANNES GEORGIO ZIMMERMAN Helveto Brugenfi.*

of, or assisted me in performing ; and since the beginning of the year 1751, I have examined several different ways, a hundred and ninety animals, a species of cruelty for which I felt such a reluctance, as could only be overcome by the desire of contributing to the benefit of mankind, and excused by that motive which induces persons of the most humane temper, to eat every day the flesh of harmless animals without any scruple. As in making these experiments I was obliged to try several which were useless, and to repeat others of them several times, to communicate the whole of them would only be spinning out this treatise needlessly ; wherefore I shall confine myself to relate those only which are of real use, and are found constantly true.

The result of all these experiments has given place to a new division of the parts of the human body, which I shall follow in this short essay, by distinguishing those which are susceptible of Irritability and Sensibility, from those which are not. But the theory, why some parts of the human body are endowed with these properties, while others are not, I shall not at all meddle with. For I am persuaded that the source of both lies concealed beyond the reach of the knife and microscope, beyond which I do not chuse to hazard many conjectures, as I have no desire of teaching what I am ignorant of myself. For the vanity

ty of attempting to guide others in paths where we find ourselves in the dark; shews, in my humble opinion, the last degree of arrogance and ignorance.

I the more willingly engaged in examining this subject, that the experiments which I have made, are the source of a great many changes, both in the physiology, pathology and surgery, and discover several truths contrary to the opinions generally received. This last reason has obliged me to be very exact in my proofs, because I was fully persuaded, that an opinion so little foreseen would at first appear improbable, and only gain assent by the clearest conviction. For this reason I was obliged to repeat and multiply my experiments, in order to convince the incredulous, by a number of authentic testimonies, so to speak, and prevent my falling accidentally into any mistake; for I am persuaded that the great source of error in physic has been owing to physicians, at least a great part of them, making few or no experiments, and substituting analogy instead of them. A second motive which encouraged me in this work, was the readiness with which some celebrated authors have laid hold of the first notions of Irritability, so as even to make use of this property of our fibres, as a basis of almost an universal system of motion in the human body, and thence deduce the functions of the fibres, vessels, nerves, muscles,

and in short of all our organs. This appears very plain in reading Dr. WINTER's oration, delivered at Franeker in 1746, that of Dr. LUP's *De Irritabilitate*, and the thesis of DE MAGNI and LA MOTTE upon the following subject, *Ergo a vasorum aucta aut diminuta irritabilitate omnis morbus*. For they all agree pretty much in the same opinion, viz. in deriving all motion from sensation: In which they are joined by Drs. KRUGER, NICOLAÏ, WHYTT, DELIUS, and some other great physiologists.

I call that part of the human body irritable, which becomes shorter upon being touched; very irritable if it contracts upon a slight touch, and the contrary if by a violent touch it contracts but little.

I call that a sensible part of the human body, which upon being touched transmits the impression of it to the soul; and in brutes, in whom the existence of a soul is not so clear, I call those parts sensible, the Irritation of which occasions evident signs of pain and disquiet in the animal. On the contrary, I call that insensible, which being burnt, tore, pricked, or cut till it is quite destroyed, occasions no sign of pain nor convulsion, nor any sort of change in the situation of the body. For it is very well known, that an animal, when it is in pain, endeavours to remove the part that suffers from the cause that hurts it; pulls back the leg if it is hurt, shakes the skin if it is pricked,

ed, and gives other evident signs by which we know that it suffers.

We see that experiments only can enable us to define what parts of the human body are sensible or irritable, and what the physiologists and physicians have said upon these qualities, without having made experiments, has been the source of a great many errors, both in this case and in a number of others.

When Dr. BOERHAAVE had established the doctrine of the nerves, being the basis of all our solids, he presently after proceeded to affirm, that there was no part of the human body which was not sensible, or capable of some sort of motion^b, and this system, which I have elsewhere^c refuted, was received almost universally all over Europe.

The most simple parts of the human body are the nerves, arteries, veins, the smaller vessels, membranes, the muscular, tendinous, ligamentous, and bony fibres, and the cellular membrane.

The more compound parts are the muscles, tendons, ligaments, bowels, glands, the great reservoirs, excretory ducts, and the larger blood vessels.

^b Institut. Med. N^o 301.
lect. Boerh. loc. cit.

^c *Commentar in Præ-*

But this by the by, for here I don't study accuracy, this being only a general list of the parts of the human body.

In order to discover which of these parts are sensible, I made the following experiments.

I took living animals of different kinds, and different ages, and after laying bare that part which I wanted to examine, I waited till the animal ceased to struggle or complain; after which I irritated the part, by blowing, heat, spirit of wine, the scalpel, *lapis infinalis*, oil of vitriol, and butter of antimony. I examined attentively, whether upon touching, cutting, burning, or lacerating the part, the animal seemed disquieted, made a noise, struggled, or pulled back the wounded limb, if the part was convulsed, or if nothing of all this happened. The repeated events of those experiments I marked down faithfully, whatever I found them to be. For what is it to me, in fact, on which side nature decides the question? nay, would it not be very foolish to hazard the reputation of a faithful and accurate observer for an imaginary fact, which the simplest experiment would prove to be false to any other anatomist who should chuse to repeat it?

As to the order of making the experiments, I think it is of no great consequence, and therefore

fore shall begin with the *skin*. For as to the epidermis, that is certainly insensible, seeing you may burn it with hot spirit of nitre till you give it a durable yellow taint, without occasioning the least degree of pain.

The *rete mucosum* of MALPIGHIUS can hardly be separated from the epidermis in experiments of this kind; wherefore I did not try any upon it, being fully convinced that it has no feeling.

The skin is sensible, and indeed more so than any other part of the body; for in whatever manner you irritate it, the animal makes a noise, struggles, and gives all the marks of pain that it is capable of. This great sensibility of the skin has determined me to take it as a fixed standard of that property; and I reckon as insensible, or at least only in a small degree, those parts which may be irritated without disturbing the animal, while it discovers signs of pain if the neighbouring skin is irritated.

The fat and cellular membrane are not sensible of pain, as is very well known, and has been shewn by other authors. What is told of DIONYSIUS the tyrant, and is commonly known to hold true in hogs, may be sufficient to confirm this; viz. upon thrusting a needle into them they shew no signs of pain, till it has got

quite through the fat and reached the flesh below^d.

The *muscular flesh* is sensible of pain, but that is owing more to the nerves than to the flesh itself. For if you tie the nerve of any joint, if there is only one, or the principal trunks, in case there are more, the whole joint is thereby rendered insensible, nor is the animal affected by those applications which hurt the joint when the nerves are tied. For it is very well known that all the muscles are capable of pain, the large hollow ones not excepted, such as the stomach, intestines, and bladder.

But the *tendons* are neither capable of sensation, nor pain. This is the first paradox which I advance against the common opinion, and it has hitherto found but few partizans. Almost all the antient, and some of the most modern authors, as La FAYE^e, HEISTER^f, and GARNGEOT^g, look upon wounds of the tendons as very dangerous and difficult of cure, and the same opinion has been adopted by BOERHAAVE, his worthy pupil VAN SWIETEN^h, by ACRELLⁱ and QUESNAY^k.

^d Commentar. BOERH. tom. III. N° 333. not. ^b

^e Chirurgie de DIONIS derniere edition, pag. 680, 681.

^f Inst. Chir. pag. 423, edit. de 1737. ^g Operat.

de chir. tom. III. ch. 7. ^h Tom. I. n. 163. p.

238. ⁱ Om Friskafor pag. 261. seqq. ^k De

la supurat. pag. 222.

I shall shew immediately, that this opinion was not altogether unknown before. For JOB A MEKREN¹, a very experienced surgeon, has said that the tendons are very insensible, and he quotes as an instance that of the *patella*. BRYAN ROBINSON^m asserts, that in a living dog irritations of the tendons did not seem to be very painful, but the muscles were more sensible. GEORGE THOMSON observedⁿ no commotion from a hurt of a tendon, and DANIEL SCHLICHTING^o was witness of the same thing both in men and in dogs. But these authors are not numerous, and made only few experiments.

For the most part I have laid bare the tendon of the *extensores recti* of the *tibia*, or the *tendo achillis*, after which I have pricked it, cut off part of the fibres, even to one half of them, nay I have cut the tendon across so as to leave one half of it intire, which wound BOERHAAVE looked upon as the most dangerous. These experiments I have repeated upwards of a hundred times since the year 1746, upon dogs, goats, rats, cats, rabbits, and various other animals, always with the same success.

The use of this experiment is to prove, that if you irritate the muscular fibres they con-

¹ Obs. cent. pag. 162.

^m Animal œconomy p. 90.

ⁿ Anatomy of human bones p. 170.

^o Traumatograph. p. 213. Eph. nat. cur. vol. VI. obs. 24.

tract ; but it is not the same case with the tendon, which you may prick and lacerate as much as you please, without occasioning the least motion of the muscle ; as indeed in general, when the muscle is contracted, there is never any contraction observed in the tendon, which WILLIS^P has taken notice of before, and I have seen it a hundred times. Hence therefore it appears, that the tendons have no organ neither of motion nor sensation.

The constant event of this experiment was, that the animal, whose tendon was lacerated, burnt or pricked, remained quiet, without shewing any sign of pain, and when it was let go, provided only a small part of the tendon remained whole, it walked easily and without any complaint. I have seen a dog walk upon his two hind feet, when the *tendo achillis* of both legs was pierced through the middle, and a kid walk freely, after I had cut away one half of each of the same tendons. And in another dog, who had only the tendon of the *solearis* muscle intire, those of the *gastrocnemii* being cut through and drawn up into a kind of knot ; I observed no bad symptom as long as I kept him. Wounds of the tendons are likewise the most easily cured of all, only by the help of nature, without any trouble or bad symptoms, so that there is nothing surprising in that obser-

^P De motu muscul. p. 118. compare BAGLIVI opera p. 317.

vation of M. la FAYE^q, viz. of the tendon of the biceps being cut without occasioning any want of motion in the arm. Neither ought we to blame VESLINGIUS and some others^r for boldly recommending the sutor of the tendons; nor M. BIENASE^f for having ventured upon it, after having first tried the experiment on a dog. Dr. ZIMMERMAN^t observed no sensation in the *aponeurosis* of the *abdomen*, upon touching it with oil of vitriol.

After I was fully satisfied of the certainty of the above event, I easily discovered the cause, viz. that there are nerves distributed to the muscles, but not to the tendons. FABRICIUS^u AB AQUAPENDENTE owned long ago, that he could not trace the nerves to the tendons, but they disappeared in a kind of membrane before they got that length; and LEUWENHOEK^x could discover only a very few nervous filaments upon the surface of the tendons.

Seeing therefore in the human body the nerves only are capable of sensation, it is neither unnatural nor improbable, that the tendons being destitute of nerves should have no sensation. I have oftener than once seen the tendons laid bare in man, and, emboldened by

^q Chir. de DIONIS pag. 681. note 2.

posth. a BARTHOLINO.

^r Epistol. ^f VERDUC. Operat. de Chirurg. c. 32.

