AN

EXPERIMENTAL INQUIRY,

&c.

AN

EXPERIMENTAL INQUIRY

INTO

THE LAWS

OF THE

VITAL FUNCTIONS.

By A. P. W. PHILIP, M.D. F.R.S.E.

IN PART RE-PUBLISHED, BY PERMISSION OF THE PRESIDENT OF THE ROYAL SOCIETY, FROM THE PHILOSOPHICAL TRANSACTIONS OF 1815, 1817, AND 1822.

WITH THE

REPORT OF THE INSTITUTE OF FRANCE ON THE EXPERIMENTS OF M. LE GALLOIS;

AND

OBSERVATIONS ON THAT REPORT.

THIRD EDITION.

ADDRESSED TO THE SCIENTIFIC PUBLIC.

Αγαθὴ δὲ διδασκαλίη ἡ πεῖρα.—ΑΡΕΤΑΙΟΣ.

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TO

SIR HENRY HALFORD, BART.,

F.R.S.

PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS, AND

ONE OF THE PHYSICIANS TO HIS MAJESTY,

&c. &c. &c.

DEAR SIR,

IT appears to me that the first attempt which has been made to present to the Scientific Public a View of the Functions of Animal Life, is properly addressed to the President of the College of Physicians; and with peculiar propriety, to the present President, in whom the Science, and the Talents which have raised him to the head of our Profession, are in no ordinary degree combined with the acquirements of the Scholar, and the general Information which distinguishes the Gentlemen of this Country, and encourages the hope that the following Inquiry will not be uninteresting to them.

I have the honour to be,

Dear Sir,

With much esteem,

Your faithful, humble Servant,

A. P. W. Philip.

Cavendish Square, Feb. 18, 1826.

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137, 8 from bottom, *for* the division, *read*, by removing a part; and line 3 from bottom, *for* division, *read* removal of a part.

138, 5, *for* dividing, *read* removing part of.

168, 9, *after* powers, *insert*, in the functions of life.

170, 21, *for* are, *read* is.

173, last line of the text, *after* animal, *insert* would.

174, 1, *for* destroys, *read* destroy.

208, 10, *for* power, *read* system.

Quand on [illegible] chose. On trouve que le système est une partie du tout et le tout est une partie d'une système

PREFACE

TO THE

FIRST EDITION.

The obscurity of the nature of Internal Diseases, of which Physicians have always complained, seems to arise from several causes; the difficulty, in those diseases, of referring the painful feeling to the seat of the injury, proceeding from the indistinctness with which we refer to internal parts, and other parts sympathizing with the part affected; the deficiency of our knowledge of morbid anatomy, in consequence of which we are not always enabled, from the train of symptoms, to infer the derangement of structure; our ignorance of the function of some internal parts, and where we have a knowledge of the function, our ignorance of the principle on which it depends. If such be the causes of the obscurity of the nature of Internal Diseases, we may easily perceive the objects which ought to be kept in view in our endeavours to obtain a more correct knowledge of them, and consequently of the means of cure adapted to them.

By the frequent inspection of diseased bodies, we learn to connect particular trains of symptoms with the changes of structure which occasion them; for although the sensations of which the patient complains are often ill defined, and sometimes not referred to the seat of the morbid action, yet the same morbid action almost always produces nearly the same train of symptoms. Nothing appears more to have retarded the progress of medical knowledge, than the obstacles which have in all ages been opposed to the inspection of dead bodies. The importance of ascertaining the changes produced by disease, however, has been slowly reconciling the public mind to it; and we may with confidence anticipate the greatest improvements from the increasing frequency of this practice; without which we can no more acquire a knowledge of the diseased states of the body, than we can of its healthy state without the aid of anatomy.

But neither anatomy nor the inspection of diseased bodies can teach us the nature of the functions. A knowledge of them can only be acquired by comparing the structure of the organs with the actions observed in them while their vital power remains. Some of these actions are the objects of simple observation in our own bodies and those of other animals. Anatomy, for example, teaches us the structure, position, and attachments of the muscles ; observation readily points out their function, and by the lesion of this function we judge of the extent and degree of their morbid affections, and are consequently guided in the application of our remedies. But there are other and more important functions, which in the entire animal are hidden from our view. To ascertain their nature, experiments must be made on the living and newly-dead animal. We shall find that many parts retain their vital actions for a certain time after what we call death, and consequently that from the inspection of the newly-dead animal the nature of many of the functions may be ascertained.

To the aversion to experiments on living animals, which every man must feel, we may, I think, in some measure, ascribe the little progress which has been made in this essential branch of medicine. Much must be ascribed to the obscurity of the subject. The internal functions of animals are of a nature so different from any thing which we are accustomed to see around us, that our previous experience gives us little assistance in attempting to trace their laws, and our progress is necessarily slow and difficult. Hence it was that the earlier Physiologists evaded the task placed before them, and endeavoured by ingenious fictions to deceive their readers. It is now universally agreed that if any progress can be made in Physiology, it is not by the contrivance of hypotheses, but by patient and laborious investigation. We are amused with the reveries of Stahl, but for instruction we look to the experiments of Haller. And to similar experiments we must look for all the information we can obtain on this subject. The hope of adding something to our knowledge of the vital functions, and thus improving the treatment of their diseases, induced the author to undertake the following Inquiry.

From the foregoing view of the subject it will be admitted, that few writers have stronger claims on the indulgence of the public than the Physiologist, provided his endeavours are rationally directed. The knowledge which it is his aim to acquire and communicate, is of the most important kind, while his means of information are always laborious, and often of a painful nature. These claims are increased by the circumstances in which he is usually placed. No person is fitted for physiological inquiries, who has not obtained a competent knowledge of the different branches of medicine. This knowledge is acquired with so much trouble, and depends so much on actual observation, that few who do not practise medicine as a profession ever acquire it. The Physiologist, therefore, generally pursues his inquiries amidst anxious and fatiguing engagements of a different kind, and of such a nature that all others must give place to them. The author does not mention these circumstances as affording any apology for inaccuracy in points of consequence, because it is due to the public that a writer should withhold his communications till he thinks himself assured of their accuracy; but they may, he hopes, be admitted as an apology for less important errors.

The errors of the following Inquiry are not those of precipitation. It is above fifteen years since some of the experiments which the author is about to relate, and many connected with them, were made. None have been made within the last year, during which he has employed the time he could devote to such engagements in arranging his experiments, comparing them together, and endeavouring to guard against hasty inferences, which it is difficult to do at the time they are made.

PREFACE

TO THE

THIRD EDITION.

It seems at first view surprising, that in an age when almost every other species of knowledge is so generally diffused, that of the functions of our own bodies should be exclusively confined to the medical profession.

The beauty of their complicated structure—the ease with which we can trace their great movements, and perceive the ends they serve—the importance of these ends, and the deep interest we necessarily take in the subject, liable as all our functions are to frequent derangement, bestow on the study of Physiology an interest which no other possesses.

To those who feel a pleasure in contemplating the works of nature, it opens an ample field. Even in the planetary system itself the wisdom and the power of the Creator are not more strikingly displayed, and certainly not in so great a variety of ways.

To the supposed difficulty of acquiring the previous knowledge necessary for this study, we must ascribe its having hitherto been confined to so small a portion of the community. But the general reader will be surprised to learn, that a few hours are sufficient for the acquisition of all the previous knowledge requisite for understanding the part of the subject to which the following Treatise is confined, the great movements of the animal system, if this expression may be used to distinguish this part from that which depends on the more minute labours of the anatomist and the chemist, which is necessarily less striking, and, with a few exceptions, less capable of useful application.

Such are the circumstances which have induced the author, in publishing a third edition of his Inquiry into the Laws of the Vital Functions, to address it to the scientific public at large; and if he succeeds in opening to them a source of knowledge, at once so pleasing and so important, at a very trifling expense of labour, he trusts he will deserve their thanks,—a reward which he is persuaded he will not fail to receive, if they experience any degree of the pleasure which he has himself derived from that source.

The following very general view of the structure and functions of the animal body comprehends all the previous knowledge requisite for understanding the present Treatise, in which all technical terms will be as much as possible avoided, and none used without explanation.

THE bones may be regarded as the foundation of the animal system. They give the general form and stability to the whole, all the other parts being more or less directly attached to and supported by them. They are very numerous, in some instances firmly attached to each other, but generally connected by joints formed by strong ligaments, admitting of various degrees of motion according to the uses of the several parts. They are of different degrees of solidity, according as firmness or extent of surface is required. The surface is extended for the attachment of muscles and other purposes, by their being rendered very cellular in some places, and hollow in others, which adds to the strength of the long bones by increasing their diameter. The cells and hollow parts are filled with marrow, and lined with a fine membrane, from which similar membranes pass in various directions to the opposite sides of these cavities, forming cells for the support of the marrow. The bones are also covered by a fine strong membrane, called by anatomists periosteum, which may be regarded as forming the medium of connexion between them and the soft parts of the body.

Every one knows that two great systems exist in the animal body, the heart and blood-vessels, and the brain and nerves. These systems pervade almost every part of it.

The blood-vessels and nerves divide like the branches of a tree, until their ramifications become so small as to escape our senses; and distinct as the central parts of these systems, the heart and large vessels on the one hand, and the brain and large nerves on the other, are; their extreme parts are so minute and interwoven that they cannot be separated by the most expert dissector. It is impossible, with very few exceptions, to wound any part of the body with the finest needle, without exciting pain and drawing blood, a proof that both vessels and nerves are injured by it.

The blood is conveyed from the heart by vessels called arteries, which are so elastic as to retain their round shape when empty. They begin by one great vessel attached to the heart, called the aorta. This fluid is returned by the veins, which are so pliable, that they collapse as soon as their contents are discharged.

The blood consists of many principles. The most remarkable are what is called the lymph, which spontaneously coagulates into a firm mass as the blood cools; a watery part called serum, which gradually separates from this mass, and minute globules of a red colour, which may be distinctly seen by a microscope of small power.

In the division of the vessels, they at length become too small to admit these globules, so that the blood in the minute vessels, which have been called capillaries, is divided into red and colourless, for to the globules the blood owes its colour. The capillaries, after pervading the minutest parts, begin to join each other, gradually forming larger vessels. Thus the veins originate, which at length terminate in two great vessels called the venæ cavæ, and these, uniting, pour their contents into the heart. The veins in all parts of the body, except the lungs, exceed the arteries both in number and size, and therefore in them the motion of the blood is proportionally slower.

A remarkable change takes place in the appearance of the blood during its passage through the body ; it leaves the heart of a bright red, and returns to it of a dark-mulberry colour. Hence the former is called red or arterial blood, the latter black or venous blood.

The heart in man and the other more perfect animals is double ; if compared with the heart of the less perfect animal, it may be regarded as two hearts joined together. Each consists of two cavities, one into which the blood is received from the veins, called the auricle, by the contraction of which it is thrown into the other, called the ventricle, from which it is propelled into the great artery. The walls of the former, like the veins, are comparatively slight, and possess little elasticity: those of the latter are thick, strong, and very elastic.

On each side of the heart valves are placed between the auricle and ventricle, which permit the blood to pass into the ventricle, but not to return to the auricle; and between the ventricle and the great artery, in like manner, there are valves which permit the blood to pass into the artery, but not to return to the ventricle; and in those of the larger veins, which are so situated as to be subjected to frequent pressure in the different motions of the body, there are valves which allow the blood to pass towards the heart, but not in the opposite direction, by which in our various exercises the rapidity of the circulation, and thus for the time, our powers are increased.

The blood in returning from the different parts of the body does not enter the same side of the heart from which it was sent, and which, on account of its position, being rather to the left of the other side, is called the left side of the heart. The side it now enters is, for a similar reason, called the right side.

By the ventricle of this side it is not thrown into the same artery as by that of the left side, but into another called the pulmonary artery, by which it is conveyed through the lungs, the only part of the body which does not receive the whole of its blood from the left side of the heart. There it is distributed in the same way as in other parts of the body, and is at length brought back to the heart by four great veins called the pulmonary veins; but, as the blood which is propelled by the left ventricle of the heart returns to the right auricle, this blood, which is propelled by the right ventricle, returns to the left auricle.

During its circulation through the lungs, it undergoes a change in its appearance as remarkable as that which takes place in its passage through the other parts of the body, but the change is now from the dark to the bright blood. It enters the pulmonary artery black blood, it is returned by the pulmonary veins red blood.

The lungs fill nearly the whole of the chest with the exception of the space occupied by the heart and great vessels. They are of a spongy texture, composed of an infinite number of membranous cells, and air, and blood, and what are termed lymphatic, vessels, of which the author will presently have occasion to speak more particularly. The air-vessels are branches of the wind-pipe which divides like an artery, into innumerable ramifications that terminate in the air-cells.

It appears from what has been said, that the quantity of blood sent through the pulmonary artery to the lungs at each contraction of the heart is equal to that sent to all the rest of the body.

The whole trunk of the body is divided into two cavities, the thorax and abdomen, by the diaphragm, in common language called the midriff, composed of membranes and muscles which give it a contractile power. It is attached all round to the lower edge of the ribs, the breast, and back-bones, and is concave upwards, which gives space for the viscera of the abdomen. The ribs are attached to the back-bone in a slanting position, the inferior angle being the acute one, and the uppermost ribs are more fixed than those forming the greater part of the chest. The spaces between the ribs are filled by muscles attached to their edges and termed intercostal muscles.

From this disposition of the parts it appears that when their muscles act, the diaphragm must be drawn towards a plane whose circumference is in its attachments to the chest, while the ribs in consequence of the upper ribs being least moveable, are drawn upwards, by which the angles they form with the spine, approach nearer to right angles. By these means, it is evident, the capacity of the chest both in its perpendicular and horizontal diameter must be enlarged, and the air consequently, by the pressure of the atmosphere passing through the wind-pipe and its branches, distends the air-cells. On the relaxation of the contracted muscles, the ribs and diaphragm returning to their former position, the air is expelled. The ribs resume their position by the elasticity of the ligaments and cartilages which connect them with the back and breast-bones, the diaphragm by the pressure of the muscles of the abdomen on the viscera of that cavity. Such is the mechanical part of the function of respiration.

In common respiration we chiefly employ the diaphragm. When we wish to inflate the lungs more fully, the intercostal muscles are also called into action, and in very laborious breathing, this function is assisted by all the muscles of the trunk. The diminution of the capacity of the chest by which the air after it has performed its office, is expelled, is not now trusted to the means just mentioned, but in addition we draw the ribs downwards and press up the diaphragm, by all the power of the abdominal muscles; and in like manner for the purpose of expanding the chest, we no longer trust to the diaphragm and intercostal muscles, but assist them by drawing the ribs upwards with all the power of the muscles of the neck and shoulders.

We here see a striking instance of the

manner in which we are enabled in emergencies to assist the usual functions. We have just seen another instance, in which the valvular structure of the veins in the extremities, by causing exercise of the limbs to increase the rapidity of the circulation, for the time increases our strength, in proportion to the increased demand for it.

The wind-pipe lies in front of the passage, called by anatomists the œsophagus, which leads to the stomach, so that in swallowing, our food passes over the upper end of the wind-pipe, which during the passage of the food, is closed by a small cartilaginous flap, called by anatomists the epiglottis, pressed down upon it by the act of deglutition. When the shutting of this flap happens not to be accurate, part of the food passing into the wind-pipe, causes the disagreeable sensation vulgarly called swallowing the wrong way, and is brought back again by coughing, which is produced by a sudden action of all the muscles that can contribute to lessen the capacity of the thorax, by which the air in the lungs being suddenly forced out through the wind-pipe, carries with it any extraneous body which happens to be there.

By the mechanism which has been described, the air and blood are brought sufficiently near to each other in the lungs, for the former to effect the necessary changes in this fluid; for the changes which the blood undergoes in the lungs are effected by the air, which is capable of producing them through the fine membranes that intervene between that agent and the blood in the lungs.

Black blood is unfit for the purposes of life. Its powers are exhausted. By its passage through the lungs, these powers are renewed, and in this renovated state it is again, by the left ventricle and the arteries, sent to every part of the body, the lungs themselves not excepted, red blood being as necessary to support their function, as that of any other organ.

Such is the circulation of the blood, unfolded to us by the experiments of Harvey, who has thus acquired an immortal name, and bestowed on the healing art incalculable advantages.

The change from red to black is not the

only one which takes place in the blood during the circulation. In its passage it affords nourishment to every part, and as the lungs are the means of restoring to it the colour and other properties which distinguish red from black blood, there must be some source from which the loss it sustains in this important function, is supplied.

For this purpose there is in the animal system a set of vessels termed absorbent vessels. These arise from every part of the body, but the most important are those which arise from the intestines, which, from the colour of the fluid they contain, have been called lacteals. They absorb the nutriment prepared from our food, by the process of digestion, and gradually uniting, at length form one trunk, which from its passing through the thorax, is called the thoracic duct. Into this duct also the greater part of the absorbents from the surface and other parts of the system, which from the appearance of their contents, are termed lymphatic vessels, open. The thoracic duct itself opens into a large vein near the right side of the heart, namely, that side which throws the blood into the lungs, so that the fluid it supplies immediately passes with the blood through them, and is there assimilated with it, and thus converted into perfect blood before it is sent to the system in general.

The food is dissolved in the stomach into a more or less homogeneous mass by a fluid called the gastric juice, formed, or secreted, as it is termed, from the blood by the stomach itself. By the contractile power of this organ, its contents are propelled into the first intestine, called the duodenum; where they are mixed with the bile, with a fluid secreted by the pancreas, in common language, called the sweetbread, and with the secretion from the surface of the intestine itself. In their passage through the other intestines they are mixed with the secretion from their surface, and the whole is separated into two parts, that which is fit for nourishment being absorbed by the lacteals, and the rest discharged from the body.

In almost all other parts, as well as in the alimentary canal, secreted fluids are formed from the blood by a set of organs called secreting, because their office is so to separate and recombine the elementary parts of the blood, as to form these fluids; which are of two kinds, either such as are necessary to maintain the functions of life, as the gastric juice, the lubricating fluid of the joints, &c., or such as are of an excrementitious nature, the secretion of the kidney, for example, by which means whatever is offensive to the system is thrown out of it.

WERE the foregoing the only functions of the animal body, it would, like the vegetable, have merely an organic existence; but to these the animal adds the faculty of perception, which we find varying from so low a degree, that it is difficult to detect its existence, to that of the more perfect brute, which possesses it in a degree in all respects equal, and in some superior, to man himself.

Intimately but mysteriously connected with this part of our frame, are the faculties of mind, which, with the exception of volition and memory, we possess in a degree that bears no comparison with other animals.

The author need hardly say that in such a Treatise as the present, he has no intention of entering into any metaphysical discussions. There are but two faculties of mind concerned in the vital functions, sensation and volition; without these, it will appear, none of the more perfect animals can exist many minutes. It is only as far as they are necessary to the continuance of life, and consequently to be regarded as vital powers, that they form any part of the subject of the following Inquiry. They so far depend on the brain and nerves, that they are influenced by all the affections of these organs, and there is no longer any indication of their presence when the functions of the nervous system cease. How the faculties of mind are disposed of at this awful consummation, without the assistance of a higher power, we have no means of judging; but it will appear, the author thinks, that by the aid of experiment, we can distinctly trace up to the moment of that consummation, the changes which necessarily precede it, and the order in which they take place.

The brain consists of two distinct masses, connected by a substance of a structure similar to their own. The largest, which fills the anterior and middle parts of the head, is more properly called the brain; the other lies in the posterior part, and is called by anatomists the cerebellum or little brain. Every one knows that the spinal marrow proceeds from the brain. It may be regarded as a kind of elongation of the brain and cerebellum ; and that part of the elongation which lies within the scull, has been termed the medulla oblongata. The spinal marrow is divided by anatomists into cervical, dorsal, and lumbar, these names corresponding with those of the different parts of the bony canal, termed the spine * in which it is lodged. The nervous system then

* The spine consists of many bones termed vertebræ, joined by strong ligaments in such a manner as to form joints allowing of motion in all directions. They differ considerably in different parts of the spine, in shape and the freedom with which they move on each other, and when they are spoken of, are generally distinguished by the part of the spine they occupy. Thus the cervical, dorsal, and lumbar vertebræ, are common expressions.

consists of the brain, cerebellum, medulla oblongata, and spinal marrow, with the various nerves which proceed from these organs.

The organs of sense owe their power to the nervous system. When their connexion with the brain by means of their nerves, is interrupted in any part of their course, the function of these organs is lost.

It is evident that the function of exciting the muscles of voluntary motion also depends on this system, because when they are deprived of the influence of the brain communicated through their nerves, they no longer obey the dictates of the will.

Thus by means of the brain and nerves, we are doubly connected with the external world. Through the medium of the sentient extremities of the nerves, we perceive the events which are passing around us, through that of the muscles of voluntary motion excited by the nerves, we are enabled to influence those events.

But there is a large proportion of the nerves which has no share in connecting us with the external world: these differ in several respects from the former. They appear to be distributed to the vital organs alone, and their only use has been supposed to be to give sensibility to those organs. Instead of arising immediately from the brain, medulla oblongata, or spinal marrow, as the former nerves do, they arise from small compact bodies formed on the nerves, which anatomists call ganglions, the uses of which have frequently been a subject of discussion, and will again be discussed in the present Treatise. From these bodies this class of nerves have obtained the name of ganglionic.

The former nerves arise in pairs, the individual nerves of each pair resembling each other, one supplying the right, the other the left side of the body, and these pairs have been numbered by anatomists, ten proceeding from the brain and medulla oblongata, and thirty from the spinal marrow. Of those proceeding from the brain and medulla oblongata, the eighth pair is that which seems chiefly to contribute to the formation of the ganglionic nerves. All the spinal nerves contribute to it, and the principal trunk of the ganglionic nerves is called the great sympathetic nerve, from its forming connexions with almost every other nerve of the body *.

The term muscle is familiar to every one. The bodies of muscles constitute what we call the flesh, consisting of fibres, which are capable of a great degree of contraction and elongation. Muscles are of two forms, either of a long shape as those of the limbs, terminating at each end in firm tendons like ropes, by means of which, attached to the different bones, we are enabled to bend and extend the various joints; or they are hollow, with their fibres disposed in a circular direction, as those of the heart and stomach, by which these cavities are enabled to contract upon their contents and expel them. The power of the muscle is only contractile, by which, in consequence of the impression of certain agents, its extremities are made to approach each other, its firmness and trans-

* The principal trunk of the ganglionic nerves being joined by some of the nerves of the head, it has been commonly regarded as proceeding from those nerves. Many facts, however, as the reader will see, oppose this opinion.

verse diameter being increased in a degree which bears a certain proportion to the approximation of its extremities.

Muscles are excited to contract by the application of various substances which are therefore called stimulants. On the removal of the stimulant the muscle is relaxed, and it is elongated, that of the former class by the action of antagonist muscles, that of the latter by the accumulation of its contents.

The muscles, in another respect, are divided into two great classes, those which are, and those which are not, subject to the will. The muscles of the trunk and extremities belong to the former class, those of the heart and other vital organs to the latter. These classes are also distinguished by the muscles of voluntary motion being supplied with nerves, proceeding directly from the brain, medulla oblongata, or spinal marrow, and those of involuntary motion, by nerves from the ganglions; and by the former being excited to contract by the nerves alone, the latter, which are the hollow muscles, by the stimulating property of their own contents, acting either directly on the fibres of the muscle, or through the medium of their nerves.

All parts of the body are connected and bound together by what is called cellular substance, which varies in its texture from being loose and cellular, where the laminæ and fibres appear variously interwoven, to the densest transparent membrane. The nerves, vessels, and muscles, are everywhere embedded in this cellular substance, and the viscera are all covered and supported by it. The strong double membrane by which the intestines are suspended from the spine, and which supports their vessels and nerves, is called by anatomists the mesentery.

In the cells of the cellular substance the fat is deposited, which is thus supported and retained in its place, by which its proper distribution is secured, and the graceful form of the body preserved.

The skin which covers the body is of considerable thickness, and forms a secreting organ for the purpose of separating and throwing off from the body the perspirable matter, and an absorbing one for that of taking in from the air what is useful to the animal economy. It is covered by the cuticle, which defends its tender and highly-sensible surface, and which is continued throughout the various internal surfaces which are in continuation with the skin.

SUCH is a very cursory view of the structure and functions of the animal body. Those who wish for more particular information on any of the subjects which have been mentioned, may find it in the systems of anatomy and physiology *: the author has confined himself to what is necessary to enable the general reader to understand the following Inquiry. He cannot help observing, even from the hasty sketch which has been laid before him, that our knowledge of the sanguiferous is more perfect than of the nervous system. It is easy to follow the course of the circulation, and conceive how the contractile power of the heart and

* As far as we can judge from the large portion of it which has already appeared, the public will soon, in Dr. Bostock's System of Physiology, be in possession of one of the best systematic works which has appeared in any science, whether we regard its arrangement, or the extent and accuracy of the information it conveys.

blood-vessels maintains it; but the manner in which the nervous system operates in its various functions is involved in much obscurity. He will even find that no satisfactory account has been given of the relation which these systems bear to each other, without which it is impossible fully to understand any of their functions, for there is none in which they do not more or less co-operate. Thus the most elementary points are left undetermined, the principle, for example, on which the heart and blood-vessels act, some maintaining that their power resides in themselves, others that they have no power but that derived from the nervous system. It was to investigate this relation, and other points relating to the laws of muscular contractility, and to throw light on the various functions of the nervous system, that the following Inquiry was undertaken.

These, we shall find, are not merely objects of rational curiosity, but intimately connected with interests of the first importance; for what interests, relating merely to our present sphere of existence, can be compared with those which regard the health both of mind and body; and it is only in proportion as we understand the functions of the animal economy, that we can arrive at rational plans of either preserving or restoring it.

AS public attention has lately been much and deservedly directed to the subject of cruelty to animals, and more or less suffering must be inflicted on them in most physiological investigations of any real utility, some observations on this subject, in such a Treatise as the present, are indispensable; and the author regards it as advisable, on many accounts, that he should here, as far as relates to such investigations, consider it at some length.

Many good and even dispassionate men have doubted whether we are entitled, with a view to the welfare of our own species, to make painful experiments on the inferior animals. The earnestness with which a dispute on this subject was conducted in the *New Monthly Magazine*, when the public feeling first began to be directed to it, induced the author to address to the editor some observations on it under the signature of Philanthropos, which appeared in that Magazine in September, 1816, from which he will beg leave to present an extract to the reader. They relate to the general subject of cruelty to animals; but, as the whole paper is too long for insertion here, and the question before the reader only relates to experiments on living animals, he will confine himself to that part of the subject. The reader will excuse the popular style which the pages of a Magazine require.

"As experiments on living animals are the topic of discussion in the papers above alluded to, the author will first offer his sentiments on this part of the subject, endeavouring, on the one hand, to guard against that unfeeling, and, in his opinion, most unjust principle, that the greatest sufferings of the brute creation are not to be placed in competition with the slightest advantage accruing to the human race; and, on the other, against that sympathy for the sufferers, which, however amiable, tends, like all other feelings when in excess, to blind the judgment.

"Whatever be the feelings of individuals, or of the moment, either on the one side of the question or the other, the author is convinced that there is some point between these extremes, where the general and dispassionate opinion of mankind will always be found. He believes, that among the best informed and most reflecting part of the community, the propriety of experiments on living animals, under certain restrictions, will always be admitted. This opinion is founded simply on the fact, that in various instances the suffering which they have prevented has been infinitely greater than that which they caused; many proofs of this assertion might be adduced. He cannot regard such experiments as justifiable on any other principle. As means of satisfying curiosity, or even of obtaining knowledge, which does not tend to prevent more suffering than they occasion, as far as he is capable of judging, they cannot be too decidedly condemned.

" Several of your correspondents have maintained that there are no circumstances under which experiments on living animals should be performed, supporting their opinion by the maxim, that we are not to do evil that good may come: but this maxim cannot be regarded as applicable to physical evil. Nobody considers it wrong to whip a child, and amputate a limb, provided they are done under proper circumstances. Those who use it as an argument on the present occasion, therefore, beg the question, the point in dispute being whether experiments on living animals are, under all circumstances, morally wrong; if so, there cannot be two opinions respecting them.

"We shall suppose a case which has actually occurred. Men are thrown on a coast, where they find nothing to maintain life but fruits with which they are unacquainted, but some of which they know to be poisonous. Is it morally wrong that they should, by experiments on their dogs, ascertain which are poisonous before they themselves eat them? Nobody surely can hesitate in answering this question. If so, can we censure Orfila for an extensive set of experiments on living animals, made with a view to discover an antidote for the poisons often taken accidentally in ordinary life, by which many human beings have been saved, and many thousands will be saved from the most painful death? Or will it be maintained that animals may be sacrificed to save one man to-day, and not to save thousands at a future period! Who can calculate what sufferings have been prevented, and how many lives have been saved, by the experiments on living animals, for example, which made known the circulation of the blood, and thus gave to the practice of medicine in many, particularly inflammatory diseases, a precision formerly unattainable*!

"In the character of those who have been engaged in such experiments, we see a sanction for them of a different kind. Where shall we find a man of a stronger moral sense or sounder principles of religion than Boerhaave was? If such a man has existed it was Haller, whose observations on the christian religion are esteemed even by those who devote their lives to its study, and whose letters to his daughter shew

* For how many improvements in surgery, to which many at present owe their lives, are we indebted to Mr. Hunter, who derived all these improvements from the same source!

how much he was alive to all the gentler feelings; yet, of all the physiologists of his time, he made most experiments on living animals, and most frequently appeals to the evidence afforded by them. Where shall we find a brighter ornament to the church, or to human nature, than the celebrated Hales, emphatically called the Christian Philosopher, yet he scrupled not to make experiments on living animals, to improve our knowledge of physiology. Many similar examples might be adduced. Let us be cautious how we arraign the judgment of such men. Your correspondents frequently refer to the opinion of the no less celebrated Johnson. The greatest advocates of this writer have lamented the prejudices which clouded his otherwise excellent understanding, and prevented his judging with accuracy of many questions in which the feelings are concerned. The author cannot help thinking that in the question before us, the example of such men as those just mentioned is a safer guide.

"While we are determining the question whether we shall voluntarily inflict suffering on the inferior animals, we must not allow our feelings to betray us into the forgetfulness, that in the present case this question involves another; shall we by such means endeavour to prevent much more extensive suffering? Let the author repeat, that neither the train of reasoning he has pursued, nor the example of the great men whom he has mentioned, can sanction experiments on living animals from a motive of curiosity, or even for the purpose of acquiring information on subjects of subordinate consequence. By pointing out the great and good purposes which such experiments may be made to serve, they seem to afford an additional argument against resorting to them on slight occasions. As it is necessary that animals should sometimes be sacrificed for the advancement of a science, in which the well-being of our species is so deeply concerned, it is doubly incumbent on us to protect them from wanton cruelty."

If the author may be allowed in addition to what is said in the first part of the foregoing quotation, to speak of any thing which he has himself done, he would say, that the employment of galvanism in Indigestion and habitual Asthma, even in its still very limited extent, and during the short period in which it has been used, has saved many times the suffering occasioned by the experiments which led to it. Nay, he could mention single cases from which alone, if it had never been employed in any other, this might be said. He may refer to a letter which the reader will find in the last part of this Inquiry, from Mr. Earle, respecting the employment of that remedy in St. Bartholomew's Hospital, to prove how much the mode of employing galvanism, suggested by those experiments, tends to relieve some of the most serious diseases we are subject to. Shall not medical men be excused for attempting by such means to relieve the sufferings with which they are constantly surrounded? And even were not the sum of suffering, on the whole, lessened by them, we might perhaps be excused for using the means afforded to us of transferring it from our own species to the inferior animals; but to say nothing of the preservation of human life, who will compare human suffering, with all its recollections, and all its forebodings with the sufferings of the animal which feels only for the present moment! What force do such reflections acquire when it is recollected that the sufferings of one animal may be substituted for those of thousands of our own species!

If we scruple *now and then* to perform experiments on living animals for such purposes, what shall we say of the *every-day* sufferings of the inferior animals, often greater than those of almost any physiological experiment, where the object is only profit, amusement, or even the gratification of the palate!

Yet let not those who view the physiologist only in his work, imagine that he inflicts suffering without reluctance. In the following page the intelligent reader will perceive sufficient proofs of that reluctance. The author uniformly endeavoured, as much as possible, to avoid experiments on living animals. Most of those related in this Inquiry were made on the newly-dead animal, and it will appear from what he is about to lay before the reader, that for many experiments, for which the living animal has been thought necessary, the newly-dead animal may be employed with equal, and sometimes with greater advantage. Eighty-six experiments are related in this treatise; of these sixty-three were performed on the dead, twenty-three on the living animal. But in many of the latter, the sensibility was destroyed by opium; in some the experiment was attended with little pain; and in some, where it was necessary to let the animal die by loss of blood, death was thus produced in the first stage of the experiment. In consequence of the author's having had recourse to the newly-dead animal, he has not employed the tenth part of the living animals employed in any other similar Inquiry of the same extent, the experiments on the dead animal being those in which the most frequent repetitions were requisite.

When it was necessary to employ living animals, he uniformly observed the following rules: to investigate no points in this way, but those of the first importance; always to destroy the sensibility previous to the experiment, when this could be done without influencing the result; when several animals were equally fit for the experiment, to choose the one which would suffer least from it; when there were several ways of performing the experiment, to choose the way which would occasion least suffering; if the experiment was necessarily fatal, to destroy the animal as soon as the purpose in view was answered, and to take such precautions as rendered as few repetitions as possible requisite.

The author most cordially agrees with those who lament that some check cannot be imposed, with a view to prevent wanton experiments on living animals.

Were it possible to confine experiments on them to the investigation of important facts, it would prevent at least nine-tenths, and he believes a great deal more, of those made at present, when every boy who attends physiological lectures, thinks himself at liberty to inflict on animals whatever suffering he pleases, and every one who reads a physiological treatise may repeat any experiment which he thinks curious.

These sentiments are not the result of a momentary feeling, nor drawn forth by any late proceedings. It is above ten years since the paper, a part of which the author has quoted, was published; and that was not the first instance in which he addressed the public on this subject.

HE was much indebted in the following Inquiry, to the kind assistance of several gentlemen, particularly Doctor Hastings, Mr. Sheppard, and Mr. Herbert Cole, of Worcester, and Mr. Cutler, assistant-surgeon to the Grenadier Guards, but he has mentioned no fact not witnessed by himself. Almost all the experiments on the newly-dead animal were performed by himself, and although, from a reluctance to operate on living animals, he employed others to perform the operative part where they were used, the experiments were all arranged, and the results witnessed by himself. He is therefore responsible for the accuracy of every part of the Inquiry, and it is particularly gratifying to him, and will secure the confidence of the reader, to state, that many of his experiments, as will appear from the following account of them, having been repeated by the Physiologists both of this country and the continent, were not, in any instance, found to be inaccurate.

IT may appear to some, that in addressing this treatise, not to medical men exclusively, but to the scientific public, the last part of it, which relates to the application of the results obtained, to explain the nature, and improve the treatment of diseases, might with propriety have been omitted. The author has not omitted this part, because without it the treatise would be defective in a very essential respect; because, as he only takes the most general view of the subject, he flatters himself that nothing is said that will not be readily understood by the general scientific reader, and because by the omission of this part, the only justifiable end of such an Inquiry would be less immediately in the reader's view.

PERHAPS some apology is necessary for the author's frequent reference to his former publications. In such a work as the present, this is unavoidable. His object is not to give the sum of our knowledge of the subject, but with the exception of the general view of the state in which he found it at the time he entered on the Inquiry, only to lay before the reader what he has himself done, without adverting to the labours of others, further than is necessary for illustrating his own. A considerable part of what he has done is dispersed through his other publications, more or less connected with the subject of the present Inquiry; and to them he is often obliged to refer, either to prove or illustrate what he says, because it would swell this volume to too great a size to republish all the passages which it would otherwise be proper to lay before the reader. He omits nothing, however, necessary for understanding the different parts of the subject, only giving the references for the use of those who wish to see a detail of the proofs, on which the positions not particularly discussed in the present Treatise, rest; or who wish for such information respecting them as would lead the author too far from the proper subject of that Treatise.

AN

EXPERIMENTAL INQUIRY,

&c.

BOTH the animal and vegetable world differ from inanimate matter, in affording a peculiar class of results when impressed either by mechanical or chemical agents. The quality on which the peculiarity of these results depends is called life. Whether it be the consequence of something added to bodies, or only a peculiar arrangement of their constituent parts, we have no means of judging. It is a state of which certain properties are the result, and it is essential that its name should convey this fact and no more.

In the present edition the author will use the term life, in the sense here defined, instead of vital principle, which has been employed so vaguely, that although he has repeatedly defined the sense in which he uses it, he has found himself very frequently misunderstood where it occurs.

The phenomena of life are more varied, and have been observed, and arranged with more care in the animal than the vegetable world. To the former the Treatise now offered to the reader is confined.

It is divided into three Parts. In the first part the reader will be made acquainted with the state of our knowledge respecting the principle on which the action of the heart and blood-vessels depends, and the relation which subsists between them and the nervous system, at the time the author began his experiments; as on these all knowledge of the vital functions, more or less, immediately depends. In the second he will relate his experiments, and point out the inferences to which they lead; and in the last, take a cursory view of their application, to unfold the nature and improve the treatment of diseases.

PART I.

On the State of our Knowledge respecting the Principle on which the Action of the Heart and Blood-Vessels depends, and the Relation which subsists between them and the Nervous System.

THE object of this Part cannot, the author thinks, be better accomplished than by laying before the reader a translation of the Report of the Committee of the Institute of France, on the Experiments of M. le Gallois, abridged in the less important parts; and such observations on it as it appears to demand.

CHAPTER I.

The Report made to the Class of Physical and Mathematical Sciences of the Institute of France on the work of M. le Gallois, *entitled* Expériences sur le Principe de la Vie, notamment sur celui des Mouvemens du Cœur et sur le Siége de ce Principe.

THE Class having charged M. de Humboldt, M. Halle and myself *, to make a report to it on the Memoir read at a meeting of the 3d of June

* M. Percy.

last, by M. le Gallois, Doctor of Medicine, respecting the nature of the power of the heart, and the source whence it derives its power *, we are about to present to it a detail which will, perhaps, be as long as the Memoir itself, because, without the necessary details and explanations, it would be impossible to appreciate all the merit of this excellent work.