^t Dissert. cit. p. 16.

^u De Fabric. muscul. pag. 27.

^x Epist. Physiolog. pag. 443.

the experiments which I made upon brutes, I once laid hold, with a pair of *forceps*, of the naked flexor of the third joint of the fore finger, of a young gentleman, without his being sensible in the least that I touched it. I have likewise seen the tendon of the long *supinator* muscle chafed with hot oil of turpentine, in order to stop an hæmorrhage; it occasioned an acute pain in the skin, but the patient felt none in the tendon. Thus it is an old practice to make use of hot oil of turpentine in wounds of the tendons, and surgeons have long ago looked upon it as an excellent remedy in such cases; but this oil would certainly occasion as much pain in the tendons, as it does in the skin, provided they were sensible.

Wherefore we need no longer be afraid of wounds of the tendons, of whatever kind they are. If a large tendon is destroyed, it is evident the patient must halt, or lose the use of a joint, seeing the muscles, being thereby deprived of their insertions into the bones, can act no longer upon it. But besides this want of motion, there is nothing else to be feared, and sometimes nature remedies this misfortune in such a manner, by means of a new cellular membrane, and the neighbouring muscles, that the motion of the joint is performed as easily as before. I have seen a new cellular substance of a bluish colour, grow in a few days, and unite the ends of the *tendo achillis* that was cut through in a dog.

dog. As soon as they were united the animal suffered no more inconvenience, and jumped with the same agility as before upon the chairs and tables.

Whence therefore can this mistake arise with regard to wounds of the tendons, into which almost all the authors have fallen, who have wrote upon the subject, even the most respectable and intelligent of them in other respects? It appears to me to be most probably owing to the signification of the word *νεύρον*, being confounded with those of *τένων* and *σύνδεσμος*, whereby it is made to signify both *nerve*, *tendon*, and *ligament*^y. But a wound in a nerve produces most violent symptoms, as will immediately appear. Thus I really believe, that in bleeding, a wound of the *median* nerve, and perhaps sometimes of a branch of the *musculocutaneous*, which runs down before the median vein, has produced those painful symptoms, which are commonly imputed to pricking the tendon of the biceps muscle, over which the vein runs. PARE^{us} has left us an account of a remarkable instance of this in CHARLES IX. of France. It is likewise to the great nerves which are distributed the whole length of the fingers, that we ought to attribute the causes of the unlucky events of some kinds of whitlow, the danger of which has long been ascribed to their seat be-

^y GALEN de usu part L. XV.

ing in the sheaths of the tendons, as was the opinion of GARENGEOT^z not long ago.

The *ligaments* and *capsulæ* of the articulation, are situated in the neighbourhood of the tendons; the former have been described under the name of *νεύρον*, and the latter are famous on account of the danger alledged to attend wounds of them, and of their being blamed by many celebrated physicians as the seat of the gout^a.

In making these experiments I met with a good deal of difficulty. For as it was necessary to remove the skin, and to stretch it a good deal in the tight joints of the smaller kinds of animals, so as to allow the wounding and irritating instrument to be introduced into the hollow of the articulation, the animal frequently complained, upon my laying hold of the skin with the *forceps*. However the experiment, even when it was made with poisons, very often succeeded. In a cat I filled the articulation of the *femur* with the *pelvis* with oil of vitriol, without its seeming to feel this violent corrosive, although I have seen the uterus of a bitch destroyed in the space of a

^z Opera de Chirurgie. tom. III. pag. 286, 301, 302.

^a BOERHAAVE *aphorif. de cognosc. & curand. morb.* 1254, 1259. It is true this great man admitts likewise the nerves to be the seat of this disease.

minute by the same application. In making these experiments upon the articulation of the knee, which is more readily come at because it is almost bare, I have frequently made use of small pieces of sticks dipt in oil of vitriol, or in the butter of antimony, with which I have burnt the lateral ligaments, that of the *rotula*, both the external and the internal surface of the *capsula*, and Havers's gland, without the animal discovering the least sense of pain. Nay these wounds, which are commonly reckoned the worst, were cured with so much ease, that the saliva alone of the animal was sufficient for it, and very often they healed without it. Now all these experiments, which have been repeated upon dogs, cats, and young kids, justify the observation of M. DE LA MOTTE^b, who found the *extensor* ligament of the *tibia* to be insensible. Sometimes instead of caustics, I tried the experiment with a knife or needle thus: I made an incision on the external side of the articulation of the knee, laid bare the *capsula*, *patella*, the ligament which goes from it to the *tibia*, and the external or internal lateral ligament; then scraping with a knife the external surface of the *capsula* and ligament, I thrust a knife or needle thorough from the inside till its point pierced the skin, during all this the animal shewed no signs of pain, till the point of the knife or needle, after having pierced the

^b Chirurg. compl. N° 365.

capsula of the articulation, came to touch the cellular membrane under the skin. This experiment I have repeated several times.

Hence it appears, that we ought not to attribute the sharp pains of the gout to the *capsula* of the joint, in which it is so difficult to find any nerves, and which is void of sensibility: Their true seat is in the skin, and in the nerves which creep upon its internal surface. And it was certainly very well ordered by nature, that those parts which are exposed to a continual friction should be void of sensation.

Wherefore if wounds of the joints are sometimes difficult to heal, we must attribute this to the humour which is continually secreted there, and which readily acquiring a rancid putrefaction, will not allow the wound to heal up. In dogs, as far as I can remember, these wounds always healed without any difficulty.

The *periosteum* resembles the ligaments and *capsulae*, and evidently in the *fœtus* forms together with them a thick pulpy membrane, which is continued from one bone to another, including the intermediate articulation. Wherefore I was not surprised to find it insensible in the numerous experiments which I made in the leg, thigh, heel, *metatarsus*, and *pericranium*, which last is of the same nature with the *periosteum*.

Physicians,

Physicians, anatomists^c, and surgeons, who are of another opinion, will I hope pardon me for differing so widely from them, and defer condemning me, till they have compared the experiments upon which that opinion is founded. A hundred times have I tore, pricked, and burnt the periosteum, without the animal's shewing the least sense of pain; nay young kids have sucked during the time, while if I pinched the skin they complained violently, and fell into convulsions.

This insensibility of the periosteum was observed before by Mr. CHESELDEN, and it is not at all surprising in a part where there are no nerves, where even NESBIT^d himself could not discover any, but concluded there must be such, though they were invisible, to account for the sensibility which he ascribed to this membrane. For those which creep in great abundance upon the *pericranium*, and do not come from the tenth pair, but from the second and third of the neck, and from the third and fifth of the brain, are bestowed upon the skin of the head, and communicate their sensibility to it.

^c WINSLOW, *Traité des os frais* N^o 6e. CLOPTON, HAVERS, NESBIT *human. osteogon.* p. 6. Phil. Ad. BOEHMER. *osteol.* p. 31. DUVERNEY *Traité des maladies des os*, tom. II. p. 431.

^d Loc. cit. ut supra.

The insensibility of the *bones* is still disputed, and indeed I have made no experiments upon this subject, for in that cruel torture which is unavoidable in order to lay bare the bones, it is very difficult to distinguish the new pains thereby produced. The sensibility of the *teeth* is known to every body, but the same reason which explains that, persuades me that the bones are void of sensation. For though the nerves of the teeth are extremely slender, yet they are easily shewn where they enter their proper foramen. But in the large bones I have never been able to find any nerve, accompanying the artery and vein at their entry into the bone^e; if there were, I should have discovered them in my numerous descriptions of the arteries; if not elsewhere, at least in the large and smooth internal surface of the cranium, and in my preparations of the nourishing arteries of the whole body. DIDIER^f says that the bones rendered soft are very sensible; but besides that it is easy to be imposed upon in so terrible a disease, M. IMBERT^g asserts the contrary; and I have seen the operation of the trepan performed upon sound persons who had the free use of their senses, without their complaining of any pain from the perforation of the *cranium*.

^e Nervi ad ossa nulli, RIOLAN enchirid. Anatomic. AL. MONRO loc. cit. p. 16.

^f Anat. rais. p. 6, 7.

^g Quæst. med. XII. p. 33.

DEVENTER^h, AMBROSE PAREⁱ, DU VERNEY^k, and most other authors, agree in saying that the *marrow* is very sensible of pain; but this is very improbable, as it is of the same nature with fat, and has no nerves bestowed upon it.

The *dura mater* is a kind of *periosteum*, every where applied to the internal surface of the *cranium*, connected to it by vessels, and has arteries transmitted through it to the skull, in the same manner as they are transmitted through the *periosteum* to the other bones of the body. PACCHIONI and BAGLIVI have ascribed to it a force equal to that of the heart, and the most part of physicians have looked upon it as the seat of the most violent diseases; but their opinions do not change the eternal nature of things. I have shewn elsewhere^l, that it was, like all the other membranes of the body, composed of the *membrana cellularis*, and this analogy has been confirmed by the experiments of Messieurs ZINN^m, ZIMMERMANⁿ, and CASTEL, and likewise by my own, which convince me that this membrane, so nearly resembling all the others produced from it, may be burnt with oil of vitriol, butter of antimony, or spirit of

^h Van beenfiekten p. 80.

ⁱ Administr. anat. p. 83.

^k Mem. de l'acad. des scienc. 1700. pag. 205.

^l Prim.

lin. physiolog. n. XI.

^m Experim. circa corpus callosum cerebellum &c. Gotting. 1749. p. 28. seqq.

ⁿ Loc.

cit. p. 6. &c.

nitre, cut with a knife, or tore with a pair of pincers, without the animals appearing to suffer the least pain. Messieurs ZINN and MEKEL observed the same insensibility in the *dura mater* of a man, which had been laid bare by a *caries* of the *cranium*; and doubtless the antient physicians, CARDANUS^o, and before him GALLEN, spoke from experience, when they recommended the sharpest applications as not only safe, but requisite in treating the *dura mater*. Comparative anatomy likewise informs us, that this membrane is a true cover for the brain, and not a muscle; for in the tortoise it is found of a cartilaginous consistence^p.

How is it possible, that a membrane so insensible and incapable of motion, should have the force to send back the spirits to the heart, and be the seat of head aches, the *phrenitis* or madness, unless you chuse to alledge, that when it is affected, the brain by its proximity ought to sympathise? wherefore the French surgeons are very right in venturing to cut it, as often as they observe *pus* or extravasated blood under it.

I must here beg leave to make a digression, which perhaps may be of use. M. SCHLICHTING^q has said, that there was a motion in the brain, that it ascended and descended al-

^o De vulner. cap. p. 139. ^p STEPH. LORENZINI obs.

^q Memoir. présentes tom I. p. 114. *et seq.*

ternately,

ternately, and is extremely angry with those who rank it amongst those parts of the body which are void of motion. Being fully convinced of the strong adhesion of the *dura mater* to the *cranium*, and of the total plenitude of the contents of the skull, I could not help wondering at the boldness with which this author maintained the contrary; however I did not think of refuting him by authorities or reasons *a priori*, but judged it best to fight him with his own weapons, that is by experiments. Wherefore I opened the skulls of several dogs with a hammer and chissel, which is a more commodious way than with a trepan, and exposed to view a large part of the brain; and this experiment I frequently repeated upon dogs, goats, rats, frogs, cats, and other animals. The event was always the same, *viz.* I saw a manifest motion in the *dura mater*, or rather in the whole brain, such as SCHLICHTING has described, for it ascended in expiration and descended in inspiration. Upon account of this motion alone I made above thirty experiments with M. WALSDORF, who proposes shortly to publish a small treatise upon this subject^r.