It was not till after the circulation of the blood was discovered by Harvey, early in the seventeenth century, that Physiologists turned their attention to the cause and mechanism of the movements of the heart, which have, since that time, given rise to so many different systems.

We shall not speak of those of Descartes †, of Sylvius de la Boe ‡, of Borelli §. They are very absurd, and serve only to prove how unfortunate were the first attempts to explain one of the most important functions of the animal economy. We shall begin with the distinction

* "Concernant le Principe des Forces du Cœur, et le Siége de ce Principe."

† L'homme de René Descartes, et la Formation du Fœtus, avec les Remarques de Louis Laforgue, Paris, 1677, p. 4 and 106.

‡ Francisci de la Boe Sylvii Opera Medica, Genevæ, 1681, p. 5, 27, 28, 33, 475.

§ Joh. Alph. Borelli de Motu Animalium. Hagæ Comitum, 1743, p. 89—92.

which Willis first pointed out between the nerves destined for the voluntary, and those for the involuntary, motions.

He placed the origin of the latter in the cerebellum or little brain, of the former in the brain, properly so called. He taught that the motions of the heart, and other vital organs, experience no interruption, because the cerebellum is in a state of constant activity; but that the organs of voluntary motion, on the contrary, require repose, because the brain acts only by intervals *. This distinction was very generally admitted till the middle of the last century.

It was chiefly with a view to it that the division of the eighth pair of nerves †, from which it was maintained that almost all the nerves of the heart proceed, was performed in different countries. The object was to prove that it is from the cerebellum that the heart derives all its power, and it was alleged that the animal died in this experiment, in consequence of the communication between these organs being interrupted. But, besides that it dies too slowly to permit us to ascribe its death to this cause, it has been proved in later times by several

* Tho. Willis Opera Omnia, edente Ger. Balsio Amstelodami, 1682, Tom. i., de Cerebri Anatome, cap. xv. p. 50.

† See the Account of the Nervous System in the preface.

Philosophers, and particularly by M. le Gallois, in a memoir which the Class ordered to be inserted in the transactions of learned correspondents, that death here proceeds from quite a different cause. It has sometimes happened, indeed, that animals have died almost suddenly after the division of the nerves in question, and the partisans of Willis have not failed to lay much stress on this circumstance, of which their adversaries could give no satisfactory explanation. But M. le Gallois has demonstrated, in the memoir to which we have just alluded, that sudden death in this case only happens in certain kinds of animals, and in these only when they are very young, and that it is the effect of suffocation*, more or less complete, from the closing of the upper part of the windpipe. There is nothing then in these facts in favour of Willis; to which we may add, that the eighth pair of nerves does not arise from the cerebellum, and that most of the nerves of the heart do not belong to this pair.

Boerhaave was of the same opinion with Willis; but, besides the power of the nerves, he admitted two other causes of the motions of the heart; the action of the blood of the arteries of the heart on its fibres, and of the venous

* Asphixie. This I translate suffocation, because we use the term Asphixia in a very different sense. Culleni Synopsis Nos. Method. Gen. 44.

blood on the surface of its cavities. According to him, the concurrence of these three causes produces the contraction, and the simultaneous interruption of their action, in consequence of the contraction, gives rise to the dilatation, during which their action is renewed*. But this explanation, with the exception of what regards the stimulant effect of the blood on the internal surface of the heart, is contradicted by fact, which has not prevented its reception in the schools, with another error that has made no less noise.

We allude to Stahl, and his presiding spirit or Archæus, as he calls it, which was supposed by him to regulate all the movements of the living body, subjecting them to the will, or rendering them independent of it, according as they are merely useful, or absolutely necessary to life, and presiding above all over those of the heart, and, through the influence of the nerves, ensuring their continuance; a species of reverie which is inconsistent with all the true principles of Physiology.

After all, where would the Stahlians place this simple and indivisible being? In the brain without doubt. But then, how does it happen that an animal may live, and the mo-

* Her. Boerhaave Instit. Medicæ, § 409.—Vanswieten in Aphorismos, &c. Lugduni Batav. 1745, Tom. ii., p. 18.

tion of its heart continue after it is decapitated? Would they place it in the heart itself? But all animals, and especially those of cold blood, live a longer or shorter time after the heart is removed *.

Other writers, such as Abraham Eus †, Stœhelin ‡, &c., have also endeavoured to explain the motions of the heart; but their systems, almost as soon forgotten as conceived, do not deserve to detain us.

Those of Boerhaave and Stahl reigned almost alone, when, in 1752, Haller published his experiments on irritability. These experiments and those of his followers tend to prove, that the contractile power belongs essentially to the muscular fibre. That property which Haller sometimes speaks of under the name of *vis insita*, sometimes after Glisson, under that of irritability, is the source of all the motions which take place in the animal; but it cannot produce them, except some cause, some stimulant determines it to act. Thus all muscular motion implies two things, the irritability on which the contraction of the muscle depends,

* For an exposition and refutation of this system, see Haller's Element. Physiolog. Tom. i. p. 480—8, and Tom. iv., p. 517—34.

† Dissertatio Physiol. de Causa vices Cordis alternas producente. Ludg. Batav. 1745.

‡ Dissertatio de Pulsibus. Basileæ, 1749.

and the stimulant which determines the irritability to act. The irritability is every where the same. It only varies in intensity in the different muscles; but it does not obey the same stimulants in all the muscles. The nervous power is the natural stimulant to all those which are under the influence of the will; and it is by exciting or suspending the action of that power on the irritability of such or such muscles, that the will causes any particular part to act or to be at rest. It is not thus with the muscles of involuntary motion; these are affected by stimulants of different kinds, which are appropriated to their different functions, and altogether different from the nervous power. It is the blood which is the natural stimulant of the irritability of the heart: alimentary substances, of that of the intestinal canal, &c.

We easily deduce from these principles the explanation of the leading circumstances which we observe in the motions of the heart. Thus its motions are involuntary, because they are independent of the nervous system; they take place without interruption during life, because the irritability which produces them belongs essentially to the fibres of the heart, and the blood which excites them is constantly supplied to this organ by the veins as it is carried off by the arteries. The contraction and dilatation succeed each other alternately and regularly, because the blood always occasions the former both in the auricles and ventricles, and the contraction itself, by expelling the stimulant, occasions the dilatation, which renews the contraction by allowing access to new blood.

Such is a summary view of the celebrated Hallerian theory of irritability. That theory was not contrived in the closet like the others of which we have spoken: it was founded, as we have said, on experiments made by Haller himself, and by the most distinguished of his scholars, who then occupied, or have since occupied, the first rank among the Anatomists and Physicians of the last age. The inferences from these experiments, which were repeated throughout Europe, found almost every where supporters; but they found also some opponents of the greatest reputation. The principal cause of this difference of opinion, and that respecting which authors have not yet been able to come to any agreement, is the question, whether the motions of the heart are really independent of the nervous system?

We may reduce to three heads the facts by which the school of Haller has supported the affirmative. 1st. If we interrupt all communication between the heart and the brain, *the* *only source of nervous power*, by dividing the nerves which go to the heart, the spinal marrow in the neck, or even by decapitation, the motions of the heart continue as before.—2d. If we cut out the heart and place it on a table, it continues to beat, and sometimes for a long time. M. de Humboldt has shewn that it beats more strongly, and for a longer time, when it is suspended. 3d. We always produce convulsions, even for some time after death, in the muscles of voluntary motion, by irritating their nerves, either mechanically or in any other way. On the contrary, the irritation of the nerves of the heart neither occasions a change in its motions nor recals them when they have ceased. The same observation is true of the medulla oblongata * and spinal marrow, the irritation of which occasions strong general convulsions, but produces no effect upon the heart.

These facts are correct, except perhaps those of the third head, respecting which there is some difference of opinion. For in admitting them, the adversaries of irritability have asked, Why, if the nervous power has no action on the heart, is this organ supplied with nerves, and why is it so evidently subjected to the influence

* See the Account of the Nervous System in the Preface.

of the passions? Haller never gave any satisfactory explanation of these objections, but every thing proves that he felt all their force. When we read with attention all that he has said of the motions of the heart, in his dissertations on irritability*, and, above all, in his great work on Physiology†, we are struck with the contradictions which we meet with in them, and which makes the perusal of them fatiguing. Through all of them his great object is to prove, that the motions of the heart are independent of the nervous system. All the facts, all the experiments, all the observations which he brings forward, tend to this end; and yet he seems to admit in several places that the nerves possess an influence over the heart. It is true that it is with an air of doubt that he admits it, and confines himself to saying, that it is possible, that it is not unlikely, that the heart derives a power of motion from the nerves‡. —These contradictions, with which several justly celebrated writers have reproached him, amongst others MM. Prochaska§, Behrends||,

* Mémoires sur la Nature Sensible et Irritable des Parties, etc. Lausanne, 1756.—Opera Minora, Tom. i.

† Element. Physiol. lib. iv. sect. 5, et lib. xi. sect. 3.

‡ Element. Physiol. lib. iv. sect. 5, p. 493, et alibi passim.

§ Opera Minora, Viennæ, 1800, Tom. ii. p. 90.

|| Vol. iii. p. 4. of the Collection of Ludwig, entitled *Scrip-*

Ernest Platner *, &c., proceed evidently from his not being able to reconcile the results of experiments with the influence of the nervous power over the motions of the heart; and, in rejecting this influence, finding it impossible to explain the use of the nerves of the heart, and the effect of the passions on this organ. Here is the great difficulty in the controversy of which we speak. Those who, like Fontana, formally reject all intervention of the nervous influence, have been forced to admit that the nerves, destined to convey to every other part life, feeling, and motion, have no known use in the heart †.

Such consequences evidently disclose the insufficiency of the theory of Haller, and several of his followers have acknowledged the necessity of some modification of it, and admit the nervous power to be one of the principles on which irritability depends. They are thus enabled to assign a use to the nerves of the heart, and to explain the influence of the passions on this organ. But when they have attempted to explain why the interruption of

tores Neurolog. Minores Selecti, Lipsiæ, 1791-5. Four volumes, in 4to.

* Vol. ii., p. 266, of the same Collection.

† Memoires sur les Parties Sensibl. et Irritab. Tom. iii., p. 234. See also Caldani, ib. p. 471, and Le Traité sur le Venin de la Vipère, Tom. ii., p. 169—171.

all communication between the brain and the heart does not stop the motions of the latter, they have been obliged to abandon the generally-received opinion, which regards the brain as the only centre and source of nervous power, and have admitted, without any *direct proofs*, that that power is generated throughout the whole extent of the nervous system, even in the smallest nerves, and that it can exist for a certain time in the nerves of any part independently of the brain. Among the authors of this opinion, the learned Professor Prochaska is one of those who has given the best account of it*. But when he applies it to the motions of the heart, and attempts to explain why they are independent of the will and yet influenced by the passions, his opinion appears undecided. He has recourse to the ganglions†, and hesitates what function to ascribe to them. Sometimes he considers them as knots, as ligatures, so tight as to intercept all communication between the heart and *Sensorium Commune*‡, in the calm

* Commentatio de Functionibus Systematis Nervosi, published in the third fasciculus of the Annotationes Academ. of this writer, and re-printed at Vienna in his Opera Minora, in 1800.

† See the preface.

‡ Those parts of the brain on which sensation and voluntary power seem to depend have been called the Sensorium Commune.

and peaceful state of the system, but not sufficient to prevent the Sensorium re-acting more or less powerfully on the heart in the agitation of the passions *. Sometimes he seems to believe that the interception is complete and constant, and that it is by the nerves of the eighth pair that the passions affect the heart †; and he seems to adopt the opinion of Winslow ‡, renewed by Winterl §, Johnstone ||, Unzer ¶, Lecat **, Peffinger ††, *&c.*, that the ganglions are so many small brains. He admits at the same time that the nerves of feeling are distinct from those of motion, so that the heart cannot contract except when the impression of the stimulant on its cavities is transmitted to the ganglions by the nerves of feeling, and reflected on its fibres by the nerves of motion ‡‡. But besides that this opinion, even by the author's confession, is only a conjecture, it supposes on

* Opera Minora, Tom. ii., p. 165.

† Ibid, p. 167.

‡ Exposit. Anatom. Traité des Nerfs, § 364.

§ Nov. Inflam. Theoria, Viennæ, 1767, cap. 5, p. 154.

|| Essay on the Use of the Ganglions, 1771.

¶ Unzer quoted by Prochaska, Oper. Minor. Tom. ii., p. 169.

** Traité de l'Existence de la Nature et des Propriétés du Fluide Nerveux. Berlin, 1765, p. 225.

†† De Structura Nervorum, Argentorati, 1782, Sect. 1, § 34, inserted in the Collection of Ludwig, vol. i.

‡‡ Opera Minor. Tom. ii., p. 169.

the one hand, that the circulation would continue after the destruction of the spinal marrow; and, on the other, that the heart would cease to beat at the moment when its communication with the ganglions and the plexuses* is interrupted. Now both these suppositions are contradicted by facts.

These fruitless attempts to modify the theory of irritability by the intervention of the nervous power, have only increased the zeal of some authors to maintain that theory in its original purity, and as the use of the nerves of the heart was among the most embarrassing objections to it, M. Sœmmering, one of the most profound Anatomists of Germany, and Behrends, one of his most distinguished scholars, maintained, in 1792, that the heart has no nerves, and that all those which appear to enter it are expended on the coats of its arteries, without the fibres of the heart receiving a single thread†; an opinion which, far from removing all the difficulties, only renders the influence of the passions on the motions of the heart more inexplicable. These two authors maintain that the nerves of the heart support and increase the irritability of its arteries; but the

* See the preface.

† Behrends Dissertatio qua Demonstratur cor Nervis Carere, Moguntiæ, 1792, inserted in the third volume of the Collection of Ludwig.

existence of irritability in the arteries is still doubtful, and were it demonstrated, it would be very strange if irritability depended on the nervous influence in the arteries; and in the heart, the most irritable of all the organs, it were wholly independent of this influence.

Science, however, has cause to rejoice at the groundless doubts proposed by M. Behrends respecting the nerves of the heart, since they have induced the learned Scarpa to take part in the dispute, and have procured for us his excellent work on the nerves of the heart*. M. Scarpa proves in that work that the nerves of the heart are as numerous, and are distributed in the same way, as in other muscles. He admits with M. Prochaska, that sensibility and irritability are essentially united, and that the nervous influence is generated throughout the whole extent of the nerves; but he does not admit that the ganglions are so many little brains†. He seems to believe that the nervous influence, such as it exists in all the nerves, is of itself sufficient for the exercise of the different functions, and that it only wants the stimulant which excites it to action. That the

* Tab. Neurolog. ad Illust. Hist. Anat. Cardiacorum Nervorum, *&c.*

† Ibid, § 30. Ticini, 1794.

stimulant of the muscles of voluntary motion comes from the brain, and that in ordinary states the blood is the stimulant of the heart: but that in vivid emotion the brain also becomes a stimulant to this organ*.

According to this opinion the heart ought to beat in the same manner, and with the same force, after decapitation, after the destruction of the spinal marrow, and after it is removed from the body. M. Scarpa himself compares the beating of the heart in apoplexy to that which we observe when it no longer communicates either with the brain or spinal marrow†. But we shall see in the sequel that it is very different. We must not omit a very important remark of this author, and which it is surprising was not sooner made: it respects the insensibility of the heart when we irritate the spinal marrow and the nerves of the heart. M. Scarpa observes, that that insensibility of which so much has been said, and which has been regarded as a demonstrative proof that the motions of the heart do not depend on the nerves, proves only that the nerves of the heart are not of the same kind with those of the muscles of voluntary motion, and that the nervous power does

* Tab. Neurolog. ad. Illust. Hist. Anat. Cardiacorum Nervorum, *&c.*, § 22, 24, 25, 26, 27, 29.

† Ibid, § 25.

not in them obey the same laws *. This reflection is without doubt very judicious, and it is by an error of experimental logic that we are surprised not to obtain the same effects from the irritation of two orders of nerves wholly different.

The work of M. Scarpa did not induce Dr. Sœmmerring † to change his opinion, nor prevent Bichat from denying that the nervous power has any share in the motions of the heart ‡. This last writer maintains the existence of an animal and organic life, distinct from each other, and of a nervous system for each of these lives. The system of the ganglions, which he regards in the same point of view with the authors above quoted, as small brains, belong to the organic life, and the system of the brain to the animal life §. To be consistent with himself, Bichat should have admitted, like M. Prochaska, that the heart, the centre of organic life, derives from the ganglions the principle of its motions; but he has

* Tab. Neurolog. ad Illust. Hist. Anat. Cardiacorum Nervorum, *&c.* § 20.

† Th. Sœmmerring de Corporis Humani Fabrica Trajecti ad Mænum, 1796, tom. iii. p. 30, 43, 46, 50, et ibid., 1800, Tom. v. p. 43.

‡ Bichat. Recherch. Phys. sur la Vie et la Mort. Paris, 1800, Part ii. Art. 11, § 1.

§ Ibid., Part i. Art. 6, § 4. Ibid., Art. 1, § 2.

not done so. It is chiefly the galvanic experiments which has brought him into this inconsistency, because he had attempted in vain to produce contractions in the heart by galvanising its nerves; experiments on which M. Sœmmerring and Behrends had also endeavoured to support their opinion. These experiments may always succeed, as one of us found in 1797 *, and three years before was found by Mr. Fowler †.

Such is a short but faithful account of the principal systems, by means of which authors have, since the discovery of the circulation of the blood, to this day, attempted to explain the motions of the heart. On taking a general view of them, we remark, that in all those invented before Haller ‡, the nervous power is considered, in one way or other, as one of the conditions essential to the production of the motions of the heart; and it is always and only in the brain that they place the seat of it. The nerves of the heart, therefore, had a determined use in all these systems, and one could easily

* M. de Humboldt. Expériences sur l'Irritation de la Fibre Nerveuse et Musculaire, publiées en 1797, et traduites en Français deux ans après, Tom. i. Chap. 9.

† Experiments on Animal Electricity, by Richard Fowler, 1794.

‡ Also in those of Ens, of Stœhelin, and others of whom we have spoken.

understand why the heart is subject to the empire of the passions; but it was impossible to explain why the circulation continues in animals without the head, and why in experiments on animals, the interruption of all communication between the brain and the heart does not stop the motions of the latter. Since Haller, irritability has been the basis of all these systems. In regarding that property as essential to the muscular fibre and independent of the nervous power, the circulation in animals without the head, and the different phenomena observed in the experiments alluded to, present nothing that is not easily understood; but the use of the nerves of the heart and the influence of the passions on that organ become inexplicable. The necessity of removing these difficulties has produced two parties among the supporters of irritability. The one, zealous favourers of the doctrine of pure irritability, called to their aid the most improbable hypotheses, and all their efforts have only served to prove how difficult it is to support the cause they espouse. The other confounded the nervous power with irritability, which they consider as one of the functions of that power; but they have been obliged to admit, either with respect to the seat, or the manner of existence, of the nervous power, conditions, which, by their own confession, are far from being demonstrated, respecting which they are not agreed, and which, in the application they make of them to the motions of the heart, either do not wholly remove the old difficulties or create new ones.

One may easily see why so little progress has been made in this great and long-disputed question. If we examine all that has been said on the subject since the days of Haller, we shall find, that both sides have constantly brought forward nearly the same facts, the same experiments, and the same reasonings. The only new experiments are the application of galvanism to stimulate the nerves of the heart; and they are only new in appearance, for, from the time of Haller, electricity has been employed with the same view*. It is evident that science had nothing to expect from our pursuing a path trodden for nearly sixty years by so many celebrated men. It was necessary to open new roads; it was necessary to find or invent new modes of interrogating nature. It was, above all, necessary to introduce into physiological experiments, that precision and severe logic to which other branches of physical science have, in our days, owed so great progress.

* See amongst others, Mém. sur les Parties Sens ib. et rritab. Tom. iii. p. 214.

It is this which the author of the memoir before us has done.

It was not the original object of M. le Gallois to explore the cause of the motions of the heart. He had adopted the theory of Haller on this subject, when experiments undertaken with other views led him to the singular conclusion, that it was impossible for him to understand his own experiments, without determining whether the nervous power influences the motions of the heart; and if so, in what way it has this effect. To make his work better understood, we shall relate on what occasion, and by what chain of facts and reasonings he was led to engage in this inquiry.

A peculiar case of labour some years ago excited in him a wish to know how long a full-grown fœtus can live without breathing, after all communication between it and the mother has ceased. That question, curious in itself, and of the first importance in the practice of midwifery and medical jurisprudence, had hardly been touched upon by authors. M. le Gallois undertook to resolve it by direct experiments on animals; and that the solution might be generally applicable, and extend to as many cases as possible, he placed the fœtus of animals in various situations similar to those in which the human fœtus is occasionally placed, when it ceases to communicate with the mother. Among these there is one which occurs too often, namely, the fœtus having the neck drawn asunder in artificial delivery by the feet. The author wished to know what happens to the fœtus in this case, whether it perishes at the instant of the separation, and how death takes place. He found that the trunk retains its life, and that if hæmorrhagy be prevented by throwing a ligature round the vessels of the neck, it dies in the same time and with the same symptoms as if, without taking off the head, respiration had been interrupted; and what completely demonstrated to him that a decapitated animal is in fact suffocated, is, that we may at pleasure prolong its existence by inflating the lungs to supply the place of natural respiration.

M. le Gallois concluded from these facts, that decapitation proves fatal by destroying the motions of inspiration, and that consequently the power on which these motions depend is in the brain; but that that on which the life of the trunk depends is in the trunk itself. Endeavouring to ascertain the precise seat of each of these powers, he found that that on which the motions of inspiration depend resides in that part of the medulla oblongata, that is, the elongation of the brain, which forms the spinal marrow*, from which the eighth pair of nerves take their rise; and that on which the life of the trunk depends, in the spinal marrow. It is not by all the spinal marrow that every part of the body is animated, but only by that portion from which it receives its nerves; so that in destroying any particular part of the spinal marrow, we only destroy life in those parts of the body which correspond to that part. Besides, if we interrupt the circulation in any particular part of the spinal marrow, life is weakened, and soon extinguished in all the parts which receive nerves from it. There are, therefore, two ways of destroying life in any part of an animal; the one destroying that part of the spinal marrow from which it receives its nerves, the other interrupting the circulation in it.

It hence results, that two conditions are necessary to preserve the life of any part of the body, viz., the integrity of the corresponding part of the spinal marrow, and the circulation of the blood, and consequently that we may preserve life in any part of an animal as long as we can preserve in it these two conditions. We may, for example, preserve the life of the anterior parts after that of the posterior parts

* See the Preface.

is destroyed, by destroying the corresponding portion of the spinal marrow, or *vice versâ*.

In all destructions of the spinal marrow, even where the death is sudden, it is instantaneous only in the parts which receive their nerves from the destroyed part, and only extends to the rest of the body at the end of a certain time ; but this time is fixed, and no means can prolong it. It is the same in animals of the same kind and of the same age ; and the longer, the nearer the animal is to the time of its birth.

M. le Gallois, who had so often decapitated rabbits of different ages, had always remarked, that the head, separated from the body, continued to gasp during a time determined by the age. This time was evidently the same as after the destruction of the spinal marrow. Now it is evident that after decapitation there can be no longer any circulation in the head, and that the gaspings which take place in that case can only continue for the time during which life may exist in the brain, after the total ceasing of the circulation. This was the first indication which M. le Gallois had, that when the partial destruction of the spinal marrow occasions death throughout all the rest of the body, it is because it suddenly arrests the circulation ; the truth of which position he ascertained by experiment.

He wished to ascertain in a more direct manner, if the circulation actually ceases at the moment the spinal marrow is destroyed. The absence of hæmorrhagy and the emptiness of the arteries were the most evident signs that he could have of the circulation having ceased; and he found, in fact, that soon after the above operation, the arteries which convey the blood to the head were found empty, and the amputation of the limbs occasioned no hæmorrhagy, though performed near to the trunk, and before life was extinct in the parts of which the spinal marrow had not been destroyed. In a word, all the signs which shew the state of the circulation demonstrated to him, that when the destruction of any part of the spinal marrow suddenly occasions death in the rest of the body, it is by stopping this function; and this effect takes place, not because the motion of the heart immediately ceases, but because it is no longer capable of throwing the blood even into the carotids.

Hence it follows, that it is in the spinal marrow that the power on which the motion of the heart depends resides, and in the whole of it, since the destruction of any one of its three portions is capable of stopping the circulation. It also follows that each portion of the spinal marrow influences life in two different ways; by the one it is essential to the existence of life in the parts which receive nerves from it;

by the other, it preserves it throughout the body in general, by contributing to furnish to the organs which receive nerves from the great sympathetic, and particularly to the heart, the life and power *(le principe de force et de vie)* necessary to the performance of their functions.

Thus we see, that to make the anterior or posterior parts of an animal live after killing the rest of the body, by destroying the corresponding parts of the spinal marrow, we must prevent the destruction of these parts from stopping the circulation. Now this is easily done by diminishing the sum of the forces, which the heart must impart for the support of the circulation, in proportion as we diminish the power which it receives from the spinal marrow. It is sufficient for this purpose to diminish by ligatures, thrown round the arteries, the extent of the parts to which the heart sends the blood. The destruction of the part of the spinal marrow which lies in the loins is quickly fatal to rabbits at or beyond the age of twenty days; but this is not the case if we previously throw a ligature round the aorta in the abdomen between the two great arteries which supply, the one the uppermost viscera of this cavity, and the other, the membrane by which the intestines are suspended *.

* See Preface.

The application of this principle to other parts of the body leads to the singular conclusion, that in order to maintain life in rabbits of a certain age, after the destruction of the cervical part of the spinal marrow, we must previously cut off the head. They certainly die if this part of the spinal marrow is destroyed without previous decapitation. This fact ceases to surprise, when we reflect that by decapitation, we lessen by the head the extent of the circulation, and that by that means the heart having need of less force to support the circulation, we may enfeeble it by the destruction of the cervical part of the spinal marrow, without destroying the circulation.

One may easily conceive that any other operation capable of suspending or considerably enfeebling the circulation in any part of an animal may produce a similar effect; and enable us, in like manner, to destroy such a portion of the spinal marrow, as would have been fatal without this previous operation. This is what happens in the partial destruction of the spinal marrow itself. It has two effects on the circulation; by the one it enfeebles it, generally by depriving the heart of that share of its power which it receives from the part of the spinal marrow that has been destroyed; by the other, without wholly destroying the circulation in the parts which are thus deprived of life, it in a great degree lessens it in a way in some measure similar to the effect of ligatures thrown round the arteries of these parts. But this effect is not remarked till a few minutes after the destruction of the spinal marrow. Thus it is the destruction of the first part of the spinal marrow which enables us to destroy a second; this a third, and so on. For example, when, by cutting off the head of a rabbit, we are enabled to destroy the cervical part of the spinal marrow, the destruction of that part in a certain number of minutes enables us to destroy the fourth part of the dorsal portion of the spinal marrow, and thus by continuing to destroy parts of similar extent, by intervals, we may at length destroy the whole of this portion of the spinal marrow without stopping the circulation, which is then supported by the lumbar portion only.

We may collect from what has just been said, that in rabbits, each portion of the spinal marrow bestows on the heart power sufficient to support the circulation in all those parts which correspond to that portion, and consequently, that in cutting a rabbit transversely, it would be possible to make each portion live for an indefinite time, if the lungs and the heart, necessary for the formation and circulation of arterial blood, could make part of it. But they can only make part of the chest, and one may very well maintain the life of the chest alone and insulated, after having cut off both the head and posterior parts, and prevented hæmorrhagy by proper ligatures, and that even in rabbits thirty days old or more.

Such are the principal results of M. le Gallois' researches, results which arising one from the other, and mutually supporting each other, are founded on direct experiments, made with a precision hitherto unknown in Physiology.

After giving an account of the repetition by M. le Gallois, in their presence, of the experiments on which his inferences are founded, the committee proceed,—

These experiments appear to us completely to confirm all the inferences which the author has deduced from them, and with which he finishes his memoir. To confine ourselves here to the principal points, we shall say, that we regard as demonstrated,

1st. That the cause of all the motions of inspiration has its seat near that part of the medulla oblongata which gives rise to the nerves of the eighth pair.

2d. That the cause which animates each part of the body resides in the portion of the spinal marrow from which the nerves of that part are derived.

3d. That in like manner it is from the spinal marrow that the heart derives its life and its powers; but, from the whole spinal marrow, and not merely from any particular part of it.

4th. That the great sympathetic nerve takes its rise from the spinal marrow, and that the particular character of that nerve is to bring every part to which it is distributed under the immediate influence of the whole nervous power.

These results readily explain all the difficulties which have arisen since the days of Haller, respecting the causes of the motions of the heart. The reader will recollect that the principal of these are, 1st. Why does the heart receive nerves? 2d. Why is it influenced by the passions? 3d. Why is it not subjected to the will? 4th. Why does the circulation continue in decapitated animals and those born without the brain? He will recollect also, that till now no explanation has been able to reconcile these points, or at least has not been able to do so without the aid of hypotheses which we have seen give rise to other difficulties. But now we easily conceive why the heart receives nerves, and why it is so eminently subject to the influence of the passions, because it is animated by the whole of the spinal marrow. It does not obey the will, because none of the organs which are under the influence of the whole nervous power are subject to it. In fine, the circulation continues in decapitated animals and those without the brain, because the motions of the heart do not depend on the brain, or only depend upon it in a secondary way. We ought to remark, that this last point, on which M. le Gallois has thrown so much light, presents only confusion and errors in authors of the old school of Haller, as well as in those of the new school. None of them have distinguished the motions of the heart which take place after decapitation, from those which we observe after the excision of this organ, or after the destruction of the spinal marrow; and they have thought that both were equally capable of maintaining the circulation. But these motions differ essentially. The latter have no power to support the circulation; they are quite similar to the feeble movements which we may excite in the other muscles for some time after death. M. le Gallois calls them motions of irritability, without attaching for the present any other meaning to the term, but that of expressing certain phenomena after death.

We have still one task to perform; to point out what particularly belongs to M. le Gallois in the work which is the object of this report, and what others are entitled to claim.

We can affirm, without fear of contradiction, that every thing in this work belongs to him. To be convinced of this, it is only necessary to read his memoir with attention. Chance suggested to him the idea of his first experiment, and that experiment led him to all the others, each of them being suggested to him, and as one may say, forced upon him by that which preceded it. In following him step by step, one observes that his own method has been his only guide, and that it is that alone which has inspired him. Thus, it is a thing without example in Physiology, to see a work of such length, in which all the parts are so connected, so dependent on each other, that to have the complete explanation of any one fact, it is necessary to recur to all those by which the author arrived at it, and in which it is impossible to deny one inference without denying all those which precede, and disturbing all those which follow it.

One might have expected that in researches so numerous, and which, by the importance of the questions they embrace, have commanded the attention of a great number of philosophers, the author would often have been led, even in confining himself to his own method, to repeat experiments which had been made by others; yet among all the experiments found in his memoir we have remarked only two which had been made before him; one by Fontana, the other by Stenon. The first* consists in inflat-

* Fontana. Traité sur le Venin de la Vipère. *&c.* Tom. i. p. 317.

ing the lungs, and thus preserving the life of an animal after decapitation. Fontana only made that experiment to supply oxygen to the venous blood; and one may easily perceive that he was a stranger to the object before us. As the experiment was unconnected with any other subject, and did not serve as a proof of any point of doctrine, little attention was paid to it; and it was confounded with many other facts, shewing that even warm-blooded animals may live after decapitation without its being suspected that it was the decapitation which enabled them to live in that state. Hence it is that this experiment remained almost unknown except in some of the Schools of England and Germany; and M. le Gallois was wholly ignorant of it when he communicated to the Society of Medicine at Paris his first inquiries into the functions of the spinal marrow. Besides, this experiment in the hands of M. le Gallois was only one of the means by which he demonstrated two of his principal discoveries, namely, that the cause of the motions of inspiration has its seat in the medulla oblongata; and that the cause of life in the trunk resides in the spinal marrow.

The experiment of Stenon is that in which the aorta * is tied in the belly, and then untied,

* See the Preface.

to shew that the interruption of the circulation in any part occasions paralysis of that part, and that the return of the blood restores life to it. This experiment is well known, and has often been repeated. Some of the authors who have made it, had in view to prove that the contractions of the muscles depend on the action of the blood on their fibres; others that the sensibility of every part depends on the circulation; and in both views it served equally to prove or disprove the point, according to the manner in which it was made. Thus, when they secured the aorta, the feeling and motion of the lower parts of the body quickly ceased*. But when the ligature was made lower, and only on one of the arteries into which this vessel divides in the loins, although in this case the circulation was wholly interrupted in the corresponding member, feeling and motion continued in it for a long time†. In these opposite results each author did not fail to adopt those which favoured his own opinion; and he thought himself authorized to do so, as the real cause of the difference was unknown.

In the hands of M. le Gallois the same experiment shews itself under a very different

* Lorry, Journal de Méd. An. 1757, p. 15. Haller, Mém. sur le Mouvement du Sang, p. 203, Exp. 52.

† Schwenke, Hæmatol. p. 8. The 57th and 58th Experiments of Haller, p. 205, are of the same kind.

aspect, and assumes a determined meaning. It is evident that feeling and motion ceasing in the hinder parts, from a ligature being thrown round the aorta, arises from its being only in this case that the circulation is interrupted in that portion of the spinal marrow which gives rise to the nerves of these parts. Such are the only experiments of M. le Gallois, as far as we know, which can be claimed by others; but besides that the manner in which they make a part of his work renders them his own, it appears to us that the new points of view, under which he presents them, and the precision of the details and clearness of the results which he has substituted for the uncertainty and obscurity in which they were formerly involved, have made them experiments wholly new.

We shall finish by a few words on an opinion of M. Prochaska, which may be believed to be similar to that, which M. le Gallois has demonstrated respecting the functions of the spinal marrow. That author places the sensorium commune in the brain and spinal marrow conjointly*. But it is necessary to be aware that he thinks that the nervous power is generated throughout the whole extent of the nervous system, so that every part derives from its own

* Opera Minor. Tom. ii. p. 51. Before him Marherr, Hartley, *&c.*, had been of the same opinion.

nerves, taken alone, the cause of its life and of its movements*. He only regards the sensorium as a central point, where the nerves of feeling, as well as those of motion, meet and communicate, and which establishes the connexion between the different parts of the body†. On the contrary, M. le Gallois has demonstrated that the spinal marrow is not merely a means of communication between different parts, but that from it the cause of the life and power of the whole body proceeds. And what proves that M. Prochaska, in announcing his opinion, which besides he only mentions as a thing probable‡, was far from suspecting the true functions of the spinal marrow, is, that he regards it as only a great bundle of nerves, *crassus funis nerveus*§.

In a word, it appears to us that we may say of the authors who have had some views on the subjects of which M. le Gallois treats, what M. Laplace has said with so much justice on a similar occasion. One may there meet with some truths, but they are almost always mixed with so many errors that their discovery belongs only to him, who, separating them from this

* Opera Minor. Tom. ii. p. 82.

† Ib., p. 151.

‡ Ib., p. 153.

§ Ib., p. 84.

mixture, succeeds by calculation or observation in effectually establishing them*.

The opinion of your Committee is, that the work of M. le Gallois is one of the most excellent, and certainly the most important, which has appeared in Physiology since the learned experiments of Haller; that this work will make an epoch in that science over which it must spread a new light; that its author, so modest, so laborious, so meritorious, deserves that the Class bestow on him its especial commendation, and all the encouragement which it can give. They cannot help adding, that the memoir of which they have given an account is worthy to occupy a distinguished place in the Transactions of learned correspondents, if the publicity of the important discoveries contained in it may be deferred to the time, perhaps distant, of the publication of those Transactions.

(*Signed*) DE HUMBOLDT.
HALLÉ.
PERCY.

The Class approve their Report and adopt its conclusions.

It moreover decrees, that the Report shall be printed in the History of the Class, and that the Committee of the Class shall make arrange-

* Mém. sur l'Adhésion des Corps à la Surface des Fluides, dans la Biblioth. Britan. Tom. xxxiv. p. 33.

ments with M. le Gallois for defraying the expenses which have been occasioned by his experiments, and enabling him to continue them.

Certified to be conformable to the original.

G. Cuvier,
Perpetual Secretary.

CHAPTER II.

Observations on the foregoing Report.

It will be necessary, before the author enters on the account of his own experiments, to make some observations on the foregoing report. As a review of the state of our knowledge of the subject at the time M. le Gallois began his experiments, it appears to be accurate, well arranged, and sufficiently comprehensive. As an account of his experiments and opinions, nothing, as far as the author can judge, can be more clear and correct; as an estimate of the merits of his work, it does not seem to him to deserve the same praise. It overlooks defects, both in the experiments and reasonings of M. le Gallois, of such moment as wholly to invalidate all his most important conclusions; and to leave him the discoverer of certain unconnected though very valuable facts, instead of the author of a new system, founded, as the report alleges, on a basis never to be shaken.

M. le Gallois has demonstrated, that the sudden destruction of any considerable portion of the spinal marrow so enfeebles the power of the heart, that it is no longer capable of supporting the circulation. He has also shewn that the same portion of the spinal marrow, whose sudden destruction destroys the circulation, may be destroyed by small parts without materially affecting it. The question then arises, if, as M. le Gallois supposes, the power of the heart is derived from the spinal marrow, and necessarily ceases when any considerable part of it can no longer perform its functions, why does the particular mode of destroying it make so great a difference in the result? This difficulty occasioned so much trouble to M. le Gallois, that it had nearly induced him to abandon the inquiry. "Après bien des efforts "inutiles pour porter la lumière dans cette "ténébreuse question, je pris le parti de l'a-"bandonner, non sans regret d'y avoir sacrifié "un grand nombre d'animaux, et perdu beau-"coup de temps." Just before, he observes, "En un mot, j'eus presque autant de résultats "différens que d'expériences." And indeed the apparent contradictions in the results of M. le Gallois' experiments are such, as at first view to have persuaded the author that some of his experiments were inaccurate ; on repeating many of them, however, the author was convinced of their accuracy.