I am too great a lover of truth to suffer pain upon account of a new discovery, however opposite it may be to my former ideas; but what gave me uneasiness was, that I could not dis-

^r He published it in 1753, since the first publication of this treatise.

cover the reason of this correspondence between the motion of the brain and respiration: For the mind loses patience at the discovery of a phænomenon which seems repugnant to reason.

But repeated experiments removed this seeming contradiction. Neither the dura mater nor the brain have any motion unless the cranium is removed, which therefore in a living sound animal is an effectual obstacle to it. This SCHLICH-
TING himself owns to be the case, nay for the most part there appears no motion in the brain, till the adhesion of the dura mater to the bones of the *cranium*, which keeps the brain immovable, is destroyed by the fingers or some proper instrument. Wherefore this consent of the motion of the brain with respiration can never be applied to a sound healthy person; for if the dura mater is not moved so long as it adheres to the cranium, and if the brain is only raised up in expiration after the dura mater has been separated from the skull, which is never the case of a person in health, this phænomenon ought not to be regarded as really existing.

Farther, I did not find this peculiar to the brain, but from repeated experiments I have observed the same in the larger veins, *viz.* in both the *cavæ*, the subclavians, the upper part of the basilic, and the jugulars; for all these swell during

* Ibid. pag. 516.

expiration, and become of a deep blue colour, whereas in inspiration, being more empty, they subside and look pale. The phænomenon therefore which SCHLICHTING has observed is not peculiar to the brain, as I took notice of before, and seems intirely to depend upon that facility with which the blood passes from the right ventricle of the heart into the lungs, which are dilated while we inspire, and upon that with which the *venæ cavæ* empty themselves into the same ventricle in the time of inspiration^t. On the contrary, in expiration the lungs being compressed, resist the passage of the blood from the heart, and the blood contained in the heart resists that returning from the extremities; hence the large veins, and amongst these the jugulars swell, and thereby the brain is rendered turgid with blood^u. I know very well that by prolonging inspiration voluntarily, we retard the blood which passes through the lungs^x; but in the ordinary alternate motion of respiration, the blood enters more easily into the lungs during inspiration, although in a preternatural state, when the lungs are filled with blood, and for want of expiration, it cannot pass to the left ventricle, there must thereby be produced a dilatation of the right, and a stagnation in the veins, almost equal to that which naturally accompanies expiration.

^t Primæ Linæ Physiol. N° 292.

^u Ibid. § 297.

^x Ibid § 294.

Allow me to add in two words, that the longitudinal sinus has no pulsation, even after the *cranium* has been removed, and that when it is pierced, the blood does not fly out in jirks, but runs in a continued stream, as when one opens a vein; and this confirms what I published before against the pulsation of the sinuses of the brain^x. It is owing to the same cause, that the small arteries which go from the *dura mater* to the *cranium*, and of which the greatest number take their origin from the surface of the large *sinus*, may be filled with injection, without its ever penetrating into the sinus itself.

The followers of STAHL, and others, especially GOHL, who deny the existence of the animal spirits, conceive the nerves to be like tense cords, which are put in motion by the impressions of objects, and communicate their vibrations to the *meninges* of the brain, which they look upon as the organ of all our sensations. This theory I have refuted by several arguments, which I see have not only satisfied Dr. FLEMMING^y, but likewise the most modern patrons of the doctrine of the soul governing the body, admit the animal spirits, as Dr. WHYTT.

But there is still another argument, which proves more fully, that the faculty of sensati-

^x Comment. ad inst. BOERH. N^o 234.

^y Of the nature of the nervous fluid, Lond. 1751. 8vo.

on, whatever it is, does not reside in the membranes of the nerves. With regard to the *dura mater* I am fully convinced, that it does not form the external coat of the nerves, although most other anatomists are of a different opinion. But there remains the *pia mater*, which actually sheaths each of the medullary fibres, which are so very slender, that we can count near a hundred of them in one of the branches of the fifth pair. If therefore I can shew that the *pia mater* is void of sensation, there can be no reason given, why the sensibility of the nerves, which is owing to the *medulla*, should be ascribed to the membranes. In order to this, I have tried the following experiment upon dogs and kids, and frequently repeated it.

I laid bare the *pia mater*, by removing a part of the skull and the *dura mater* contiguous to it; I touched it with butter of antimony (which ought in this case to be preferred to the oil of vitriol, as this consumes the membranes too fast, and it is almost impossible to prick the *pia mater* with a knife without wounding the brain at the same time) whereupon it was burnt to an *escar*, without the animal's complaining in the least, making any sort of struggling, or being at all convulsed. But upon touching the brain, in whatever manner I did it, the animal was instantly seized with violent convulsions, which bended its body to one side in the form of a bow.

If

If the *dura* and *pia mater*, and likewise the *periosteum*, are found void of sensation, it appears evidently, that the sensibility of the *other membranes* must be given up. This conjecture is confirmed by repeated experiments, which I have made very carefully upon the *peritonæum*, after removing the *recti* muscles, upon the *pleura* freed from the intercostal muscles and nerves (which is a difficult experiment, but I have frequently done it, and most successfully on a kid, it being a very quiet animal) and even upon the *pericardium*, in all which cases the membranes being cut or irritated, occasioned no sense of pain, nor the least change in the animal. The celebrated STORCH, as appears by the journal of the disease of which he died, was not sensible of any pain, while, in performing the *paracentesis*, the *trocar* pierced the *peritonæum*. Upon this occasion I foresee that many eminent physicians, who place the seat of the violent pain of the pleurisy in the membrane of the *pleura*, will differ from me in opinion. But I only relate facts which I have actually seen.

Neither ought these truths to be looked upon as paradoxes, because they contradict some pathological doctrines. BOERHAAVE² affirmed long ago, that in inspiration the *pleura* was less upon the stretch, because by the ribs at that time approaching nearer to one another,

² In his *Praxis Medica*, 1745, tom IV. pag. 162.

the intervals between them were thereby diminished; and that on the contrary, in expiration, that membrane was more tense, because then the ribs receded to a greater distance from each other. But in the pleurisy the patient suffers most in inspiration, that is, when the *pleura* is least distended, and *vice versa*; wherefore that great man did not place the seat of this disease intirely in the *pleura*, but he likewise joined to it an inflammation in the muscles which serve to bring the ribs nearer to one another. But according to my system, in order to explain this phænomenon, it is sufficient that the intercostal nerves suffer, be the case as it will in other respects.

The *mediastinum*, being a very fine membrane, and very much resembling the *omentum*, I consider in the same light with the *pleura*. All these membranes being destitute of nerves, are of the same nature with the cellular membrane, and therefore are not sensible of pain.

The *arteries* and *veins* seem not susceptible of pain; for upon irritating or laying hold of a nerve the animal complains, but if you lay hold of an artery it does not feel it. The sensibility which is occasionally met with in the membranes of the carotid, the lingual, temporal, pharyngal, labial, and thyroidal arteries, and of the *aorta* near the heart, depends upon the nerves which I commonly demonstrate there,
and

and which do not appear to make any farther progress. In those places it is natural to imagine, that the arteries must be sensible, seeing the nerves run immediately upon them, but otherwise they are endowed with little or no sensation. I have frequently ordered the arteries to be tied very tight in men, without their complaining of pain in the least.

The *internal membranes* of the *stomach*, *intestines*, *bladder*, *ureters*, *vagina* and *womb*, being of the same nature with the skin, ought of consequence to enjoy the same sensibility.

The *heart* is likewise sensible, which I don't say from my own experiments, but from those of other persons ; and it is natural to think it should, seeing it is a muscle, and has nerves bestowed upon it. The reason why I have not discovered it myself is, that an animal whose *thorax* is opened is in such violent torture, that it is hard to distinguish the effect of an additional slight irritation.

On the other hand, I am fully convinced, from a great number of experiments, that the *viscera*, properly so called, *viz.* the *lungs*, *liver*, *spleen* and *kidneys*, have very little sensation ; seeing I have irritated them, thrust a knife into them, and cut them to pieces, without the animal's seeming to feel any pain. Dr. ZIM-

MERMAN

MERMAN^a has tried the same experiments with the like success. Hence ulcers of the lungs are not attended with any pain, and persons have had stones in their kidneys for several years without ever being sensible of them.

If it should be objected that there are nerves in these viscera, my answer is, I do not alledge that they are void of all sensation, but only that it is very weak in them, *viz.* such as one would expect in a part which has very few nerves bestowed on it in proportion to its bulk. For all the *viscera* have large blood vessels and small nerves, the liver not excepted, but especially the spleen and the kidneys.

The *glands* in general have only an obtuse sensation, owing to the few nerves which are bestowed upon them. Hence schirrous and encysted tumours are so indolent, and it is very surprising, that M. DU BORDIEU, so severe a critic of the writings of others, should lately have established it as an axiom, that a great many nerves are bestowed upon the glands, and upon this founded a system to explain the nature of their functions, in which he alledges, that it is not compression, but irritation that makes them discharge their liquors. In the mean while it is easy to prove, that there are no considerable nerves bestow'd upon the largest of the glands nor the *thymus*, as far as hitherto we have been able to discover; that

^a Loc. cit. p. 17.

the nerves which go to the thyroid gland are a great deal smaller than those of a muscle ten times less in bulk than that gland, and that there is not a gland in the whole body, in which we can demonstrate any nerve of the larger sort. Besides upon opening the mouth, when we are not at all hungry, we can see the *saliva* squeezed out, by the sole compression of the *digastric* muscle.

The breasts are covered with a great deal of skin, and furnished with many nerves. The *pænis*, which has likewise a great deal of skin, and receives a greater number of nerves than any other part of the body of an equal size, has also a proportionable degree of sensibility. The *tongue*, which has likewise a great many nerves, is endowed with a sensation more acute and delicate than the touch, which form the taste. We may judge of the sensibility of the *eye*, and especially of the *retina*, from the irritation and inflammation occasioned by too glaring a light. The *tunica choroidis* and the *iris* seem likewise to be sensible. But I do not find that the *cornea* has any nerves, and hence it may be pierced with a needle without occasioning any pain; and what persuades me that the *iris* is much less sensible than the *retina*, is, that if after having pierced the *cornea*, you irritate or cut the *iris*, it is not thereby contracted, whereas the least increase of light makes it contract; which evidently proves, that this contraction does not depend upon the proper

proper sensibility of the *iris*, but on that of the *retina*. The *gutta serena* serves likewise to prove the same thing; the *iris* being no ways changed in that disease, any farther than that it is deprived of motion, from the sensation of the *retina* being destroyed by a palsy of the optic nerve.