He attempts, we have seen, to explain the difficulty in the following manner. He has shewn that if ligatures be thrown round the large vessels, at no great distance from the heart, so as greatly to lessen the extent of the circulation, this organ can still support it, notwithstanding the destruction of such a portion of the spinal marrow as would, under ordinary circumstances, have destroyed it. On the same principle accoucheurs apply tourniquets to the limbs in cases of profuse hemorrhagy. Now, M. le Gallois supposes, that the power of the blood-vessels, as well as that of the heart, depending on the spinal marrow, we greatly impair the vigour of the circulation in any part by destroying that portion of the spinal marrow by which its nervous power is supplied ; and, therefore, that when any portion of the spinal marrow is destroyed by small parts, the vigour of the circulation in the corresponding parts of the body being greatly impaired, nearly the same effect is produced as if ligatures had been thrown round their vessels. It might here be objected, that when a considerable portion of the spinal marrow is at once destroyed, the power of the vessels corresponding to this portion being lost, the effect produced by the ligatures should still be observed. To this the author supposes M. le Gallois would have replied, that as it requires some time for the destruction of any part of the spinal marrow to produce its effect on the vessels, when a large portion is destroyed at once, the vessels not accommodating themselves to the rapid destruction of the successive parts of the spinal marrow, the circulation is lost.

The foregoing explanation resting wholly on the position, that the vessels of any part are debilitated when deprived of the influence of the corresponding part of the spinal marrow, it was incumbent on the Committee to inquire by what experiments M. le Gallois had established it. This question, however, is overlooked by them; and, on reviewing the experiments of M. le Gallois, we find none from which any such inference can be drawn. He attempts to support it only by experiments not properly bearing on the point; although, if the position be correct, the simplest experiments are sufficient to establish it. It is impossible from his experiments to say, whether the diminished circulation in the parts in question arose directly from the destruction of part of the spinal marrow, or from the lessened power of the heart.

Another error, of even greater consequence than the foregoing, in the reasonings of M. le Gallois, which is also overlooked by the Committee, is his inference that the spinal marrow possesses an influence over the heart not possessed by the brain; because he found that removing the brain produces little or no effect on the action of the heart, while crushing the whole, or a considerable part of the spinal marrow, greatly enfeebles it. But to obtain this inference, it is evident that the brain and spinal marrow must be subjected to the same power. They ought both to have been removed or both crushed.

The inferences which M. le Gallois makes from the effects of instantly killing the animal by crushing the spinal marrow are in another respect incorrect. There are two ways in which we may account for the power of the heart, or of the blood-vessels, being destroyed by crushing the spinal marrow. Either the heart and blood-vessels derive their power from the spinal marrow, and consequently lose it on the destruction of the whole or a considerable part of that organ; or, deriving their power from some other source, they are influenced by agents acting on the spinal marrow. It was incumbent on M. le Gallois, therefore, to ascertain by experiment in which of these ways crushing the spinal marrow produces the effects he observed. But he does not even seem aware, that it may act in any other way than that which he supposes.

By the same mode of reasoning, the inference which he draws from the restoration to life of the lower parts of an animal when a ligature, which has been thrown round the abdominal aorta, is removed, is inadmissible ; namely, that when the circulation in every part is destroyed by crushing the spinal marrow, and we find that we cannot by any means restore it, this is to be ascribed to the absence of the influence of the spinal marrow. The same result may arise, it is evident, from the heart and blood-vessels, supposing them to derive their power from some other source, being so deranged by a powerful agent acting through the spinal marrow, that they are no longer capable of performing their functions. M. le Gallois relates no experiment to prove that his explanation ought to be admitted in preference to this; and the Committee speak as if no inference, but that of M. le Gallois, could be drawn from the experiment.

Nor is M. le Gallois' inference respecting the origin of the great sympathetic nerve warranted by his experiments ; namely, that it arises wholly from the spinal marrow. It is true that he has found, that through this nerve a powerful agent, applied to any considerable portion of the spinal marrow, is capable of enfeebling the power of the heart; but nothing said by M. le Gallois proves that the heart may not also be affected in the same way through the brain.

A position on which much of the reasonings of M. le Gallois rests, which is admitted by the Committee, but of which we find no proof in the experiments of this author, is, that the contractions of the heart, which remain after the sudden death caused by crushing the spinal marrow, are of a nature different from those which support the circulation. Observing that after the spinal marrow is thus destroyed, the contractions of the heart are too feeble to support the circulation, without farther inquiry he concludes, that these contractions do not merely differ in degree from those which support the circulation, but, existing independently of the spinal marrow, are wholly of a different nature.

The contractions of the heart, after it is removed from the body, are regarded by M. le Gallois as analogous to those which remain after the spinal marrow is crushed, and he regards in the same light the contractions which may be excited for a short time after death in the muscles of voluntary motion. Had M. le Gallois' mind been unbiassed by his peculiar views of the subject, he would have easily observed a striking difference between the action of the heart, immediately after death caused by suddenly crushing the spinal marrow, and its action immediately after it is removed from the body. In the former instance it is feeble and fluttering, gradually, especially in the cold-blooded animal, becoming stronger and more regular; in the latter instance, it is comparatively strong and regular, gradually, and in the cold-blooded animal very slowly, becoming more feeble. With respect to the contractions of the muscles of voluntary motion after death, it is generally known that these muscles may for some time be excited to the perfect performance of their function. They can be made to move the limbs precisely as they did before the death of the animal. But whether they move them as forcibly or not, and whether or not the heart beats as forcibly after it is removed from the body, as while it supported the circulation, as far as we can see, the action of both is of the same nature as when they performed their usual functions; and M. le Gallois has adduced no proof whatever of its being of a different nature. The experiment, indeed, in which he lessens the extent of the circulation by ligatures, and thus enables the heart to support it after such a portion of the spinal marrow is crushed, as would otherwise have destroyed it, is a sufficient refutation of his own opinion. It proves that the effect of crushing a large portion of the spinal marrow is merely that of enfeebling, not changing the nature of the action of the heart.

Another position of M. le Gallois, admitted by the Committee, which does not seem to be warranted by his experiments, is, that the power, on which all the motions of respiration depend, has its seat near that part of the medulla oblongata*, which gives rise to the eighth pair of nerves. On this subject the author will hereafter have occasion to make many observations; and he will only remark here, that inspiration is a complicated function; and that if any of the powers necessary to it are withdrawn, its most essential motions are as quickly destroyed as if all these powers had ceased. Now M. le Gallois made no experiments to ascertain whether it is by the destruction of one or all of these powers, that inspiration is destroyed by destroying this part of the medulla oblongata.

The argument employed by the Committee in favour of M. le Gallois' opinions from the existence of fœtuses without the brain, is wholly invalidated by the fact, that fœtuses have been born alive without either brain or spinal marrow; for instances of which M. le

* See the Preface.

Gallois himself refers, in the two hundred and fifty-first page of his Treatise, to the *Hist. de l'Acad. des Sciences, An.* 1711, *Obs. Anat.* 3, and *An.* 1712, *Obs. Anat.* 6, but without attempting to shew how it is possible to reconcile his opinions with the existence of such cases. The reader will find, in the fifth volume of the Medico-Chirurgical Transactions, the case of a full-grown fœtus, without either brain or spinal marrow, described by Mr. Lawrence. I have seen a similar case. In this case, as in that mentioned by Mr. Lawrence, in the place of the spinal marrow, there only appeared a vascular membrane.

An inconsistency, of great importance in M. le Gallois' work, which he makes no attempt to explain, is overlooked by the Committee. He observes, in the commencement of his work, "Ce que j'y ai dit du cœur pouvant s'appliquer "aux autres organes des fonctions involon-"taires, la question peut étre considérée plus "généralement, comme la determination du "siége du principe qui préside à cet ordre de "fonctions *." Yet he shews that decapitation does not influence the function of the heart, while the division of the eighth pair of nerves injures that both of the lungs † and stomach.

* Avant-propos, page 1.

† I speak here of the functions of the lungs themselves, not of the muscles of respiration.

It appears from what has been said, as far as the author is capable of judging, that the experiments of M. le Gallois do not warrant any of the positions stated by the Committee as the results of these experiments*.

If these results be not legitimate inferences from the experiments of M. le Gallois, the explanations of the long-contested points respecting the action of the heart, founded on them, are inadmissible; namely, That the heart is supplied with nerves because it derives its power from the spinal marrow; That it is influenced by the passions, because the brain acts upon it through the spinal marrow; That it does not obey the will, because no organ influenced by every part of the nervous power,

* " 1°. Que le principe de tous les mouvemens inspira-
" toires a son siége vers cet endroit de la moëlle allongée qui
" donne naissance aux nerfs de la huitième paire;

" 2°. Que le principe qui anime chaque partie du corps
" réside dans ce lieu de la moëlle épinière duquel naissent les
" nerfs de cette partie;

" 3°. Que c'est pareillement dans la moëlle épinière que
" le cœur puise le principe de sa vie et de ses forces; mais
" dans cette moëlle toute entière, et non pas seulement dans
" une portion circonscrite;

" 4°. Que le grand sympathique prend naissance dans la
" moëlle épinière, et que le caractère particulier de ce nerf
" est de mettre chacune des parties, auxquelles il se distribue,
" sous l'influence immédiate de toute la puissance nerveuse;"
that is of the whole of the spinal marrow, which M. le Gallois regards as the seat of the nervous power.

that is, of the spinal marrow, does obey the will; (it may here be remarked, that were this position admitted, it would by no means explain why the motions of the heart are independent of the will, though influenced by the passions;) and, That the circulation continues in decapitated animals, and those born without the brain, because its direct dependence is not on the brain, but on the spinal marrow.

If the foregoing observations be correct, we must dissent from the following opinion of the Committee: " Ces résultats resolvent sans peine " toutes les difficultés qui se sont élevées depuis " Haller sur les causes des mouvemens du " cœur." The experiments of M. le Gallois, indeed, by ascertaining some facts of great importance, while others immediately connected with them escaped his observation, have left the subject in greater confusion than he found it. Instead of removing the difficulties which formerly existed, the valuable additions which he has made to our knowledge have shewn us others.

The heart's being subject to the passions, yet independent of the influence of the brain, on which so much has been written, does not seem to imply a more direct contradiction, than that the destruction of the same part of the spinal marrow should, according to the way in which it is effected, either destroy the function of the heart, or little, if at all, influence it. The author has had occasion to observe, that M. le Gallois' explanation of this apparent contradiction is not a legitimate inference from his experiments; and he will soon relate some, so simple, that it is impossible to be deceived in their result, which directly refute that explanation.

Why, if the power of the heart depends on the spinal marrow, as it appears to do from the experiments of M. le Gallois, the accuracy of which the author has ascertained by repeated trials, have fœtuses been born alive where no spinal marrow had ever existed?

Why, if the power of the heart depends on the spinal marrow, does it continue to perform its usual motions after it is removed from the body?

Why, if (as M. le Gallois maintains, and it is generally admitted) the various organs of involuntary motion bear the same relation to the nervous system, is the function of the heart uninfluenced by decapitation, and that of the stomach immediately impaired by dividing or throwing a ligature round the eighth pair of nerves?

Why does respiration cease on the destruction of a certain part of the medulla oblongata, since the nerves of the muscles employed in respiration arise from the spinal marrow, which

M. le Gallois has proved to be capable of exciting the muscles independently of the brain? He considers this subject at length in the thirty-fifth and following pages of his treatise, and admits that he can give no explanation of it, calling it "one of the great mysteries of the nervous power, the discovery of which will throw the strongest light on the mechanism of the functions of that wonderful power."

These apparent contradictions, it is evident, as well as those which existed before the discoveries of M. le Gallois, must be reconciled before we can be said to understand the relation which the thoracic and abdominal viscera bear to the nervous system. The doctrine which cannot reconcile them must be erroneous.

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PART II.

Experiments made with a view to ascertain the Laws of the Vital Functions and the Inferences to which they lead.

It appears from the general view of the structure and functions of the animal body, laid before the reader in the preface, that two great systems, the sanguiferous and nervous, pervade every part of it.

The sanguiferous system, it is evident, may be divided into three parts; the heart, the vessels of circulation, and the vessels of secretion. In the following Inquiry, the author will, in the first place, endeavour to ascertain the principle on which the action of the heart and the vessels of circulation depends, and the relation which subsists between them and the nervous system. He will then consider the principle on which the action of the muscles of voluntary motion depends, a subject immediately connected with the former, and the relation which they bear to this system. The comparative effects of stimulants acting through the brain and spinal marrow, on the heart and muscles of voluntary motion, will next be investigated. An account of the experiments on these branches of the subject, though not in the order in which they are here related, was presented to the Royal Society in two papers, composed while the author was still engaged in the Inquiry, and published in the Philosophical Transactions of 1815.

The next object of inquiry will be the principle on which the action of the secreting vessels depends, and the relation which they bear to the nervous system. The principle on which the action of the alimentary canal depends will then be considered, and its relation to this system. These subjects will lead to some experiments and observations on the use of the ganglions*, and the cause of animal temperature.

The relation which the different functions of the animal body bear to each other, and the order in which they cease in dying, will form the next subjects of investigation. The author will then endeavour to point out how far we are enabled to advance in ascertaining the nature of the vital powers; and this Part of the Treatise will conclude with a review of the inferences obtained from the various experiments and observations which shall have been laid before the reader.

* See the account of the nervous system in the Preface.

CHAPTER I.

On the Principle on which the Action of the Heart and Vessels of Circulation depends.

As it is now generally admitted by Physiologists, as appears from the report just laid before the reader, that the heart is capable of performing its functions after the brain is removed, the first question which presents itself is, How far does the power of this organ depend on the influence of the spinal marrow, from which, we have seen, M. le Gallois maintains, that it is wholly derived?

Exp. 1. A rabbit was killed by a blow on the back of the head. The author uses the words death and kill in the usual acceptation, not implying the ceasing of all the functions. After this explanation no ambiguity can arise from the use of these terms. When the rabbit is killed in this way, the respiration immediately ceases; but the action of the heart and the circulation continue, and may be supported for a considerable length of time by artificial respiration, that is, by alternately throwing air into the lungs and allowing it to escape, as practised by Fontana, and since by Chirac, Mr. Brodie,

M. le Gallois, and others*. This mode of killing the animal does not influence the result of the experiment, and has the double advantage of preventing the animal's sufferings, and his motions. Its greatest inconvenience is, that if care be not taken, considerable vessels are sometimes ruptured, and there is almost always some rupture of vessels, which of course tends to impair the vigour of the circulation. It is the usual mode of killing rabbits, and is that which was adopted in all the experiments on the dead animal, except where the contrary is mentioned.

As soon as the animal was killed, the spinal marrow was laid bare from the head to the beginning of the dorsal vertebræ, the circulation being supported by artificial respiration. The chest was then opened, and the heart found beating regularly, and with considerable force. The spinal marrow, as far as it had been laid

* It appears from the first volume of the Philosophical Transactions, that Mr. Hook, in the year 1667, shewed in the presence of the Members of the Royal Society, not only that the life of a dog could be preserved for an hour after the thorax had been opened, and a great part of the diaphragm removed, by alternately inflating the lungs and allowing them to collapse so as to imitate respiration; but that the effect is nearly the same if the lungs are preserved in a state of permanent distention, by air constantly thrown into them, and allowed to escape by small perforations made in their surface.

bare, was now wholly removed, but without in the least affecting the action of the heart. After this, the artificial respiration being frequently discontinued for a short time, we repeatedly saw the action of the heart become languid, and increase on renewing it. The skull was then opened, and the whole of the brain removed, but without any abatement of the action of the heart, which still continued to be more or less powerful, according as we discontinued or renewed artificial respiration. This being for a considerable time discontinued, the ventricles of the heart ceased to beat about half an hour after the removal of the brain. On renewing the respiration, however, the action of the ventricles was restored. The respiration was again discontinued and renewed, with the same effects.

Exp. 2. A rabbit was made insensible by applying opium to the brain. The spine was then opened between the cervical and dorsal vertebræ, the thorax laid open, and the action of the heart supported by artificial respiration. The force with which it beat was carefully observed, and the spinal marrow destroyed by running a small wire up and down the spine, through the opening made in it, by which the action of the heart was not at all affected.

Exp. 3. In the foregoing experiments, it may be said, there was no direct proof of the continuance of the circulation after the spinal marrow, was destroyed or removed. On this account, this and several of the following experiments were made. A rabbit was killed, and the circulation supported by artificial breathing. The large arteries which convey the blood to the head, being exposed, were seen beating, a proof that the circulation continued *. The cervical part of the spinal marrow was then destroyed by a hot wire, after which these arteries were still found beating.

Exp. 4. In a rabbit killed in the usual way, the whole spinal marrow was destroyed by a small hot wire, and the breathing artificially supported. One of the arteries of the neck was then laid bare. Its beating was evident, and on dividing it, florid blood flowed from it freely.

Exp. 5. The only difference between this and the last experiment was, that artificial breathing was not performed. In both, the spinal marrow was destroyed, by introducing a small hot wire through an opening between the cervical and dorsal vertebræ, first through the upper portion into the brain, then through the under portion to the end of the spine. On laying open one side of the neck, the artery was found beating. On dividing it, blood of a much darker colour than in the former experiment was thrown out copiously *per saltum.*

* Towards the end of the Treatise, the author will have occasion to make some observations on the nature of this beating.

Exp. 6. A rabbit was killed in the same way, and artificial respiration maintained. The spinal marrow from the base of the skull to the beginning of the dorsal vertebræ was removed, and the remaining part of it destroyed by a small hot wire. The same artery was then found beating, and, on dividing it, florid blood rushed out with great force *per saltum.*

Exp. 7. This experiment resembled the last, except that the spinal marrow, instead of being partly removed, was wholly destroyed by a hot wire, and artificial breathing was not performed previous to opening the artery, from which dark-coloured blood flowed *per saltum.* The lungs were then inflated, and florid blood soon began to flow copiously from the vessel, and appeared like a red stream mixing with the dark-coloured blood which had previously come from it. This experiment was repeated in the same manner, and with the same result.

Exp. 8. In this experiment the rabbit was rendered insensible, by a blow on the back part of the head, except that the breathing still continued. The spine was opened, and the spinal marrow destroyed, as in the preceding experiment. On introducing the wire through the spine into the brain, the breathing immediately ceased. The artery of the thigh was laid bare about two or three minutes after respiration had ceased. The beating of the artery was evident. On opening it, a dark-coloured blood flowed from it freely. Artificial respiration was now employed. In about half a minute, the blood, which continued to flow copiously from the artery, became of a highly florid colour. The other femoral artery was then opened, from which florid blood also flowed freely. When about an ounce of blood had flowed from the two vessels, the inflation of the lungs was discontinued, and the blood again flowed of a dark colour. On renewing the inflation of the lungs, in less than half a minute, it became of a florid colour. It continued to flow from the arteries altogether for seven minutes. Three minutes after the blood had ceased to flow from them, artificial respiration being continued, one of the great arteries of the neck was opened, from which a florid blood flowed in a free stream, to the amount of a drachm and a half. The flow from the artery ceased in eleven minutes after the first artery had been opened. Most of the blood was now of course evacuated. A good deal had been lost in opening the spine, which always happens. The left cavities of the heart were found nearly empty. The blood which remained in them was florid. The right cavities were full of dark blood.

Exp. 9. From various trials, we found that in such experiments the circulation ceases quite as soon without, as with the destruction of the spinal marrow. Loss of blood seems to be the chief cause which destroys it. If the living animal were operated upon, pain would also contribute to this effect. After the skull and spine were laid open, it was frequently found that the circulation was lost before either the brain or the spinal marrow had been disturbed. The circulation is particularly apt to fail, if artificial respiration is not carefully performed after the animal ceases to breathe. In making such experiments, after opening the bone, it is always necessary to ascertain whether the circulation continues, before we destroy or remove the brain or spinal marrow. As little blood is lost in this part of the operation, when the arteries are beating before, we always find them beating after it. The result of this experiment is still more striking in cold-blooded animals, in which death takes place so slowly, that the circulation continues long after the destruction of the brain and spinal marrow.

Exp. 10. The brain and spinal marrow of a frog were removed at the same time. On opening the thorax, the heart was found performing the circulation freely.

It appears from these experiments that the action of the heart is as independent of every part of the spinal marrow as of the brain; and, consequently, that the opinion of M. le Gallois, that it derives its power from that organ, and particularly from the cervical part of it, must be regarded as erroneous. The author will soon have occasion to consider the facts which led M. le Gallois to this opinion; it will appear that they admit of a very different explanation. He is now to inquire, whether the action of the vessels of circulation is also independent of the brain and spinal marrow.

The following experiments, and some others which he will have occasion to relate, were made on the capillaries of the frog, which, from the extent and transparency of the web of its hind feet, and from its great tenacity of life, appeared the best subject for such experiments. It has been questioned, how far inferences drawn from experiments made on cold-blooded animals, can be supposed to apply to those of warm blood. Both Fontana and Dr. Monro observe, that in their experiments they found the systems of both obeying the same laws. The experiments I have had occasion to make on both sets of animals tend to confirm this observation. There are certain circumstances in which they evidently differ, in all others they seem to agree. As there is no part of the warm-blooded animal on which such experiments on the vessels of circulation, as I shall have occasion to relate here and in the next chapter, can be made except the mesentery, many of them would be attended with much greater suffering in this, than in the cold-blooded animal; and some of them, from the warm-blooded animal being less tenacious of life, could not be so satisfactorily performed on it.

Exp. 11. A frog was killed suddenly by cutting off the head after a ligature had been applied round the neck to prevent loss of blood; much loss of blood immediately destroys the circulation in the extremities. The spinal marrow was then destroyed by a small wire. On bringing the web of one of the hind legs before the microscope, the author found the circulation in it vigorous for many minutes, and in all respects resembling that in the web of a healthy frog. This experiment was repeated with the same result.

Exp. 12. A frog was immediately killed by destroying the brain and spinal marrow by a small wire. After it had lain dead for several minutes, part of the web of one of the hind legs being brought before the microscope, the blood was seen circulating in it as rapidly as in the web of a healthy frog. In making such experiments it is necessary to be aware, that handling and stretching the web tends to impair the vigour of the circulation in it. If this experiment is objected to on account of its being made on an animal of cold blood, the author might, as far as the larger vessels are concerned, refer to several experiments just related, in which the circulation in the arteries of the neck and thigh of rabbits was free after the animal had been killed by a blow on the head, and the spinal marrow had been removed or destroyed.

It appears from these experiments that the vessels of circulation, like the heart, retain their power after the brain and spinal marrow are destroyed or removed, for it will hardly be maintained, that in these instances the power of the heart supports the motion of the blood in the vessels. Should this opinion be maintained, the reader will find it refuted, respecting animals of cold blood, by experiments related in the next Chapter, and respecting animals of warm blood, by those related in Chapter the Tenth.

From the whole of the foregoing experiments it must be inferred, that the position of M. le Gallois, that the circulation in every part of the body depends on the corresponding part of the spinal marrow, and consequently his explanation of the destruction of the same portion of the spinal marrow, destroying the circulation if suddenly effected, but failing to do so if effected slowly, are erroneous*.

It appears a necessary inference from the

* See page 54, et seq.

experiments related in this chapter, that the action of the heart and vessels of circulation depends on a power inherent in themselves, and having no direct dependence on the nervous system. Yet many facts, laid before the reader in the first part of this Inquiry, prove that a certain relation subsists between the nervous and sanguiferous systems. What this relation is we are now to inquire.

CHAPTER II.

On the Relation which subsists between the Heart and Vessels of Circulation and the Nervous System.

It is generally admitted, we have seen, that the action of the heart cannot be influenced by stimulants applied to the brain and spinal marrow; and it seems almost a contradiction to suppose that it should be so, when we see that it cannot be influenced by the total removal of these organs. Many reasons, however, induced the author to try the effect on the heart, of stimulants so applied to the brain and spinal marrow, as not to excite any of the muscles of voluntary motion, whose action, both by throwing more blood towards the heart, and by agitating the animal, prevents our judging of their effect.

Exp. 13. A rabbit was killed in the usual way, the action of the heart supported by artificial respiration, and the brain and cervical part of the spinal marrow laid bare. The thorax was now opened, and the action of the heart, which beat with strength and regularity, observed. Spirit of wine was then applied to the spinal marrow, and a greatly increased action of the heart was the consequence. It was afterwards applied to the brain with the same effect. The increase of motion was immediate and decided in both cases. We could not perceive that it was greater in the one case than the other.

Exp. 14. The foregoing experiment was repeated, with the difference, that the whole of the spinal marrow was laid bare. The motion of the heart was nearly, if not quite, as much influenced by the application of the stimulant to the dorsal, as to the cervical portion of the spinal marrow; but it was very little influenced by its application to the lumbar portion.

Exp. 15. In this experiment, the rabbit was prepared in the same way as in the preceding experiments, except that that part of the brain alone which occupies the anterior part of the head was laid bare. The spirit of wine applied to this part of the brain, produced as decided an effect on the motion of the heart as in those experiments. The spirit of wine was washed off, and a watery solution, first of opium, then of tobacco, applied, with the effect of an increase, but a much less increase of the heart's action than arose from the spirit of wine. The increased action was greater from the opium than from the tobacco. The first effect of both was soon succeeded by a more languid action of the heart than that which preceded their application to the brain. This effect was greatest, and came on soonest when the tobacco was used ; and an evident increase in the action of the heart always occurred when the tobacco was washed off. This could also be perceived, though in a less degree, when the opium was washed off.—Little or none of this debilitating effect was observed when the spirit of wine was used. After its stimulating effect had subsided, the action of the heart only returned to about the same degree as before the application of the stimulant.

Exp. 16. The foregoing experiment was repeated on an animal of cold blood.

Dr. Hastings had found, that immersing the hind legs of a frog in tincture of opium, in less than a minute deprives it of sensibility. This does not arise from any action of the opium; a watery solution of opium, we found, however strong, does not produce the effect. It is produced by simple spirit of wine. It is remarkable, that if simple spirit of wine is used, the animal expresses pain; if tincture of opium, little or none. The author has just mentioned the reason why it is necessary, in order to judge of the result of this experiment, that the animal should be rendered insensible. A frog, being thus deprived of sensibility, the brain and spinal marrow were laid bare, and the chest opened. The heart was found contracting with vigour. Spirit of wine was applied to the spinal marrow with an immediate and evident increase of the action of the heart. It was then applied to the brain with the same effect. Watery solutions of opium and tobacco were also applied to both, with precisely the same effects as in the rabbit. The increase of action from the opium and tobacco was much less than from the spirit of wine, and was soon followed by a great diminution of action. The increase of action was least, and the diminution greatest from tobacco. On washing off the opium and tobacco with a wet sponge, the heart immediately beat more strongly. The different parts of this experiment were frequently repeated with the same result. It is remarkable that the motion of the heart could be affected by stimulants applied to the brain and spinal marrow, after they had all ceased to produce any effect on the muscles of voluntary motion through the medium of the nervous system, and long after the circulation had ceased.

Exp. 17. This experiment only differed from the last, in the cervical part of the spinal marrow and lower part of the brain being removed, and the stimulants applied only to that part of the brain which lies between the eyes of the frog. Spirit of wine, opium and tobacco, thus applied, affected the motion of the heart quite as much, and precisely in the same way, as when they were applied to the entire brain or spinal marrow. When opium and tobacco were applied to the lower part of the spinal marrow, the motion of the heart appeared to be hardly at all affected by them. It was evidently increased when spirit of wine was applied to the same part.

Exp. 18. It was found from repeated trials on the newly dead animal, that considerable pressure either of the brain or spinal marrow produces little or no effect on the action of the heart.

Thus it appears from the preceding experiments, that although the power of the heart is independent of the brain and spinal marrow, it is capable of being influenced through these organs, both by stimulants and sedatives.

All that has been said of the power of the heart is strikingly illustrated by the following experiments.

Exp. 19. The reader has seen that if the head and spinal marrow of a frog be removed, the heart continues to perform its function perfectly for many hours, nor does it seem at all immediately affected by their removal. But we find the effect very different when the most sudden and powerful agent is applied to them. If either the brain or spinal marrow be instantly crushed, the heart immediately feels it. The brain of a large frog was crushed by the blow of a hammer. The heart immediately performed a few quick and weak contractions. It then lay quite still for about half a minute. After this its beating returned, but it supported the circulation very imperfectly. In ten minutes its vigour was so far restored, that it again supported the circulation with freedom, but with less force than before the destruction of the brain. An instrument was then introduced under the heart, and after ascertaining that this had produced no change on its action, the spinal marrow was crushed by one blow, as the brain had been. The heart again beat quickly and feebly for a few seconds, and then remained still, and seemed wholly to have lost its power. In about half a minute it again began to beat, and in a few minutes acquired considerable power, and again supported the circulation. It beat more feebly, however, than before the spinal marrow was destroyed. It ceased to beat in about an hour and a half after the brain had been destroyed. In another frog, after the brain and spinal marrow had been wholly removed without any further injury being done to them, the heart beat for nine hours, gradually becoming more languid.

From this experiment it appears that the heart not only retains its power long after the brain and spinal marrow are removed, but that if they are destroyed in such a way as to impair and almost destroy the action of the heart, it can recover the power of performing its function, after they no longer exist; precisely as a muscle of voluntary motion will by rest recover its excitability, although all its nerves have been divided.

Exp. 20. The foregoing experiment cannot be performed in the same way on warm-blooded animals, but it may be performed in a way equally satisfactory. Two rabbits were instantly killed by crushing the brain by a blow with a hard substance. In both the heart immediately beat with an extremely feeble and fluttering motion.

Exp. 21. A rabbit was instantly killed by crushing, in the same way, only the anterior part of the brain. The effect on the heart was the same as in the preceding experiment.

Exp. 22. A strong ligature was thrown round the neck of a rabbit, and at the moment it was tightened, the head was cut off. The bleeding, except from the vessels defended by the bone, was restrained by the ligature. General spasms made the body hard for the space of between one and two minutes, so that the beating of the heart could not be felt. At the end of this time, the heart was felt through the side, both by Dr. Hastings and the author, beating regularly, and not more quickly than in health. The rabbits used in the three last experiments were of the same age.

Exp. 23. The following experiments are still more conclusive. A rabbit was instantly killed by crushing the anterior part of the brain by a blow, as in Experiment 21. The side was rendered hard by spasm for about half a minute. Neither during this, nor after it, could the author perceive any motion of the heart by applying the hand to the side. The head was then cut off, about three-quarters of a minute after the brain had been crushed. No blood spouted out, and very little ran from the vessels.

Exp. 24. A strong ligature was passed round the neck of a rabbit of the same age with that used in the preceding experiment. It was suddenly tightened, and the head cut off. In this instance little spasm took place, and the heart was found beating regularly under the finger for about three-quarters of a minute. At the end of this time the ligature was slackened, and the blood spouted out to the distance of three feet, and continued to spout out with great force, till nearly the whole blood was evacuated.

Exp. 25. From the strength of the spine of a rabbit, and the situation of the neighbouring parts, it is impossible to crush it, without directly influencing the state of the heart by the blow. It was opened between the cervical and dorsal vertebræ, and the animal suddenly killed by a steel rod of considerable thickness, passed through the cervical part. As in the experiments of M. le Gallois, the action of the heart was immediately debilitated. In the preceding experiments, the reader has seen, both the cervical and other portions of the spinal marrow repeatedly slowly destroyed, or removed entirely, without at all influencing the action of the heart.

These experiments point out an easy solution of the difficulties stated by M. le Gallois in the 119th and following pages of his treatise. When in his experiments the greater part of the spinal marrow was destroyed by small portions at a time, comparatively little effect was produced on the heart; but when a considerable part of it was crushed at once, the power of the heart was so impaired, that the circulation ceased. Thus in some of his other experiments, where the injury was inflicted slowly, and where it was inflicted suddenly, the result was found to be different. He observes, that if the spinal marrow be divided near the back of the head, and a certain part of it immediately destroyed, the circulation ceases. If some time intervene between the division and the destruction of precisely the same part, the circulation is not interrupted.

In his experiments, the spinal marrow was always crushed by a stilet, of the same dimensions with the cavity of the spine. In the corresponding experiments related in this treatise, the spinal marrow was either removed, or destroyed by a comparatively small wire, moved about in it till its functions ceased. The reader will easily understand, from the experiments just laid before him, why this apparently slight circumstance occasions so essential a difference in the result. The effect on the heart is the same whether the animal be previously killed by means that do not immediately affect this organ, or death be occasioned by the blow which causes the instantaneous destruction of the brain or spinal marrow.

M. le Gallois, the reader has seen, maintains that affections of the brain influence the heart only through the medium of the spinal marrow. But in Experiment 17th, after the lower part of the brain and cervical part of the spinal marrow of a frog had been removed, agents, applied only to the anterior part of the brain, affected the action of the heart as much as when applied to the brain, while both this organ and the spinal marrow were entire. To remove any objection which may arise from the subject of this experiment having been an animal of cold blood, the following was made:—

Exp. 26. A rabbit was suddenly killed by dividing the spine near the head. The spine was then divided near the lower end also, and by means of a wire, introduced at these parts, the spinal marrow was destroyed. Spirit of wine was then applied to the brain, which influenced the action of the heart as readily, and to as great a degree, as it does when the spinal marrow is entire.

We are now to inquire how far the vessels of circulation are capable of being influenced through the brain and spinal marrow.

In order to ascertain whether the vessels can be stimulated through these organs independently of their action on the heart, it is necessary, in the first place, to determine how far the vessels can support the motion of the blood independently of the heart.

M. Bichat* has shewn that in a frog the motion of the blood continues in the capillaries after the heart no longer propels it. This observation, indeed, we shall afterwards find applies to the warm, as well as the cold-blooded animal.

Exp. 27. When a frog is decapitated without much loss of blood, and then a ligature thrown round all the vessels attached to the heart, on the web of one of the hind legs being brought before the microscope, the circulation in it is found to be vigorous, and will continue so for many minutes; at length gradually becoming more languid.

In endeavouring to proceed farther, the author found much difficulty. It is necessary, in order to ascertain the effect of stimulants applied to the brain or spinal marrow on the vessels of the web, to destroy the sensibility, to remove the heart, and to lay open the cranium or spine; were not the sensibility destroyed, the voluntary motions of the animal, which would continually occur, could not fail to accelerate the motion of the blood in the web.

Exp. 28. A frog was deprived of sensibility

* Recherches Phys. sur la Vie et la Mort.

and voluntary power, by the upper parts of the body being immersed in laudanum; part of the cranium was then removed, after a ligature had been thrown round the neck to prevent loss of blood. The thorax was now opened, and the vessels attached to the heart included in a ligature. But, notwithstanding this experiment was repeatedly performed with the greatest care, the circulation by all these preparatory means was so enfeebled, that although the blood still moved in the web, it was in so irregular and uncertain a way, that the author never could arrive at any positive conclusion respecting the effect of the stimulant applied to the brain. After several fruitless attempts, therefore, he abandoned this mode of making the experiment.

Although the action both of the heart and muscles of voluntary motion so influenced the effect of stimulants, applied to the brain, on the circulation in the foot, that, without wholly preventing it, no conclusion can be drawn, it is evident that the sedative lessening the power of the heart will not affect the result of the experiment, if it be made on the web of the frog. We have just seen, that the total ceasing of the action of the heart does not, for a considerable time, affect the circulation in it. The following experiments appear to be decisive respecting the effect of the sedative, and of the stimulant, as far as this can be decisive, the action of the heart remaining. It is evident that the action of either stimulant or sedative is equally conclusive respecting the direct influence of the nervous system on the blood vessels.

Exp. 29. Part of the cranium of a frog was removed, the web of one of the hind legs brought before the microscope, and the circulation in it observed. The animal was then rendered insensible by the immersion of the other hind leg in laudanum. The insensibility did not in the least affect the circulation in the web before the microscope. Spirit of wine was then applied to the brain, with an evident increase of the velocity of the blood in the web. The same effect was produced in a less degree by watery solutions of opium and tobacco. After the tobacco had been applied for about half a minute, the motion of the blood was much less rapid than before its application. On washing off the tobacco the velocity of the blood increased, and was again lessened on applying it. This was repeated several times with the same effects. The following way of performing the experiment is equally conclusive.

Exp. 30. A frog was rendered nearly insensible by having its back immersed in laudanum. A ligature was then thrown round the neck to prevent loss of blood, part of the cranium removed, the web of one of the hind legs brought before the microscope, and the circulation in it, which was rapid, observed. A strong infusion of tobacco was then applied to the brain, with the effect of at first rendering the circulation more rapid. In about half a minute it became more languid, and soon stopped altogether. On the infusion of tobacco being washed off, the circulation returned and regained considerable vigour. The tobacco was several times applied to the brain and washed off, with the same effects. It may be observed, that when the circulation in the web had almost ceased after the tobacco had been washed off, its velocity was immediately increased on applying spirit of wine to the brain.

The power of the blood-vessels, like that of the heart, is capable of being directly destroyed through the medium of the nervous system.

Exp. 31. The web of one of the hind legs of a frog was brought before the microscope, and while Dr. Hastings observed the circulation, which was vigorous, the brain was crushed by the blow of a hammer. The vessels of the web instantly lost their power, the circulation ceasing; an effect which cannot arise, we have seen, from the ceasing of the action of the heart. In a short time the blood again began to move, but with less force. This experiment was repeated with the same result. If the brain is not completely crushed, although the animal is killed, the blow, instead of destroying the circulation, increases its rapidity.

Exp. 32. The spine of a frog was laid open at the lower end, and the animal suddenly killed by a wire of nearly the same dimensions with its cavity, passed through it, as in M. le Gallois' experiments. The web of one of the hind legs was then brought before the microscope, and the circulation in it was found to have wholly ceased. In another frog, as the reader has seen*, the spinal marrow was destroyed by introducing in the same way, and moving in various directions, a wire much smaller than the cavity of the spine, yet the circulation in the web continued vigorous.

Exp. 33. Analogous to what happens with respect to the heart, as the author will have occasion to point out more particularly in the fourth chapter, he could never, either by chemical or mechanical agents, if we except crushing the brain or spinal marrow, excite any irregular action in the blood-vessels. Their action was only rendered more or less powerful.