The *nerves*, which are the source of all sensibility, are themselves of course extremely sensible. It is impossible to represent to one's self, without having seen it, the vast pain and disquietude which an animal is thrown into, upon touching, irritating, or even tying a nerve. I have learnt from experience, that by tying a considerable branch, not only of the eighth pair, but even of the extremities, dogs have died after a few days; which has made me still more afraid than I was before of tying the nerves so common in amputations. But if a nerve is cut, and irritated below the section, the animal feels no sensation therefrom, which is a proof that pain is not propagated from one nerve to another by their anastomosing.

We have seen that the sensible parts of the body are the *nerves* themselves, and those to which they are distributed in the greatest abundance; for by intercepting the communication between a part and its nerve, either by compression, tying, or cutting, it is thereby immediately deprived of sensation, which is a fact proved

proved by well known experiments, some of which you may see in my commentaries upon BOERHAAVE^b. Wherefore the nerves alone are sensible of themselves, and their whole sensibility resides in their medullary part, which is a production of the internal substance of the brain, to which the *pia mater* furnishes a coat.

SECTION II.

I proceed now to irritability, which is so different from sensibility, that the most irritable parts are not at all sensible, and *vice versa*, the most sensible are not irritable. I shall prove both these propositions by facts, and at the same time I shall demonstrate, that irritability does not depend upon the nerves, but on the original fabric of the parts which are susceptible of it.

In the first place the nerves, which are the organs of all the sensations, are not at all irritable. This will appear surprizing, but at the same time it is very true. If you irritate a nerve, the muscle to which it is distributed is immediately convulsed. I never saw this experiment fail, and I have frequently by this means seen the diaphragm and abdominal muscles of a rat, and the hind and fore legs of a frog, thrown into convulsions. You may see experiments of the same nature in SWAM-

^b De irritabilitate n. 284. not. g.

MERDAM, and in making them I have found, as OEDER^c mentions, that the irritation of a nerve communicates the motion only to those muscles to which it is distributed, and does not effect those which derive their nerves elsewhere.

I have likewise constantly remarked, that the convulsion of a muscle only took place when the nerve was irritated with a knife, and not when the experiment was made with corrosives.

But if you irritate the nervous fibres which are scattered through a muscle, there is thereby no contraction produced in the nerve. I have often carefully observed in dogs, and more especially in frogs, that whatever irritation I gave to a muscle, it never communicated the least motion to the nerve.

I afterwards tried the same experiment which ZINN made at Berlin, viz. I applied a mathematical instrument, marked with very small divisions, lengthways to a long nerve of a living dog, in such a manner as I could thereby perceive the slightest contractions; and upon irritating the nerve, it remained perfectly quiet.

These experiments clearly prove, by the by, that the force of oscillation, which has been attributed to the nerves, is not agreeable to experience.

Neither the skin, which is the seat of feel-

^c De irritabilitate n. 22. p. 5.

ing, nor the nervous membranes of the stomach, intestines, and *Urethra*, are irritable. Here we must take care not to confound with this property, a kind of vermicular motions produced by the corrosion which oil of vitriol, or spirit of nitre, communicates to the nerves, or arteries, when they are cut into pieces, or the contraction of the skin of the membrane of the *Urethra*, of the bladder, or gaul bladder, occasioned by the same application. The lungs have been constricted by oil of vitriol after death according to ZIMMERMAN^d. And a motion has been observed some hours after death, in the skin, fat, and tails of animals^e. Wherefore this corrosive force has nothing in common with life, and continues to take effect twenty four hours after death, as I have learnt by experience.

Neither is irritability proportioned to sensibility. The stomach is extremely sensible, and the intestines are rather less so, seeing they certainly are not liable to such violent pain, and yet I have found them to be more irritable. The heart, which is very irritable, has but a small share of sensation, and upon touching it in a living person, a fainting is thereby occasioned rather than pain.

Farther, we must not conclude that a part is sensible, because it is irritable; for the tying or cutting of a nerve, which destroys the sensibility of that part to which it is sent,

^d De irritabilitate p. 17.

^e *Ibid.* p. 13.

does

does by no means destroy its irritability. I have frequently repeated BELLINI's experiment, but the event was somewhat different from what is commonly related, viz. I laid hold of, and compressed the phrenic nerve of a living animal, or of one very lately dead, for the experiment succeeds alike in both cases; this compression irritating the nerve, put the diaphragm in motion; if I tied the nerve the same thing happened; if I cut it, and irritated it below the section, where there was no sensation left, because the communication with the brain was thereby destroyed, the diaphragm was convulsed the same as before. In the same manner by cutting the crural nerve of a dog, you thereby deprive the limb of all sensation, and you may tear it as much as you please without putting the animal to pain; but at the same time if you irritate the nerve which has been cut, the muscles of the leg are seized with a trembling motion; wherefore at that time it is irritable, though quite insensible. This experiment of BELLINI has been too much embellished. It is true that the compression and irritation of the nerve puts the diaphragm in motion, but this is equally the case whether you press the nerve from above downwards, or from below upwards. The experiment succeeds better if the nerve is tense than if it is relaxed. If you compress the nerve, and irritate it above the compressed part, in whatever manner you do it, the dia-

phragm remains without any motion, and therefore it is not true what FREDERICK ORTLOB^e has said, viz. that it is put in motion if you direct the compression of the nerve downwards, and ceases upon sliding the finger towards the upper part of the *thorax*.

Lastly, in small animals I have tied the trunk of the nerves which go to the extremities, and thereby rendered the limbs insensible and paralytic. Afterwards I have irritated the muscles, and seen them contract the same as before, though they were no longer subject to the command of the will.

I have tried experiments of the same kind upon parts separated from the body. The intestines in this state after being deprived of all communication with the brain, preserve their peristaltic motion; and if you touch them with a knife or corrosives, they put on the same appearances, as if they were in their natural situation, and still preserved their connexion with the nerves and brain^f. The same thing is to be observed in the heart, and in any other muscle cut off from the body^g. In an eel, the heart continues its motions during the space of several hours, with the greatest regularity, even after it has been pulled out of the *thorax*.

If therefore we say that an animal only feels,

^e In præf. ad *anatomien. rationalem* DANIELIS TAURY.

^f WOODWARD'S supplement, pag. 76. ^g ZIMMERMAN, pag. 19.

when any external impression is represented to the mind, certainly that part of the body must be void of sensation, whose communication with the brain is destroyed by the nerve being cut, or the part being taken quite out of the body. In asserting that there was no motion of our body but by the soul, Dr. WHYTT has found himself obliged to admit the divisibility of the soul, which he believes to be separable into as many parts as the body^h. I have frequently repeated the experiment just now mentioned: *viz.* I pulled out the intestines as quickly as I could, and cut them into four or eight pieces, all which moved separately, still preserved their peristaltic motion, and contracted in whatever manner they were irritated. Dr. WOODWARDⁱ tried experiments of the same kind before in the intestines, and BAGLIVI^k upon the heart of a frog, as did AURELIUS SEVERINUS^l long before the others. I have seen the heart divided into several small pieces, each of which moved upon the table. M. LUPS^m has observed in the membranes of an egg an irritability which cannot proceed from any nerve, because there is no such thing there; but I have had no experience upon this article. I find that BAGLIVIⁿ has made use of the same arguments to establish the feat of irritability in the solids, and we ought to take care not to

^h L. C. pag. 383. ⁱ L. C. pag. 80. ^k De
fibra motrice pag. 7. ^l Vipera pythia, pag. 119.
^m L. C. pag. 34. ⁿ De fibra motrice & morbosa, p. 7.

call in here the analogy of insects, which are both irritable and sensible all over°.

The soul is a being which is conscious of itself, represents to itself the body to which it belongs, and by means of that body the whole universe. I am myself, and not another, because that which is called I, is changed by every thing that happens to my body and the parts belonging to it. If there is a muscle, or an intestine, whose suffering makes impressions upon another soul, and not upon mine, the soul of that muscle or intestine is not mine, it does not belong to me. But a finger cut off from my hand, or a bit of flesh from my leg, has no connexion with me, I am not sensible of any of its changes, they can neither communicate to me idea nor sensation; wherefore it is not inhabited by my soul nor by any part of it; if it was, I should certainly be sensible of its changes. I am therefore not at all in that part that is cut off, it is intirely separated both from my soul, which remains as entire as ever, and from those of all other men. The amputation of it has not occasioned the least harm to my will, which remains quite entire, and my soul has lost nothing at all of its force, but it has no more command over that amputated part, which in the mean while continues still to be irritable. Irritability therefore is independent of the soul and the will.

These same experiments farther prove, that

° Theolog. des insect t. 2. pag. 84, 85.

the whole force of the muscles does not depend upon the nerves, because after these have been tied or cut, the muscular fibres are still capable of irritability and contraction; and some time or another perhaps, the use of the nerves with regard to the muscles will be reduced to convey to them the commands of the soul, and to increase and excite that natural tendency which the fibres have of themselves to contract, in whatever manner that is brought about.

But I return to my experiments, whereby I have discovered which are the irritable parts of the human body, and in what degree they are so.

I have excluded the skin. The cellular membrane and the fat, which the oil of vitriol devours so greedily, is generally agreed to be void of motion, at least a gentle irritation does not affect it. So neither the lungs (tho' violent acids constrict them) the liver, the kidneys, nor the spleen, have any irritability; because they are composed of the cellular substance, which, as I said just now, is not endowed with it, and of vessels which are equally void of irritability.

This character of irritability seems to me to be that which distinguishes the cellular fibres from the muscular, which resemble each other so much, that they are confounded together every day, as appears for example in the *dartos*, which so many still look upon as a muscular

membrane, as also in GLISSON's *capsula*, and the round ligament of the womb, in which a great number of anatomists have been at a vast deal of pains to discover muscular fibres.

The disposition of the cellular membrane to irritability; is precisely the same with that of the fibres of dead flesh; it yields to the touch, dimples if you press it, and recovers itself if the pressure is removed. If you cut it, the fibres retract on both sides, and leave an empty space between. But the muscular fibres, if you irritate them in a living body with a knife or corrosives, become shorter; their extremities approach nearer to one another, presently they are relaxed again, and these contractions and relaxations are repeated alternately for some time,

The *tendons* are as void of irritability as they are of sensation. No irritation made with a knife or with any gentle corrosive can excite any convulsion in them, or produce any motion in the muscle to which the irritated tendon belongs. If the electric spark produced from the *tendon* is violent, the celebrated M. JALLABERT has observed, that sparks of the same kind may be produced equally as violent from other parts of the body which are more solid and hard.

The ligaments, *periosteum*, *meninges* of the brain, and all the membranes, being composed of the cellular membrane, are void of irritability: and the following experiment may serve to convince those of their error, who have fancied that they
have

have observed muscular fibres in the *dura mater* and *pericardium*; viz. if you cut, burn, or tear those membranes, you can observe no sensible motion excited in them thereby. I have repeated this experiment an hundred times, and the same has likewise been done by Mess. ZINN, WALSTORF, CASTEL, OEDER and others, always with the same success.

The *arteries* by some are thought to be irritable, both upon account of their muscular membrane, and the necessity of finding the cause of their contraction, which is constantly alternate to that of the heart; and we know that Drs. SENAC and WHYTT^o have looked upon this irritability as essential to the arteries. The first of these authors takes it for a more efficacious cause of the circulation, than the heart itself; and I own that this system has an appearance of reality. The intestines, which by their peristaltic motion push on their contents; the principal artery of the silk worm, which performs the office of a heart; and animals in whom after the heart has been cut out, the motion of the fluids has been continued for some time, which appears to be owing entirely to the arteries; lastly, local inflammations occasioned by irritating topics, form so many analogies, which unite to render this system probable. Upon examining with the microscope the blood in a fish, and in a frog, after the heart was pulled out, it continued for some time to move in the vessels,

and I saw it pass up and down the vessels of a small fish, which had no motion neither in the heart nor gills, nor shewed the least sign of sensibility.

In the mean while all those facts do not yet prove the irritability of the arteries. Irritate the *aorta* of any animal you please, either in its internal or external surface, and either with a sharp instrument, corrosives, or hot spirit of nitre, you will not be able to observe any contraction thereby produced, except that from oil of vitriol, which I mentioned before^p, and which succeeds equally as well, if the experiment is made several hours after the animal has been quite dead. In living frogs I have frequently irritated the arteries with alcohol, spirit of nitre, and other acrid liquors; and though I examined them attentively during the time with a microscope, I could never discover any motion thereby occasioned in them, while the blood which they contained was changed into a thick jelly of the colour of earth.

Farther, upon examining the circulation in animals with the microscope, I have never observed any contraction in the arteries. I have viewed the circulation for whole hours in fishes and frogs, and during the whole time the sides of the vessels remained as quiet as those of the tube with which I examined them. If the pulse of the artery had occasioned any motion in the neighbouring vein, I could not have failed to

^p I. G. ZIMMERAAN p. 24.

discover it with the microscope. With regard to the experiment which HEYDE^a relates, *viz.* that upon cutting an artery of a frog, it contracts itself so as to become impervious, I have frequently seen the event very different, the orifice of the artery retaining its former figure, without being either contracted or dilated.

Wherefore, although I will not absolutely deny the irritability of the arteries, yet I do not at all see that it is confirmed by experiments.

Neither am I more convinced that it obtains in the *veins*. It is true I can perceive a motion in them, which depends upon respiration, and I have frequently observed, especially in frigid animals, that of the *vena cava*, which contracts near the heart, and drives the blood which it contains into the auricle. We likewise know that upon touching the veins with some very sharp corrosive, as spirit of vitriol, or hot spirit of nitre, they contract much more sensibly than the arteries, and push on the blood, as I have seen both in a kid, and also in a cat. But as neither the knife, nor gentle corrosives injected, produce such a contraction, and there is no acrimony of that force in any of the humours of the human body, I look upon the veins as intirely void of irritability, or at least very gently endowed with it.

The *laeteal vessels* contract and empty themselves, upon being touched with oil of

^a Obs. 35.

vitriol, and what proves that they have a considerable share of irritability is, that though they are ever so full of chyle at the time when the animal dies, they empty themselves, and contract in such a manner, that you cannot discover any cavity remaining.

The different *excretory ducts* are not endowed with more irritability than the veins. The gall-bladder, the *ductus coledochus*^r, the ureters, and the *urethra* contract themselves upon being touched with a very acrid corrosive, but the application of a weak one, or the knife, produces no change in them.

The ureters cannot be irritated even by oil of vitriol, and therefore they seem to be destitute of any muscular force, as indeed there has never been any muscular fibres plainly shewn in those canals.

I have convinced myself by an experiment of the nature of the bladder. For by pricking it with a knife or needle, in a dog that was almost dead, I have seen it, though not always, very frequently contract itself considerably, and drive out the urine through the opening of the abdomen. I have likewise observed it immediately after death contract itself spontaneously, and empty itself of all the urine with which it was full before; which observations were formerly made by WEPFER^f, and I have quoted them before from him.

^r I. G. ZIMMERMAN 46 ab oleo vitrioli.

^f De cicuta aquat. p. 250.

The watering of the eyes, which irritating *effluvia* or snuffs produce, and the discharge of *mucus* from the *urethra*, occasioned by sharp injections, together with other *phænomena* of the same kind, prove that the glands and mucous sinuses in man are irritable; but in the experiments which I have tried upon living animals, I have not discovered any such property in these parts.

The *uterus* in quadrupeds is irritable, and at least as sensible of motion as the intestines, whether it still remains in the body, or has been cut out of it. The strong contraction of the human *matrix*, which in a great measure expels the child, and is so evidently felt by the hand when it is introduced into it, proves its irritability, and this determined RUYSCH to leave the delivery of the after-birth to nature, as every body knows.

The Irritability of the genitals seems to be of a particular nature, in so far as voluptuous ideas are the most proper *stimulus* to put them in motion. However so far it resembles that of the other parts, as it is rendered active and produces an erection, if it is excited by a quantity of urine, or *semen*, the acrimony of cantharides, or the sharp discharge of a *gonnorrhœa*. But in whatever manner these irritations are produced, the effect of them is always to constrict the veins, and to retard the motion of the blood through them. As Dr. WHYTT ascribes the erection of the *penis* to a greater afflux of arterial blood, he
seems

seems to be unacquainted with the experiments, whereby it appears, that by tying the *penis* either in men or brutes, an erection is produced, and that in the *paraphimosis*, the stricture of the prepuce occasions a prodigious swelling of the *glans*, though a greater afflux of arterial blood cannot be suspected in either case.

But all the *muscles* are irritable. I do not know one that has not a natural palpitation after death; they all tremble, and are alternately contracted and relaxed. I have observed it in the temporal, pectoral, and sternocostal muscles, in the *recti abdominis*, *cremaster*, and *sphincter* of the *anus*. Dr. WHYTT^t has observed the same in the *sphincter* of the bladder, and other anatomists in other muscles of the body. With regard to the sternocostals, I have often seen with pleasure, that upon cutting away the *sternum*, they still preserved a sufficient force to bend the cartilages of the ribs, and pull them inwards. The same muscles preserved their Irritability a long time, nay even longer than the diaphragm. Farther, as the flesh in general of animals palpitates spontaneously after death, so it appears from an old experiment, which is known to every body^u, that this motion may be revived after the muscles have been some time quiet, either by irritating the nerve which goes to a muscle, or the mus-

^t P. 93. ^u HIGHMOR, *disquis. anat.* p. 137. B. LANGRISH *de mot. musc.* p. 51. WOODWARD p. 74. l. c. PARSONS *de mot. musc.* p. 68. W. CROONE *de mot. musc.* p. 10. MAZINI *de mechan. medic.* p. 13. HUGHES of Barbados p. 309.
cle

cle itself, with a knife, or corrosives. Dr. ZIMMERMAN^x has made experiments of the same kind with mine, as also Dr. WOODWARD^y upon the muscles of oxen, CROONE^z upon a muscle of the human thigh, and BREMOND^a upon a frog; and OEDER^b observed the muscles to be violently convulsed upon being touched with salt. In making the former experiment it is of little consequence, whether the nerve is intire and communicates with the brain, or whether it has been cut through^c. In both cases the muscular fibre contracts, its extremities approach nearer to one another, and the succession of these motions represents a kind of undulation on the surface of the muscle. On examining in a frog with a microscope the muscle thus convulsed, you do not see that the blood is pushed out; and it circulates as before. There is no animal in which the muscles grow pale during their action, and I have long ago observed, that the paleness which HARVEY^d said he had observed in the heart during its contraction, has been a source of errors into which the greatest men have fallen.

In most part of the muscles the Irritability is so great, that after one single irritation, the muscle contracts and is relaxed several times, by oscillations which diminish gradually, till they intirely cease^e. This alternate motion is very

^x P. 19.^y P. 73, 74, 75, 76.^z *De mot. muscul.*

p. 30.

^a Mem. de l'Acad. des Sciences 1739, p. 476.^b P. 2.^c CL. OEDER p. 5.^d *Comment. BOERH.*^e *n. cordis* 400. *Phys. prim. lin. n. 4.*^e WHYTT pag. 18.

evident

evident in the *recti* muscles of the *abdomen*, the sternocostals and others, where there is no difference in the direction of the fibres, which therefore HAMBERGER^e, and another author, had no manner of occasion to introduce into the structure of the heart, seeing the muscles just now mentioned oscillate perfectly well, though all the fibres are straight and parallel to one another. In the mean time Dr. WHYTT^f is mistaken when he says, that this oscillation takes place in all the muscles, for it does not happen in the bladder, which after it begins to act, continues its contraction without interruption till all the urine is expelled.

The *iris*, which you will be surprized at, has no Irritability, when the cause of the irritation applied to it is mechanical, *viz.* a knife or the like, as I said before. Now that I mention the *iris*, I have observed, contrary to Dr. WHYTT's^g opinion, that its dilatation does not depend upon muscular force, because after death it remains very large. This I had remarked before a great many times, and I have a proof of it before me in a cat which was tortured to death, and has the pupil opened so wide, that you can scarce see any part of the *iris* at all. In a frog it also appears to be void of Irritability.

There are some muscles which have a stronger contracting force than others, and retain it a longer time after the animal is dead. The chief of these I reckon the *diaphragm*, which I have always observed to continue to move a long while

^e In progr. *de causa dilat.*

^f Pag. 243.

^g Sect. 7.
after

after the others when the animal was dead, or at least by irritating the nerves the motions could be renewed. I have seen it irritable and tremble an hour or more after death, when the motion of the intestines had intirely ceased; and Dr. ZIMMERMAN^h has been witness of the same along with me. WEPFERⁱ has observed long ago, that the diaphragm contracted after the stomach was cut out. At the same time I must own, that I have sometimes seen in animals while they were still warm, other muscles, and the œsophagus continue their palpitations, after the heart was perfectly quiet, an example of which is related by OEDER^k. But for the most part the diaphragm, heart and intestines may be irritated, or the heart and intestines palpitate spontaneously, after the motion of all the other parts is quite ceased.

The *œsophagus*, if it is irritated above the diaphragm, contracts itself very sensibly, and I have plainly seen its peristaltic motion without any irritation, and a morsel contained in it pushed upwards and downwards, by the peristaltic motion renewed upon irritation, which I think is sufficient to refute the opinion lately advanced against the motion of this muscular tube.

The *stomach* is considerably irritable, and when touched with a corrosive, its impression immediately produces a long superficial furrow in it.

^h P. 19.
porali p. 4.

ⁱ *De cicut. aquat.* p. 195.

^k *De tem-*

If you irritate it with a knife, either at the *pylorus* or elsewhere, it presently contracts; and I have seen it, especially upon touching the left side of the *pylorus* with a corrosive, contract itself circularly. If after you have opened it, you touch it with a corrosive, it throws out a froth, and the borders of the wound are rolled together*, as do those of the intestines. I have likewise seen the stomach shining through the diaphragm while the *abdomen* was intire, agitated with a peristaltic motion, and the same may be observed upon viewing it through the *peritonæum* after it has been laid bare, which contradicts the opinion of SCHWARZ, who attributes that motion to the access of the external air. I have seen it very evidently in a cat, a rabbit, and a rat, continue more than an hour after that of the intestines had ceased.