The irregular appearances in the circulation in the web of a frog's foot, mentioned by Dr. Thompson, lately Professor of Military Surgery in the University of Edinburgh, in his Lectures

* See Experiment 12.

on Inflammation, published a few years ago, and which he ascribes to inflammation, may be observed in any case, if the vessels be at all compressed in applying the foot to the microscope ; and although they are not compressed, these appearances very generally occur when the circulation begins to fail. The blood will then stop and go on at intervals, and move backwards and forwards in the same vessel. The author has often watched the capillaries from the commencement of inflammation to its greatest height, when the part is about wholly to lose its vital power, in the mesentery of a rabbit, the web of a frog's foot, and the fins of fishes, without perceiving the least tendency to this irregular motion when the part viewed was so applied to the microscope as not to compress any of its vessels*. When the circulation fails without any morbid distension of the vessels, the motion of the blood becomes irregular before it stops altogether ; when it fails from morbid distension of the vessels, which gives rise to the phenomena of inflammation, this irregularity is not perceived, the motion of

* An account of these experiments is published in the introduction to the author's Treatise on Symptomatic Fevers, and a plate given representing the state of the vessels in the different stages of inflammation. See also the last part of this Inquiry.

the blood gradually becomes slower, till it ceases altogether.

What are the simple results of the experiments related in this and the preceding chapter? The first set prove, that the power of the heart and vessels of circulation is independent of the brain and spinal marrow, for we find that the functions of the former organs continue after the latter are destroyed or removed, and that their removal is not attended with any immediate effect on the motions of the heart and vessels. The second set prove, that the action of the heart and vessels of circulation may be influenced by agents applied either to the brain or spinal marrow. It is as readily influenced by agents applied to the anterior part of the brain, as by those applied to the cervical part of the spinal marrow. This is what we should expect when we trace the origins of their nerves.

If it be said that the results of these experiments imply a contradiction, that we cannot suppose the power of the heart and vessels to be wholly independent of the brain and spinal marrow, and yet influenced by agents applied to them, the reply is, that such are the facts, of the truth of which any one may easily satisfy himself by experiments on the newly-dead animal.

On a closer examination of the phenomena of the nervous system, we shall find other similar difficulties. The experiments of M. le Gallois prove, in the most satisfactory manner, that a principal function of the spinal marrow is to excite the muscles of voluntary motion, and that it can perform this office independently of the brain. It performs it after the brain is wholly removed, and its powers seem not at all immediately impaired by the removal of the brain; yet we constantly see injuries of the brain impairing the functions of the spinal marrow. Of this apparent inconsistency, M. le Gallois justly remarks, that two facts well ascertained, however inconsistent they may seem, do not overturn each other, but only prove the imperfection of our knowledge.

Whichever of the disputed opinions, respecting the functions of the nervous system, we adopt, the foregoing phenomena seem to imply a contradiction. The experiments related in the following chapter point out still another instance of this apparent contradiction, and seem to suggest the principle on which it as well as the others depend.

CHAPTER III.

On the Principle on which the Action of the Muscles of voluntary Motion depends, and the relation which they bear to the Nervous System.

WE are now to consider how far the principle on which the action of the muscles of voluntary motion depends, and the relation which they bear to the nervous system, resemble those of the heart and vessels of circulation.

Exp. 34. By the application of stimulants to the spinal marrow of a newly dead frog, strong and repeated contractions were excited in the muscles of the hind limbs, as long as the stimulants would produce the effect. On examination, the muscles of these limbs were found wholly deprived of their excitability. We should conclude from this experiment, that the power of the muscles of voluntary motion is dependent on the nervous system ; on the other hand, it is well known, that although all the nerves supplying the limbs of a frog be divided, and cut out close to the place where they enter the muscles, the latter still retain their excitability, which appears to be not at all less than when the nerves are entire ; the fact which suggested to Haller the independence of the muscular power.

His opponents, however, objected to his inference, because although the division of the nerves may prevent the muscle from receiving more nervous power, it does not deprive it of that already bestowed on it, either forming a necessary part of its power, or dispersed through its substance in nerves too small to be removed; and this objection appears to be greatly strengthened by the muscle soon losing its excitability after it is separated from the body, and those muscles, whose function is supported by stimulants peculiar to themselves, being still supplied with nerves.

It appeared to the author that this question could only be determined by some experiment capable of ascertaining whether the excitability of muscles is maintained by the influence they receive from the nervous system, or impaired as it is found to be by other stimulants; for if on the one hand it can be proved that the permanency of their excitability is unimpaired by cutting off all supply from the nervous system; and on the other, that the influence of that system exhausts it as other stimulants do, a doubt, the author conceives, cannot remain respecting the dependence of muscular power on the muscular fibre itself.

Exp. 35. The nerves of one of the hind legs of a frog were divided, and thus the limb wholly deprived of sensation and voluntary power. The skin was removed and a stimulant constantly applied to the muscles till no further contraction could be excited in them, which happened in twelve minutes. The corresponding muscles of the other limb were in the same manner subjected to the same stimulant. Here the muscles were subjected to the stimulant, while the influence of the nervous system was as usual freely communicated to them; and as the animal, while the nerves are entire, by influencing the muscles to which the stimulant is applied, always endeavours to counteract its effect, the muscles were now exposed both to the effect of the artificial stimulant and that of the nervous power.

They lost their contractile power in ten minutes; and in a repetition of the experiment, they lost it in half the time required for exhausting those whose nerves had been divided.

It is evident from these experiments, that the influence of the nerves, so far from bestowing excitability on the muscles of voluntary motion, exhausts it like other stimulants. The excitability, therefore, is a property of the muscle itself. Yet all admit that these muscles are under the influence of the nervous system. It even appears from Experiment 34, that their power may be wholly destroyed by changes induced on that system. On the same principle we explain the seeming contradiction respecting the action of the heart and vessels. We have seen that their power exists as independently of the brain and spinal marrow, as that of the first muscles to which the artificial stimulant was applied, whose nerves had been divided; but, while the brain and spinal marrow retain their functions, and the connexion of nerves is entire, the heart and vessels, as well as the muscles of voluntary motion, may be influenced by agents acting through the nervous system. It is not difficult to account for these muscles being more copiously supplied with nerves than the heart, influenced as they are in all their usual functions through their nerves; while the heart is only occasionally excited through the nervous system, its usual stimulant being as immediately applied to it, as the artificial stimulant was to the muscles of the limb in the foregoing experiments, and acting as independently of the nervous system.

In the experiments which have been laid before the reader, he cannot surely see any difference in the nature of the muscular power of the heart, and that of the muscles of voluntary motion, except their being fitted to obey different stimulants, a difference which, as might be expected, appears, from direct experiment, to exist in the two sides of the heart itself, the natural stimulant of the one being red, of the other black, blood.

It may here be objected, that in apoplexy the power of the muscles of voluntary motion is lost, while that of the heart is often little or not at all impaired. Were such the fact, this objection would be unanswerable; but the author has repeatedly examined the muscles of voluntary motion in a state of apoplexy, both in warm and cold-blooded animals, and found their excitability unimpaired. It is not their power, but the stimulant which excites them, that is lost in apoplexy. In this disease, the heart continues to contract, because its stimulant is still supplied; the muscles of voluntary motion cease to contract, because their stimulant is no longer supplied*.

The conclusions afforded by the foregoing experiments so far agree with those of Haller, that they prove the heart and other muscles to possess an excitability independent of the nervous power; but they prove, in opposition to the system of that great physiologist, that the heart is, equally with the muscles of voluntary motion, capable of being stimulated by that power.

* See Chap. 10.

In the report of the Institute of France, which has been laid before the reader, it is observed, " the adversaries of irritability have " asked, why, if the nervous power has no ac- " tion on the heart, is this organ supplied with " nerves, and why is it subjected to the influ- " ence of the passions? Haller never gave any " satisfactory reply to these objections, but " everything proves that he felt all their force." These objections, we have seen, prevented Haller's doctrine of irritability from being generally admitted by physiologists, and at length led M. le Gallois to suppose that he had wholly refuted it.

We may, it appears to the author, trace the subject farther. The reader has seen that the spinal marrow is capable of exciting the muscles, and we shall find it capable of its other functions, independently of the brain, yet we constantly see the spinal marrow influenced through the brain. Thus the excitability of the spinal marrow bears the same relation to the brain, which that of the muscles bears to the spinal marrow and nerves. Even M. le Gallois, although his experiments lead to an opposite conclusion*, observes, that the brain seems to act on the

* He infers from his experiments that the power of the heart ceases on the destruction of the spinal marrow, but that that of the spinal marrow remains after the destruction of the brain.

spinal marrow as the latter does on the parts it animates. We know the peculiar office of the brain, by observing what functions are lost by its removal, the sensorial functions. It would appear, therefore, that as the muscular obeys the nervous; the nervous obeys the sensorial, system.

In a future part of this Inquiry, the author will endeavour, by experiment, to point out with more precision than has hitherto been done, the line of distinction between the sensorial and nervous functions, which appears to be very different from that assumed by M. le Gallois†; and to ascertain the relation which they bear to each other.

CHAPTER IV.

On the comparative Effects of Stimulants, applied to the Brain and Spinal Marrow, on the Heart and Muscles of voluntary Motion.

In making the experiments related in the preceding chapters, it was evident, that although the muscles of involuntary are equally with

† See Chap. 11.

those of voluntary motion subject to the effects of stimulants applied to the brain and spinal marrow, the laws which regulate these effects on the two sets of muscles are very different. The author has endeavoured by the following experiments to ascertain in what this difference consists.

Exp. 36. Part of the cranium of a rabbit was removed, and a wire passed in various directions through the brain, which, so destitute of sensibility is the brain, occasioned no pain. The muscles of voluntary motion could not in this way be at all excited, except when the wire approached those parts of the brain from which the spinal marrow and nerves originate. Some of the muscles of voluntary motion were then excited. The whole of the upper and anterior part of the brain was sliced off, without giving any pain or affecting the muscles of voluntary motion. The instrument only excited their action when it approached the source of the spinal marrow and nerves.

Exp. 37. A rabbit was killed in the usual way, part of the cranium was then removed and the thorax laid open. The heart was found beating regularly. By passing a wire through the brain in any direction, the beats of the heart were accelerated and rendered stronger. The author could not perceive that this effect was produced more powerfully when the wire was directed towards the source of the nerves, than when any other direction was given to it, provided it passed through an equal portion of the brain. When an instrument was merely pressed on the surface of the brain, the effect was similar. When a pair of scissors, or any other thing of larger bulk than the wire, was passed into the brain, the effect on the heart was greater than from the wire. It was still greater when the brain was wounded rapidly in many directions.

Exp. 38. Part of the cranium of a rabbit was removed, and after passing an instrument through the brain in various directions near to the origin of the nerves, which excited strong spasms in the muscles of voluntary motion, the blood being absorbed by a sponge, spirit of wine was applied to the surface of the brain, and dropt it into its substance, without causing any pain, or at all affecting the muscles of voluntary motion. The upper part of the brain was then wholly removed, and spirit of wine applied to it, but neither signs of pain nor spasms of the muscles of voluntary motion ensued.

Exp. 39. Another rabbit was killed in the usual way, and part of the cranium removed. The thorax was then laid open, and the heart found beating regularly. Spirit of wine was now applied to the surface of the brain, by which the frequency and force of the heart's beats were immediately increased. Several cuts were then made in the brain, and the spirit of wine dropt into them, by which the action of the heart was increased in a much greater degree. Spirit of wine increased the action of the heart more than any mechanical injury, which never produced the strong action in this organ, that it does in the muscles of voluntary motion.

This experiment was repeated with a watery infusion of opium instead of spirit of wine; the result was in all respects the same, except that the action of the heart was less increased than by the spirit of wine.

Exp. 40. Under the term brain, the author means to include the cerebellum as well as the brain properly so called. From many trials on the newly-dead rabbit made to ascertain the point, he could not perceive that the heart is more or less affected either by chemical or mechanical stimulants applied to the cerebellum than to the brain; nor are the muscles of voluntary motion affected by wounding the cerebellum, except we approach the source of the spinal marrow and nerves. In some of his experiments, the author thought that stimulants applied to the cerebellum affected the action of the heart rather more powerfully than when applied to the brain; but this was contradicted by other experiments.

Exp. 41. The head of a rabbit was cut off close to the occiput. For some time the trunk and limbs were affected with strong spasms. The slightest touch of a wire applied to the cut end of the spinal marrow, after the spasms had subsided, immediately excited the action of some of the muscles of voluntary motion. The strongest spirit of wine and watery infusion of opium were applied to it, without producing the least effect on those muscles. The application, however, of stronger chemical stimulants, the nitric and muriatic acids, threw the muscles of the fore-legs into powerful contractions. A repetition of this experiment gave the same results.

In a rabbit killed in the usual way, the reader has seen that both spirits of wine and a watery infusion of opium applied to the spinal marrow, increase the action of the heart*.

Exp. 42. It was repeatedly found both in newly-dead rabbits and frogs that, after all stimulants applied either to the brain or spinal marrow had ceased to produce any excitement in the muscles of voluntary motion, both chemical and mechanical stimulants so applied still increased the action of the heart; the former more than the latter.

Exp. 43. The author tried in every possible way, both by mechanical and chemical stimulants, to excite, through the brain or spinal

* Exp. 13.

marrow of newly dead rabbits and frogs, any irregular action in the heart which is so readily excited in the muscles of voluntary motion, but could not. Nor could he by sedatives (such substances as tend to allay action), applied to any part of the nervous system, occasion any irregular action in this organ. Its action was rendered quicker or slower, more or less frequent, stronger or weaker, but never irregular. Irregular action was never excited in the heart, except when its power was nearly destroyed by crushing the brain or spinal marrow.

Exp. 44. He found from many trials both on newly-dead rabbits and frogs, that the excitement of the muscles of voluntary motion took place chiefly at the time the stimulant was applied to the brain or spinal marrow. It was generally necessary to move the instrument, thus applying it to a new surface, in order to support the effect. It is known that in the living animal repeated contractions of the muscles of voluntary motion will sometimes continue, assuming the form of a fit, as long as an extraneous body remains in the brain. The increased action of the heart could generally be observed as long as the stimulant, whether chemical or mechanical, was applied, unless it was of a nature to produce the sedative after the stimulant effect. The sedative effect is so far from being the consequence of previous excitement, as many physiologists have supposed, that spirit of wine and mechanical stimulants, which produced no sedative effect, but continued to stimulate the heart as long as they were applied, produced a much greater degree of excitement than tobacco, whose slight stimulant effect was quickly succeeded by a powerfully sedative one.

It appears from these experiments, that chemical stimulants, applied to the brain and spinal marrow, exert a greater power over the heart than mechanical stimulants, while the latter exert a greater power over the muscles of voluntary motion than chemical stimulants ; that both chemical and mechanical stimulants, applied to the brain and spinal marrow, excite the heart, after they cease to produce any effect on the muscles of voluntary motion ; that stimulating every part of the brain and spinal marrow equally affects the action of the heart, while the muscles of voluntary motion are only excited by stimulants applied to the parts of those organs from which their nerves originate ; that stimulants applied to the brain and spinal marrow never excite irregular action of the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion ; and that their effect on these muscles is felt chiefly on their first application, but continues on the heart as long as the stimulant is applied. These differences in the effects of stimulants applied to the brain and spinal marrow on the muscles of voluntary and those of involuntary motion, must be explained before we can be said to understand the relation which subsists between the nervous system and the heart.

It appeared to the author probable, from many experiments, that the cause of chemical stimulants, applied to the nervous system, producing a greater effect on the heart than mechanical stimulants do, is, that the former from their nature act on a larger portion of the brain and spinal marrow. If this opinion be correct, the mechanical stimulant will be rendered the most powerful by confining the chemical to a smaller space than the mechanical stimulant occupies.

Exp. 45. In newly-dead frogs and rabbits, the author applied to various parts of the brain and spinal marrow, and particularly to those parts from which the nerves originate, minute portions of strong spirit of wine, without at all influencing the action of the heart. When these small portions were applied to a great many parts, the heart began to beat more frequently. This, of course, was much the same thing as at once applying the spirit of wine to a larger part.

It appears from the foregoing experiments, that mechanical stimulants, applied to any considerable portion of the nervous system, increase the action of the heart; and from the following, that we cannot affect the heart by such stimulants confined to any small part either of the brain or spinal marrow.

Exp. 46. In a rabbit, killed in the usual way, different small parts of the brain were wounded with a wire, particularly all those parts near which the nerves of the heart appear chiefly to originate; but the motion of this organ remained uninfluenced, while at the same time passing the wire through any considerable portion of the brain immediately accelerated it.

Exp. 47. The cervical part of the spine of a newly-dead rabbit was laid open, and a wire was repeatedly passed transversely through the spinal marrow, without being able at all to affect the motion of the heart; but on the wire being passed longitudinally, so as to bring it into contact with a larger portion of the spinal marrow, the motion of the heart was immediately accelerated. On the same principle, when the wire was made to wound many minute portions of the brain and spinal marrow in quick succession, the action of the heart was increased. In another newly-dead rabbit, the spinal marrow was divided close to the head, without at all affecting the heart.

Mr. Clift, in an account of experiments on the carp, published in the *Philosophical Transactions for* 1815, observes, that on dividing the spinal marrow at the occiput, the action of the heart was greatly accelerated for a few beats; but he divided the spinal marrow while the animal retained the power of the muscles of voluntary motion, which never fail to be called into action, by wounding it, and whose action, by increasing the flow of blood, always accelerates the motion of the heart*.

Thus we see that neither chemical nor mechanical stimulants, applied to the brain and spinal marrow, affect the action of the heart, unless they make their impression on a large portion of these organs. In the various experiments just related, every part of them was stimulated individually, without the action of the heart being influenced; and the stimulant being the same, the force with which it acted on this organ was always proportioned to the extent of surface to which it was applied. The

* It is particularly satisfactory to the author that Mr. Clift, on repeating his experiment, in which the spinal marrow was destroyed by a small wire, found the same result in the carp, which the author had done in rabbits and frogs. Mr. Clift did not ascertain whether the circulation continued after the destruction of the spinal marrow, but from this occasioning little or no diminution in the action of the heart, we cannot doubt the continuance of the circulation.

author could not find that it was of any importance what part of the brain was stimulated. Even stimulating the surface alone, either mechanically or chemically, immediately increased the action of the heart. The muscles of voluntary motion, on the contrary, it appears from the foregoing experiments, are wholly insensible to stimulants applied to the brain, except near the origin of the nerves and spinal marrow.

Another circumstance, which appears to be of great importance in tracing the cause of the different effects of stimulants applied to the brain and spinal marrow on the muscles of voluntary and involuntary motion, is, that the heart obeys a much less powerful stimulant than the muscles of voluntary motion do. It appears from what has been said, that only the most powerful chemical stimulant affects them, while all that were tried readily influenced the action of the heart. Mechanical stimulants which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical stimulants, indeed, from their effects on the heart, we should consider the most powerful. But their greater effect on this organ is readily explained by the influence of stimulants applied to the brain and spinal marrow on the heart, being proportioned to the extent of surface to which they are applied. It is evident that the stimulant can be applied to a greater extent of surface in the fluid, than in the solid form. When the effect of the mechanical agent is rendered extreme, we find its influence on the heart far greater than that of any chemical agent. From experiments related in the Second Chapter of this Part, it appears, that suddenly crushing any considerable portion of the brain or spinal marrow instantly destroys the power of the heart.

The conclusions then at which we arrive are, that the heart is excited by all stimulants applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only excited by intense stimulants applied to certain small parts of them.

These facts being ascertained, the other differences observed in the effects of stimulants applied to the brain and spinal marrow, on the heart and muscles of voluntary motion, are easily explained.

Irregular action of a muscle arises from stimulants acting partially, or at intervals, on its nerves, or on the particular part of the brain or spinal marrow, from which its nerves arise. But very partial action of a stimulant on the brain and spinal marrow, we have just seen, is incapable of exciting the heart, and while the stimulant is applied to any part of these organs, as all parts of them seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion when it is moved from place to place in the brain.

When in the newly-dead animal the instrument is kept still after it is introduced into the brain, the action of the muscles of voluntary motion ceases; its merely being in contact with the parts of the brain which excite these muscles, not being sufficient to call them into action. As the muscles of voluntary motion feel the impressions made on a very small part of the brain only, in proportion as this part is small, the impression must be great to affect them; but the heart, which is influenced through all parts of it, though not very powerfully through any one, feels all the impressions made on this organ, provided they are made on a sufficiently extensive portion of it; thus, within certain limits, as long as the instrument remains in the brain, its stimulant effect on the heart continues.

It is true, that although the heart is only influenced by agents applied to a large portion of the brain, we may conceive them so applied as to produce irregular action in it, and we find that certain irritations of the nervous system have this effect. But it is evident, that the heart not being subject to stimulants whose action is confined to a small portion of this organ, and being equally affected through all parts of it, must render it much less subject to irregular action; which may be one of the final causes of the heart, whose regular action is of such importance in the animal economy, being made subject to the whole, and not to any one part of the brain*; and readily accounts for our not being able to produce irregular action in it, in the experiments above related.

What has been said also explains why those who have endeavoured to influence the heart, by stimulating the parts of the brain from which its nerves seem chiefly to originate, have failed. When indeed the source of the nerves of the heart is considered, it will be found to derive its nervous influence from every part of the nervous system, and not very remarkably from any one part; a circumstance which well corresponds with the result of the foregoing experiments.

From the same facts it is evident, why the heart is stimulated through the brain and spinal marrow, after their power is so far weakened as no longer to convey the effect of the stimulant to the muscles of voluntary motion. As these obey stimulants applied to only one part of those organs, if the change in this part is not suffi-

* In the course of this inquiry, another and apparently more important reason will appear, why it is necessary that the heart should be subject to the influence of every part of the brain and spinal marrow.

ciently strong to produce the effect, it cannot be assisted by any other. Thus it appears from experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, because no one part suffers greatly; but it produces a great effect on the heart, because it feels the sum of all the impressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart*.

* It may be proper to make a few observations on what has been called the sympathy of nerves, a doctrine very generally diffused in medical writings. Their authors have not been satisfied with the simple fact, that the nerves are the means of conveying impressions to and from the more central parts of the nervous system, but finding that certain parts particularly sympathize with each other, they have supposed that their nerves have a power of communicating impressions to each other independently of the intervention of the brain or spinal marrow.

The first objection which presents itself to this doctrine is, that it is an unnecessary one. All nerves convey impressions to the source of nervous power, and every nerve is capable of being influenced by it. These are acknowledged facts, and they are capable of explaining the phenomena in question. It is possible, however, that some collateral facts may prove that the former is the just explanation. Is it found that parts never sympathize unless their nerves are connected in their progress? Do parts, whose nerves are most connected, most sympathize?

Numberless

CHAPTER V.

On the Principle on which the Action of the Vessels of Secretion depends, and the Relation which they bear to the Nervous System.

It not only appears from the experiments which have been laid before the reader, that the power of the heart and vessels of circulation is independent of the nervous system, but that that of

Numberless facts reply to these questions. What nervous connexions exist between a vital organ and the skin which covers it, between the liver and the ligaments of the shoulder, between the intestines and the muscles of the abdomen, the stomach and the cartilages of the ribs, *&c.*? Why does inflammation of the membrane of the ribs spread as readily to that of the lungs, which is only in contact with it, as to that in continuation with it which is supplied from the same branches both of nerves and blood-vessels? The same question may be asked respecting inflammation of the membranes of the abdomen and the head, for even the interposition of bone itself does not prevent this sympathy of parts. Such facts, as far as the author is capable of judging, leave no room to doubt that nerves sympathize only from their connexion in their common source. That the phenomena of sympathy depend on changes in that source would appear from the fact alone, that feelings still continue to be referred to a limb which is lost, because this seems to be a law of general application at whatever part the separation has been made. These observations are farther illustrated by the able investigations of Mr. Charles Bell, which have afforded new and important views of the distribution of certain nerves.—*Philosophical Transactions* for 1821.

the muscles of voluntary motion is so likewise; and that these, like the former, are only subjected to this system in the same way in which they are subjected to every other agent that is capable of exciting them. Thus we find, that all the moving powers of the animal body, as far as we have hitherto traced them, are independent of the nervous system; but that this system is equally capable of affecting them, although in different ways, whether they are subject to the influence of the will or not. Is the power of secretion also independent of, though influenced by, the nervous system?

I was soon convinced, that although the powers of circulation are independent of the nervous system, those of secretion are very far from being so. M. le Gallois, in the treatise so frequently alluded to, enumerates many physiologists who divided the eighth pair of nerves; and he gives a minute account of the consequences of their division, particularly of those which he himself observed in rabbits and guinea-pigs. The chief are, oppressed breathing, and loss of power in the stomach. Mr. Brodie also gives an account of experiments, in which he divided the eighth pair of nerves in dogs, in a paper published in the *Philosophical Transactions* for 1814. But the animals always died of oppressed breathing before he could judge of the effect on the stomach. He proved, however, by another set of experiments, that arsenic introduced into the system after the division of the eighth pair of nerves, does not produce the copious secretion from the stomach and intestines which it is found to do under ordinary circumstances. He found a similar result when he divided the stomachic nerves immediately above the cardiac orifice of the stomach.

The lungs are affected differently according to the part at which the nerve is divided. If in the rabbit, it be divided before the inferior laryngal nerve is sent off, or this nerve itself be divided, great difficulty of breathing, with a croaking noise, immediately follows; arising, as M. le Gallois has shewn *, from the opening of the glottis becoming much narrowed as soon as the nerve is divided. If the eighth pair of nerves are divided below the place at which they send off this nerve, there is little or no dyspnœa for some time. Dr. Hastings, who watched the progress of the following experiments with great care, observed, that when

* M. le Gallois says, that the difficulty of breathing comes on from dividing the recurrent nerves; but Dr. Hastings, who frequently performed the experiment, always found that there was little or no dyspnœa induced in the rabbit by dividing the recurrent nerves. It was when he divided the laryngal nerves that the sudden dyspnœa, mentioned by M. le Gallois, took place

the eighth pair of nerves were divided below the inferior laryngal nerve, the dyspnœa, although it often came on sooner, was greatly increased by the attempts to vomit, which generally happened almost immediately after eating; but which, if the animal was not allowed to eat, frequently did not occur for an hour and a half or two hours after the division of the nerves. The dyspnœa increases, and the animal seems at last to die of it. The lungs are found after death considerably altered in their structure, appearing of a more compact texture than healthy lungs, and sinking in water. They are clogged with a frothy fluid, which fills the air-tubes and cells, and covered with patches of a dark red colour, often of great extent, which give the appearance of blood having escaped into the cellular substance.

As M. le Gallois' account of the effects of the division of these nerves on the stomach does not altogether agree with that of the authors he quotes, and is also in other respects contradictory, Dr. Hastings repeated the experiment on rabbits, removing part of the nerve. The author had thus an opportunity of ascertaining, that even during the space of more than twenty hours, although the stomach was full of food (parsley), no change on it had been effected. It continued in the same state as when it left the mouth, simply divided by mastication, preserving perfectly both its appearance and its smell. It was impossible to distinguish it from parsley chopped small with a knife.

It occurred to the author, that the pain and irritation occasioned by the operation might in these experiments have induced such a degree of disease as to destroy the powers of digestion, independently of any specific effect on the stomach. He, therefore, requested Dr. Hastings to perform the experiment in the following manner:—

Exp. 48. Two rabbits, of about the same age, were fed in the same way. In both the eighth pair of nerves were brought into view. In the one rabbit a part of each nerve was removed; in the other, after being raised on a probe, they were replaced without injuring them. Both rabbits were allowed to eat as much parsley as they chose after the operation. When that, in which part of the nerves was removed, died, which did not happen for more than twenty hours after the operation, the other was killed. In the former the food was found wholly undigested; in the latter the digestive process had gone on as usual, and the food was found in the same state as in a healthy rabbit.

The stomach is generally distended to a greater size than usual, when the eighth pair of nerves has been divided, the cause of which will hereafter appear. This happened in the present case. It is remarkable, that the passage from the mouth to the stomach, termed the œsophagus, also is found to contain food, and is often very much distended. From these circumstances, and from an experiment of M. le Gallois', in which one only of the eighth pair of nerves being divided in a guinea-pig, the animal survived several days, and the stomach became excessively distended with undigested food; it occurred to the author, that the sensation by which an animal judges when he has received enough of food, being destroyed by the division of the nerves, the animals had perhaps occasioned over-distension of the stomach, and thus destroyed the power of digestion, for they often ate a great deal after the operation. He therefore requested Dr. Hastings to repeat the experiment, allowing the animal to eat as much as he chose before the operation, but none after it.

Exp. 49. This he did, but the result was the same. The food with which the rabbit had filled its stomach just before the division of the nerves, remained wholly unchanged; and it was remarkable, that the œsophagus was just as much distended with the food as when the animal had eaten after the operation. This arises from the fruitless efforts to vomit, which always come on in an hour or two after the division of the nerves*. It deserves notice, that although part of the eighth pair of nerves has been removed, the food is found covered with apparently the same semifluid which is found covering the food in a healthy stomach.

These experiments leave no room to doubt, that the office of the stomach is suspended by removing part of the eighth pair of nerves. A similar observation applies to the lungs.

In the animal in which the eighth pair of nerves was merely raised on the probe, the lungs continued perfectly to perform their office, and were found of a healthy appearance after death. In all the instances in which a part of the nerves was removed, great dyspnœa soon came on, the structure of the lungs was evidently altered, the air-cells and tubes were found clogged with frothy mucus, and the surface of the lungs marked with dark-coloured patches.

It appears then, that the extreme parts of the sanguiferous and nervous systems are connected in a way very different from that in which these systems are connected in other

* The author has already mentioned that Dr. Hastings observed the dyspnœa greatly increased by the fruitless attempts to vomit. As the passage to the stomach in the rabbit lies contiguous to the yielding part of the windpipe, the distension of the former cannot fail to lessen the capacity of the latter.

parts. The heart and vessels of circulation can perform their functions after the nervous influence is withdrawn. The power of secretion immediately ceases on the interruption of this influence. We must suppose, therefore, either that the influence of the nervous system bestows on the extreme vessels the power of separating and re-combining the elementary parts of the blood, or that the vessels only convey the fluids to be operated upon by this influence.

Experiments, related in the second chapter of the present part of this Inquiry, shew that the most minute vessels which can be seen by a microscope in the web of a frog's foot, are independent of the nervous system. The motion of the blood is as rapid, and the circulation in the foot presents precisely the same appearance after, as before the destruction of that system. It is hardly consistent with these experiments to suppose that any part of the sanguiferous derives its power from the nervous system. If the power of the vessels of secretion had been lost by the destruction of the latter, this must have occasioned some change in the distribution and motion of the blood in the web. The conclusion from these experiments is strengthened by other facts. In those experiments in which the power of secretion was destroyed by withdrawing the nervous influence, there appeared to be no defective supply of fluids. Both in the stomach and in the lungs they were sufficiently copious. The fault seemed to be, that a due change on them had not been effected. We have no reason to believe, as far as the author is capable of judging, that the vessels possess any other than a muscular power, if we except the mere power of elasticity *. Now it has been proved, as the reader has seen, by direct experiment, that the muscular power throughout the whole animal, namely, in the muscles of voluntary motion, the heart and the vessels of circulation, is independent of the nervous system. Can we suppose that the vessels of secretion, which

* The author does not mean here to enter on the arguments which seem to prove that the power of the vessels is, strictly speaking, a muscular power. For this subject he refers the reader to the observations of Mr. Hunter, in his work, entitled, A Treatise on the Blood, Inflammation, and Gunshot Wounds. In some animals the muscular structure of the vessels is apparent on the slightest view. In man, and animals resembling him, it is very obscure, the reason of which Mr. Hunter has pointed out. In them the proportion of the muscular to the elastic coat of the vessels is inversely as the size of the vessels; so that in the larger vessels there is comparatively little muscular fibre, and in the vessels possessing a larger proportion of it, the parts are too minute to enable us to detect it. The arguments of Mr. Hunter are surely much strengthened by the foregoing experiments, which shew, that the laws which regulate the power of the vessels of circulation are the same with those which regulate that of the heart, whose muscularity cannot be questioned.

are only a continuation of those of circulation, all at once assume a different nature? Or, is it at all consistent with our knowledge of the phenomena of chemistry, to conceive that, by any influence, the muscular and elastic powers can be enabled to separate and re-combine the elementary parts of the blood?

The first of the above positions, therefore, may, as far as the author can judge, be regarded as set aside. This admitted, it seems a necessary result of the preceding experiments and observations, that in the function of secretion the vessels only convey the fluids to be operated upon by the nervous power. The reader will soon be presented with other facts which tend to confirm this inference.

Thus it appears that the vessels of secretion, like those of circulation, are independent of the nervous system; secretion failing when the influence of this system is withdrawn, not because the vessels of secretion fail to perform their office, but because the necessary changes on the fluids which they supply no longer take place.

We know that the nervous power occasionally influences the vessels of secretion, as we have seen it does those of circulation, because affections of the mind frequently influence the flow of fluids to secreting surfaces. The vessels of secretion, therefore, thus far obey the same laws as those of circulation; they are independent of, but influenced by, the nervous power. Are they influenced by certain parts of the brain alone, or, like the heart, by every part of the brain and spinal marrow? To save repetition, this question will be considered, with another intimately connected with it, in the second section of the seventh chapter.

It is not to be overlooked, that the vessels convey the fluids, to be operated upon by the extreme parts of the nervous system, in a peculiar way. By the lessening capacities of the capillary vessels, the blood is divided as by a fine strainer, some of its parts being too gross to enter the smaller vessels. How far the blood may thus be subdivided, and how far it may be differently divided in different organs, we cannot tell. As this structure of the vessels is uniform, we have reason to believe that its effect on the blood is necessary, to prepare this fluid for the due action of the nervous power.

CHAPTER VI.

On the Principle on which the Action of the Alimentary Canal depends.

In order to ascertain how far the peristaltic motion of the intestines is independent of the brain and spinal marrow, the following experiments were made.

Exp. 50. A rabbit was killed in the usual way. The whole of the spinal marrow was then destroyed by a small hot wire. On opening the abdomen, we found the peristaltic motion of the stomach and small and great intestines quite as strong as when the nervous system is entire, as we ascertained by exposing the abdominal viscera of other newly-dead rabbits. This motion is as strong in the newly dead, as in the living animal.

Exp. 51. The spinal marrow was wholly removed in another newly-dead rabbit, without at all affecting the motion of the stomach and intestines. The removal of the brain produced as little effect upon it, as that of the spinal marrow. When both were removed at the same time, it remained unaffected. It continues till the parts become cold, so that when the intestines exposed to the air have lost their power, that of those beneath still remains.

From these experiments, compared with those made with a view to ascertain the principle on which the power of the muscular fibre depends, it appears that the muscular power of the stomach and intestines, like that of the heart and blood-vessels, resides in themselves, and is wholly independent of any influence derived from the nervous system*.

CHAPTER VII.

On the Relation which the Alimentary Canal bears to the Nervous System.

The alimentary canal is of such importance in the animal economy, that it is of the first consequence, in tracing the laws of the vital functions, to ascertain the principle on which its action depends, and the relation which subsists between it and the nervous system ; the former of these points the author has endeavoured to ascertain by the experiments related in the preceding chapter, the latter he is now to consider.

Exp. 52. He endeavoured in the newly-dead animal, to ascertain how far the motion of this canal is influenced by stimulants applied to the brain and spinal marrow ; but, from its nature, it is in every way so irregular, that no certain result could be obtained. It often

* On reading this experiment, Mr. Andrew Knight informed me that he had often observed in the newly-dead animal that the peristaltic motion of the bowels was unimpaired for some time after they were removed from the body.

appeared that spirit of wine, applied to the brain and spinal marrow, increased it.

The admission of air into the cavity of the abdomen, in the newly-dead animal, throws the bowels into strong spasmodic action, which alone would obscure any effect that can be supposed to arise from stimulating the brain. The abdomen of the newly-dead animal was therefore opened under tepid water, but this was found to excite even stronger spasms than the air had done.

The effects of the passions on the alimentary canal, however, leave no room to doubt that it is capable of being stimulated through the nervous system. It remains to be ascertained whether it is subject only to certain parts of the brain, or, like the heart, to every part of that organ and of the spinal marrow.

It is evident from the circumstances just mentioned, that it is impossible to answer this question respecting the alimentary canal as respecting the heart, by agents applied to different parts of the brain and spinal marrow. Before the experiments which were resorted to for this purpose are related, it will be proper to direct the reader's attention to the process of digestion in the animal on which these experiments were made, which will place in a clearer point of view both their results, and those of some experiments already laid before him.

SECTION I.

On the Process of Digestion.

On the functions of the stomach all other functions of the animal body may be said to depend, as their various organs derive from it that supply, without which they can exist only for a very short time. In another point of view we find the stomach equally important. There is no other organ whose diseases are at once so frequent and so varied, or which partakes so much of the diseases of other parts, or of the whole system.

It is not, however, my intention to enter fully into that part of the process of digestion which is performed in the stomach. The experiments of Spalanzani and others sufficiently prove that the change which the food undergoes in this organ is effected by a fluid secreted by it. The author will confine himself to such circumstances attending this change as serve to elucidate the results of the experiments which have been or are about to be laid before the reader.

He has inspected after death, under various circumstances, and at different periods after taking food, the stomachs of about a hundred and thirty rabbits, which has enabled him not only to ascertain some points that will place the result of many of his experiments in a clearer light, but to observe the process of digestion more particularly than has hitherto been done.

The experiments on this part of the subject were so frequently repeated, that it would be tedious and unprofitable to give an account of each experiment. Under the head Experiment, therefore, the result of all the experiments on each particular part of the subject will be given. Mr. Sheppard was so good as to assist me in these experiments.

Exp. 53. The first thing which strikes the eye on inspecting the stomachs of rabbits which have lately eaten, is, that the new is never mixed with the old food. The former is always found in the centre, surrounded on all sides by the old food, except that on the upper part, between the new food and the smaller curvature of the stomach *, there is sometimes little or no old food. If, as was ascertained by more than twenty trials, the old and the new food are of different kinds, and the animal is

* The stomach is like a bag suspended by its extremities, which, when filled, bulges out on the lower part. Thus the distance of the extremities measured by the lower is greater than that measured by the upper part. Hence the lower part of the stomach is by anatomists called the greater, and the upper part the smaller curvature of the stomach.

killed after taking the latter, unless a great length of time has elapsed after taking it, the line of separation is perfectly evident, so that all the old may be removed without disturbing the new food. For this purpose we fed rabbits on oats, and, after making them fast for sixteen or seventeen hours, allowed them to eat as much cabbage as they chose, and killed them at different periods, from one to eight hours, after they had eaten it.

On opening the stomachs of rabbits three or four weeks old, who both sucked and ate green food, the curdled milk was always found unmixed with the green food. Before the stomach was opened, it was evident, from its transparency, where the green food and where the milk lay. The rabbits, used in this and all the experiments which are related in this section, were killed in the usual way.