There is however in the stomach something less active, compared with the intestines; and upon irritating it in a frog with corrosives, it does not at all contract. I have frequently given these substances to them, and never but once saw them produce those motions which occasion vomiting, and consist in strong and short succussions repeated from time to time. Another time I saw the corrosive sublimate constrict the stomach, and make it perfectly flat.

The *intestines* both large and small, and the *cæcum* in those animals where it is large, are extremely irritable; for after having cut the mus-

* Et revolvit labia vulneris.

cles of the abdomen entirely away, I have seen the excrements driven out by the force of the intestines alone, as WEPFER and STHAL¹ have observed before. We may add to these facts, so contrary to the system of those who attribute too much to the contraction of the abdominal muscles, that in the case of an obstinate costiveness, when the excrements resist the most violent straining, and repeated efforts of respiration, there is often nothing more required to expel them, than rousing the Irritability of the intestines by a glister. There is no part in the animal body which continues longer to move, frequently longer than the heart, as I have observed fourteen times; and when the heart moves the longest, it appears to me to be owing to the *abdomen* being first opened, and the intestines thereby rendered cold^m. Nevertheless the brisk and perpetual motion of the heart, together with other reasons, give it the preference to the diaphragm. Opium, which destroys the peristaltic motion of the intestines, and almost the whole Irritability of the body, leaves the force and motion of the heart intire, as I have frequently seen. In several experiments, the motion of the heart has continued longer than that of the intestines, whereof I find seven examples in the journal of my dissections.

Very often after the motions of the intestines have ceased, they are renewed again, and in-

¹ Theor. vit. & mort.

^m Comp. OEDER p. 5. &

I. G. ZIMMERMAN.

creased by little and little, either by the cold or some obscure cause irritating them. Besides, after they have been taken out of the body, I have observed this motion rather to increase, which according to the common received opinion ought to be totally destroyed, and Dr. FELIXⁿ, my pupil, has already made this remark. They may likewise be irritated externally either with a knife, a needle, alcohol, or corrosives; but their internal surface is much more irritable. Upon opening the intestinal canal, and dropping a corrosive into its cavity, the bile descends and flows out with a great deal of froth, and is alternately resorbed into the intestine. I never saw the peristaltic motion more plain than in a cat, which had swallowed corrosive sublimate. The lips of the intestine where it is cut through turn back, so as to embrace the part immediately above the section, and thus being turned inside out, readily adhere to the neighbouring parts. When the intestine is only slightly wounded the lips are equally retracted.

But it is so difficult to observe the peristaltic motion, that it is very hard to reduce it to any certain rule. In general however the intestines are evidently constricted where that motion obtains, while the part below the constriction is dilated, and receives the contents which the constricted part sends to it. Thus, when a part of the intestine is irritated, it contracts so strongly there, that the cavity is quite shut up, and the contents are pushed into the neighbouring parts, either up-

ⁿ De motu peristaltico n. 11.

wards or downwards, which dilate, and soon afterwards, in consequence of this dilatation, contract themselves, and push along whatever is contained in them.

I have seen an intussusception in a rabbit, which had poison given it. In this case the contracted part of the intestine is received into the neighbouring part which is larger, and it is easily disengaged; but in the mean time it squeezes the contents equally upwards and downwards. It is likewise certain, that the intestine changes its situation from side to side, moving itself alternately from the left to the right, which motion renders the longitudinal fibres very conspicuous, as the constriction does the transverse.

In frigid animals the intestines appear to me to be proportionably less irritable. An hour after having opened the belly of a frog, I still found the stomach and the intestines to be irritable, but the motion of the heart continued a good deal longer.

Thus by degrees I am at last got to the most irritable organ of all, *viz.* the heart, which as it is the cause of all motion in the human body, so is the best constructed for it, and therefore ought to be endowed with the greatest Irritability. That it is actually so, appears from experiments, especially in frigid animals, in which it is constantly irritable, and much more than the intestines. For first, in these animals it moves a great deal longer than any other part of the body, even after death, and sometimes for

four and twenty, ° or thirty^p hours, or longer; but in animals whose blood is hot, it moves till the fat is rendered stiff by the cold, at which time the motion of all the muscles commonly ceases. I have observed in frogs, that generally the heart continues its motion from noon till pretty far in the night, but rarely till next morning. Farther, after the heart has ceased to move, its motion may very easily be renewed by external irritation, either with a knife, a needle, salt^q, poison, and sometimes even with simple hot water, according to Dr. WOODWARD^r. The auricle being irritated by poison, contracted itself several times successively, and I have seen the same thing happen in the heart. But very often in those irritations produced by poison, the motion which follows upon them is very short, oft enough local, and confined to the place alone which is irritated. The best method of renewing the motions of the heart, is to irritate its internal surface, and I have frequently succeeded by blowing into it, after having tried corrosives without any success; and the injection of other fluids, which have more consistence than air, produces the same effect. The motion of the heart is equally renewed, whether you inject water, or blow air into it, either by the *vena cava*, the *trachea arteria*, or the thoracic duct^s, which last experiment I tried upon a dog.

° In a large viper CHARAS *de la theriaque* p. 43. ^p In a turtle 1. CALDESI.

^q OEDER p. 3.

^r L. C. p.

52. ^s WEPFER *cicut. aquat.* p. 29.

In short, it is sufficient if the air gets any how into the left ventricle, which experiment usually goes by the name of Hooke's, and I have tried it myself frequently with success. This irritation of the internal surface of the heart, produces oscillations much more lasting, than those which are caused by irritating it externally, and they become weaker very insensibly. Neither does this irritation at all diminish the Irritability of the heart, whereas that which is produced by poisons, renders whatever part they touch almost incapable of motion.

It is difficult to determine which is the most irritable part of the heart. Anatomists have commonly preferred the right ventricle and auricle. But, if I am not mistaken, I have proved that in this respect the right has no advantage over the left, seeing the oscillations of this last continue longer, when the irritating cause has been applied longer to it than to the right^t. It does not appear that the weight of the liquor which is made use of contributes to increase the irritation, because air produces the same effect as water, although it is near a thousand times lighter, and the heart of a *fœtus* beats stronger and quicker than that of adults, whose blood is a great deal thicker and heavier. Hence I conclude that the different weight of the blood has no great influence upon the motion of this organ; and the experiments just now mentioned, made with air and water, prove that there is no need

^t See the essay published at the end of this treatise.

of acrimony in the fluids to produce an irritation of the heart; though at the same time I must observe that acrimony increases it, as appears from the example of salt. But acrimony and irritation do not increase in the same proportion; for hot spirit of nitre, applied to the internal surface of the heart, produces no contraction.

If you ask me whence proceeds this greater Irritability of the heart than of the other muscles, I shall find it very difficult to answer the question. The nerves are not more numerous there than elsewhere, and they are rather smaller than those of the muscles of the eye. Dr. WHYTT^u is of opinion that the nerves there are more sensible, and therefore can less resist any stimulus. But whence can this excess of sensibility in the heart proceed? Is it because the nerves are more exposed, and nearer the internal surface of the heart, and by that means nearer the irritating cause? Anatomy gives us but little light into this subject, unless we might take for example the auricles, which are both very thin and very irritable. What would induce me to adopt this hypothesis, is the great Irritability which is observed in the intestines, which have only few nerves, but these very much exposed. To convince ourselves how much this circumstance increases the sensibility, we need only examine the symptoms which arise, when the mucus of the bladder and *urethra* is abraded; and of the intestines, when upon being de-

^u Pag. 311.

prived of their mucus, their naked villous membrane oozes out blood. But it is difficult to support this system by anatomical facts; for so far from being able to demonstrate, that the last ramifications of the nerves are extremely exposed in the heart, we find it difficult to discover their principal trunks. Besides, of all animals an eel appears to me to have the heart and other muscles the least capable of irritation.

From all these experiments collected together it appears, that there is nothing irritable in the animal body but the muscular fibre, and that the faculty of endeavouring to shorten itself when we touch it is proper to this fibre. From the same experiments it likewise follows, that the vital parts are the most irritable; the diaphragm frequently moves after all the other muscles have ceased, the intestines and stomach move still longer, and lastly the heart continues its motions after all the other parts are quiet. This furnishes us with a distinct character between the vital organs and the others, *viz.* the first, being extremely irritable, require only a weak stimulus to put them in motion, whereas the others, which are endowed with very little Irritability, are not to be moved but by the determinations to the will, or by very strong irritations, the application of which is capable of producing in them violent commotions, known by the name of convulsions.

I shall easily prove, that this power of producing motion is different from all the other properties

perties of bodies^x. Elasticity, which seems to have the most resemblance to the other, differs from it in this, that it is proper to dry fibres, in which state they are deprived of all Irritability, as easily appears upon drying a frog. Besides elasticity is the property of hard bodies, and Irritability of the softest. The polypus is so irritable, that though it has no eyes, the light affects it very sensibly. Gelatinous animals are extremely irritable, but far from being elastic. Dr. WHYTT^y adds, that the motion of the heart ceases and is renewed spontaneously, which is not observed in any elastic fibre, and upon pricking steel with a needle you produce no irritation in it. Dr. BATTIE^z has observed, that Irritability is less considerable in old subjects than in young, although the fibres of the former are more elastic than of the latter.

But the muscular fibres being composed of earthy particles and a glutinous mucus, it may be asked in which of these Irritability resides. It appears most probably to reside in the latter, because this when it is pulled endeavours to shorten itself; whereas on the contrary, dry earth never changes its figure of itself, and being extremely brittle, when its parts are separated, they constantly remain so. This opinion is strengthened by observing that children, in whom the mucus predominates, are much more irritable than adults; which is evidently proved from the quick-

^x ZIMMERMAN in addend. OEDER p. 7. ^y P. 231, &c.

^z De princip. anim. p. 34.

ness of their pulse, which vibrates one hundred and forty times in a minute, whereas in old persons the vibrations are not above sixty or sixty-five in the same space of time. Farther, the most solid and earthy parts of our body, *viz.* the bones, teeth and cartilages, are void of Irritability, and the most irritable parts are deprived of that quality, only by robbing them of their mucus by drying.

We should next inquire how this mucus, produced from insensible lymph, can become irritable. Dr. WHYTT, and the followers of STHAL, alledge that it is owing to the soul, which being sensible of the touch, contracts the fibres that are touched, and pulls them back, to prevent their being injured.