Exp. 54. If the old and the new food be of the same kind, and the animal be allowed to live for a considerable time after taking the latter, the gastric juice, passing from the old to the new food, and changing as it pervades it, renders the line of separation indistinct, so that on a cursory view we should suppose the old and new food mixed together; but towards the small curvature of the stomach, and still more towards the centre of the new food, we find it, unless it has been very long in the stomach, comparatively fresh and undisturbed. All around, the nearer the food lies to the surface of the stomach, the more it is digested. This is true even with regard to the food in the small curvature, compared with that nearer the centre, and the food which touches the surface of the stomach is more digested than any other found in the same part of the stomach. But unless the animal has not eaten for a great length of time, that in contact with the surface of the stomach is in very different stages of digestion in different parts of this organ. It is least digested in the small curvature, more in the large end, and still more in the middle of the great curvature.

These observations apply to the left portion of the stomach which first receives the food, and is called the cardiac portion, from the orifice by which the food enters, termed by anatomists the cardia. Sir Everard Home, in his work on Comparative Anatomy, has shewn that the stomach is divided into two portions, in such a way, that the length of the first is to that of the other nearly as two to one. The latter is called the pyloric portion, from the orifice by which the food leaves the stomach, termed by anatomists pylorus. The line of division may generally be seen in some animals after death. He says it is more evident while digestion is going on. I have sometimes observed it very distinctly after death in the stomach of the rabbit, and have then found the food in the two cavities divided by an evident line of separation as described by this author. The two portions of the stomach form an angle with each other, which is well expressed by the plates in Sir Everard Home's work, and appears to the author to cause the line of division just mentioned.

Exp. 55. The food in the pyloric portion of the stomach of the rabbit, is always found in a state very different from that just described. It is more equally digested, the central parts differing less in this respect from those which lie next the surface of the stomach; it is evident, however, that all the change effected in the stomach is not completed when the food enters this portion of it, because we find it the more digested the nearer it approaches to the pylorus, where being ready to pass into the intestine, it has undergone all that part of digestion which is performed in the stomach.

One of the most remarkable differences between the state of the food in the cardiac and pyloric portions of the stomach, is, that in the latter it is comparatively compact and dry, in the former mixt with a large proportion of fluid, particularly when digestion is pretty far advanced, and time consequently has been given for a considerable secretion from the stomach. In the rabbit, indeed, which is fed only with solid food, in the early stage of digestion it is nearly as free from liquid in the cardiac as in the pyloric portion of the stomach. When digestion is very far advanced, the whole contents of the former are often reduced to the state of a semi-fluid. But even then the contents of the pyloric portion, particularly those parts which are near the pylorus, are comparatively compact and dry. In rabbits so young as to live wholly on milk, the curdled milk is considerably softer and moister in the cardiac than in the pyloric end of the stomach.

Exp. 56. Although the food is in the most digested state in the pyloric end, it appears from several circumstances that the change is chiefly effected in the cardiac, which from its greater capacity is also called the great, end of the stomach. The food found in the pyloric end, we have just seen, is comparatively dry, while that found in the great end, if digestion is much advanced, is mixed copiously with the juices of the stomach, and there is a more evident difference in the state of the food before it comes into this part, and when it is about to leave it, than in any other part of the stomach. The author will presently have occasion to mention a fact ascertained by Mr. Hunter, which seems to confirm this opinion. Dr. Hastings, on examining the stomach of a woman who had died under his care, found it everywhere in a state of ulceration, except in the great end, where it was healthy. The stomach had performed its functions to the last, and the contents of the bowels proved that the food had been properly digested.

It appears that in proportion as the food is digested, it is moved along the great curvature, where the change in it is rendered more perfect, to the pyloric portion. Thus, the layer of food lying next the surface of the stomach is first digested. In proportion as this undergoes the proper change, it is moved on by the muscular action of the stomach, and that next in turn succeeds to undergo the same change. As the gastric juice pervades the contents of the stomach, though apparently in no other way than by simple juxtaposition, (for the arrangement of the food, above described, was never found disturbed), the change in each part, which in its turn comes in contact with the stomach, is far advanced before it is in actual contact with it; and, consequently, is soon after this in a proper state to be moved on towards the pyloric end.

Thus a continual motion is going on, that part of the food which lies next the surface of the stomach passing towards the pylorus, and the more central parts approaching the surface. When rabbits have fasted sixteen or eighteen hours, the whole food found in the cardiac portion, which is in small quantity compared to what is found in it immediately after a full meal, seems frequently to be all nearly in the same state with that next its surface, the gastric fluid having pervaded and acted upon the whole, and is consequently apparently fitted to be sent to the pyloric end. Sir Everard Home found that the stomach of a rabbit never empties itself, containing, even when the animal dies after long fasting, a considerable quantity of food. The first impression on the food is made in the small curvature, because the upper part of the new food, which lies contiguous with this part of the stomach, or nearly so, is always found more changed than the more central parts of it.

In the large end of the stomach the small round masses or balls, about the size of the largest kind of shot, mentioned by Sir Everard Home, were frequently found by the author. These balls are very constantly found in the great end of the stomach of rabbits, especially when fed on green food, never in any other part of it. They are often very numerous, sometimes forming the whole contents of that part of the stomach. They cannot be fewer, in many cases, than from two to four hundred. At other times they are much less numerous, and mixed with food of the same consistence with that of which they are composed. It is difficult at first view to account for their appearance. The ingenious idea of Sir Everard Home, that they are produced by the rabbit occasionally ruminating, is opposed by several circumstances; the frequency of their appearance, their sometimes forming one half or more of the contents of the stomach, their being always found at a considerable distance from the opening by which the food enters the stomach, unless their number is so great as to fill the greater part of the stomach, food much less digested than that composing them generally lying between them and this opening, their being always too soft to retain their shape in the act of deglutition, and no appearance of this kind being found in ruminating animals.

It was long before the author could form even a probable conjecture respecting the formation of these balls. He has now, from inspecting many stomachs containing them, very little doubt of the cause to which they are to be ascribed. When the stomach of the rabbit is laid open, the great end is found corrugated, the rugæ giving it a honey-comb appearance. These rugæ disappear when it is stretched, and as soon as the stretching power is withdrawn, again appear, the surface of the rest of the stomach being comparatively even. The balls seem to be formed in the hollows of these rugæ, which are about the same size with the balls.

It would appear that the food, by the action of this part of the stomach, is rolled up into these masses after it has undergone that part of the digestive process which takes place in the great end of the stomach, and, consequently, after it has been exposed for a considerable time to the action of the gastric juice; in which form it is sent towards the pyloric end, where the balls are broken down, and the whole again formed into one mass of a firmer consistence than the balls. The formation of these balls appears to be in some degree similar to the process by which, in many animals, the discharge from the bowels is formed into similar balls by the cells of the intestines. The author has observed that when all the food in the great end of the stomach is composed of them, it contains no fluid but that which is mixed up with the food in them. Sometimes no balls are formed. This is comparatively rare. We never found the curdled milk formed into balls, consequently there are none in the stomachs of very young rabbits. With this exception they are frequently, it may be said very generally, found under all circumstances of diet, situation, &c. Sometimes when rabbits had lived precisely in the same way, they were not found in all. They are sometimes found, when the more central parts of the contents of the stomach have undergone little or no change.

Their formation is evidently no essential part of the digestive process, and is probably prevented by the food or air occasionally so distending the stomach during that process as to prevent the formation of the rugæ just described. It would appear from several observations, that when the whole of the contents of the great end of the stomach have equally undergone the action of the gastric juice, so that there is no fresher food in the central parts, they are very slowly passed into the pyloric portion. Hence it seems to arise, that they are often, when the animal has fasted for a considerable time, and thus the stomach, by the diminution of its contents, is allowed to fall into the rugæ wholly formed into balls; when it has fasted for a great length of time, so much fluid is secreted in the great end of the stomach, that the balls lose their consistency, and the whole runs into a uniform semi-fluid mass.

Exp. 57. It is in the great end of the stomach where digestion appears to go on so rapidly, that Mr. Hunter found the stomach itself dissolved ; and by the most satisfactory arguments shewed that this is the effect of the gastric juice after death. His observations on this subject confirm the foregoing view of digestion, for he found part of the stomach digested when the food it contained remained undigested, in the case of a man killed immediately after a full meal. This the author has often observed in rabbits, when the animal has been killed immediately after eating, and allowed to lie undisturbed for some time. On opening the abdomen he has found the great end of the stomach soft, eaten through, sometimes wholly consumed, the food being only covered by the membrane which covers the viscera, or lying quite bare for the space of an inch and a half in diameter, and part of the contiguous intestines, in the last case, also consumed; while the cabbage, which the animal had just taken, lay in the centre of the stomach unchanged, if we except the alteration which had taken place in the external parts of the mass it had formed, in consequence of imbibing the gastric juice from the half-digested food in contact with it. The great end of the stomach was sometimes found digested within an hour and a half after death; this was more frequently the case when the animal had lain dead for many hours. It is not always digested, however long the animal has lain dead. This seems only to take place when there happens to be a greater supply than usual of gastric juice. Thus it was always observed most apt to happen when the animal had eaten voraciously. Why it should take place without the food being digested is evident from what has been said. Soon after death the motion of the stomach, which is constantly carrying on towards the pylorus the most digested food, ceases. Thus, the food, which lies next to the surface of the stomach, becoming fully saturated with gastric juice, neutralizes no more ; and no new food being presented to it, it necessarily acts on the stomach itself, now deprived of life ; and on this account, as Mr. Hunter justly observes, equally subject to its action with other dead animal matter. It is remarkable that the gastric juice of the rabbit, which in its natural state refuses animal food, should so completely digest its own stomach, as not to leave a trace of the parts acted on*. The author never saw the stomach eaten through except in the large end. In other parts its internal membrane is sometimes injured.

If the foregoing account of the process of digestion in the rabbit be kept in view, it will be interesting to trace the effect produced on it by depriving the stomach of a great part of its nervous influence by dividing the eighth pair of nerves.

* The rabbit when hungry will often eat animal food.

The division of the eighth pair of nerves, which the author has so frequently had occasion to mention, is one of the oldest physiological experiments of which we have any account. It was performed by several of the ancients, and has been repeated by a great many physiologists in modern times. Valsalva is among the first who gave any distinct account of its effect on the stomach. He observes that it impedes digestion, and even prevents the food passing from the œsophagus into the stomach. The cause of part of the food being found in the œsophagus, the author has had occasion to point out above*.

If the animal be allowed to live for a considerable time after a part of the eighth pair of nerves is removed, the food remaining in the stomach is always found undi-

* It is said that M. Magendie has divided the eighth pair of nerves immediately above the diaphragm, and found that the stomach is still capable of performing its functions. Of the effects of the division of the eighth pair of nerves at this place, the author cannot speak from his own observation, as he has never seen the experiment made. Its effects on the stomach, it is evident, may be different from that of the division of these nerves in the neck, because they form various connexions with the great sympathetic nerve in the thorax. By dividing the eighth pair of nerves in the neck, the stomach is deprived of the whole, or nearly the whole, power of these nerves.

gested, and nearly in the same state in all parts of the stomach, a circumstance which the author was at first greatly at a loss to explain. This effect was uniform, he never saw it otherwise. Yet we must conceive that at the time the animal last eats, there is some food more or less digested in its stomach, and some gastric juice to act on part of that just received into it. The foregoing statements explain the difficulty. The division of the eighth pair of nerves destroys the formation of the gastric juice; but the animal still living, and the motions of the alimentary canal, as the reader has seen, being independent of the nervous power*, the usual motions of the stomach continue, and send onwards into the intestines all the food which is digested, and, consequently, can so affect the stomach as to excite its natural motions.

Thus it is evident from the foregoing observations, that the undigested food must at length come into contact with it. As soon as this happens, the usual secretions not being supplied to produce the proper change in the food, an unnatural motion is excited; hence the efforts to vomit, which always ensue in about an hour, an hour and a half, or two hours, after the division of the nerves, marking the time when

* See Chap. VI.

the stomach, having sent towards the pylorus its digested contents, begins to feel the effects of undigested food coming into contact with it. If the animal be allowed to eat after the operation, the efforts to vomit almost immediately ensue; the new food, as appears from the above statement, and indeed as is evident from the way in which it enters the stomach, directly coming in contact with some part of the small curvature.

We thus see the cause of the efforts to vomit, which follow the division of the eighth pair of nerves; and why, if the animal be allowed to live for a certain time after the operation, nothing but undigested food is found in the stomach. It also appears, from the same circumstances, why the stomach is generally more distended than usual after the eighth pair of nerves have been divided, particularly if the animal has been allowed to eat after the operation; all the food, not digested by the gastric juice, present before the division of the nerves, remaining in the stomach, and there swelling from the heat and moisture.

SECTION II.

On the Effects on the Stomach and Lungs of destroying certain Portions of the Spinal Marrow, compared with those of dividing one or both of the eighth Pair of Nerves.

From the extreme irregularity of the motions of the alimentary canal, the author has already had occasion to observe, we cannot ascertain whether it is subject to the influence of the different parts of the brain and spinal marrow in the way in which this has been done respecting the heart. He, therefore, endeavoured to ascertain this point by withdrawing from the most important part of this canal, the stomach, the influence of different parts of these organs, and observing the effects produced on it.

As the reader has seen the office of the stomach destroyed by the division of the eighth pair of nerves, he might at first view infer, that it is from the brain alone that the stomach derives its nervous power. But although the process of digestion is suspended by the division of these nerves, it does not follow that the stomach may not derive nervous power from other sources, because the loss of any considerable part of its nervous energy may destroy its function. Besides its remaining sensibility, indicated by the efforts to vomit, proves that its nervous power is not wholly withdrawn by dividing the eighth pair of nerves.

If then this power be not supplied to the stomach by the eighth pair of nerves alone, but also, as we have reason to believe from the evidence of anatomy, by nerves arising from all parts of the spinal marrow, it is evident, that cutting off its supply from any considerable part of this organ, while we leave the eighth pair entire, must affect its power, though probably not so much, because the brain, we have reason to believe, constitutes the most important, as well as the largest part of the nervous system. To ascertain this point, the following experiments were made.

Exp. 58. A hole was made about the middle of the spine, and the lower part of the spinal marrow destroyed by a small wire. The only apparent effect of the operation was the loss of power and feeling in the lower part of the animal. It seemed to be otherwise in health. It may here be observed, that the sufferings of the animal in those experiments, in which portions of the spinal marrow were destroyed, was much less than at first view they appear to be; because as soon as the spinal marrow is divided, the sensibility of all parts below the division is lost. The animal, used in this experiment, was allowed to eat nothing for twelve or fourteen hours after the operation. At the end of this time it ate parsley very readily, and in large quantity, without any tendency to vomit. It lived twenty-four hours after the operation, and ate parsley from time to time.

On examination after death, the stomach was found distended to a great degree, apparently containing the whole of the parsley which had been eaten after the operation, in an undigested state. The bladder had not been emptied after the operation, and it was so much distended, that it rose above the umbilicus. The lower intestine was also distended, although there had been some discharge from the bowels. The lungs collapsed on opening the thorax, but were slightly congested, that is, obstructed with phlegm.

Exp. 59. In a full-grown rabbit a small wire was introduced into the spine at the fourth lumbar vertebra, by which it was attempted to destroy the spinal marrow as far as the first dorsal vertebra. The hind legs were rendered insensible and motionless. Respiration was a little disordered. In a short time after the operation the animal appeared lively, and ate some parsley. The respiration continued to be slightly affected. Some hours after the operation Dr. Hastings, who watched the animal, observed it to be very cold, and it shivered, although it was kept in the same temperature with other rabbits, who shewed no signs of being cold. The rabbit used in the last experiment also seemed cold, but not in the same degree. The respiration now seemed much disordered, and the animal refused parsley. It was then brought near a fire and wrapped up in flannel. By these means it was soon relieved, the shivering ceased, its eyes looked more lively, and the breathing became more free. It was kept near the fire as long as it lived, and frequently ate parsley. It died in twenty-seven hours after the operation.

The stomach was not much distended. The parsley near the cardiac orifice was not at all changed, and that near the pyloric orifice very slightly. The membrane of the windpipe and its branches was more vascular than natural. The cells were slightly loaded with frothy and bloody mucus, and there were the same red patches in the lungs as after the division of the eighth pair of nerves.

Blood was extravasated in different parts of the spinal marrow, as far as the wire had passed, and the membranes were inflamed. Immediately above the opening, the spinal marrow was quite destroyed for about an inch in length. In other places it did not appear much injured.

Exp. 60. The author wished to ascertain the effect of destroying a smaller portion of the spinal marrow than that destroyed in either of the last experiments, and requested Dr. Hastings to perform the following experiment, noting the temperature of the animal at the different periods of it. In a rabbit about two months old, fed on parsley, a small wire was introduced into the spinal canal, at the first lumbar vertebra, and that part of the spinal marrow which lies below this vertebra destroyed. After mentioning the other circumstances of the experiment, the author threw together the observations made on the temperature. The animal lost the power and feeling of the lower extremities, but seemed in no other way immediately affected by the destruction of this part of the spinal marrow. It lived thirty-five hours.

On examination after death, the stomach was found no larger than natural, the parsley retained its colour, smell, and fibrous texture, although such a change had taken place in it, as demonstrated a very slight degree of the digestive process. The duodenum, for about an inch below the pylorus, was filled with parsley in the same state. The bladder and rectum were distended, but not so much as in the two last experiments. The lungs were slightly congested.

It is difficult to destroy a large portion of the spinal marrow without immediately killing the animal. It must be done very slowly, and even with this precaution the attempt will not always succeed. Its destruction is attended with no suffering to the animal, if we previously divide it at the upper part of the portion which we wish to destroy. On examination after death, the lumbar portion of the spinal marrow was found completely destroyed.

The following are the observations on the temperature.

The bulb of Fahrenheit's thermometer introduced into the mouth, and kept there for two minutes previous to the experiment, stood at 98°.

The animal was kept in a warmer temperature after than before the destruction of the lumbar portion of the spinal marrow. The temperature was always measured by putting the bulb of the thermometer into the mouth, and keeping it there for two minutes.

Immediately after the operation, therm. 98°
In twelve minutes after it .... 92°
In half an hour after it ... . 92°

In two hours and a half after it . . . . 98°
In five hours and three-quarters after it 98°
In seven hours and a quarter after it . 98°
In nine hours after it . . . . 96°
In ten hours after it . . . . 95°

The animal during all this time appeared lively, and ate parsley.

In eleven hours after it, therm. . . . . . . 96°
In twelve hours after it . . . . 97°

Night coming on, the temperature was not measured again for thirteen hours. In the morning the rabbit appeared lively and ate readily.

In twenty-five hours after the operation,
therm. . . . . 88°
In twenty-seven hours after it, therm. 84°
In twenty-nine hours after it. . . . . 88°
In thirty hours after it . . . . 84°
In thirty-one hours after it . . . . 84°
In thirty-three hours after it . . . . 80°

The animal still continued to eat. In thirty-four hours after the operation the temperature was 75°. In an hour after this, the animal died. This animal did not appear nearly so cold as that in the preceding experiment, in which a larger and more important part of the spinal marrow was destroyed.

Thus we find the function of the stomach impeded by depriving it of the influence of any considerable part of the spinal marrow, and it seems only more affected by the division of the eighth pair of nerves, in proportion to the greater extent and importance of the brain.

It is remarkable, that the result of the first of these experiments is the same with that which M. le Gallois obtained when he had divided one of the eighth pair of nerves in a guinea-pig. The animal did not vomit, and the stomach was found distended to a great size, apparently containing all the food it had taken after the operation in an undigested state. This coincidence demonstrates how much the same the effect on the stomach is, whether we deprive it of part of the nervous power, which it receives from the brain, or part of that which it receives from the spinal marrow. Dr. Hastings, at my request, made the following experiment.

Exp. 61. One of the eighth pair of nerves was divided in a rabbit. It ate soon after the operation, but did not attempt to vomit till two hours and a half after it, and then dyspnœa came on. The breathing at times was almost free, and the efforts to vomit only occurred at intervals. Both subsided when it was prevented from eating. It died in forty-five hours after the operation.

The stomach was found after death larger than natural, being distended with air, and containing more food than usual. For the most part the parsley was in the same state as when taken into the stomach, both in appearance and smell. In some places it was slightly changed. There was undigested parsley in the duodenum, to the distance of about an inch from the stomach. The lower end of the passage to the stomach contained a little parsley. There was none in any other part of it.

The membrane of the windpipe was of a darker colour than natural, its vessels being distended with blood, and there was some frothy mucus in it. The lungs were slightly congested. The membrane of the air tubes was too vascular, and the air-cells contained some frothy mucus. All these appearances existed in a much less degree than when both nerves were divided. The lungs collapsed imperfectly on opening the chest. There were some dark coloured spots on them.

If the reader will take the trouble to compare these appearances with those observed when part of the spinal marrow was destroyed, particularly in Experiment 59, he will see that the division of one of the eighth pair of nerves produces nearly the same effect on the lungs and stomach, as the destruction of part of the spinal marrow.

The author wished to see the effect on the stomach and lungs of destroying nearly the whole spinal marrow. But with all the precautions that could be taken the animal died almost immediately. It is difficult indeed to prevent immediate death, when as much of it is destroyed as in Experiments 58 and 59.

There is still another point in this part of the subject, which remains to be ascertained. Do the effects observed in the stomach and lungs, when part of the spinal marrow is destroyed, arise from the destruction of that part; that is, from the ceasing of its office, or from the influence of the brain on the spinal marrow being thus limited? It is evident, that if the former opinion be correct, the division of the spinal marrow in the middle, will not produce the same effects as the destruction of the lower half. If the latter be correct, these must produce precisely the same effects.

Exp. 62. The spine was divided in an old rabbit, about the same place at which it was opened in order to destroy the lower half of the spinal marrow in Experiment 58, after which there was no motion in the lower extremities. The rabbit seemed lively after the operation, and continued to eat frequently till within six hours of its death. It died in twenty-seven hours and a half after the division of the spinal marrow. It had not vomited, and had had little or no difficulty of breathing.

On examination after death, the stomach was not found more distended than natural.

The food it contained was nearly as well digested as in the stomach of a healthy rabbit. The contents of the duodenum had completely undergone the proper change. The bladder and rectum were distended, but not so much as after the destruction of the lower part of the spinal marrow.

The lungs collapsed on opening the thorax, but contained a little frothy mucus.

On examining the spine, it was found to have been completely divided.

On a comparison of this experiment with Experiment 58, it appears that here the lower part of the spinal marrow still performed its office, and supplied its portion of nervous power to the stomach and lungs, although the communication between it and the brain was cut off.

The reader must have perceived through the whole of these experiments, that any considerable diminution of the nervous influence almost wholly deprives the stomach of its power; and even the slightest diminution of it seems to be felt. The author has no doubt, that the very slight derangements observed in the stomach and lungs in the last experiment, may be ascribed to the destruction of function, that must have taken place in the part of the spinal marrow at which it was divided. The wound must of course have destroyed the function of a small part on each side.

Thus it appears that although we cannot by agents, applied to different parts of the brain and spinal marrow, ascertain, as was done with respect to the heart, how far the stomach and lungs are under their influence; yet, in consequence of the secreting power being under the influence of the nervous system, we may, by withdrawing that of different parts of the brain and spinal marrow, prove that the stomach and lungs, like the heart, are capable of being influenced through every part of those organs.

As the functions of the stomach and lungs depend on the secreting power, the same inference, it is evident, applies to it; and consequently, as will appear more fully from what is said in the ninth chapter, to the vessels of secretion.

CHAPTER VIII.

On the Cause of Animal Temperature.

The temperature of the animals in the preceding experiments, in which portions of the spinal marrow were destroyed, is now to be considered. It appears from them, that while the destruction of part of the spinal marrow impedes the office of secreting surfaces, it also lessens the disengagement of caloric.

Mr. Brodie, in the Croonian Lecture for 1810, gave an account of experiments which led to the inference, that the maintainance of animal temperature is under the influence of the nervous system; and in the Philosophical Transactions of 1812, he relates additional experiments, tending to strengthen this inference.

In the second section of the last chapter, the author has had occasion to relate experiments made for other purposes, which tend in a striking manner to confirm the opinion of Mr. Brodie. He found that poisons, impairing the vigour of the nervous system, impair the temperature. In the foregoing experiments, lessening the extent of this system, by destroying part of the spinal marrow, had the same effect.

Towards the conclusion of the latter of the above papers, Mr. Brodie observes, " The facts " here, as well as those formerly adduced, go " far towards proving, that the temperature of " warm-blooded animals is considerably under " the influence of the nervous system; but " what is the nature of the connexion between " them? Whether is the brain directly or in- " directly necessary to the production of heat? " These are questions to which no answers can " be given, except such as are purely hypo- " thetical. At present we must be content with

" the knowledge of the insulated fact : future " observations may, perhaps, enable us to refer " it to some more general principle."

The various phenomena of animal temperature, and the experiments on this subject, related in the last chapter, compared with the effects on secreting surfaces, observed in these experiments, seem to the author to prove, that the caloric*, which supports animal temperature, is disengaged by the same means, namely, the action of the nervous power on the blood, by which the formation of the secreted fluids is effected, and, consequently, that it is to be classed with the secreting processes.

The author will relate some additional experiments on animal temperature, both when treating of the order in which the functions cease in dying, and in the observations which he will have occasion to make on the nature of the vital powers, tending to confirm this view of the subject.

* The author uses caloric, the term employed for the cause, whatever that may be, the action of which on the animal body produces the sensation of heat, without entering on the question whether it is a substance or only a peculiar state of the particles of bodies. What he here says is said on the supposition, which appears to him by many degrees the most probable, that it is a substance. The intelligent reader will easily perceive how this passage must be modified, if it is ever proved to be only a peculiar state of the particles of bodies.

CHAPTER IX.

On the Use of the Ganglions.

It appears from experiments related in the first and second chapters of the present part of this Treatise, that the motion of the heart, though independent of, may be influenced through, every part of the brain and spinal marrow. It seems also ascertained by experiments related in the same chapters, that the blood-vessels bear the same relation to the nervous system with the heart. Their power is equally independent of this system, and they are influenced in the same way by agents acting through it. We cannot, we have seen, affect the muscles of voluntary motion in the extremities by agents applied to the upper parts of the brain, yet the vessels of their most extreme parts obey agents applied even to the upper surface of this organ*.

It appears from experiments related in Chapter VI., that the muscular power of the alimentary canal is also independent of the nervous system. It is impossible, for reasons which have been laid before the reader, to ascertain by experiments similar to those relating to the

* See Experiment 29.

heart and blood-vessels, whether the alimentary canal is subject to the influence of every part of the brain and spinal marrow. With respect to it, therefore, the author attempted, in the second Section of Chapter VII., to determine this point by experiments of a different nature. The result was, that, like the heart, it is capable of being influenced by every part of the brain and spinal marrow. The same observation, it appears from those experiments, applies to the lungs.

Here the question arises, By what means is the one set of organs subjected to the influence of every part of the other? We cannot suppose that the former receive nerves from every part of the brain and spinal marrow. We know, indeed, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the brain and spinal marrow, others only by small parts of them. In the latter instances, we see directly proceeding from those small parts, the nerves of the parts influenced. In the former instances, namely, where it is found that the part is influenced by all parts of the brain and spinal marrow, we do not in any case see nerves going directly from all parts of these organs to the part influenced, but we always see this part receiving nerves from a chain of ganglions*, small

* See the account of the Nervous System in the Preface.

compact bodies formed on the nerves, to which nerves from all parts of the brain and spinal marrow are sent. It is, therefore, evident from direct experiments, that the nerves issuing from ganglions convey to the parts, to which they send nerves, the influence of all the nerves which terminate in these bodies.

Such then is the relation which the most important organs of involuntary motion bear to the brain and spinal marrow. Their powers are independent of both, yet they are subjected to the influence of every part of both, communicated through the medium of the ganglions. Thus it would appear, that the ganglions may be regarded as a secondary centre of nervous influence, receiving supplies from all parts of the brain and spinal marrow, and conveying to certain organs the influence of all those parts.

If the nervous influence of the thoracic and abdominal viscera be thus supplied from a common source, why, in affections of the spinal marrow, it may be said, is the breathing most affected when the disease is in the dorsal portion of this organ, and the action of the bladder and rectum most affected when its chief seat is in the lumbar portion? This arises from the muscles of respiration deriving their nerves from the dorsal portion, and the abdominal muscles from the lumbar portion of the spinal marrow. The latter muscles excite, and generally increase, the action of the bladder and rectum, by pressing them against their contents, and also by this pressure contribute mechanically to expel their contents. Thus, in the above cases, in addition to the failure of nervous power in the viscera, there is a failure of excitement in the muscles of voluntary motion, which conspire with these viscera in certain parts of their functions*.

We can trace the communications of nerves issuing from the great chain of ganglions, placed, it would seem, to facilitate these communications in the centre of the animal system, with all the nerves of the body. And many circumstances, regarded by anatomists as anomalous, namely, nerves becoming larger after they appear to send off branches, apparently taking a retrograde course, *&c.*, are readily explained, if we admit that nerves, arising from ganglions, join and again separate from those proceeding in an opposite direction from the brain and spinal marrow. It is worthy of remark that none of these anomalous appearances are observed in the extremities, where the

* See what is said of the muscles of respiration in Chapter X.

ganglionic must take the same course with the other nerves. Bichat, although his opinions respecting the use of the ganglions are very different from those which the author has, from the results of the preceding experiments, been led to form, and indeed wholly inconsistent with their results, was induced from his observation of the situation and distribution of the ganglions and their nerves, to regard them as the centres of nervous systems.

From a comparison of all that has been said, we have reason to believe, that the system of ganglionic nerves is quite as extensive as that of the nerves proceeding directly from the brain and spinal marrow. We everywhere find blood-vessels, which receive their nerves from the ganglions; in the larger vessels, we can often trace the ganglionic nerves attached to and supplying them. The following case, related by Dr. Parry, in the 139th page of his Treatise on the Arterial Pulse, might alone be regarded as proving the existence of two sets of nerves in the extremities; the one supplying the muscles of voluntary motion, the other the organs supporting the circulation; and strikingly illustrates what has been said on this subject. " I have seen," he observes, " a total " loss of pulse in one arm, with coldness, but " complete power of motion in that part; while " the other arm was warm, and possessed a

" perfectly good pulse, but had lost all power
" of voluntary motion."

From the foregoing observations the question arises, For what purpose has nature thus combined the influence of every part of the brain and spinal marrow, to bestow it on particular parts? This question appears to be answered by the experiments which shew, that when the influence of any considerable part, either of the brain or spinal marrow, is withdrawn from secreting surfaces, the secreting power is deranged. This the reader has seen ascertained by repeated experiments, both with respect to the surface of the stomach and lungs. Among the secreting processes, the author ranks the disengagement of caloric, although not taking place on any particular surface, because it appears to be performed by the same power acting on the same fluid; and because, like the secreted fluids, it fails when any considerable part of the influence of the brain or spinal marrow is withdrawn. In the case just quoted from Dr. Parry's work on the pulse, the reader has seen it affected along with other functions influenced by the ganglionic nerves.

Admitting, it may be said, that the due performance of secretion requires the united power of all parts of the brain and spinal marrow, and that we may, therefore, explain why their united influence is bestowed on secreting surfaces; the question still remains. Why should their united influence be bestowed also on the muscles of involuntary motion?

It is evident that affections of the nervous system could produce no occasional increase of the secretions, were not the sanguiferous system, and particularly the vessels of secretion, capable of being stimulated by the same influence which operates in the formation of the secreted fluids. The increase of secreting power in any part would be vain, were there not at the same time a corresponding increase in the supply of the fluids on which it operates. A similar observation applies to the excretory muscles as far as they are muscles of involuntary motion. The same increase of nervous influence which occasions an increased flow of secreted fluids, excites these muscles to carry off the increased quantity. Nature does not seem to trust this to the increased stimulus occasioned by the increased flow of the secreted fluid, which we have reason to believe from the *modus operandi* of certain causes of inflammation, would often occasion morbid distension*. Now, the vascular system and the muscles of excretion, if in them we include the alimentary canal, comprehend all the muscles which are supplied with nerves from the ganglions.

* See Part III., Chap. II.

Thus, we see the necessity of every part of the function which the ganglions appear to perform. A combination of the whole nervous influence is necessary for the due formation of the secreted fluids, and that there may be, under all circumstances, both a due supply of the fluids to be acted upon, and a due removal of those prepared, whether for the functions of life or for the purpose of being thrown out of the system, it is necessary, as appears from what has just been said, that the powers which convey these fluids should be subjected to the influence by which secretion is performed.

The constant presence of fluids in secreting surfaces appears to solicit a continual supply of nervous influence to them, so that they go on during our sleeping as well as waking hours. The more copious the supply of fluids to secreting surfaces, we find the secreting power the greater, and *vice versâ*. The function of secretion, it is evident, requires a more regular supply of fluids than could have been obtained, had the usual action of the vessels depended on the nervous system which is subject to continual variation; but had not this system been capable of influencing the vessels, no change in it could have influenced the flow of secreted fluids. Thus, it is necessary that the power of the sanguiferous should be independent of the nervous system, yet capable of being influenced by it; as it is ascertained to be by the experiments related in the first and second chapters of the present part of this Treatise.

That the reader may see how far the observations of the anatomist correspond with the result of the preceding experiments, the following cursory view of the nervous connexions of the ganglions is presented to him; by which he will find that they may receive the influence of every part of the brain and spinal marrow, and communicate that influence to every part of the body.

The great sympathetic nerve receives nerves from every part of the spinal marrow, being largest near the middle of the spine, and becoming smaller as it ascends and descends, forming ganglions, which give out nerves on all sides. When these circumstances are compared with the fact of its conveying the influence of every part of the spinal marrow, we cannot, I think, hesitate to regard it as arising from this organ; especially as its slender communications with the nerves of the head present the appearance of its gradual termination in that direction. This inference is farther strengthened, by other means being provided for conveying the influence of the brain to the ganglionic system. The eighth pair of nerves, which, from the course it takes, has been termed, by anatomists, the *par vagum*, the reader has seen, from the effects of dividing it, performs this office; for which it is admirably fitted, by its numerous and extensive communications with the ganglions of the great sympathetic. After various connexions with those in the neck and chest, it sends a large branch to the stomach, whose filaments are intermixed on this organ with those of nerves sent by the abdominal ganglions; and at length terminates in forming, with branches of the sympathetic, a great ganglion, termed, from its shape, semilunar. From this ganglion or collection of ganglions, for it is formed of smaller ones, many nerves issue, forming networks on the different large arteries, from which they derive their names. Nerves from these networks alone, or intermixed with other branches from the sympathetic nerves, supply the whole abdominal viscera.

Before the sympathetic nerves finish their course by uniting at the end of the spine, they form ganglions in the loins, which send branches to the lumbar nerves; and others in the lower belly, which send branches to the nerves issuing from the lowest part of the spine; thus forming communications between the ganglionic system and the nerves of the lower extremities.

The nerves of the upper extremities communicate with this system, both by means of the middle ganglion of the neck, and through the sympathetic nerves *, by branches from the second and third of those nerves called intercostal from their passing between the ribs, which go to these extremities.

The ganglionic system communicates by branches of the sympathetic nerves with the internal nerves of the head. One branch is sent to the second branch of the fifth pair before it leaves the head, and one, two, or sometimes three small filaments to the sixth pair, and a branch to what is called the *portio dura* of the seventh pair, at the under part of the ear. These connecting branches are generally regarded as proceeding from the nerves of the head, but for reasons already assigned, we must, as far as the author is capable of judging, agree with those writers who regard them as proceeding from the sympathetic nerves. The extensive connexions of these nerves with the eighth pair have already been mentioned.

The external nerves of the head and neck, namely, the higher spinal nerves, communicate with the ganglions of the neck; and lastly, the ganglionic system communicates with the external parts of the trunk by means of the connexions of the sympathetic with the spinal nerves which supply those parts†.

* See the account of the Nervous System in the Preface.

† Here we have reason to believe a double communication takes place, the spinal nerves conveying to the sympathetic

Thus the sympathetic nerve, conveying the influence of the spinal marrow, and the eighth pair that of the brain, unite in forming the ganglions, which constitute a secondary centre of nervous influence, a channel through which the influence of every part of the brain and spinal marrow flows, to be bestowed on the thoracic and abdominal viscera, on the vessels and all secreting surfaces; the most important of which parts, as the reader has seen proved by direct experiment, are subjected to every part of the brain and spinal marrow; while all other parts, subjected to the influence of the nervous system, obey only those parts of the brain or spinal marrow from which their nerves directly proceed.

It also appears from what has been laid before the reader, that while through the cerebral and spinal nerves, the animal is connected with the external world, the ganglionic system is strictly a vital organ. It has no object but that of maintaining the functions necessary to life.

It seems to be superfluous, after the experiments which have been related, to say any thing in refutation of the opinion of Bichat, that the ganglions are centres of nervous influence, independent of the brain and spinal

the influence of the spinal marrow, and the sympathetic sending with them to the parts to which they are distributed, filaments conveying the influence of the ganglionic system.

marrow. "Les nerfs des ganglions ne peuvent
" transmettre l'action cérébrale ; car nous avons
" vu que le système nerveux partant de ces
" corps, doit etre considéré comme parfaite-
" ment independant du système nerveux céré-
" bral ; que le grand sympathique ne tire point
" son origine du cerveau, de la moelle épinière
" ou des nerfs de la vie animale ; que cette
" origine est exclusivement dans les ganglions ;
" que ce nerf n'existe même point, à propre-
" ment parler, qu'il n'est qu'un ensemble d'au-
" tant de petits systèmes nerveux ; qu'il y a de
" ganglions, lesquels sont des centres particu-
" liers de la vie organique, analogues au grand
" et unique centre nerveux de la vie animale,
" qui est le cerveau."—*Recherches Physiologiques sur la Vie et la Mort, par Xav. Bichat,* page 355 & seq.

CHAPTER X.

On the Sensorial Functions.

A VIEW has now been presented to the reader of the general laws of the muscular and nervous functions, and the means by which these laws have been ascertained.

A set of functions still remains to be considered, which, as far as the author can judge, will be found equally distinct from both of these, although they have never been correctly distinguished from the nervous functions, and the maintainance of which seems to be the final cause of both the others. The nervous and muscular functions maintain the life and health of the animal, and are the immediate means of intercourse between it and the external world; by the sensorial functions it is rendered capable of enjoyment. Were this, however, their only object, they would not fall within the scope of the present treatise, which is confined to the vital powers.

According to generally-received opinions, therefore, except as far as the sensorial powers are necessary for obtaining a supply of food, they form no part of the subject of this Inquiry. The author has, however, been led by it to a conclusion, in which he confidently expects the concurrence of the reader, when the whole of the facts shall have been laid before him, that two of the sensorial functions, sensation and volition, are more immediately necessary to life in the perfect animal.

Nothing can be more indistinct than the opinion of Physiologists respecting the powers by which life is maintained. Constantly engaged in the vain attempt of ascertaining what life is, they have been less anxious to trace the causes on which its preservation immediately depends, an object open to experiment, and even if the other were attainable, one of far greater importance.

It was at once evident that the muscular power is essential to the continuance of life; but it will be admitted that any ideas which have been entertained respecting the share contributed by the nervous system towards this end, have been extremely vague; and the sensorial functions, except in the indirect way just mentioned, have been regarded as out of the question.

It appears to the author that what he is about to say of these functions cannot be better introduced and connected with the preceding part of the Inquiry, than by the following observations on the division of the muscles into those of voluntary and involuntary motion.

A great variety of opinions have prevailed respecting the cause of some muscles being subjected to the will, while others are independent of it. One of the most plausible, and which professes to be the result of experiment, is that of Dr. Johnstone *, " that the ganglions " are the instruments by which the motions of " the heart and intestines are from the earliest

* Med. Essays and Observations, 1795.

" to the latest periods of animal life rendered " uniformly involuntary*?"

Dr. Johnstone's experiments, an account of which is given in the 25th and following pages of the work just quoted, and other experiments of a similar nature to which he refers, of Haller, Whytt, and others, were made with a view to prove that it is impossible to affect the action of the heart by stimulants applied to the brain and spinal marrow. These physiologists appear to have been deceived by the following circumstances. They did not employ the precaution of preventing the action of the muscles of voluntary motion, which renders it impossible to judge of the effect of the stimulant on the heart; and they were not aware that the heart will not obey a stimulant applied to the brain and spinal marrow, however powerful, unless it be applied to a portion of considerable extent. They were probably deceived also by expecting to see in the heart the irregular motions excited by artificial stimulants in the muscles of voluntary motion. Any person who attends to these precautions, will find, that the heart is not only as easily stimulated through the brain and spinal marrow as the muscles of voluntary motion are; but that it may be stimulated through

* The above-mentioned Treatise, p. 16.

them in the newly-dead animal for a considerable time after these muscles can no longer be influenced in this way; proving that the ganglions oppose no obstacle to the influence of the brain and spinal marrow being extended to the muscles of involuntary motion.

These muscles being exposed to the constant or constantly-renewed effects of stimulants, over which the will has no power, in a great measure accounts for their action being involuntary. What power of volition could prevent the blood from exciting the contractions of the heart? But the writers on this subject, as far as the author can judge, wholly overlook the point of most consequence in deteɪ mining the question, namely, that the action of the muscles of involuntary motion can effect no end desired. We will to move a limb, not to excite a muscle. We wish to handle, for example; and, on trial, find that we can move the fingers; but what act of volition could we perform through the medium of the heart or blood-vessels? If we had had no wish to handle, the muscles of the fingers, of course, would never have become subject to the will. Few have any command over those of the external ear, the position of the human ear but little influencing the sense of hearing. The animals whose ears are so shaped that their position materially assists the sense, move the external ear as readily as any other part of the body. It deserves to be remarked, that the will influences the lowest part of the intestines and bladder, the only internal organs which can assist in accomplishing an end desired.

Such is the first instance in which the sensorial power is found to influence the nervous and muscular powers. The reader will soon find it influencing them in a function of still greater importance.

CHAPTER XI.

On the relation which the Vital Functions bear to each other, and the Order in which they cease in Dying.

THAT the reader may follow the author in his endeavours to ascertain the degree and manner in which the sensorial functions are necessary to life in the more perfect animals, it is requisite that he should bear in mind the result of what has been laid before him respecting the muscular and nervous functions.

It appears from the preceding experiments and observations, that the muscular power resides in the muscle itself; that the influence of the nervous system, whatever that may be, stands in no other relation to it but that of a stimulant, and that this power is the same in all the muscles, the means of exciting it alone varying in those of voluntary and involuntary motion.

With respect to the nervous system, he has seen that the nerves afford the sole stimulant of the former, and an occasional stimulant of the latter set of muscles, and are the means of conveying impressions to and from its own more central parts, the brain and spinal marrow; there being no evidence that impressions are ever communicated from one nerve to another, independently of the intervention of one of these organs*.

The nervous system also, as appears from what is said in the fifth and seventh chapters, maintains by its action on the blood the secreting and other assimilating processes, on which the structure of the various organs depends; and from what is said in the eighth chapter and the passages there referred to, causes the disengagement of caloric from the blood, which supports the temperature necessary to life in the more perfect animals.

The subject of the present chapter may be divided into two parts. In the first the author will attempt, by the aid of experiment, to draw a correct line of distinction between the sen-

* See the Observations on the Sympathy of Nerves.

sorial and nervous functions; and in the second, by the same means, to trace the manner in which all the functions are so connected as to render the sensorial power essential to the continuance of life in man and the animals which resemble him.

The seat of the sensorial and nervous powers is not so well defined as that of the muscular power. M. le Gallois appears to regard the brain as the seat of the one, and the spinal marrow, of the other; but many observations are in opposition to this opinion, nerves proceeding wholly from the brain exhibit the phenomena of nervous power properly so called, and that the spinal marrow possesses sensorial power may be made apparent by very simple experiments.

Exp. 64. Even in the warm-blooded animal, the rabbit for example, after all feeling, as far as the brain is concerned, is destroyed, when one of the limbs are wounded, the others are moved, demonstrating that it still possesses the means of receiving the impression from one set of nerves and communicating it to another*. In the cold-blooded animal, the same thing is observed in a greater degree. For a considerable time after the head is cut off, the frog will sometimes even sit in its usual position, and ap-

* See the Observations on the Sympathy of Nerves just referred to.

pear sensible to an injury inflicted on any part of it. It is evident, from many observations, however, that the sensorial power chiefly resides in the brain, and that the power possessed by the spinal marrow is chiefly nervous.

If these powers, it may be said, are thus blended in their organs, what proof have we of their being distinct powers? This proof, it appears to the author, will be found in carefully observing the process of dying, of which what we call death appears to be only the first stage.

However blended the organs of the sensorial and nervous powers may appear to be, we are assured that they are distinct organs, by the fact that while those of the nervous power evidently reside equally in the brain and spinal marrow, those of the sensorial power appear to be almost wholly in man, and chiefly, in all the more perfect animals, confined to the former. It may be possible, therefore, to withdraw the power on which the one set of functions depends, without immediately destroying the other, as we find we can withdraw the influence of the nervous from the muscular system without destroying the power of the latter.

At the instant of death, it is evident the sensorial functions cease, no impression is perceived or followed by any act of volition. It is, however, equally evident to the physiologist, that the muscular power still remains. If under these circumstances the heart or muscles of voluntary motion be stimulated, they still possess the power of contraction, which is only lost by degrees, and not till after the sensorial power has for a considerable time been withdrawn.

It is also evident to the physiologist, that some part of the nervous power still exists, for if the nerves themselves, or those parts of the brain or spinal marrow from which they originate be irritated, the corresponding muscles are thrown into action. The nerves, therefore, are still capable both of conveying impressions and exciting the muscles. Are they capable of their other functions? Can they effect the formation of the secreted fluids, and cause a disengagement of caloric from the blood after the sensorial power is withdrawn?

The author has already had occasion to refer to Mr. Hunter's observations respecting the digestion of the stomach after death. It is perhaps superfluous to observe that this is not to be regarded as a vital action. It is a mere chemical process. But Mr. Hunter, as appears from the following observations, suspected that a truly vital action continues in the stomach for some time after what is called death. " This is ex-
" actly the case with the experiments of Spal-
" lanzani, which although they prove that meat
" was digested in the stomach after the animal

"was killed, which no one doubted," that is, no one doubted that the gastric juice already in the stomach would continue to perform its office there, "yet are not at all calculated to "shew that the stomach itself may be digested. "In fact, the manner in which they were "managed rather tended to prevent that effect "from taking place, the gastric juice having "substances introduced on which it could act, "was less likely to affect the coats of the "stomach*. That the digestion was not car-"ried on merely by the effects of the gastric "juice secreted before death is evident from "his own account, some of the food which had "been introduced and digested being found in "the duodenum; a thing which could not have "happened if a cessation of the actions of life "in the involuntary parts had taken place "when visible life terminated. There had "been an action, *and most probably a secretion* "*in the stomach*†."

It appeared to the author that the question might be reduced to the test of experiment, by dividing, immediately after death, the eighth pair of nerves in the neck, which seems at once to impair and in the newly-dead animal probably

* This observation, as appears from what is said above, is incorrect. It is soon after a full meal that the stomach is most apt to be digested after death, the cause of which the author has had occasion to explain. See Chap. VII. Sect. I.

† Observations on the Animal Economy.

destroys the formation of gastric juice*. We are not, it is evident, to expect that any great secretion of gastric juice can take place after death, or consequently that any great difference can be observed between the food in the stomach of an animal in which the eighth pair of nerves has been divided immediately after death, and one in which they are left entire, and many circumstances which we cannot estimate, particularly there being more gastric juice in the stomach of the one animal than the other at the time they are killed, or one having eaten more than the other, must influence the result. It will not answer the purpose, it is evident, to confine the animals to the same quantity of food, because the stomach of the animal which is most hungry will digest it most quickly. The quantity of old food in the stomach also influences the result. The question, therefore, can only be determined by making the experiment on a large scale, to which, as it is made on the dead animal, there can be no objection.

Exp. 65. This experiment was made on twenty-six rabbits; eight full grown, eight half grown, six two months old, and four one month old. They were made to fast for sixteen hours, at the end of this time allowed to

* The author was not aware, when this experiment was made, that the mere division of these nerves will not wholly prevent its formation in the living animal, as will soon be more fully explained.

eat as much cabbage as they chose, and then killed in the usual way. Immediately after death the eighth pair of nerves was divided in one half of those of each description, and they were all allowed to lie undisturbed for about twenty-two hours. The stomachs were then laid open, and those of the rabbits of the same age, who had eaten most nearly the same quantity, were compared together. The result was, that in twelve pairs the food was most digested in those animals whose nerves were left entire. In one pair it was most digested in the animal whose nerves had been divided. In several of those whose nerves had been divided, the cabbage appeared quite fresh and green. This did not happen in any whose nerves were left entire. In these the colour was always changed more or less to a brown. The difference in the state of the cabbage was sometimes more sensible to the touch than to the eye, that least digested feeling hardest. This experiment, at the same time that it proves the continuance of secretion after the sensorial power has ceased to exist, shews more than any experiment made on the living animal could do, how quickly the formation of gastric juice is impaired by the division of the eighth pair of nerves in the neck.

It is remarkable that the division of these nerves after death almost always produced the same appearance of dark-coloured patches upon the surface of the lungs, but generally in a less degree, observed from it when the operation had been performed during the life of the animal; an effect, equally with the state of the stomach, demonstrating that some of the involuntary functions continue for a certain time after visible death. These patches now and then appear in the lungs of an animal whose nerves are entire, after it has lain dead for some time; but much less frequently, and to a much less degree, than when the nerves have been divided immediately after death.

The appearance of dark-red patches on the surface of the lungs, the reader has seen, is always observed to a great degree when the eighth pair of nerves has been divided during the life of the animal, and it has survived the operation many hours. It may be regarded, and is mentioned by various physiologists, as the characteristic effect of the operation on this organ. The obstruction of the lungs, which is also an uniform consequence of it, appears under many other circumstances, but the author knows of no other in which there is an appearance like this patching, except, as he has just mentioned, that a certain degree of it, or rather something like it, now and then appears in the lungs of the entire animal after it has lain dead for many hours. In the experiments in which the eighth pair of nerves were divided in the living animal, it was always proportioned to the degree in which the secreting power of the lungs was deranged, appearing to the greatest degree when the change of structure and congestion of the lungs was greatest.

In considering the result of the foregoing experiment, the question arises, What occasions any supply of fluids to secreting surfaces, after the circulation has ceased, and thus enables the remaining nervous influence to produce any secreted fluid after death? The result of the following experiments appears to afford a ready answer to this question.

Exp. 66. A rabbit, about two months old, was killed in the usual way. The chest was then laid open, and a ligature thrown round the aorta. Part of the mesentery was now brought before a microscope, and the blood in its vessels seen both by Mr. Sheppard and the author, moving with great velocity. By examining different parts of it, and choosing those which had not been previously disturbed, and consequently still retained some warmth, they found the circulation, in the smaller vessels, going on with rapidity for a quarter of an hour after the aorta had been secured, and an irregular motion of the blood in these vessels was evident for twenty minutes longer, the blood stopping and going on, and sometimes moving backwards and forwards in the same vessel. This could be distinctly seen long after the part had become quite cold. This experiment was performed in the sun-shine, in the open air, where there happened to be a good deal of wind, and the exposed part of the mesentery quickly became parched; which, as we found from other trials, destroyed the motion of the blood in the capillaries long before it naturally ceases.

Full-grown rabbits are bad subjects for this experiment, on account of an accumulation of fat which takes place in the mesentery and obscures the vessels. Rabbits about six weeks old, when they have been fed for some time on green meat, are generally thin, and consequently the best subjects for it.

Exp. 67. A dead rabbit, about a month old, whose intestines had been submitted to examination, after a ligature had been thrown round all the vessels attached to the heart and this organ removed, was thrown aside. An hour and a quarter after the heart had been removed, the author brought part of the mesentery, which had long been quite cold, before the microscope, and still found the blood in some of the capillary vessels moving freely. He has no doubt that the blood continues to move in the capillaries of a full-grown rabbit (whose temperature will sink much more slowly, and whose abdomen has not been laid open,) for several hours after death. This at the same time accounts for the supply of fluids to secreting surfaces, and for a certain power of the nervous system remaining after death, and when the impetus of the blood has wholly ceased, except as far as it depends on mere elasticity and the action of very small vessels. It seems to be owing to this cause also that the larger arteries of dead animals are found empty*. That the continued action of the capillaries must readily empty them, will be evident, when we recollect how much the sum of the areas of the branches of arteries exceeds the areas of their trunks.

We are now to inquire whether the nervous power is capable of occasioning a disengagement of caloric from the blood after the sensorial power has ceased to exist. The author here considers it as proved by experiments already laid before the reader, or referred to, that the disengagement of caloric depends on the nervous power. But it seems so immediately connected with the existence of the circulation, and is so generally proportioned to its vigour, that we cannot, he thinks, adopt a better means of answering the present question, than by ascertaining whether supporting the circula-

* The author cannot agree with Dr. Parry in ascribing this fact to the contractility of the larger arteries. This may reduce, but it cannot wholly expel, their contents.

tion by artificial respiration, after death, occasions a greater disengagement of caloric than takes place when the dead animal is left undisturbed. On this subject there has been great difference of opinion. The following experiments seem to point out how this difference may have arisen, on the supposition that all the experiments which have been made on the subject are correct, which we have every reason to believe them to be.

Exp. 68. Two rabbits of the same size were killed in the usual way, the temperature of the air being 61°, that of both rabbits 104°. The lungs of one were inflated six times, those of the other from twenty-six to thirty times, in a minute. The temperature of the first in half an hour was 102.25°, in an hour 100° ; the temperature of the second at the end of half an hour was 101.5°, at the end of an hour 98°.

It is evident that all the air thrown into the lungs, beyond what is necessary to effect the proper change in the blood, must tend to reduce the temperature in proportion as that of the air is less than that of the animal. The living animal receives but little air into the lungs in one inspiration. It is impossible in the dead animal to throw in the quantity which the blood still demands, and no more.

The following experiments, in which Mr. Sheppard was so good as to assist the author, as indeed he did in all the experiments which he made on this part of the subject, strikingly illustrate these observations.

Exp. 69. Two rabbits were chosen of the same size, and each of the temperature of 102.5°.—They were killed in the usual way, in the temperature of 65° ; one was left undisturbed. In the other, the lungs were inflated about thirty times in a minute. In half an hour the temperature of the undisturbed rabbit was 98.75°, while that of the other was only 98.5°. In the last the lungs were then inflated only about twelve times in a minute. In half an hour its temperature was 96°, so that it had lost 2.5°, while that of the other left undisturbed had in the same time sunk to 95.25°, so that it had lost 3.5°.

Exp. 70. Two rabbits were killed in a temperature of 61.5°. The temperature of the one was 106°, of the other 103°. The lungs of the first were inflated twelve times in the minute, the other was left undisturbed. In half an hour the first had lost 3.5°, its temperature being 102.5°. The other in the same time had lost 4°, its temperature being 99°. The first being of the highest temperature, would have cooled fastest had both been undisturbed, although probably not in a sensible degree. The author may here observe, that it happened in the course of such experiments as those which he is relating, that the temperature of the room varied, but as the experiment was always made on both rabbits at the same time, and placed together, this could not influence the result, and is therefore unnoticed. The lungs of the first of the above rabbits were now inflated at the rate of from twenty-six to thirty times in a minute. At the end of half an hour its temperature was 98°, that of the other at the same time being 94.5°, so that each had now cooled 4.5°, the disengagement of caloric in consequence of the inflation of the lungs being here sufficient to counteract the cooling effect of the rapid change of air, and no more. In one experiment of this kind, in which the lungs were inflated only a few times in a minute, the temperature was found to have risen nearly 1° between two of the examinations.

While the author was making experiments on this subject in Worcester, Dr. Hastings was, without his knowledge, making similar experiments at Edinburgh. He shewed the author the detail of several, which prove that throwing air into the lungs of the dead rabbit about fifteen times in a minute, occasions it to cool more slowly. In one of his experiments, the rabbit, in which artificial breathing was performed, cooled only 4°, while that which was left undisturbed cooled 7.5°. This was the greatest difference he observed. He frequently saw the thermometer rise a little in those animals in which the lungs were inflated after death. In those in which they were not inflated, the cooling was always uniform.

There appears to be no doubt, from the preceding experiments, that when the lungs are not inflated so frequently as to constitute a powerfully cooling process, their inflation by occasioning a disengagement of caloric after what we call death, retards the cooling of the animal.

The author's next object was to ascertain how far the disengagement of caloric after death is influenced by the destruction of the brain and spinal marrow.

Exp. 71. Two rabbits of the same size, whose temperature was 98°, were killed in the usual way. In one, immediately after death, the brain and spinal marrow were destroyed by introducing, through a hole in the cranium, a wire of nearly the same diameter with the cavity of the spine, repeatedly pushing it on to the end of this cavity, and then moving it about for some time in the cavity of the head. The other rabbit was left entire. A hole was made about the centre of the abdominal muscles in each, to admit of a thermometer being introduced into the cavity of the abdomen. They were placed near each other in a temperature of 50°. During the first twenty minutes each lost exactly 4°, and they both lost, during the succeeding three-quarters of an hour, just 2° during each quarter. Something, which we could not ascertain, accelerated the rate of cooling during the next quarter, and so exactly did it correspond in both rabbits, that each lost during this quarter 2.5°. After this their temperature diminished more slowly, and still more so, of course, as it approached more nearly to that of the air, but still in both it was found to correspond. At the end of a hundred and ten minutes the temperature of both rabbits was 84°.

Exp. 72. The foregoing experiment was repeated, with the difference that in both rabbits the lungs were inflated; but we could not perceive that the one rabbit cooled faster than the other.

Exp. 73. Two rabbits, of the same size and temperature, being killed in the usual way, in the one the brain and spinal marrow were wholly removed, the other being left entire. In both the lungs were inflated. We could not perceive that the one cooled faster than the other.

The author was particularly careful in repeating these experiments, because they appear at first view to contradict the inferences afforded by some of those which precede them. On a closer attention, however, the different results will be found perfectly consistent.

It appears from the foregoing experiments, that after the destruction of the sensorial, the nervous power is still capable of performing all its functions, except that it can no longer give evidence of conveying impressions to the sensorium, the necessary consequence of the destruction of the sensorial power. On comparing these experiments, however, a considerable difficulty presents itself. We have seen it ascertained by those on digestion, that the brain and spinal marrow retain sufficient power after visible death to form secreted fluids. Yet it would appear, from the experiments on temperature, that the influence of the brain and spinal marrow has no effect under the same circumstances in promoting the disengagement of caloric, although it is evident that the system still retains the power of disengaging it, and from former experiments, that this power depends on the state of the nervous system. The secreted fluids are no longer formed, if the influence of the brain be withdrawn; the evolution of caloric takes place in the same way whether the influence of both the brain and spinal marrow be withdrawn or not.

A well-known fact appears to remove the difficulty. Although we have reason to believe, the author thinks, from every observation on the subject, that the brain and spinal marrow are the only sources of nervous power; yet it is evident that a certain portion of this power remains in the nerves when separated from these organs, as appears from the contractions excited in the muscles by irritating their nerves under such circumstances. The muscle will thus be made to contract as long as any power remains in the nerve, but this being once exhausted, the nerve has no means of renewing it. Now the first nervous power which is employed in the stomach after death is of course that already in its nerves. In proportion as this is exhausted, the brain and spinal marrow are called upon for a further supply. It is evident that they cannot be long so called upon, because there cannot long be any supply of proper fluids. If then, instead of the nerves which belong to the stomach, the whole nerves of the ganglionic system terminated in this organ, there is reason to believe, that the supply of fluids, which takes place after death, would never be sufficient to exhaust the nervous power already in its nerves; and consequently, that in that case it would never receive any part of this power formed after death; and the same degree of digestion would take place after death, whether the influence of the brain remained or not.

Now this is precisely what seems to happen with respect to the temperature after death. As long as we can by artificial respiration occasion such a change in the blood as elicits the power of the nervous system, the blood draws it from all the nerves of the ganglionic system;

and it does not appear that we can support this change long enough to exhaust the nervous power already in the nerves, and occasion any farther demand for it. It appears from the experiments above related, that the greatest disengagement of caloric, which can be effected after death, is but very inconsiderable. Hence the result is the same whether the brain and spinal marrow exist or not. There is already in the nerves more nervous power than the blood can use.

The disengagement of caloric occasioned by inflating the lungs after death being so small, may arise from our being able but very imperfectly to imitate natural respiration. It is true that we can imitate it sufficiently to give the arterial colour to the blood, but it will appear that the disengagement of caloric from this fluid is not essentially connected with the change of colour. It is evidently impossible to proportion the quantity of air thrown in, to the demand for it, which is constantly becoming less, so that we are either supplying too much or too little. In the former case the superfluous quantity can, as far as relates to the temperature, have no other effect but that of reducing it, as happened in the above experiments; but although we could supply air in the due proportion, we should still be very far from being able to imitate natural respiration, from which artificial respiration, among other things, differs, in the increased pressure to which the lungs are subjected in the latter, in which the ribs and diaphragm are moved by the force of the injected air; whereas in natural respiration the ribs and diaphragm being moved by their muscles, the lungs are subjected to no pressure in addition to that of the atmosphere. But the great diminution of nervous influence in artificial respiration appears to be the most essential difference between it and natural breathing. It would be very desirable, although attended with considerable trouble, accurately to ascertain the effects of passing the galvanic influence through the lungs while artificial respiration is performed *.

A very decisive experiment by Mr. Brodie, to whose labours on the subject of animal temperature we are so much indebted, related in an addition to the Croonian Lecture above referred to, proves that the change of oxygen gas into carbonic acid gas takes place when the lungs are inflated after decapitation.

Thus it appears, from the experiments related in this chapter, that the nervous as well as the muscular power is capable of its functions after the sensorial power is withdrawn.

THE next question which presents itself is,

* See what is said of suspended animation in the third part of this Inquiry, page 336, *et seq.*

whence does it arise that, notwithstanding the independence of the two first of these powers, they never long survive the sensorial power.

Here a difficulty presents itself, on which M. le Gallois makes the following observations:—

" Il est donc certain que la vie du tronc n'a " son principe immédiat ni dans le cerveau, " ni dans aucun des viscères de la poitrine et " de l'abdomen ; mais il ne l'est pas moins " que tous ces viscères sont indispensables à " son entretien. Or, en considérant sous quel " rapport ils le sont, les faits énconcés plus " haut prouvent évidemment que, quant au " cerveau, les phénomènes mécaniques de la " réspiration, c'est-à-dire, les mouvemens par " lesquels l'animal fait entrer l'air dans ses " poumons, dépendent immédiatement de ce " viscère. Ainsi, c'est principalement en tant " que l'entretien de la vie dépend de la réspira- " tion, qu'il dépend du cerveau ; ce qui donne " lieu à une grande difficulté. Les nerfs dia- " phragmatiques, et tous les autres nerfs des " muscles qui servent aux phénoménes mécan- " iques de la réspiration, prennent naissance " dans la moëlle épinière, de la même manière " que ceux de tous les autres muscles du tronc. " Comment se fait-il donc qu'après le décapita- " tion, les seuls mouvemens inspiratoires soient " anéantis, et que les autres subsistent ? C'est " là, à mon sens, un des grands mystères de la

" puissance nerveuse ; mystère qui sera dévoilé
" tôt ou tard, et dont la découverte jettera la
" plus vive lumière sur le mécanisme des fonc-
" tions de cette merveilleuse puissance."

This difficulty seems to the author to arise from respiration being regarded as a function depending wholly on a combination of the nervous and muscular powers ; whereas the sensorial power appears evidently to share in it.

The muscles of respiration are, in the strictest sense of the word, muscles of voluntary motion ; we can at pleasure interrupt, renew, accelerate, or retard their action ; and, if we cannot wholly prevent it, it is for the same reason that we cannot prevent the action of the muscles of the arm, when fire is applied to the fingers. The pain occasioned by the interruption of a supply of air to the lungs is greater than can be voluntarily borne. Respiration continues in sleep for the same reason that we turn ourselves in sleep when our posture becomes uneasy. It continues in apoplexy for the same reason that the patient generally moves his limbs if they are violently irritated.

If respiration continues in apoplexy when no irritation of the limbs, however powerful, can excite the patient to move them, it arises from the interruption of a supply of air to the lungs producing a greater degree of irritation than any other means we can employ. We have heard of the hand voluntarily held in the fire, but we know of no instance where the breathing has been voluntarily discontinued till the lungs were injured. As the insensibility increases in apoplexy, the breathing becomes less frequent; and when the former becomes such that no means can longer excite any degree of feeling, the breathing ceases.

By a certain sensation, a wish is excited to expand the chest. This is an act of the sensorium. Till this act take place, the nervous as well as the muscular power, by which its expansion is effected, is inert; it is in vain that these powers remain, if the power which calls them into action be lost. Thus the removal of the brain puts a stop to respiration.

Is it said that the motions of respiration must be involuntary, because we are in general unconscious of them? But do we not become more or less so of all habitual acts of volition? "If I did so, I did it unconsciously," is a common expression. If we stop a person who is walking, he cannot tell which leg he last moved, or a person who is playing on an instrument, he cannot tell which fingers he last employed; yet all such acts are strictly acts of volition. If we are reminded of them, we can always interrupt, renew, retard, or accelerate them at pleasure. We have no difficulty in perceiving and changing in any way we please the motions of respiration, when we choose to attend to them; but as there is no other act of volition so habitual, there is none so apt to escape our attention.

The foregoing explanation of the manner in which the removal of the brain puts a stop to respiration will be readily admitted, when the reader turns his attention to the part of the brain to which impressions from the lungs are conveyed. It is evidently to the part where the eighth pair of nerves, which supplies them, originates, and from which the spinal marrow proceeds. These nerves are no ways connected with the muscles of respiration, they only convey the sensations excited in the lungs; yet it appears from the experiments in which M. le Gallois removed the brain by slices, that respiration continued till he removed this part of it, and then instantly ceased. In these experiments, the power of the muscles of respiration and the nervous power which excites them still remain, as may be easily ascertained by stimulants properly applied to the spinal marrow. It is the sensation which excites to inspire, that is the influence of the sensorial power which is withdrawn.

If these observations are kept in view, the physiologist will perceive how well the gaspings of the head, after it is separated from the body, correspond with the interesting investigations of Mr. Charles Bell, to which the author has already had occasion to refer*.

* Philosophical Transactions.

We cannot, perhaps, have a better instance of the distinct operation of the sensorial, nervous, and muscular powers, than in the case before us, although they all here conduce to the same end. We may destroy any one of them, and leave the others unimpaired. The destruction of the sensation by which we will to inspire, the reader has just seen, does not destroy the nervous or muscular power employed in respiration. By means applied to the muscles of respiration, we may destroy their mechanism without depriving any part of the spinal marrow of its power, or at all impairing the above sensation; and we may destroy the nervous influence which excites these muscles by destroying a certain part of the spinal marrow, while they, as may be ascertained by the application of stimulants, perfectly retain their vigour, and the sensation which excites the wish to inspire, though, as in the last case, useless, remains unimpaired; nay, if any two of these powers be destroyed, they leave the remaining power unimpaired. The destruction of the muscles of inspiration, and of the nervous influence which excites them, does not destroy the sensation by which we will to inspire; nor does the destruction of this sensation and the nervous influence at all impair the power of the muscles; and we may destroy the sensation in question, and the power of the muscles, without impairing the nervous influence which excites them. So far from true is the position of M. le Gallois, that the power on which all the motions of inspiration depend, resides in the medulla oblongata.

Much has been written by Whytt and many other physiologists, respecting the cause of the first inspiration. The author cannot help thinking that the difficulty vanishes, when we regard the muscles of inspiration as merely muscles of voluntary motion. The young animal throws them into action to remove a painful sensation occasioned by the want of that change in the blood, which is produced by the influence of the air in the lungs ; a process necessary to the existence of the animal as soon as its connexion with the mother ceases, and which can only be effected by expanding the chest, and thus receiving air into the lungs. It seems to be expanded for the first time, precisely for the same reason that the fœtus changes its position for the first time by acting with the muscles of the trunk and limbs. In both cases he endeavours to remove an uneasy sensation, and nature has given him the power to remove it by calling into action certain muscles subjected to the will. The first act of deglutition, if it does not occur in the fœtal state, appears to be an act of precisely the same nature with the first inspiration. In both, a certain set of muscles of voluntary motion is thrown into action to satisfy a craving which had no existence in that state.

It may be objected to this view of the first inspiration, that the animal often breathes before a ligature is thrown round the vessels which connect it with the mother; but we have no reason to believe, that the secondary change, effected in the blood of the fœtus by the vicinity of the maternal blood in the after birth, although this gives it the florid colour, as may be seen by opening the vessels, is sufficient for the functions of the perfect animal. One of these functions, which, as the reader has seen, is intimately connected with the change effected on the blood by the air, the disengagement of caloric, it is evident, is immediately after birth required to be in a state of much greater activity than in the fœtus, which is surrounded by a medium of its own temperature.

Why the sensorial powers are the first which fail in dying will be considered in the next chapter; it is sufficient at present to refer the reader to the experiments which prove that the nervous and muscular powers, with the exception of those cases in which the nervous system is so impressed as immediately to destroy all the functions, survive them.

A necessary consequence of the sensorial powers being the first which fail, is that respiration is the first vital function which ceases, being the only one to which the sensorial powers are necessary.

When respiration ceases, the change in the blood effected by this function no longer taking place, most of the pulmonary vessels lose their proper stimulant, red blood, and feel more directly perhaps the debilitating influence of black blood. Their functions, therefore, begin to fail. In proportion as this happens, the blood accumulates in the lungs. The right side of the heart consequently experiences an increased difficulty in emptying itself, and the due supply of blood to the left side fails. By the operation of these causes, both sides of the heart, in warm-blooded animals, soon lose their power after respiration ceases. The arteries, under such circumstances, it is evident, cannot long supply fluids proper for the purposes of secretion, the nervous and muscular solids, therefore, deviate from the state necessary for the functions of life, which at length cease in every part.

Such appears to be the order in which the functions always, with the exception just mentioned, cease in death, whether it be occasioned by injury of the sanguiferous, or nervous system, or both.

The acute and indefatigable Bichat has

been at great pains to ascertain the effects of black blood on the lungs and other organs. To his experiments on this subject the author refers the reader. There are but few parts of his physiological works, however, which can be confidently referred to. In general, he has allowed his reasonings to go beyond the evidence afforded by his observations and experiments. The author will take this opportunity of making a few remarks relating to the principal points in which he has differed from him. He was unacquainted with the fact, that the spinal marrow performs its functions independently of the brain *, and therefore did not see the difficulty respecting respiration stated by M. le Gallois, but seems to think that the division of the spinal marrow near the head occasions death, by preventing the nervous influence of the brain from reaching the intercostal muscles and diaphragm. The want of this knowledge leads him into inaccuracies, both in his observations on death, and other passages; which are increased by his not being aware, that the sensorial and nervous powers

* The independence of the spinal marrow on the brain, as far as relates to its power over the muscles of voluntary motion, appears from the experiments of M. le Gallois; and as far as relates to secretion, from experiments laid before the reader in the second section of the seventh chapter of this Inquiry.

have no direct dependence on each other. He is led into more obvious errors, as far as the author is capable of judging, in various parts of his works, particularly in those which relate to the passions and the death of the brain, by his not knowing that the heart and blood-vessels may be directly influenced, and even their power directly destroyed, by agents acting either on the brain or spinal marrow; by his not being aware of the dependence of the process of secretion on the brain and spinal marrow, and by his supposing that the ganglions are capable of preparing nervous influence independently of these organs; a supposition which the reader has seen contradicted by many experiments, and which Bichat does not attempt to support by any observation or experiment directly bearing on the point.

These circumstances have even led him into the most striking inconsistencies in his great division of the functions into organic and animal. If the experiments which have been laid before the reader be correct, the sensorial with the cerebral part of the nervous functions constitute the animal, and the ganglionic and muscular functions the organic life. To this, it may be objected, that plants and the less perfect animals have no ganglionic system.— Wherever secretion is performed, a power resembling the ganglionic must exist. In order that a being, possessed of the muscular and secreting systems alone, may live in perfect vigour, it is only necessary that respiration should be performed, as circulation is, by powers of involuntary motion. A being so formed, though possessed of all the powers of life, would be wholly unconnected with the external world, except as far as food and the influence of air and light were necessary to its existence ; all other intercourse with that world depending on the sensorial and cerebral part of the nervous functions. Such is the life of vegetables, and we have reason to believe, that that of the lowest class of animals differs from it in little else than degree*. An animal of this class approaches, as nearly as facts will allow us to suppose, to one possessing merely organic life, according to Bichat's definition of it; yet, in the second section of his sixth article, he maintains, that every thing relative to the passions belongs to the organic life ; an inconsistency, itself sufficient to prove a radical defect in his system. Can the passions belong alone to that life in which they never can be excited, in

* The author does not mean that the change effected on the air by plants is of the same nature with that effected by animals, or that they possess a circulation similar to that of animals; but we know that air is necessary to their existence, that some change in it is effected by them, and that in their vessels or canals there is a continual motion of their fluids.

which they never could operate? Even according to Bichat's definition of organic life, it is common to the animal and vegetable world.

The various functions necessary to the preservation of life in the more perfect animals have now been considered. It appears from the experiments and observations which have been laid before the reader, that as the muscular is independent of the nervous power, yet influenced by it; the nervous is independent of the sensorial power, yet in like manner influenced by it: that the sensorial can be withdrawn without destroying the nervous power, as the nervous can be withdrawn without destroying the muscular power; but in the entire animal, as the muscular obeys the nervous, the nervous obeys the sensorial power; and that they are all so connected, that the existence of each indirectly depends on that of the others. They may, therefore, be justly called vital powers. In the following chapter, an attempt is made to point out how far it is possible to advance in ascertaining the nature of these powers.

CHAPTER XII.

On the Nature of the Vital Powers.

The first observation which strikes us, in comparing the sensorial and nervous functions, is, that the latter bear a striking, the former no analogy to the effects observed in inanimate nature. The act of secretion and the maintenance of animal temperature are analogous to the processes of the laboratory ; and the transmission of impressions through the nerves, both to chemical and mechanical processes ; while the excitement of the muscular fibre is the ready effect of many inanimate agents. But what analogy can we detect between the functions of the sensorial power, sensation and volition for example, and the effects of inanimate agents. We are now in a new world, and at once perceive that it is in vain to look for the analogies which necessarily suggest themselves on reviewing the phenomena of the nervous system. It seems to require but a moment's reflection to teach every sober and unprepossessed understanding, that, in our study of the sensorial power, we must be satisfied with observing and arranging its phenomena without attempting to refer them to any more general principle.

On a review of all that has been laid before the reader, it is evident that the nervous and muscular powers are, on the one hand, the direct means of maintaining the life of the animal; and on the other, of connecting it with the external world; the former receiving impressions from that world, and through the latter communicating impressions to it. All the functions of both powers bear a strong analogy to the properties of the world with which they are thus associated; and we have reason to believe that all these functions, as is evidently the case with many of them, are the results of inanimate agents acting on vital parts.

As vital properties do not differ from the properties of inanimate nature, in degree or by any other modification, but have nothing in common with them, it follows that when living bodies affect each other only by their vital properties, the result must be such as bears no analogy to any of the properties of inanimate nature; and, consequently, that in all processes which have any such analogy, one of the agents must operate by the properties of inanimate matter.

In the animal body itself, the nervous system alone appears to be the connecting link between the sensorium and inanimate matter. It consists of living parts capable of acting in concert with that matter, receiving impressions from it, and independently of the intervention of the muscular system impressing it; for there can be no stronger analogy than that which subsists between the secreting processes, effected by the nervous power in living surfaces, and the chemical processes of inanimate matter; and if an inanimate agent be employed in the former processes, its supply and application must be regulated by the vital powers of the nervous system. Whether this agent be a distinct being, or only a peculiar state of the constituent parts of bodies, is not the question. All the essential inferences are in either case the same. The phenomena of electric animals are here in point. We see their nervous system collecting or forming and applying, even according to the dictates of the will, an inanimate agent.

With respect to the sensorial functions, they have only an indirect effect in maintaining animal life, and are excited by no impressions but those communicated through the nervous system. They are therefore the results of vital parts acting on each other. Hence it is that they bear no analogy to the processes of inanimate nature, and are the first functions which cease when the vital power begins to fail. In the nervous and muscular functions, an inanimate agent excites the languid powers of life.