However simple this theory may be, and however commodious for disembarassing us from several difficulties, yet it is not agreeable to the phenomena which are observed. For in the first place Irritability differs intirely from sensibility, and the most irritable parts are these, which are not subject to the command of the soul, which ought to be quite the reverse if the soul was the principal of Irritability. In the second place, Irritability continues after death, and parts quite separated from the body, and intirely insensible, after the heart is taken out, and the head is cut off, remain still irritable. There is nothing more common than to see the heart of a frog beat, and the muscles remain irritable, after the head has been cut off, and the spinal marrow taken out.

out. Dr. WHYTT^a shuns this difficulty with great address by saying, that the time of death is very uncertain, and that frequently an animal has life still remaining, after it has been looked upon as dead for some time; and this he proves by the example of persons who have been drowned, and those who fall into *syncofes*. But as it is certain that the seat of the soul is in the head, and that it has no command over the rest of the body, after the nerves have been cut or destroyed; and farther, as the Irritability remains intire after the head is lopped off, or the nerves cut through, it appears that this quality still subsists after the seat of the soul is removed, or its commerce with the body quite intercepted, and therefore it does not depend upon the soul. This is so evident that it is needless to add, that Irritability acts without the soul being sensible of it, and that it is not subject to the command of the will, both which are proved by the example of the heart. To avoid the consequences of this argument, the anatomists are obliged to introduce an insensible sensation, and involuntary acts of the will, that is to say, to admit contradictory propositions.

What therefore should hinder us from granting Irritability to be a property of the animal *gluten*, the same as we acknowledge attraction and gravity to be properties of matter in general, without being able to determine the cause of them. Experiments have taught us the existence

^a P. 367, 389, &c.

of this property, and doubtless it is owing to a physical cause which depends upon the arrangement of the ultimate particles, though the experiments that we can make are too gross to investigate them.

Irritability is destroyed by drying, by the congealing of the fat, but especially by the use of opium in a living animal. This medicine destroys so effectually the peristaltic motion of the intestines and stomach, that it cannot be revived again by any irritation. I have seen it myself, and Dr. KAAU BOERHAAVE^b has observed the same thing before. Once however in a cat I found opium not capable of subduing the peristaltic motion. It has the same efficacy in destroying the peristaltic force of the bladder; nay in a frog I have seen it destroy the peristaltic motion and irritability of the intestines, and the convulsive force of the nerves. Dr. WHYTT says, that it likewise destroys the Irritability of the heart, but that I could never discover^c.

To conclude, Some celebrated authors having asserted that Irritability was a property unknown till now, and having given me the honour of the discovery, while others have neither looked upon it as new, nor really existing, I have thought proper to add a short history of it here. Some obscure experiments which offered themselves naturally have been known

^b In impetum faciente HIPPOCRATICO.

^c P. 371, 372.

in all ages, and VIRGIL has observed the trembling of the flesh cut off from the body immediately after death, but I don't see that the ancients have tried any experiments to irritate the fibres and renew their motions. FRANCIS GLISSON^d, who discovered the active force of the elements of our bodies, was the first who invented the word *Irritability*, which he attributes to a *natural perception*, that is not accompanied with any sensation, and depends upon Archæus who is the framer of his own body^e. He distinguishes it into two sorts, the one of which depends upon the external senses, and the other upon the internal appetite^f. He likewise mentions some facts to prove, that this motion is produced independent of sensation, that after death the fibres contract upon being touched with acrid and pungent liquors; nay he makes this property so general, as to allow even the bones and human juices to be irritable^g. He likewise distinguishes the different degrees, and amongst the rest that excessive Irritability, which BOERHAAVE called *prurient*^h.

BELLINIⁱ mentions a *natural contraction*, and explains mechanically how acrid bodies, which might irritate the fibres, are repelled by means of this property, and thence he accounts how irritating substances may occasion contractions of the muscles, accelerate the motion of the

^d De ventriculo & intestin. c. vii. ^e N. 6. ^f N. 11.

^g C. 8. n. 1. ^h *Ib.* n. 6. ⁱ de *Stimulis* amongst his *opuscula* and in his book *de sanguinis missione*.

blood,

blood, occasion an inflammation, produce a revulsion, or some certain evacuation. But he does not confirm these opinions by any experiments. BAGLIVI^k by his experiments has approached nearer to a discovery of it. He observed pieces of the heart without the assistance of any nerve, tremble and oscillate, and preserve their alternate motion of contraction and relaxation^l; and he has remarked, that the muscular fibres contract themselves upon being touched, without the soul's having any share in it, or even being sensible of it^m.

Since that time all the followers of STAHL have enlarged much upon the tone and natural contraction of the fibres, but they attribute it to the soul, and as they have always had an aversion to anatomy, they have made no experiments to illustrate this quality.

Dr. BOERHAAVEⁿ has acknowledged an active force in the heart, and a latent principle of motion in the pieces of it which are cut; but as he attributes the cause of muscular motion to the nerves, this proves that he did not sufficiently know, that the cause of this motion was in the muscles themselves, that the nerves had no other office but to subject them to the will of the soul^o, and that they could indeed increase or diminish it, but were not the cause of it, because it is extended far beyond the nervous system, and we meet with it in in-

^k *De fibra motrice & morbosa.* ^l P. 7. ^m P. 12.

ⁿ Institut. rei med. n. 187. not. 1. 11. ^o *Ib.* n. 402.

fects which have not even a head. We find a number of curious experiments upon this subject, in Dr. WOODWARD's posthumous supplement, published by HOLLOWAY. Dr. STEWART^p has likewise observed several very useful facts, and he formerly remarked, that the fibres preserved their irritability after the nerves were detached from them.

We find scattered in several other authors, a good many things relating to this subject, which seem rather to have been dropt accidentally than wrote on purpose.

In my commentaries upon BOERHAAVE's^q Institutions, published in 1739, I have expressed myself as follows, *Wherefore the heart is moved by some unknown cause, which neither depends upon the brain nor the arteries, but lays concealed in the very structure of the heart itself.* The nature of the thing obliged me to differ in opinion from my preceptor. Three years afterwards I published the following doctrine^r, viz. *That all animal fibres when they were irritated contracted themselves, that this character distinguished them from those of vegetables, and that the perpetual irritation alone was the cause of the continuation of motion in the vital organs, while the animal organs ceased to act.* In my abridgement of the physiology^f I have positively ascribed the motion of the heart to the force of

^p De mot. muscul. p. 13. ^q Ad. n. 187. Instit. rei med not. i. 11. ^r Tom. 4. pag. 586. ann. 1743.
^f Ann. 1747. n. 113. pag. 51.

a *stimulus*, and in the second edition, I have been more explicit on the Irritability of the muscular fibres^t, asserting that it was independent of the nerves, and of every other known property. If any person denies the truth of this assertion, I shall be glad to be informed by him, what the property is upon which that motion depends. Since that time, numerous experiments made upon living animals, had convinced me of the truth of the doctrine above advanced.

It gave me no small pleasure to see, that almost at the same time Dr. DE GORTER^u published several observations to the like purpose, and that the celebrated Dr. WINTER^x, physician in ordinary to the house of Orange, in an oration *de certitudine in medicina practica*, attributed all the motions of the human body to the Irritability of the fibres, and the force of a *stimulus*. These two famous physicians have been followed by several others. Dr. KAAU^y, nephew to the great BOERHAAVE, has made a great number of experiments, although with another view; and not long ago the celebrated Dr. WHYTT^z has attributed all the motions of the human body to the force of a *stimulus*; but with this difference between him and the others, that he imputes Irritability to the soul, which feeling the impression of the irritation, occasions

^t N. 408, p. 252.

^u Exercitat. de motu vitali.

^x Franeker 1746. fol.

^y De impet. facient HIP-

POCRAT.

^z Of vital motions, Edinburgh 1751. 8.

the contraction of the fibre. I cannot help complaining of this gentleman's want of candour, both with regard to myself, and to several others, seeing he never mentions me but when he wants to criticise me, and has adopted several of my ideas, without mentioning whence he had them. He has made but a small number of experiments upon dying animals, which he has published with a view to support his system, but they have not been often enough repeated to be established as certain, and some of them are contradicted by those which I have made.

Two of my pupils, Drs. ZIMMERMAN and OEDER, have hit upon the proper method of arriving at the knowledge of this property, both these gentlemen endeavouring by experiments to explain this quality of a fibre, as a law of nature resembling attraction, without entering into useless researches in theory. CASTEL has likewise confirmed the experiments upon sensibility. The deceased M. DE LA METTRIE^a has made Irritability the basis of the system which he advanced against the spirituality of the soul; and after saying that STAHL and BOERHAAVE knew nothing of it, he has the modesty to assume the invention to himself, without ever having made the least experiment about it. But I am certainly informed, that he learnt all he knew about it of a young Swiss with whom I am not acquainted: who never was my

^a L'Homme machine n. 18. 22.

pupil,

pupil, nor is he a physician, but he had read my works, and seen some of the famous ALBINUS's experiments, and upon these LA METTRIE founded his impious system, which my experiments totally refute. For if Irritability subsists in parts separate from the body, and not subject to the command of the soul, if it resides every where in the muscular fibres, and is independent of the nerves, which are the *satellites* of the soul, it is evident, that it has nothing in common with the soul, and it is absolutely different from it; in a word, that neither Irritability depends upon the soul, nor is the soul what we call Irritability in the body.

SUPPLEMENT by the AUTHOR.

Having seen, since the publication of my essay on Irritability, M. LE CAT's objections to what I had advanced, in a paper which he sent to the Royal Academy at Berlin^b, I thought it incumbent upon me to answer it in a few words.

I do not know what motive could engage this author, or M. DELIUS, to refute me before I had published my opinion. They have either laid hold of some *theses* of my pupils, or expressions which have escaped me in some letter to a friend. This is certainly the case with M. LE CAT, and both of them might

^b At the end of an essay upon muscular motion.

have spared a great part of their criticism, if they had only waited till the publication of my treatise.

In my first essay upon this subject, I inquired if the *dura mater* or tendons were irritable, if they contract, when any external violence is applied to them, and if in effect they can act like muscles. This enters essentially into BAGLIVI's system, and I plainly aver the contrary. All the animals which I have examined, have the *dura mater* strongly attached to the *cranium*, and when it is detached from it, it is always void of motion. In vain do we attempt to irritate it with a knife, needle, or corrosives more or less gentle; there is thereby no commotion raised in the animal. The same is the case with the *pia mater* likewise. In M. LE CAT's experiment, the application of spirit of wine is scarcely felt in the *dura mater*, whereas it produces a very violent pain in the skin; which shews that the first has no sensibility when compared with the second. Upon irritating the *medulla* of the brain, and that of the spine, convulsions are thereby quickly produced; wherefore the cause of motion is in the *medulla*, and the *meninges* have nothing at all to do with it.

The second doctrine which I have advanced is this, *viz.* that wounds of the *periosteum*, tendons, ligaments and *dura mater*, do not hurt the animal, and are cured without any accident. The experiments which M. LE CAT has recourse to against me are of no signification.