In the sensorial functions, the functional power and the agent which excites it, being equally vital powers, fail together.

When the nature of the sensorial functions is kept in view, we cannot be surprised that the attempts to refer them to a more general principle should have proved so futile. To what other principle shall we refer the effects of the vital parts of animals on each other, when it is in animals alone that such parts ever influence each other? Even in vegetable life we find nothing analogous to the sensorial functions. All its processes bear the same analogy to the properties of inanimate nature which we observe in the functions of the nervous and muscular systems of animals, and are therefore the results of inanimate agents acting on living parts.—Much less can we look for any analogies of this kind in inanimate nature itself. Such fancied analogies may please in the creations of the poet, but by the philosopher they are justly rejected. While we are charmed with the flights of Lucretius, we see only the perversion of philosophy in the reasonings of Hartley.

While these observations point out the necessity of limiting our study of the sensorial functions to a careful observation and arrangement of their phenomena, they encourage us to inquire into the nature of the functions of the nervous power, that is, to endeavour to refer them to some more general law whose phenomena, however modified, are not wholly changed by one of the agents in these functions being a vital part. The power which operates in many other instances may be the means of exciting the muscles, of conveying impressions to and from the sensorium, of effecting the formation of the secreted fluids, and of causing the disengagement of caloric which maintains animal temperature.

Such is the train of reasoning which seems unavoidably to lead to the conclusion, that the nervous power is an inanimate agent. It appeared to the author that it might not be impossible to submit this conclusion to the test of experiment. Nobody will hesitate to admit that the vital functions depend on the organization of the parts in which they reside. If the organization of the muscular fibre be deranged, its function ceases. The same is true of the functions of the brain, spinal marrow, &c. Whether the function depends merely on that organization, or on something superadded to the organ, the presence of which depends on the organization, we know not. The fact is, that when the peculiar organization is disturbed, the function ceases. A power which can exist independently of the peculiar organization of the part in which it resides, is not a vital power. It must be classed with those powers which are capable of existing in inorganized bodies. It is a mere inanimate agent.

The nervous power, whatever it be, is evidently conveyed by the nerves. If it be a vital power, its existence depends on the organization of the part in which it exists. Its propagation along the nerve, therefore, depends on the peculiar organization of the nerve, and cannot be performed by any other part. If the nervous power can be conveyed by other parts, it is not a vital power, but one that may reside in inorganized bodies.

With a view to determine this question, the author, with the assistance of Dr. Hastings and Mr. Sheppard, made many experiments on nerves directly proceeding from the spinal marrow, but could obtain no evidence of the nervous power having been conveyed by any part but the nerve itself. Circumstances which the author will soon have occasion to mention, led him to make the experiment on the ganglionic nerves, in which Mr. Cutler was so obliging as to assist him.

Exp. 73. The reader has seen, that removing a part of the eighth pair of nerves prevents the formation of the gastric juice, the fluid secreted in the stomach being no longer capable of acting on the food. At the author's request, Mr. Cutler divided, without otherwise disturbing the eighth pair of nerves in the neck of a rabbit, which had been allowed, immediately before the operation, to eat as much as it chose after a fast of many hours. The divided ends of both nerves retracted, so as to cause a separation between them of at least a quarter of an inch on both sides. The animal died in about eight hours, and although the digestion was much deranged, such a change was found to have taken place in the food, as proved that part of the nervous power conveyed by the eighth pair of nerves had reached the stomach. The result was such as left no doubt in the mind of any of those who witnessed it, among whom was Mr. Brodie. M. Breschet, Dr. Milne Edwards, and M. Vavasseur of Paris * have since repeated this experiment on a large scale, and confirmed its result.

It seems, at first view, surprising, that the influence of the nervous system should pass so readily by the ganglionic nerves after their division, since it appears from the experiments above alluded to, made on the spinal nerves, and, indeed, we have reason to believe from every day's experience, that this does not hap-

* De l'Influence du Système Nerveux sur la Digestion Stomachale, par MM. Breschet, D.M.P., chef des Travaux Anatomiques de la Faculté de Médicine de Paris, etc. H. Milne Edwards, D.M.P. et Vavasseur D.M.P. (Mémoire lu à la Société Philomatique, le 2 Août, 1823.) Extrait des *Archives Générales de Médécine*, *Août* 1823.

pen in these nerves; but the different circumstances in which the two sets of nerves are placed seem readily to explain the difficulty. We know that the power of secreting surfaces is increased for the time, if they retain their healthy state, by any cause which occasions a greater than usual determination of blood to them. The presence of this fluid in such surfaces, therefore, solicits towards them a corresponding supply of the influence of the nervous power. Thus there is a cause soliciting a flow of this influence to the extremities of the ganglionic nerves, which has no existence in the case of the cerebral and spinal nerves. There is nothing in the muscular fibres to solicit this influence. They are passive till it is applied to them.

Thus that the nervous power is an inanimate agent appears from direct experiment, as well as the simplest train of reasoning. There is still another evidence of the same fact, which alone perhaps would be sufficient. In its most simple function we can substitute for the nervous power a variety of inanimate agents. The muscles can be excited to contract in precisely the same way, as far as we can see, both by mechanical and chemical agents, as by the nervous power. As soon as it was ascertained that the power of the muscular fibre resides in itself, and that the nervous power acts only as a stimulant to this fibre*, the question which has just engaged the reader's attention appears to have been determined. If there be nothing in common between vital properties and those of inanimate agents, how comes it that a vital power here performs a function apparently as well performed by those agents. If the nervous power imparts no power to the muscular fibre, its effect on it is precisely the same with that of other stimulants.

In considering the inference from this power having passed the space between the divided ends of the nerve, the reader must recollect that it is not merely capable of passing along the nerve from the brain and spinal marrow, but of remaining in the nerve when those organs no longer exist. See Page 185, *et seq*.

There is no better proof of having arrived at truth, than that arguments drawn from dissimilar sources conspire in confirming the result of our inquiry. Here a simple train of reasoning, unequivocal experiment, and every day's experience of the effects of stimulants on the muscular fibre, tell us that the nervous power is an inanimate agent.

It only remains to inquire whether this agent is peculiar to the animal body, or the same which operates in the production of other phe-

* Experiment 35.

nomena. This is a question which can be determined by experiment alone.

When we look throughout inanimate nature for an agent capable of the phenomena of the nervous power, the most subtile, electricity, naturally suggests itself, and when voltaic electricity and its signal influence on the muscular system was discovered, a material step, it was imagined, had been made towards ascertaining the nature of the nervous power. On more mature reflection, however, it was admitted that to ascertain that galvanism is capable of exciting the muscular fibre, a property possessed in common with so many other bodies, is to go but a very short way towards establishing its agency in the phenomena of the nervous system, and of late the opinion appears to have been abandoned. If the nervous power and galvanism be really the same agent, the latter must be capable of the more complicated, as well as the more simple, functions of that power.

On comparing the properties of galvanism with the phenomena of the nervous power, the analogy between them seemed to the author to warrant the investigation thus suggested.

The reader has seen that by the removal of part of the eighth pair of nerves, the power of digestion, and consequently the formation of gastric juice, is wholly lost, and the structure of the lungs as well as their secreting power deranged. This appeared to offer an excellent opportunity of ascertaining how far galvanism is capable of effecting the more complicated functions of the nervous system. It is not difficult, by coating the lower part of the divided nerves with tin-foil, and applying a plate of metal to the skin over the stomach and lungs, to expose these organs, by means of a galvanic trough, to any degree of galvanic power which may be judged proper. The author explained his views to Dr. Hastings, who, at his request, was so good as to make the following experiment.

Exp. 74. The hair was shaved off the skin over the stomach of a young rabbit, and a shilling bound on it. Part of the eighth pair of nerves was then removed, and about a quarter of an inch of the lower remaining portion of each nerve coated with tin-foil. The tin-foil and shilling were connected with the opposite ends of a galvanic trough, containing fifty-two four-inch plates of zinc and copper, the intervals being filled with muriatic acid and water, in the proportion of one of acid to seven of water. The galvanic influence produced strong contraction of the muscles, particularly of the fore limbs.

For five hours the animal continued quite free from the symptoms which follow the division of the eighth pair of nerves in rabbits. It had neither vomited nor been distressed with difficulty of breathing. It had not eaten any thing after the nerves were divided. At this time the power of the trough became much weaker, so that it produced no visible effect on the muscles. The respiration now began to be disordered, and soon became very difficult. Acid was put into the trough till the galvanic power became as great as at first, and the animal soon breathed with greater freedom. The galvanic process was several times discontinued and renewed, so that we repeatedly saw the extreme dyspnœa return on discontinuing, and abate on renewing it. The animal died in six hours after the division of the nerves.

On examination no food was found in the passage to the stomach. This organ was not larger than usual. The food had undergone a considerable change. The appearance and smell of the parsley were gone. The smell was that peculiar to the rabbit's stomach while digestion is going on. Both Dr. Hastings and the author, who had been much accustomed to examine the stomach of rabbits under various circumstances, thought that digestion was nearly as perfect as it would have been in the same time in a healthy rabbit. This rabbit had not eaten any thing for twelve hours till within three hours of the experiment; it was then very hungry, and allowed to eat as much parsley as it chose.

The membrane of the wind-pipe was of its natural colour, and there was no fluid in it. The ramifications of the air tubes in the left lung were quite free from frothy mucus. There was some fluid in the right lung, though it did not appear much gorged; there was one dark spot on it. The lungs collapsed imperfectly on opening the chest. The author requested Dr. Hastings to make the experiment in the following manner:—

Exp. 75. Two full grown rabbits were kept without food for twelve hours; within half an hour of the experiment, they ate as much parsley as rabbits usually do at one time. After the hair was shaved off the region of the stomach in one of them, and a shilling bound upon it, part of the eighth pair of nerves was removed, and the lower remaining portion of the nerve coated with tin-foil, to the extent of half an inch. Difficulty of breathing was evident immediately after the operation. It was an hour after it before the animal was brought properly under the galvanic influence; till then the respiration was very much oppressed; it soon improved on keeping up a regular and gentle twitching of the muscles of the chest and forelegs. In this instance a less galvanic power was employed, the third, half, or whole of the trough being used, according to its effects. A gentle twitching of the fore-legs was regarded as the measure of a due degree of galvanic power. The animal made a croaking noise in respiration, the cause of which will presently be explained. The breathing always began to get worse when the galvanic power became too weak to produce any twitching in the fore-legs.

About twelve hours after the operation, each rabbit ate the same quantity of parsley. Fourteen hours after the nerves had been divided, the galvanic influence had become too feeble, and the animal made one attempt to vomit. The power of the trough was increased, and no further attempt to vomit took place. After this, however, the breathing continued more or less oppressed. It died in seventeen hours after the removal of part of the nerves. The other rabbit, which had been left undisturbed, was killed at the same time.

The stomachs of both were laid open. That of the rabbit, in which the nerves had been divided, was not more distended than the stomach of the other. The food in it had the appearance which it has in a healthy stomach while digestion is going on. The only differences between the contents of the two stomachs were the following:—The food which the healthy rabbit had taken during the experiment, was found in the cardiac portion of the stomach, and digestion was going on rapidly in it, while that, which the other had taken at the same time, was still in the passage to the stomach, and consequently unchanged. The position of this animal was unfavourable to the food's reaching the stomach. There was about a quarter of an inch of the passage between the food and the stomach quite empty. The contents of the middle portion of the two stomachs could not be distinguished from each other; those in the pyloric end only differed in being of a firmer consistence in the healthy rabbit. In both, the contents of the pyloric end of the stomach were most digested, and in both the food had equally lost the appearance and smell of parsley, and acquired the smell peculiar to the stomach of the rabbit while digestion is going on. The food in the duodenum was equally digested in both. The reader has seen, that after part of the eighth pair of nerves is removed, parsley will remain in the stomach of a rabbit wholly unchanged for six-and-twenty hours. Neither Dr. Hastings nor the author could, from any thing observed in the stomach of the rabbit which was galvanized in this experiment, have doubted its being the stomach of a healthy rabbit.

If we compare the foregoing experiment with Experiment 61, in which only one of the eighth pair of nerves was divided, we find the difference of result very striking. In the latter, parsley remained in the stomach unchanged for nearly two days. In the former, although both nerves had been divided, the whole food contained in the stomach, notwithstanding it had lain in it a comparatively short time, was nearly as much changed as in the stomach of a nealthy rabbit.

The membrane of the wind-pipe appeared of a very deep red colour, but there was not much fluid in it. The lungs did not collapse on opening the thorax, the air cells being full of a frothy and bloody serum. The lungs were externally of an uniform dark red colour. The heart was a little increased in size, and highly vascular. Throughout the whole of the body there was a general increase of vascularity. This, as appeared from what was observed in other cases, was the effect of the galvanic influence.

The cause of the peculiar difficulty of breathing in this experiment, and of the croaking noise, was, that the nerves of the upper part of the wind-pipe were injured by being stretched, in order to divide the eighth pair as near to them as possible. The reader will see from what is said above, that difficulty of breathing and a croaking noise are the effects of injuring those nerves. They, it is evident, were out of the galvanic circle.

We learn from the state of the lungs after the division of the eighth pair of nerves, why an animal cannot be long preserved by artificial respiration after the brain is removed. M.

le Gallois, who was perhaps inferior to no physiologist in accuracy of observation, remarks, that artificial respiration produces the same loaded state of the lungs. This is the effect of the nervous power being withdrawn. The artificial respiration, by preserving the life of the animal, only gives time for the effect to take place.

In the foregoing experiments, the galvanic influence had been directed chiefly to the region of the stomach. In order to ascertain the effect of directing it more particularly to the lungs, the author requested Dr. Hastings to make the experiment in the following manner.

Exp. 76. A full-grown rabbit was kept without food for twelve hours. Within two hours of the experiment, it was allowed to eat as much parsley as it chose. The hair being then shaved off the skin of the chest, it was covered with tin-foil, but not so low as the pit of the stomach. Part of the eighth pair of nerves was now removed lower down in the neck than in the last experiment, and the lower remaining portion of both nerves coated with tin-foil for about a quarter of an inch. The nerves of the upper part of the wind-pipe were not disturbed, and no croaking ensued. It was an hour after the operation before the galvanic influence was so applied as to keep up a gentle twitching of the muscles of the chest and fore-legs. This effect of the galvanism was kept up uniformly for five hours, during which time there was hardly any dyspnœa.

In about an hour after this, an uniform effect from the trough could not be kept up, on account of the tin-foil having been torn. The breathing became much worse, and the tin-foil could not again be generally applied to the chest. It was kept imperfectly applied till the death of the animal, which happened in about two hours and a half after this; that is, about nine hours and a half after the division of the nerves. The animal had shewn no tendency to vomit.

The lungs collapsed on opening the chest, though not so perfectly as when they are healthy: they swam in water. The inner membrane of the wind-pipe was redder and more vascular than usual: there was no frothy mucus in it. The air-cells, near the great division of the wind-pipe, were full of phlegm; but on tracing them further, there was very little, and none at all towards the surface of the lungs, which was of a deeper red than natural, but not shewing patches of red, as where the eighth pair of nerves are divided without the application of galvanism. The heart was much more vascular than natural. The same observation applies to every part of the thorax. The thoracic viscera, in short, were rather in a state of inflammation, an effect always produced by a considerable galvanic power in the part to which it is directed, than in that in which they are found after the division of the eighth pair of nerves.

The stomach was larger than natural, and the food but little altered, retaining the colour, smell, and stringiness of the parsley. There was no food in the passage to the stomach. It is evident that the stomach, in this experiment, was but little exposed to the galvanic influence. It was sufficiently so, however, to prevent the vomiting, and to occasion more change in the food than happens when a part is cut out from the nerves without the application of galvanism.

The reader may remark, that as the stomach is here found in a state intermediate between that of this organ, when part of the eighth pair of nerves is removed without the application of galvanism, and when the galvanism is chiefly directed to it; so the state of the lungs in Exp. 74 and 75, is intermediate between what it was in this experiment, and what it is when part of the nerves is removed without the application of this power.

As the foregoing experiments were made on a granivorous animal, and some objections were started to them on this account, a carnivorous animal was chosen for the subject of the following experiment, which the author requested Mr. Sheppard to perform. He may observe that the three preceding experiments were made in the presence of three, and the following experiment in that of four medical men besides himself.

Exp. 77. Two small dogs, of the same size and age, were kept without food for about thirteen hours; they were then permitted to eat as much lean raw mutton as they chose. In both, the eighth pair of nerves were divided immediately after they had taken the mutton. In one of them the nerves were coated with tin-foil, as they had been in the rabbits, a three-shilling piece having been previously bound on the pit of the stomach and lower part of the thorax, after the hair had been shaved off; and galvanism applied as in the foregoing experiments.

The apparatus had been so arranged, that the galvanism was applied as soon as the nerves were divided.

The dog which was not galvanised was almost immediately affected with dyspnœa, and within ten minutes with repeated efforts to vomit. The other, to which the galvanism was applied, of sufficient strength to occasion a very gentle motion in the fore-legs, but not any expression of pain, breathed as free as before the division of the nerves, and never made any effort to vomit. The application of the galvanism was twice discontinued for a few seconds, during which the animal breathed very laboriously, but on renewing the galvanism the breathing immediately became free. This dog lived two hours and a quarter.

On opening the stomach after death, we found the mutton half digested. It had lost its red colour, and was reduced to a soft pulpy substance, in which there was little or no appearance of muscular fibre. That part of the mutton which lay in the pyloric end of the stomach was most digested, a proof that digestion was going on in the usual way. The vessels in some parts of the stomach, and throughout the whole of the small intestines, were highly injected, giving those parts a very florid appearance. The lungs were rather redder than natural, but otherwise quite healthy, collapsing perfectly on the thorax being opened. It is of great consequence, in judging of the effects of galvanism on the lungs, that the galvanic apparatus should be arranged before the commencement of the experiment, that the animal may be subjected to its influence as soon as the nerves are divided, and thus the difficulty of breathing wholly prevented; another proof of this the reader will see in an experiment soon to be related.

The other dog, which was still alive at the end of four hours after the nerves had been divided, was killed at this time. The mutton, although it had been in its stomach so much longer than in the other dog, was as firm as when it was swallowed, and perfectly retained both its red colour and fibrous appearance, except that on the outside the bits seemed as if they had been dipped in boiling water: immediately below the surface they were quite red. The lungs exhibited the same appearances as those of rabbits under the same circumstances. They were so obstructed, that they collapsed very imperfectly, and their surface was covered with patches of a dark red colour.

There was nothing in the stomach of either dog but the mutton, which was taken at the commencement of the experiment, and no part of it had been thrown back into the passage leading to the stomach in either. All present at this and the three preceding experiments examined the state of the stomach and lungs, and expressed their entire satisfaction in the results.

These results were thought so improbable by many, that several gentlemen of the highest reputation as physiologists thought it proper that the experiment should be repeated. On repeating it, the result they obtained was in opposition to that which had occurred to the author, the digestive process not appearing to be promoted by the galvanic influence. This led to a second repetition of the experiment, (an account of which was published in the London Medical and Physical Journal, for May, 1820, Vol. XLIII. p. 385,) by Clarke Abel, M.D. F.R.S., *&c.*

The results obtained by Dr. Abel corresponded with those which had occurred to the author, but naturally failed to convince the gentlemen who had themselves met with a different result; and when the author came to London in 1820, even those who were most averse to experiments on living animals, agreed that it was necessary to set the question at rest by a public repetition of the experiment, by which all who felt any interest in it might have an opportunity of witnessing its different stages, and judging for themselves of the results. It was consequently repeated in 1821, and the President of the Royal Society, who is not more distinguished by the splendour of his discoveries, than eminent for his liberality and love of science, permitted the experiment to be made in the rooms of the Royal Institution, where free access was allowed to every one who desired it.

Among those who witnessed the result were the President of the Royal Society, Mr. Andrew Knight, and other members of the Royal Society, and many distinguished members of the medical profession, all of whom acknowledged the accuracy of the author's statements respecting the effect of galvanism in restoring the digestive power of the stomach.

One of the gentlemen who had assisted at the first repetition of the experiment, Mr. Brodie, whose abilities and whose candour have justly placed him so high in public estimation, and who on this occasion did the author the honour of performing the operative part of the experiment, was the first to acknowledge the accuracy of those statements; and Mr. Broughton, who had publicly expressed his doubts of them, in an account of the experiment published in the ensuing number of the *Journal of Science*, in the handsomest manner acknowledged their accuracy. The results both of this and the seventy-third experiment were laid before the Royal Society, and published in the *Philosophical Transactions* of the following year. The author cannot recur to these circumstances without expressing in the strongest terms his sense of the candour and liberality he experienced from all with whom he had any intercourse on this occasion. To the politeness of Mr. Brande, of the Royal Institution, he was at this time, and has since been much indebted.

In the foregoing experiment the nerves were divided and treated in the same way in two rabbits of the same age, and in all respects prepared as in the preceding experiments, one only being galvanised, that those who witnessed the result might have an opportunity of comparing the contents of the two stomachs.

The apparatus was arranged previous to the commencement of the experiment, and one of the rabbits was subjected to the galvanic power, as soon as its nerves were divided. No dyspnœa whatever, and no effort to vomit occurred in it, while the other was affected with both to a great degree. The galvanic power was uniformly such as to keep up a gentle motion of the fore-legs, but to produce no other sensible effect. Both rabbits were killed in about six hours after the division of the nerves.

With respect to the lungs, those of the non-galvanised rabbit, as in the preceding experiments, were much obstructed, and evidently changed in their structure, with patches of a dark-red colour on their surface. The lungs of that galvanised appeared perfectly healthy, the galvanism not having been continued for a sufficient length of time to inflame them. The experiment having been made with a view to the state of the stomach alone, most of the gentlemen had left the room before that of the lungs was examined, which could not be done without disturbing the contents of the stomachs.

In the first stage of the experiment a difficulty presented itself, which led to a detection of a principal cause of the difference of result which had occurred in the first instance. Before it can be ascertained whether galvanism is capable of restoring to the stomach the power of digestion, it is evidently necessary to deprive it of this power; for which purpose, Mr. Brodie divided the eighth pair of nerves in the neck of a rabbit, by which digestion was deranged, but not arrested in the way in which the author had seen it in his own experiments, the operative part of which had been performed by Dr. Hastings.

On minutely comparing Dr. Hastings and Mr. Brodie's mode of operating, the only difference which the author could detect was, that Mr. Brodie only divided the nerves, while Dr. Hastings had always removed part of them. This circumstance appeared too trifling to occasion the difference of result observed; and when the author, influenced by views which have already been explained, suggested that it was possible that the influence of the nervous system might pass along a divided nerve, he found none to second his opinion, which was regarded as little better than visionary. It was on this occasion that Mr. Cutler, at the author's request, was so good as to perform the experiment which has been related above (73), which, as has been stated, convinced those who witnessed it, that the nervous power had passed along the nerves, although their divided ends had retracted from each other to the distance of a quarter of an inch.

The reader will readily perceive, that in the first repetition of the experiment, the digestion having only been partially disordered by simply dividing the nerves, the effect of the galvanism could not have been so remarkable as in the original experiment.

He has seen that the experiment just referred to was repeated, and its result confirmed by M. Breschet, Dr. Milne Edwards, and M. Vavasseur, of Paris. These physiologists also repeated the other experiments related in this chapter on a scale much more extensive than was attempted in this country, and on a greater variety of animals *. The author considers himself much indebted to them for the ample confirmation they have afforded of the results just laid before the reader, and which will make any future repetition of the experiments unnecessary.

In the experiment performed at the Royal Institution, neither Mr. Brodie's nor Dr. Hastings's method was followed; but, at the author's request, after the division of the nerves, a thread having been passed through the lower portion of each close to the divided end, they were raised out of their place, and their posi-

* *Archives Générales de Médécine, Août*, 1823.

tion secured by the thread being tied round the neck of the animal. This the author judged the most effectual means of preventing the influence of the brain from passing from the upper to the lower portion of the nerve.

Exp. 78. It was suggested, that, by the power of galvanism, the degree of digestion, which takes place after death, might be increased. But we could not cause the galvanism to produce any appearance of inflammation, either in the stomach or bowels, an effect which the powerful continued action of galvanism always produces in the living animal. The absence of inflammation here seems to arise from the galvanism producing little or no increased impetus of the blood in the dead animal. Thus, whatever increase of nervous influence there may be, there can be no increased supply of fluids for it to act upon, without which, it is evident, there can be no increased secretion.

The author has no doubt that the slight degree of digestion, after death, which is prevented by dividing the eighth pair of nerves, might, after the division of these nerves, be restored by galvanism. This is an inference from the experiments which have been laid before the reader. To make the experiment would be very laborious, because, for reasons mentioned above, it must be made on a very large scale ; and the galvanism continued in each instance as long as the motion of the blood in the capillaries continues after death, probably for two or three hours.

Exp. 79. Although it is difficult to ascertain whether galvanism influences the state of the stomach after death, the case is very different with respect to the lungs. In them it is easy, from the degree of patching, with precision to ascertain the state of the secretions. The patches are occasioned, we have seen, by dividing the eighth pair of nerves after death. Here, however, there was no occasion for minute observation, for in nine instances, in which the galvanic influence was sent through the lungs for only about a quarter of an hour after the eighth pair of nerves had been divided immediately after death, all appearance of patching was prevented ; the lungs, after the animal had lain dead about twenty hours, appearing quite sound.

Thus it appears that galvanism is capable of performing the functions of the nervous power both in the stomach and lungs. By depriving these organs of a great part of their nervous power, the reader has seen, the formation of gastric juice is destroyed, and not only the secretions of the lungs disordered, but their structure in the space of a few hours evidently deranged. By means of the galvanic power all these consequences are prevented. The gastric juice continues to be formed, the secretions of the lungs remain healthy, and their structure unimpaired. It remains to be inquired whether galvanism is capable of causing a disengagement of caloric from the blood. To ascertain this point the following experiments were made :

Exp. 80. A cup was placed in water of the temperature of 98° of Fahrenheit's thermometer, which was ascertained to be the temperature of the rabbit on whose blood the experiment was made, by placing the bulb of the thermometer in the rabbit's mouth, and allowing it to remain there for two minutes. The temperature of all the rabbits used in the following experiments was ascertained in the same way. Blood was received into the cup from one of the carotid arteries. The bulb of a small thermometer, raised to 98°, and the wires from the different ends of the galvanic trough above-mentioned, the whole trough being charged, were immersed into it. The blood had been in the cup about two minutes before the whole apparatus was arranged. The same quantity of blood, taken from the same vessel of another rabbit of the same age and temperature, was received into another cup, placed also in water of the temperature of 98°. So far, however, from there being any disengagement of caloric from the effects of the galvanism, the blood in the galvanised cup seemed to cool rather faster than that in the other. The appearance of the blood in the two cups, however, was very different; that in which the wires were immersed assumed a dark venous colour, and most of the coagulum, which had formed more rapidly in this than the other blood, was soon dissolved, the blood again becoming liquid. The blood in the other cup retained the florid colour, and coagulated as usual.

It occurred to the author, that the galvanism in this experiment had, perhaps, been applied too late to produce all its effect on the blood. For we must suppose the changes of this fluid to commence as soon as it leaves the vessels; with the assistance of Dr. Hastings, and another gentleman, therefore, the experiment was repeated in the following manner:

Exp. 81. The rabbits were chosen of the same size and temperature, the thermometer in the mouth of each standing at 98°. The cups were disposed as in the last experiment; the water, in which they stood, being at the temperature of 98°. Into the one cup nothing was put but the thermometer; into the other, the thermometer and the two wires from the different ends of the galvanic trough; the thermometer, raised to 98°, being put into the cups at the moment the blood began to flow. Assistants held the rabbits while Dr. Hastings divided the large arteries of the neck. The author observed the thermometer, and a person, having a watch marking seconds, noted down the changes of the thermometer, as the author mentioned them, and the times at which they took place. The experiment was made, first on the blood of the one rabbit, and then on that of the other; but to save repetition, it is related as if it had been made on both at the same time. The temperature of the mouth is always the same as the temperature of the blood on its first flowing from the vessel. In the cup, in which there was only the thermometer, one minute after the blood began to flow into it, the thermometer stood at 97°, in a quarter of a minute more it stood at 96°, and so on gradually falling; for it is to be observed, that although the cups stood in water of 98°, the air in them was more than ten degrees lower.

In the cup, in which the wires were placed, one on each side of the bulb of the thermometer, one minute after the blood had begun to flow into it, the thermometer stood at 100°, in half a minute more at 102°, in half a minute more at 100°, in a minute more at 99°, in half a minute more at 98°; that is, in three minutes and a half after the blood had begun to flow into the cup. After this, the thermometer gradually fell.

While the above changes of temperature went on, the blood in the galvanised cup began to assume a dark colour about the positive wire. But it appears from the preceding experiment, that the disengagement of caloric was not connected with this change of colour, which took place as quickly where no caloric was disengaged. Besides, the caloric ceased to be disengaged soon after the dark colour appeared about this wire, and the supply of galvanism being continued, the dark colour extended till the whole blood in the cup assumed this colour. Air-bubbles arose around both wires. Around the negative wire they continued to rise in such quantity as to form a considerable accumulation of froth. All these appearances took place equally, whether the wires were immersed in the blood at the moment it flowed from the vessel, or after the time had elapsed, at which they ceased to occasion a disengagement of caloric.

Exp. 82. That the author might be assured that there had been no deception in the first experiment, blood was allowed to flow from the large artery of the neck of a rabbit into a cup placed as above, and after it had remained in the cup only about a minute and a half, during which no change of appearance took place in it, the wires and thermometer were immersed into it. The change of colour, and other phenomena mentioned above, took place exactly as before; but there was no disengagement of caloric; the blood continued gradually to cool.

Exp. 83. In a rabbit, whose temperature was only 96°, both of the large arteries in the neck were divided, and the blood allowed to flow into the cup; a thermometer, raised to 96°, being at the same moment placed in the cup between the galvanic wires. In a quarter of a minute after the blood began to flow the thermometer rose to 98°, in half a minute afterwards to 99°. In a quarter of a minute more it had fallen to 98°, in a quarter of a minute it was still 98°, in half a minute more 97°, in a quarter of a minute more, that is, two minutes after the blood began to flow, it returned to 96°, after this it continued gradually to fall. The low temperature of this animal, and the disengagement of caloric being less than in Experiment 81, probably arose from the same cause.

The author wished to ascertain, whether galvanism occasions a similar disengagement of caloric from venous blood.

Exp. 84. Blood was taken from the arm of a person, whose temperature, as appeared by putting the bulb of a thermometer into the mouth, was 98°. The blood was received into a cup placed in water of the same temperature, into which were put the wires from the galvanic trough. The thermometer, raised to 98°, was put into the cup between the wires, as soon as the blood began to flow into it. It continued gradually to sink, at no moment giving the least indication of the disengagement of caloric. This experiment was repeated in the same way, and with the same result.

Exp. 85. In order to ascertain the effect of galvanism on blood returning from the viscera, the following experiment was made on a cat, whose temperature was 97°. The vena cava was opened, and the blood from it allowed to flow into a cup placed in water rather above 100°. The thermometer raised to the same temperature, and the galvanic wires being placed in the cup while the blood was flowing freely into it. The thermometer indicated no disengagement of caloric. The galvanism produced the same visible effects on the venous as it had done on the arterial blood, except that the colour of the former remained unchanged.

In the seventh volume of the Medico-Chirurgical Transactions, Mr. Henry Earle notices cases of palsy, in which the temperature of the paralytic limb, although the pulse was good, was lower than that of the rest of the body. In the first case which he mentions, he subjected the limb to the influence of electricity, which raised its temperature.

Exp. 86. By the foregoing experiments the idea is suggested that some gaseous fluid probably escapes from arterial blood, soon after it leaves the vessel. To ascertain whether this is the case, a glass of such a shape that the smallest globule of air could be seen in it, was filled with and inverted over mercury. A considerable part of the femoral artery of a large rabbit, whose sensibility had been nearly destroyed by opium, was then exposed and divided under the glass. The blood immediately rose into the glass, and was allowed to remain undisturbed for a quarter of an hour, but no gaseous fluid was disengaged from it. In performing this experiment, if great care be not taken, the hair of the animal and hands of the assistants may convey a little air under the glass, by which we were repeatedly foiled in making the experiment. The artery must not be held deep in the mercury, else the weight of the metal, by compressing it, will prevent the escape of the blood.

The glass, into which the blood was received, rose only about an inch and a half above the surface of the mercury in the basin. Had it risen sufficiently high to take off any considerable part of the pressure of the atmosphere, the experiment, it is evident, would not have been a fair one. What elastic fluids may be disengaged from arterial blood, when that pressure is removed from it, is a different question; it appears from this experiment, that the difference of the effect of galvanism on the blood at the moment it leaves the vessel, and two minutes after it has left it, does not arise from the escape of any gaseous fluid.

It thus appears that galvanism, as it has been found capable of performing the other functions of the nervous power, is capable also of causing a disengagement of caloric from the blood.

Mr. Hunter *, whose penetration and originality are ever conspicuous in the midst of his greatest errors, has shewn that the blood newly drawn from the vessels of a living animal possesses some of the properties peculiar to living matter, and therefore justly regards it as living, that is, living in the same sense in which an egg must be considered as alive, whilst it can resist the natural changes to which inanimate matter is subject, and by the application of caloric alone produce a living animal. The same degree of cold, for example, will neither freeze an egg newly laid, nor blood newly drawn, which will freeze them after they have been exposed to such causes as tend to destroy life. However fre-

* His Treatise on the Animal Economy.

quently an inanimate fluid is frozen and thawed, the same degree of cold is required to freeze it again, but the degree of cold required to freeze blood newly drawn from a living animal is greater than that required to freeze it a second time*. It appears from the preceding experiments that galvanism fails to occasion any disengagement of caloric from the blood in about a minute and a half after it has been drawn from the vessel, although no change which we can detect has taken place in it; yet, at the moment it flows from the vessel, galvanism occasions such a disengagement of this principle as raises the thermometer three or four degrees, provided the blood has not undergone the effects of the nervous power in the course of circulation. From venous blood the reader has seen it occasions no disengagement of caloric. In the venous state, the blood is returning to have those properties renewed which have been exhausted by the operation of the nervous power. Here also we observe the correspondence of the effects of that power and galvanism.

The experiments, which have been laid before the reader in the present chapter, prove two positions; that the nervous power is an inanimate agent, its existence not being essen-

* See the experiments of Mr. Hunter in the work just referred to.

tially connected with the organization of the part in which it resides; and that galvanism is capable of its functions. From the experiments on which the latter position rests, one of two inferences is unavoidable; either galvanism performs the nervous functions properly so called; or, the powers on which these functions depend remaining in the part after it is deprived of the nervous power, galvanism calls them into action. But if they still remain in the part, it is evident that the effect of the nervous power itself is only that of calling them into action. Thus by either inference the identity of the operation of the nervous power and galvanism is established.

The answer to the question, Is any degree of the same effect produced by any other stimulus? will go a great way towards determining which of these inferences is correct. It will be generally admitted, I believe, that this question must be answered in the negative.

Does not every step we take, in a knowledge of the operations of nature, tend to prove their simplicity; to point out from how few principles her boundless variety springs? Many phenomena, formerly supposed to have nothing in common, we are now assured proceed only from different modifications of the same principle. Who now doubts the identity of electricity and magnetism? Shall we in the case before us alone, find the course of nature reversed, and many principles capable of the same effects.

We have been accustomed to regard every thing intimately connected with the operations of life as mysterious, without attempting to ascertain how far this mystery goes, or in what it consists. What is meant by saying that life is mysterious? only that its phenomena cannot be referred to any more general principle. It exists in living bodies alone, and no task can be more hopeless than to search for it elsewhere. Electricity might as reasonably be called a modification of life, as life of electricity. Here, as with respect to the sensorial functions, we must be content merely to observe and arrange the phenomena. But because we cannot class the phenomena of life with those of inanimate powers, shall we not inquire into the manner in which it influences and is influenced by those powers? What stops us? Is not this inquiry as open to experiment as the laws of any other principle; or will experiment, our sure guide in every other branch of knowledge, fail us here?

Were it necessary to argue farther on the subject, the author might refer to various phenomena of electric animals, to the fluids, as well as the solids of the animal body being under the influence of the nervous power; to both being affected in the same way by the extreme application of the nervous power, and electricity *; to the galvanic phenomena exhibited by a pile composed of alternate layers of brain and muscle, and to many other similar facts, some of which the reader will find in the third part of this Inquiry. One conclusive argument is sufficient to establish truth, but there are always many less powerful ones ready to give it their aid.

* The reader has seen that the muscles are deprived of their contractile power by crushing the brain. This experiment suggested to Mr. Andrew Knight, that by killing the smaller animals used at table in this way, we should at the same time that an easy death would be substituted for the present often cruel mode of killing them, always find them tender, the stiffening of the muscles being the cause of the toughness. In a letter the author had the honour to receive from him, he says, that the effect of killing poultry in this way exceeded his expectation, the flesh was not only found perfectly tender, but the tenderness was more perfect, and gave a better flavour to it than keeping for any length of time was found to do.

The Author takes this opportunity of observing that after the greater part of the present edition was printed, a friend informed him that in an old and valuable publication which is now much less consulted than it deserves to be, *The Edinburgh Med. and Phys. Essays*, an experiment is related by the celebrated Dr. Whytt, in which the brain and spinal marrow were slowly destroyed without affecting the action of the heart. This experiment not being mentioned in the works of Whytt, has escaped attention. Had it occurred to him to compare its result with that of suddenly crushing these organs, he would have saved much trouble to M. Le Gallois, and the author of this Inquiry.

If, then, it may be said, the nervous power is a mere inanimate agent, is the nervous system to be regarded as devoid of any peculiar vital powers? The peculiar vital powers of the nervous system consist in those by which it controls the agent it employs. In electric animals we find electricity no longer obeying the laws which it obeys in inanimate nature. Even the will of the animal, without any change that we can observe in the arrangement of the conductors, directs it to any part of the body, and renders its intensity greater or less without our being able to perceive that any of the usual means are employed for this purpose. Precisely in the same way all animals regulate the stimulant by which they excite the muscles of voluntary motion.