They

They are vague and inconclusive. He must produce patients where a tendon, ligament, or one of the *meninges*, has been indisputably and singly wounded, and where troublesome accidents have ensued thereupon. What he says of the *dura mater*^c, is explained by the compression which is produced in the brain, in consequence of that of the *meninges*. When we separate the *dura mater* from the *cranium* with the finger, the animal complains, even a moderate compression of the brain makes it suffer, and if it is pretty strong, the animal falls asleep. In the patient named CLERMONT, whom M. LE CAT^d mentions, the optic nerve was hurt, as he himself owns, and it is very difficult in a cursory dissection to know, whether in the one called COURVET, the nerves of the eyes, and especially those that creep in the bottom of the orbit, towards the temples, were really preserved. The spasm may have causes not to be penetrated into by our senses, and founded upon the most delicate structure of the nerves. The hysteric *tetanus*, and that in animals which have been poisoned, confirms this opinion, and the observation of M. LE CAT^e proves absolutely nothing at all, seeing it does not exclude this disorder, which is too deep seated to be visible. The history of PERCHEPIE^f ought not to be produced against me, seeing this patient had *pus* in the ventricles and under the *basis* of the brain, which is more than sufficient to occasion

^c P. 113.^d P. 115.^e P. 118.^f P. 119.

a delirium. In order to refute me, M. LE CAT must bring experiments such as mine; the *dura mater* irritated, and convulsions thereby produced; the tendons pierced and wounded, and the ligaments pricked or burnt, and followed by violent symptoms. But no such experiments can be produced; nature is too constant, and I have seen her act too often to be deceived in this point. Neither should the difference between man and brutes be here quoted against me; for if wounds of the tendons had any influence upon their motion, a kid, a rabbit or a dog would not be able to leap up on a chair, immediately after the *tendo achilles*, had been pierced, cut, or destroyed^s.

Lastly, I have said, that the tendons, *periosteum* and *dura mater* are all insensible. I am not the very first who has advanced this truth, and I have quoted observers, who having no system to defend, had remarked the same thing before me. M. LE CAT mentions no experiments against me here, but with regard to the *dura mater*^h. He relates that one, named FLEURI, complained upon his pressing this membrane with a crotchet; and that another wounded patient, called MABIREⁱ, was sensible of the motion of a tooth-pick upon the *dura mater*, that he felt the application of spirit of wine^k, and the action of the surgeon who washed the wound^l; and consequently that the *dura mater* must ei-

^s See the thesis of Dr. CASTEL. ^h P. 122.

ⁱ P. 124.

^k P. 129.

^l P. 125.

ther be almost cartilaginous, or ossified, in those subjects which do not seem to have any sensation in this membrane. It even appears by his own expressions, that he has seen examples of this insensibility^m.

A thousand times have I scratched, burnt, and cut the *dura mater*, in I don't know how many different animals, without their ever complaining, and they have seemed to feel still less the spirit of wine, which is infinitely less active than the butter of antimony or spirit of nitre. Young animals have sucked, or lapt milk, during the tearing of this membrane, without seeming at all sensible of what was a doing. It is absolutely impossible to imagine that the *dura mater* should be cartilaginous, or almost bony, in young healthy animals. The same animals were very sensible when the skin was pinched or pulled, which they discovered by making a noise, and endeavouring to pull away the part. The experiment has been made upon animals fierce and impatient, such as cats for example, which become furious when they find themselves in danger and pain, nay, the same experiment has been tried upon man, and M. ELLER has confirmed the truth of this doctrine at Berlin, upon the *dura mater* of a man, which had been laid bare by a *caries*. If the wounded person mentioned by M. LE CAT felt the pressure, it was no more than brutes themselves are sensible of; they feel

^m Page 129. near the bottom.

very well the detaching of the *dura mater*, and the finger resting upon it, as I just now remarked. It might not even be impossible for very penetrating remedies to act beyond the *dura mater*, as cold water and acids affect the nerves of the teeth, through the nervous and bony substance which intervenes. But I have never observed this fact to happen, and I repeat it once more, that the *dura mater* being only a cellular membrane, becoming evidently such where it accompanies the nerves, and having no nerves itself, it cannot be susceptible of any sensation.

I have only one word more to add, which is, to entreat physicians and surgeons, to lay hold of all opportunities of instructing themselves with regard to the insensibility of the *periosteum*, tendons, ligaments, and membranes of the brain. Such opportunities must frequently enough occur, and after they have laid hold of a tendon with a pair of *forceps*, as I have done that of the *flexor* of the third articulation of the finger, they will find themselves emboldened to make experiments, which are neither attended with danger nor inconvenience to the patient.

Allow me to add here that M. LA MURE published in tome 1749, of the *Memoirs of the Royal academy of arts and sciences at Paris*, an essay concerning the motion of the brain, analogous to that of respiration, which was read the 7th of August 1752^m. This of mine was read before

the royal society at Gottingen, the 22d of April and 6th of May 1752. Hence you see that it was published previous to M. LA MURE's experiments, and it is plain that mine are anterior to his, seeing they are quoted by himⁿ, and I was entirely unacquainted with those which he had made. Envy takes a pleasure in laying hold of slight occasions of detraction, and an essay wrote in 1752, but published in the volume of 1749, might furnish occasion for it in this case; and it would not be agreeable to be thought to have copied after another, when a person has followed nature alone. BERN 10th Sept. 1754.

ⁿ Memoirs of the academy 1749, page 542. I communicated them in a letter wrote to M. DE SAUVAGE the beginning of January 1752, who sent me an answer as follows, dated March 1, 1752. *The dog was trepanned, and we observed a very considerable motion of the brain, exactly like that which you described in your letter to me. In order to be certain whether it is the reflux of the blood which causes this elevation during the time of expiration, M. LA MURE opened more than ten dogs: In short we have found the case to be exactly as you describe it, and I am extremely obliged to you for communicating this discovery.*

AN

AN
E S S A Y
ON THE
CAUSE of the MOTION
OF THE
H E A R T.

Read November 10th, 1751*.

ALthough this essay is but short, yet it will have its use; seeing you will find an experiment in it which I have made several times, and which proves that the motion of the

* Although this essay is independent of that which was read before, and even appeared sooner; yet as it contains experiments upon the Irritability of the heart, which determine the cause of these motions, I imagine it will be agreeable to the reader to see it subjoined to the preceding, seeing both may be considered as pieces belonging to the same subject, which reciprocally establish each other.

heart,

heart, by perpetual alternate contractions and relaxations, depends upon irritation, occasioned by the venous blood which is sent thither. All the explanations which have hitherto been given of this phænomenon, are destroyed by the human or comparative anatomy.

We know perfectly well that the right ventricle, and especially its *auricle*, are the last parts of the body which preserve any motion. GALEN^o, HERVEY^p, and BOERHAAVE^q, were convinced of this by experiments.

I have long suspected^r, that the continuance of this motion depended upon the blood, which the *venæ cavæ*, contracted by the cold, and compressed by the palpitations and weight of the muscles, send continually to this ventricle; whereas the lungs of the dying animal, being motionless and collapsed, no longer admit the blood by the pulmonary artery; and that all the quantity which its contraction can force into the left auricle, is too inconsiderable, compared with that which returns from the whole body to the right auricle, to produce a sensible effect. It may therefore be affirmed, that if the right ventricle and auricle, move longer than the left auricle, it is because the venous blood is sent longer thither.

I determined to confirm my conjecture by experiments, and in order to this, it was ne-

^o Admin. anatom. lib. 7. c. 15. ^p Dis. 1. p. 39. 44, &c. ^q Instit. rei med. No. 159. ^r Commentar. in BOER. c. 4. p. 609, prim. lin. phys. No. 113. cessary,

cessary, if possible, to prevent the entry of the blood into the right ventricle, and if by this means its motions were stopt, that was a proof that they actually depended upon the access of the blood.

I attempted this experiment at first by ligatures, because I remembered to have read in BARTHOLIN^f, and BERGER^t, that the tying of the veins makes the motion of the heart cease, and that it begins again when the ligature is cut: and HERVEY says that he has made the same experiment upon a serpent^u.

But the experiment done in this manner did not succeed, because as long as the animal is still warm, the blood contained in the right auricle continues to move it, though there is none sent to it by the *venæ cavæ*; and after having tied them in three young cats, the motion of the blood continued as before. The same thing happened to BLANQUET, in the experiments related by Dr. SENAC^x.

This made me resolve to slit both the *cavæ*; I should have cut them quite off, if I had not been afraid, that then the cessation of the motions of the heart, would have been attributed to the want of its necessary supporters. After having slit them, I emptied them of all their blood, and tied them. I next emptied the auricle; and then the success of the experiment was

^f Anat. p. 379. ^t De nat. hum. p. 62, 63. 306.
See likewise D. SORGELOOS de œconom. corp. 66. 69.
^u L. C. p. 99. ^x Trait. du Cœur t. 1. p. 449.

was always constant. As soon as I had freed the auricle of all its blood, and prevented it from receiving any afresh, immediately its motion entirely ceased. As it is more difficult to empty the ventricle than the auricle, and as it yields to the impressions which are communicated to it by the left ventricle, I sometimes observed there a gentle motion, but incomparably weaker than that, which it has when it receives the blood from its auricle and the *venæ cavæ*.

But there still remained a more authentic experiment for me to make. In the natural state, the right ventricle moves longer than the left; because, as I said before, it receives the veinous blood longer than the other. To prove to a demonstration that the blood is actually the cause of the heart's motion, there is nothing farther required to be shewn, than that if the right ventricle and its auricle are deprived of the blood, while the left ventricle is not, the first immediately loses its motion, while the other still continues to act.

In order to succeed in this experiment, the right ventricle must first be perfectly emptied, by opening the pulmonary artery and *venæ cavæ*, and the emptying of the left ventricle prevented, by tying the *aorta*; and after this is done, we must examine narrowly, if the motions of the right ventricle cease, and if the left and its auricle continue theirs.

After some attempts, which the difficulty of so delicate an undertaking, and the sudden death

of the animals rendered fruitless, the experiment at last succeeded to my wish; *viz.* the right auricle remained perfectly quiet, and its ventricle preserved no other motion than that which was a necessary consequence of the connection of its fibres with those of the left ventricle, and which pulled the external *parietes* towards that which separates the two ventricles from each other. The left auricle preserved its motion for a certain time, the left ventricle still longer; and I have sometimes observed this last contract two hours after the other had ceased.

When the experiments succeeded exactly, the blood mounted from the point to the basis of the left ventricle, and descended again from the basis to the point, and then the left ventricle, as long as it preserved any motion, appeared likewise to descend. At other times, as I have seen in a kid, it had no motion at all. This experiment succeeded best when the left auricle emptied itself freely into the ventricle, and the passage of the blood from the ventricle into the *aorta*, which was tied, was intirely stopt. The point of the left ventricle was always the part which preserved its motion the longest. By this means the property of being the last living part of the body, is transferred from the right ventricle to the left, by preserving longer in the last, the irritation produced by the contact of the blood.

A new force may be given to this experiment, by trying to blow into the right ventricle; by which irritation its pulsations are renewed.

Farther, I have always remarked, that the internal surface of the heart is much more irritable than the external. For when I irritated this last with the strongest poisons, the motion thereby communicated to the heart soon ceased; whereas the irritation communicated to the internal surface, by means of air alone, has occasioned, especially in frogs, and even in cats, motions which subsisted a very long time, even after all the parts were quite cold.

I have repeated this last experiment nine times, *viz.* of preserving the motion of the left ventricle, after all the other parts were deprived of theirs; seven times upon cats, and twice on kids. The violent struggling and agitation of dogs, render them improper subjects for this purpose.

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