ON the nature of the muscular power a few words will be sufficient, but it is necessary, in order to place what will be said in a clearer point of view, to premise some observations on the laws of muscular contractility, which, notwithstanding all that has been written on this subject, appear, as far as the author can judge, to have been observed with less care than the importance of the subject requires.

It appears on the most cursory view of the phenomena of life that they depend on a capacity of action in living parts, and the operation of agents capable of exciting them. The degree of excitement produced is proportioned to the force and continuance of the exciting cause and degree of excitability possessed by the part acted on. By the action of the stimulant, the excitability is impaired, and by its continued action at length exhausted. Thus excitement continues, unless the agent is withdrawn till the part is so far deprived of its excitability, that it will no longer obey the same degree of the same agent. To produce further excitement, a more powerful agent must be applied, or the excitability of the part acted on must be increased.

The excitability is, within certain limits, increased by the abstraction of agents. Thus, for example, sensibility, one species of excitability, is exhausted by the various agents which affect us during the day; and we find by degrees the same agents no longer excite us. If more powerful agents are not applied, we soon fall into a state of insensibility, sleep, during which the operation of the usual agents being withdrawn, we again become sensible to them.

Such are the more evident laws of excitability; and it would be easy, the author thinks, to prove that they are the laws which regulate the cerebral system in a state of health. But it has been maintained that the same laws regulate the excitability of every part of the animal. To this position a very obvious objection occurs.

When the eye becomes wearied with seeing, the ear with hearing, &c., they cease to be excited; their excitability is thus allowed to accumulate, and they are again fitted for their functions: they are not concerned in the preservation of life. An animal may be in perfect vigour, as far as relates to the powers on which his existence depends, although he neither sees nor hears. The vital powers remaining in sleep, restore vigour to the exhausted organs of sense; but were the vital organs themselves subject to similar exhaustion, what, during such intervals, would preserve the life of the animal? and by what powers would the vigour of these organs be restored?

It has been said, that the relaxation of the fibres of the heart arises from the stimulant effect of the blood exhausting their excitability during its contraction, which is restored to it during the interval that elapses between its contraction and the occurrence of that degree of distension by the blood, which again excites it. But a very simple experiment shows the fallacy of this opinion.

If the heart is exhausted by the stimulant effect of the blood, and recovers its excitability during the absence of such a quantity of this fluid as is capable of exciting it, it ought not to recover its excitability if it is prevented from expelling any part of the blood which has excited it; for, as has just been observed, the continued application of the same stimulant which has produced exhaustion, cannot again excite the exhausted part, as no renewal of excitability can take place while the agent which exhausted it is still applied. The retina will never recover its powers under the impression of the same degree of light which exhausted it. We cannot recover from fatigue while the cause of our fatigue still operates. But the alternate contractions and relaxations of the heart still take place, as may be ascertained by experiments on the newly-dead animal, although a ligature be thrown around the aorta, in consequence of which the heart remains uniformly gorged with blood.

If we sprinkle salt on a muscle, it does not occasion permanent contraction followed by exhaustion, but a constant alternation of contraction and relaxation, although the salt is never removed. The state of the muscle, however, in the relaxations which intervene between the contractions, is evidently very different from that in which it is left when the salt can no longer excite any contraction in it.

The foregoing facts prove that the nervous and muscular excitability obey different laws. While the effect of uniform stimulants acting on the former, is permanent excitement, followed by exhaustion, the habit of the muscular fibre under the influence of uniform stimulants, is to act by intervals. This is probably the reason why moderate excitement does not exhaust the muscular fibre, while the nervous system suffers proportional exhaustion from every degree of excitement. The heart continues to act through life, but no degree of excitement can be long sustained by the brain.

Two circumstances appear to be capable of counteracting this habit of the muscular fibre, and producing in it permanent contraction, a peculiarly strong stimulant, and the influence of the will. It is only, however, occasionally and for a limited time, that either the will or the most powerful stimulants have this effect. After a certain time, the natural tendency of the muscle to alternate contraction and relaxation prevails, and the limb which we wish to keep steady begins to tremble ; or if the artificial stimulant prevails, the muscle soon loses its power, the state of permanent contraction under such circumstances, being a morbid one.

There is another species of debility of a very different nature from exhaustion, to which the reader's attention has been directed, which appears to bear no relation to any previous excitement, but to be the direct effect of agents; for while some agents increase, others lessen, the action of the living solid. The former have been called stimulants, the latter, sedatives.

It has been maintained, indeed, that as exhaustion is the effect of moderate excitement, the species of debility we are now considering is always the consequence of excessive excitement; and therefore, that, like exhaustion, the sedative effect is never the direct effect of the agent: and this opinion seems at first view to be countenanced by the fact, that stimulants act as sedatives when applied in excess. Thus a moderate quantity of distilled spirits received into the stomach produces excitement, which, within certain limits, is greater in proportion to the quantity taken; but if a very large quantity be suddenly received into the stomach, it produces no degree of excitement, but immediate debility, or even death. A draught of spirit of wine has immediately proved fatal.

It is surely a strained explanation of the latter effects, to suppose them the consequence of excessive excitement, no symptom of which appears. The supposed existence of this excitement rests wholly on the preconceived opinion, that exhaustion, in consequence of previous excitement, is the only debility which can arise from the operation of agents on the living solid; and it has been maintained, that however imperceptible the excitement produced by large quantities of distilled spirits, for example, we must suppose that their first effect is excitement, and their debilitating effect, consequently, only secondary; and so much has the idea laid hold of the minds of many, that in an account of this Inquiry, lately published in a journal of great respectability, the author's opinion of the nature of inflammation is opposed, on the ground that the operation of agents in producing this disease, must, in the first instance, be stimulant; and the debility of the vessels, which, it is admitted, exists in inflamed parts, the consequence of previous excitement; and this is maintained without questioning the accuracy of his experiments, from which it appears that none of this previous excitement could be perceived with the aid of powerful microscopes. Now the author may surely be allowed to maintain that where no increased action can be perceived, none should be supposed. We must not substitute hypothesis for plain matter of fact.

But if this argument, which is, of all, the most conclusive, were out of the question, there is another, which, as far as he can judge, would be unanswerable, to which even the supporters of the hypothesis in question must listen. It must be admitted, even by them, that if the tendency of different agents to produce debility, arises from the degree of excitement occasioned by their first impression on the living solid, those best calculated to produce excitement should be found capable of the greatest sedative effect; but this is so far from being true, that we find that the agents which produce the greatest degree of this effect, are the worst stimulants. Tobacco, for example, which is one of the most powerful sedatives, cannot, by any management, be made to produce the degree of excitement which arises from opium or distilled spirits.

The sedative, like the stimulant effect, may be communicated to the muscular, through the nervous system. When tobacco is applied to any considerable part, either of the brain or spinal marrow, the heart, as the reader has seen, soon begins to act more languidly, but this languor is preceded by little, if any, increased action, unless the tobacco be applied in a very diluted state; in which case, it produces comparatively little languor, and the excitement is much less than that produced by opium, which is followed by no sensible languor.

If we disregard preconceived opinions, and fix our attention on facts alone, we shall, as far as the author is capable of judging, arrive at the following conclusions. Every agent capable of affecting the living solid, acts both as a stimulant and sedative, according to the degree in which it is applied. Within a certain degree, it is a stimulant; and in proportion as it stimulates, it exhausts the excitability: this, if the power of the stimulant exceed certain limits, being equally true of the interrupted excitement produced in the muscular, as of the permanent excitement produced in the nervous system. Applied in a greater degree, agents no longer produce excitement followed by proportional exhaustion, but direct debility arising from the operation of the agent itself, and wholly unconnected with previous excitement; the stimulant and sedative powers existing in no particular proportion to each other, but in different agents in every possible proportion. The author has just had occasion to mention the comparative effects of tobacco and opium on the heart. The stimulant power of distilled spirits is great, its sedative power small. It must be used in very great quantity to produce the sedative effect: while a small quantity of digitalis produces this effect, and its stimulant power is so slight, that it must be used in very minute quantity to make it perceptible.

These observations apply to agents affecting the mind as well as the body. Grief and fear possess great sedative power; they act as stimulants only when they are present in a comparatively small degree. Joy and anger, on the other hand, are powerful stimulants, and only act as sedatives when in excess. There is no exception, the author believes, to this law. There is no agent which may not be made to produce a stimulant effect by applying it in a very small quantity, and none which does not act as a sedative when applied in excess*.

Such appear to be the more evident laws of muscular contractility. With respect to the nature of the power on which it depends, as the author has already had occasion to observe, a very few words will be sufficient. We can have no hesitation in regarding it as a vital power; it depends wholly on the organization of the muscular fibre, and we find no power in inanimate nature which resembles it.

It is true that an evident analogy exists between the contraction of this fibre, and the contraction and coagulation of certain inanimate substances; but the contraction of the muscular fibre is its function, the effect of its power called into action by an inanimate agent. The reader has seen, that in this effect an inanimate agent always co-operates. Hence its analogy to the effects of inanimate nature. The contraction of the muscular fibre can no more take place without the co-operation of such an agent, than without the muscular power itself, which is quiescent till it is thus excited.

* The foregoing observations on muscular contractility were first published in the *Annals of Philosophy* for 1818.

CHAPTER XIII.

A Review of the Inferences from the preceding Experiments and Observations.

From the various experiments and observations which have been laid before the reader it appears,—

1. That the vessels of circulation possess a power capable of supporting a certain motion of the blood independently of the heart. Exp. 27, 28, 29.

2. That the power both of the heart and vessels of circulation is independent of the brain and spinal marrow. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12.

3. That the nervous power is capable of acting as a stimulant both to the heart and vessels of circulation. Exp. 13, 14, 15, 16, 17, 18, 26, 29, 30, 31.

4. That the nervous power is capable of acting as a sedative both to the heart and vessels of circulation, even to such a degree as to destroy their power. Exp. 15, 16, 17, 18, 19, 20, 23, 24, 25, 29, 30, 31, 32, 33.

5 That the proof of the vessels possessing a principle of motion independent of their elasticity, which bears the same relation to the nervous system with the excitability of the heart; not only as far as respects the kind of

influence which they derive from that system (47,) and the way in which it is supplied to them (46,) but also as far as respects the purposes for which it seems to be bestowed on them (47,) affords a strong argument for believing that this power is of the same nature with that of the heart. See the Experiments related in the two first Chapters of the present part of this Inquiry, also Exp. 43, 48, 49, 58, 59, 60, 61, and Chap. V. and IX.

6. That the excitability of the muscular does not obey the same laws with that of the nervous system; the usual excitement of the muscles of involuntary motion never occasioning exhaustion in them; while all excitement of the nervous system is followed by proportional exhaustion. See the observations on muscular contractility in Chap. XII.

7. That the effect of the sedative is not the result of the excess of stimulant effect, but, like excitement, the direct operation of the agent. See the observations after Exp. 44, and the experiments there referred to, and the observations on muscular contractility in Chap. XII.

8. That the power of the muscles of voluntary motion is independent of the brain and spinal marrow, and that their relation to the nervous system is of the same nature with that of the heart and vessels of circulation, the nervous power influencing them in no other

way than other stimulants and sedatives do. Exp. 34, 35, and the observations under Exp. 35.

9. That the cause of the muscles of voluntary and involuntary motion appearing at first view essentially to differ in their nature, is their being excited by stimulants essentially different, the former being always excited by the nervous power, the latter, though occasionally excited by this power, in all their usual functions obeying other stimulants. See the observations under Exp. 35.

10. That the brain and spinal marrow act, each of them, directly on the heart as well as on the muscles of voluntary motion. Exp. 17, 18, 19, 26.

11. THAT the laws which regulate the effects of stimulants applied to the brain and spinal marrow on the heart and muscles of voluntary motion are different. Exp. 36, 37, 38, 39, 40, 41, 42, 43, 44.

12. That mechanical stimulants, applied to the brain and spinal marrow, are better fitted to excite the muscles of voluntary motion, and chemical stimulants the heart. Exp. 36, 37, 38, 39, 40, 41, 42.

13. That neither mechanical nor chemical stimulants, applied to the brain and spinal marrow, excite the muscles of voluntary motion, unless they are applied near to the origin

of their nerves; and consequently that these muscles are excited by stimulants applied to very minute parts of the brain and spinal marrow. Exp. 36, 38, 40.

14. That both mechanical and chemical stimulants, applied to any considerable part of the brain or spinal marrow, increase the action of the heart, which cannot be increased by any stimulant applied to a minute part of these organs. Exp. 37, 39, 42, 45, 46, 47.

15. That the heart obeys a much less powerful stimulant, applied to the brain and spinal marrow, than the muscles of voluntary motion do. Exp. 38, 39, 40, &c., and observations after Exp. 36.

16. That stimulants, applied to the brain and spinal marrow, excite irregular action in the muscles of voluntary motion. Exp. 36, 38, 41.

17. That no stimulant applied to the brain or spinal marrow, excites irregular action in the heart or vessels of circulation, nor is their action rendered irregular by sedatives, except by a blow, which crushes a considerable part of the brain or spinal marrow. Exp. 32, 43, &c.

18. That the excitement of the muscles of voluntary motion takes place chiefly at the moment at which the stimulant is applied to the brain and spinal marrow, while that of the heart may generally be perceived as long as the stimulant is applied. Exp. 44.

19. That after all stimulants applied to the brain and spinal marrow fail to excite the muscles of voluntary motion, both mechanical and chemical stimulants so applied still excite the heart. Exp. 41.

20. That all the foregoing differences in the effects of stimulants applied to the brain and spinal marrow on the heart and muscles of voluntary motion are referrible to the following law: That the heart is excited by all stimulants applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only excited by intense stimulants applied to certain small parts of these organs. See the observations under Exp. 47.

21. THAT the function of secretion is destroyed by cutting out a part from the nerves of the secreting organ: Exp. 48, 49. Or by dividing them and displacing one of the divided ends. Page 227, *et seq.*

22. That it may be restored after it is thus destroyed by the galvanic influence. Exp. 75, 76, 77, 78.

23. That this function, although deranged, is not destroyed by simply dividing the nerves, even when the divided ends retract to a considerable distance from each other. Exp. 73.

24. That lessening the extent of the nervous system by destroying the influence of any con-

siderable part either of the brain or spinal marrow, deranges the secreting power. Exp. 58, 59, 60, 61.

25. That dividing the spinal marrow, does not derange the secreting power. Exp. 62.

26. That the vessels of secretion only convey the fluids to be operated upon by the nervous power. See Chap. V., and the experiments there referred to.

27. That these vessels, like the vessels of circulation, are independent of, but influenced by, the nervous system. Ib.

28. THAT the motions of the stomach and intestines are independent of the nervous system. Exp. 50, 51.

29. That they are capable of being influenced through this system. See the beginning of Chap. VII.

30. That the food when received into the stomach remains at rest in the central part of this organ, and unmixed with the food previously taken; and that it is changed in proportion as it approaches the surface of the stomach, in consequence of that, previously there, being moved on towards its lower orifice. Exp. 53, 54, 56.

31. That the food is most mixed with the fluids of the stomach, and the greatest change

is effected in it in the large end of the stomach. Exp. 56.

32. That the food is much drier and of a more uniform consistence, in the small than in the large end of the stomach. See Exp. 55, and the observations under it.

33. That the efforts to vomit occasioned by the division of the eighth pair of nerves, arise from fresh food coming into contact with the surface of the stomach, no longer covered with its proper fluids. See the observations near the end of the first Section of Chap. VII. and the experiments there referred to.

34. That the muscular power of the stomach remains after the division of the eighth pair of nerves, by which all that part of the food which has undergone the action of the gastric juice is carried into the intestine, undigested food alone remaining in the stomach. Ib.

35. That the secreting power of the stomach is almost as much deranged by destroying a considerable part of the spinal marrow, as by dividing the eighth pair of nerves. Exp. 58, 59, 60, 61.

36. That a similar observation applies both to the structure and to the secreting power of the lungs. Ib.

37. That the stomach and lungs, like the sanguiferous system, are influenced by every

part of the brain and spinal marrow. Exp. 48, 49, 58, 59, 60, 61.

38. THAT the destruction of any considerable part of the spinal marrow lessens the temperature of the animal. Exp. 58, 59, 60.

39. That the galvanic influence occasions a disengagement of caloric from arterial blood, if it be subjected to this influence as soon as it leaves the vessels. Exp. 80, 81, 82, 83.

40. That the galvanic influence occasions no disengagement of caloric from venous blood, although subjected to this influence as soon as it leaves the vessels. Exp. 84, 85.

41. That there is no evolution of gaseous fluid from arterial blood on its leaving the vessels. Exp. 86.

42. That, if caloric be admitted to be a substance, its disengagement from the blood, being effected by the same means by which the secreted fluids are formed, must be classed with secreting processes. See Chap. VIII.

43. THAT the division of the spinal marrow does not destroy any of the functions of either half of it, (22) the paralysis of the lower part of the body, occasioned by its division, arising from that part having its communication with the principal source of sensorial power destroyed. See Exp. 63, and the observations on the sensorial power in Chap. XI.

44. THAT the ganglions are a secondary centre of nervous power, whose nerves are as extensively distributed as those which proceed from the brain and spinal marrow. See Chap. IX.

45. That through the ganglions, the influence of every part of the brain and spinal marrow is bestowed on the parts, which have been found to be influenced by every part of these organs. See Chap. IX., and the experiments there referred to.

46. That the influence of every part of the brain and spinal marrow is bestowed on all parts directly or indirectly necessary to the due performance of secretion, this function requiring the influence of every part of these organs. (21.) Exp. 48, 49, 58, 59, 61, 62.

47. That the position of the ganglions and the distribution of their nerves tend to confirm the view of their use afforded by the foregoing experiments. See Chap. IX.

48. That we have reason to believe, that the great sympathetic nerve arises from the spinal marrow*. Ibid.

49. THAT the various functions of the animal body may be divided into sensorial, nervous,

* This position does not rest, as M. le Gallois maintains, on the fact, that crushing any considerable part of the spinal marrow destroys the power of the heart, while the removal of the brain leaves it unimpaired; because the reader has

and muscular. See Chap. XI., and the experiments there referred to.

50. That the muscular and nervous functions are the result of inanimate agents acting on vital parts; the sensorial, of vital parts acting on each other. See Chap. XII.

51. That the sensorial power is not wholly confined to the brain, nor the nervous power to the spinal marrow, both powers in a greater or less degree residing in both organs. See Exp. 63, and the observations which precede and follow it.

52. That the muscles both of voluntary and involuntary motion appear to bear the same

seen that crushing the brain in like manner destroys the power of the heart, which remains uninfluenced by the removal of the spinal marrow.

It appears to rest on the following circumstances:—the sympathetic nerve is largest about the middle of the spine, becoming less as it ascends and descends. The reader has seen from direct experiment that the thoracic and abdominal viscera are influenced by every part of the spinal marrow, which can only be through the medium of this nerve, the influence of the brain on the other hand, as appears also from direct experiment, being conveyed to these viscera by the eighth pair of nerves. Bichat found that the effects of dividing the eighth pair of nerves in the neck are not increased by dividing in the same place the great sympathetic nerve, and lastly, the division of the great sympathetic alone in the neck produces little, and sometimes no sensible, inconvenience to the animal, as the author has repeatedly seen ascertained. Before it arrives at the neck it has freely communicated with the eighth pair of nerves. See Chap. IX.

relation to the brain properly so called, and the cerebellum. Exp. 36, 37, 38, 39.

53. That we have no control over the latter set of muscles, because they are incapable of any act of volition, and are excited by stimulants over which the will has no influence. Chap. X.

54. That what we call death is the ceasing of the sensorial power alone, the nervous and muscular powers still continuing. Exp. 64, 65, 66, 67, 68, 69, 70, 71, and the observations preceding and following these experiments.

55. That in the function of respiration the sensorial, nervous, and muscular powers cooperate. See the observations after Exp. 72, and the experiments there referred to.

56. That it is owing to the ceasing of respiration, that the destruction of the sensorial power is followed by that of the nervous and muscular powers. Ibid.

57. That whatever be the cause of death the functions cease in this order, unless the sensorium be so impressed as instantly to destroy all the functions. The power of the muscular system may be instantly destroyed through the nervous, and that of the nervous through the sensorial, system. Ibid.

SUCH are the immediate inferences from the experiments and observations which have been laid before the reader. By comparing them together we arrive at the following conclusions.

The circulation is supported equally by the power of the heart and the blood-vessels; and we have reason to believe that in both, the power which supports it is the muscular power.

The power of the muscles both of voluntary and involuntary motion is independent of the nervous system, and depends on the organization of the muscular fibre itself. Both these sets of muscles are equally capable of being excited by the nervous power, but while this power is the sole stimulant to which the muscles of voluntary motion are subjected, it acts only occasionally on the muscles of involuntary motion, which are excited in all their usual actions by stimulants over which the will has no control, for which reason, and because they are incapable of any act of volition, their motions are involuntary.

When these muscles are excited by the nervous power, it is not applied to them in the same way as to the muscles of voluntary motion, to which it is sent directly from the brain and spinal marrow, each muscle receiving its nervous power from a particular part of these organs; while to the muscles of involuntary motion, it is sent through the great chain of ganglions, each muscle receiving its nervous power from every part of the brain and spinal marrow.

The consequence of which is, that the muscles of voluntary motion are only excited by powerful stimulants applied to those small parts of the brain or spinal marrow from which their nerves originate; while the muscles of involuntary motion are excited by all stimulants applied to any considerable portion of these organs.

The excitement of the muscles of involuntary motion in all their usual functions, occasions no exhaustion in them; the muscular excitability not obeying the same laws with that of the nervous system. Their excitement appears to be rendered independent of the nervous power, because their functions require a more uniform excitement than could have been derived from this source; and they appear to be subjected to the influence of the whole brain and spinal marrow, because they are directly or indirectly subservient to the function of secretion, which requires for its due performance the influence of every part of these organs; for the nervous power is not supplied by the brain alone, the spinal marrow supplying a necessary part of it, and that independently of any operation of the brain on this organ.

In the function of secretion, the sanguiferous system appears only to supply the fluids to be operated upon by the nervous power; and the disengagement of caloric, which supports animal temperature, is also effected by the action of this power on the blood. Thus it is that those vital functions which depend on the secreting power are immediately subject to the nervous system, while those which depend on the muscular power alone are only indirectly subject to it.

The destruction of the nervous power influences the latter set of functions through those of respiration and secretion.

The nervous power is an inanimate agent, being capable of existing independently of the peculiar organization of the brain and nerves. It passes the interval between the divided ends of the nerve, although it is capable of remaining in the nerve when the brain and spinal marrow no longer exist.

We have reason to believe it to be the galvanic influence, collected by the brain and spinal marrow, and sent along the nerves; galvanism being, not only of all artificial means of exciting the muscles, that which seems best adapted to this purpose, but capable of forming the secreted fluids, preserving the healthy structure of the organs, and causing a disengagement of caloric from the blood, after the nervous power is withdrawn.

The nervous power is not more distinct from the muscular, than it is from the sensorial power. We find the first capable of its functions after the last is withdrawn.

The nervous and muscular functions being the effects of inanimate agents acting on vital parts, survive the sensorial functions, which are the effects of vital parts acting on each other.

The only function essential to animal life, in which the sensorial power is concerned, is respiration; and consequently it is by the interruption of this function that the removal of the sensorial power proves fatal, except where the sensorium is so impressed as immediately to destroy all the functions.

THE foregoing conclusions seem to reconcile all the apparent contradictions stated in the first part of this Inquiry.

The heart continues to act for some time after it is removed from the body, and performs its functions in the fœtal state when neither the brain nor spinal marrow has existed; because it has no direct dependence on the nervous system, and is only influenced by the removal of the brain and spinal marrow in the perfect animal, in consequence of the failure of respiration *.

* It is evident from what has been said, that could we perfectly imitate the function of respiration after the removal of the brain and spinal marrow, the heart would soon begin to feel the effects of the general failure of the secreting power; the failure of the secreting power in the lungs, indeed, as

The heart is supplied with nerves, and subject to the influence of the passions, because, although independent of the nervous system, it is capable of being influenced through it.

Thus, when we remove the brain and spinal marrow, the action of the heart is unimpaired, because it is independent of these organs. When we crush them it is enfeebled or destroyed, because it is influenced through them; and the greater the portion destroyed, and the more sudden its destruction, the greater injury the heart sustains. These facts reconcile the apparent contradictions in the experiments of M. le Gallois.

The heart is independent of the will, because it is exposed to the constantly-renewed action of a stimulant, over which the will has no control; and because there is no act of volition which could be performed through the medium of the heart.

The function of the stomach is destroyed by withdrawing the influence of the brain or spinal marrow, while that of the heart is unimpaired; because the function of the heart depends wholly on the muscular power, which has been found in every part of the body independent of the nervous system; while the func-

has already been observed, probably constitutes one of the chief differences between natural and artificial respiration.

tion of the stomach chiefly depends on the secreting power, which has been found everywhere dependent on this system. As far as the function of the stomach is muscular, it also continues after the nervous power is withdrawn: all the food which is so changed as to become the proper stimulant to the muscular fibres of the stomach is still propelled into the intestine. Thus the muscular power of the stomach is indirectly dependent on the nervous system.

The difficulties stated by M. le Gallois respecting the function of respiration, seem to disappear, when it is admitted, that, although the muscular and nervous powers, concerned in this function, are, as M. le Gallois states them to be, independent of the brain, the sensorial power is here necessary to call them into action: and that the lungs, being supplied with nerves from the eighth pair, the sensorial power must, as far as regards them, cease, when that part of the brain, from which these nerves originate, and to which all impressions communicated through the spinal marrow must also be sent, is destroyed. The powers of respiration remain after the destruction of this part, but the sensation which excites the animal to call them into action is gone.

PART III.

On the Application of the foregoing Experiments and Observations to explain the Nature and improve the Treatment of Diseases.

A CONSIDERABLE length of time alone can shew how far the principles, which seem to be established by the experiments which have been laid before the reader in the preceding Inquiry, may tend to improve the knowledge and treatment of diseases. It is the author's intention, in this part of the treatise, to point out in what instances they at present appear to promote these ends. He will first speak of the diseases which arise chiefly from a fault in the sanguiferous, afterwards of those whose cause chiefly exists in the nervous, system. He uses the qualifying words of the preceding sentence, because there is hardly any disease of the sanguiferous, whose symptoms do not in some degree depend on the state of the nervous system; and, on the other hand, in almost all the diseases of the latter, the sanguiferous system is more or less affected. It is evident, however, from a review of the symptoms of these two sets of diseases, that the nervous more amply partakes of the affections of the sanguiferous system, than the sanguiferous of those of the nervous system; the cause of which is evident. The circulation is immediately necessary to the functions of the brain and spinal marrow; but it appears from the experiments which have been related, that those of the heart and blood-vessels may go on for a certain length of time after the former organs have ceased to exist, having only on these organs an indirect dependence through the functions of respiration and secretion *.

Of the diseases of the sanguiferous system there are some in which the force of the circulation is diminished, so that the due supply of blood to the brain fails, producing, according to the degree in which this happens, various symptoms of debility or actual fainting; and others, in which the vessels of this organ are distended with more than their due proportion of blood, either in consequence of the increased action of the heart and large vessels, or of the vessels of the brain being so far weakened that their power of resistance is not in due proportion to the usual impulse of the blood. The two last states produce the same train of symptoms, except that in the former

* See the experiments related in the first and seventh chapters of Part II.

the symptoms of a morbidly-increased impetus of the blood throughout the system are more considerable *. The species of apoplexy depending on these states of the sanguiferous system is very different from that which the author will soon have occasion to speak of, in which the cause originates in the brain itself. These species, it will be found, are frequently combined; but when apoplexy, from distension of the vessels, exists alone, the brain seems to react but little on the sanguiferous system. The reader has seen, that it was found by experiment, that considerable uniform pressure either of the brain or spinal marrow produces little or no effect on the motions of the heart †.

* I shall not here enter on the question how far there can be more blood in the scull at one time than at another; but only observe that, however incompressible the brain, and immoveable the parietes of the head, may be, if the brain is compressed by an increased force of circulation, there must then be more blood, however little, in the scull than when the brain is not compressed; and when we consider how many openings there are in the scull, filled only by soft medullary matter, we may easily perceive why there may exist within this cavity very evident accumulations of blood, without corresponding depletion in some other parts of it, the necessity of which certain anatomists have maintained.

† Exp. 18.

CHAPTER I.

On Sanguineous Apoplexy.

THE only change which, in this species of apoplexy, appears to take place in the action of the heart, is, that it becomes slow and oppressed, as if acting against a stronger opposing force, and consequently with greater effort than usual. This is easily accounted for by the circulation through the lungs becoming more difficult, owing to the muscles of respiration being less readily called into action in proportion as the insensibility increases, and the vessels of the pulmonary system being less powerfully stimulated in proportion as the blood less perfectly undergoes the change effected by the air. But this is not the only change which takes place in the lungs in apoplexy. They soon begin to be clogged with phlegm, which, in protracted cases, more than the lessened action of the muscles of respiration, at length appears to occasion suffocation. This is readily explained by the experiments which have been laid before the reader, from which it appears, that when a considerable portion of the nervous power is withdrawn from the lungs, the fluids destined to form their secretions, being no longer properly changed, accumulate in them till the air-cells and bronchial tubes are so clogged, that their function at length is wholly destroyed *. Now, in those cases of apoplexy, in which the brain is so oppressed that the various organs are deprived of a great part of their nervous power, but not sufficiently oppressed immediately to put a stop to the action of the muscles of respiration, the above change necessarily takes place in the lungs; and as they are of more immediate importance to life than any other organ whose function depends on the nervous system, this derangement of the lungs is here the cause of death. We see the patient to the last endeavouring to breathe through the phlegm which clogs them, and at length produces suffocation.

If such be the immediate cause of death in sanguineous apoplexy, we have reason to believe, from experiments † which have been laid before the reader, that by passing the galvanic influence through the lungs, they may be enabled to perform their functions for a longer time than without this aid, and thus the life of the patient for a certain time preserved. There is an evident limit to this effect of galvanism. It is only while the sensibility con-

* Page 110, &c. † Exper. 74, *et seq.*

tinues such as to induce the patient to expand his chest to a certain extent and with a certain frequency, that advantage can arise from it. Whatever be the supply of nervous power in the lungs, if the air is not admitted with sufficient frequency, the proper changes, it is evident, cannot go on. Galvanism, under these circumstances, must fail to relieve the breathing, having no other effect but that of a stimulant in promoting the sensorial functions *.

On employing galvanism in apoplexy, the author had the satisfaction to see his expectations confirmed. After the rattling breathing had come on, and the patient seemed about to be suffocated, he was at least a dozen times made to breathe with ease, the accumulation of phlegm gradually disappearing on the application of galvanism, by which his life was prolonged. The inspirations, about an hour and a half or two hours before death, becoming very imperfect, and less frequent, the galvanism failed to relieve him. The relief obtained, as may be supposed from what has been said, was always of very short duration, the breathing sometimes becoming oppressed as soon as the galvanism was discontinued. The author directed it never to be applied for

* See the observations on the office of the sensorial power in respiration, in Chap. XI.

more than ten minutes at a time, and no greater power to be employed than what he had found a person in health could bear without inconvenience. It appears, from the experiments which have been laid before the reader, that a long-continued and powerful application of galvanism excites inflammation. Its proper employment in the present case will appear from what the author has had occasion to say of its use in indigestion, in his treatise on this disease.

It is evident, from the foregoing observations, that the use of galvanism is not suggested as a means of cure in apoplexy ; but it will always, the author believes, in the species of this disease which we are considering, by far the most common, prolong the patient's life ; and may thus, under certain circumstances, by giving more time for the use of those means which tend to remove the cause of the disease, be the means of saving it.

Such are the observations which are suggested by the experiments which have been laid before the reader, respecting the action of the heart and the state of the breathing in sanguineous apoplexy. The character of this species of apoplexy seems evidently to arise from the power of the heart being independent of the brain, so that the action of the former seems to be no otherwise affected by the state of the latter, than necessarily arises from impeded respiration *.

With regard to the other symptoms of sanguineous apoplexy, the reader has seen it proved by direct experiment, that the loss of power in the limbs does not here arise from any change having taken place in their muscles, whose power seems equally unimpaired † with the muscles of involuntary motion; but from the nervous power, the only stimulant of the former, being no longer applied. The contents of the bladder and bowels often pass involuntarily; not that any change is produced in their sphincters, but because these being muscles wholly of voluntary motion ‡, although they still retain that degree of contraction which constitutes their state of rest, when the pressure from within increases, as the will no longer influences them, they yield to this pressure.

* See the observations on respiration in Chap. XI.

† See the observations after Exper. 35.

‡ Observations, similar to those which have been made respecting the muscles of respiration, apply to the sphincters; see Chap. X. There is no such muscle, as far as the author is capable of judging, as what physiologists call a mixed muscle, that is, one partaking of the nature both of the voluntary and involuntary muscles. All the muscles referred to this head are muscles of voluntary motion, but the nature of whose function is such that we are frequently obliged to call them into action.

The species of apoplexy which the author has been considering, provided there be no effusion of blood or serum, is the most favourable. By the abstraction of blood the brain is relieved from pressure, and its functions are restored, and continue, unless, as frequently happens, especially where the patient has suffered from previous attacks of the disease, the vessels again yield to the increasing impulse of the blood. We thus often see the symptoms relieved by blood-letting, but soon recur; and they sometimes continue to do so till the powers of the constitution are exhausted.

With respect to the two varieties of this species of apoplexy just alluded to, in which the distension of the vessels has occasioned rupture, or an effusion of serum, they are cases of a more fatal tendency, and are with great difficulty distinguished from those of mere distension. The only diagnostics on which it has appeared to the author we can rely are, that in the two first cases, particularly in the first, the symptoms generally increase more rapidly than in apoplexy from mere distension, and are much less relieved by blood-letting. That form of serous apoplexy, which is the effect of general debility, and takes place with little or no distension of the vessels of the brain, may, for the most part, be distinguished by the habit of the patient, and by the tendency to effusion in other parts. To enter further on these cases, and the distinction between them and the different forms of water of the brain, would be foreign to the present purpose. The author has enumerated them that the reader may the more clearly understand what he means by nervous apoplexy, on which he will soon have occasion to make some observations.

It will be necessary here to say more of another affection of the head which insensibly runs into apoplexy from distension,—inflammation of the brain. In both, we find the vessels of the brain preternaturally distended, and in many cases can detect no other morbid appearance; yet their nature must essentially differ. In the one, the patient often resembles a furious maniac, while insensibility is always the characteristic of the other. This subject leads to the consideration of the most important of all the diseases of the sanguiferous system, perhaps it may be said, of all the diseases to which we are liable.

CHAPTER II.

On Inflammation.

It will be necessary, in order to place what is about to be said of inflammation in a clear point of view, to premise some observations on the powers by which the circulation is maintained.

Much has lately been said of the dilating power of the heart, and what has been called the resilience of the lungs, that is the tendency of the lungs, when distended, to contract. Those powers are not imaginary. Very simple experiments prove their existence ; and we are indebted to Dr. Carson for having particularly called our attention to them, but the inferences which have been drawn from them appear to the author inadmissible.

An experiment made many years ago by Dr. James Johnson *, demonstrates, in a very satisfactory way, the dilating power of the heart. He immersed in water the heart of a turtle newly killed, and could, from the tinge

* Editor of the Medico-Chirurgical Journal, and the well-known author of several valuable works.

given by the blood, observe it continually expelling, during the contraction, the water it had drawn in during the act of dilatation. But if we grasp the heart either of a warm or cold blooded animal, we find that, during the latter, it offers little if any greater resistance to the hand than other muscles in a state of relaxation do. In short, it is quite soft and compressible, till the contraction recurs: from which, it is evident, that its dilating power is very small. It is also to be observed that the dilating power seems chiefly to reside in the ventricle, the auricle being much thinner, and more inclined to collapse. Nor will the dilating power of the heart be much aided by the resilient power of the lungs. This power cannot operate in many of the lower classes of animals, or in the fœtal state in any class; and in the perfect animal of the higher classes, its effect is very inconsiderable. It appears, from an experiment related in the 111th page of Dr. Carson's Treatise, that, even in the bullock, it is not equal to the pressure of a column of water of ten inches. It does not indeed appear, from the experiments of Dr. Carson, how much the effect he observed depends on what he calls the resilience, or merely on the weight of the lungs. It is evident that, as far as this effect goes, whether it depends on the weight or resilience of the lungs, it tends to dilate the heart; the atmosphere, through the medium of the blood, pressing as much on the internal surface of the heart as on that of the lungs.

But, admitting that the dilating power of the heart, and tendency of the lungs to collapse, are sufficient in degree to support the motion of the blood in the veins, another insurmountable and surely most evident objection to this hypothesis, presents itself. What is the effect of the action of an exhausting syringe fitted to a tube filled with a fluid? If the fluid must rise before it can enter the syringe, after the first portion has entered it, either the fluid continues to rise, or the sides of the tube begin to collapse. If the sides of the tube are firm enough to resist a pressure equal to that of the column of fluid to be raised, it rises; if not, the tube collapses. Could the sides of the veins resist a pressure equal to that of the column of fluid to be raised, even if they were not constantly pressed upon by the surrounding parts? Nay, so small is their elastic power, that they collapse by their own weight as soon as emptied. Yet it is through these tubes that a column of blood of many feet is to be raised by suction! Besides, the elastic power of the ventricle can have no operation on the veins, the auricle being interposed between them, and contracting during the dilatation of the ventricle.

What purpose then is served by the dilating power of the ventricle increased by the tendency of the lungs to collapse? It favours the entrance of the blood suddenly propelled into it by the contraction of the auricle; and the degree of dilating power is well proportioned to this office. Without this dilating power, the tendency of the ventricle would be to remain in a state of collapse after the contraction by which the blood is expelled, and part of the power of the auricle would be expended in dilating the ventricle. Here, as in many other instances, both in man and the inferior animals, we see nature saving the muscular, by the substitution of the elastic power. The latter is the simplest, and its action tends little to exhaust it. In like manner the arteries are rendered elastic, to favour the ingress of the blood on the contraction of the ventricle. It is evident that a greater power must have been bestowed on the ventricle had the arteries been wholly inelastic. Their elasticity, by resisting the pressure of the surrounding parts, and thus tending to preserve an uniformity of calibre, facilitates both the ingress and passage of the blood suddenly thrown into them. Had the blood entered by a continued stream, and been carried on merely by the power of the vessels themselves, this elasticity would not only have been of little use, but evidently injurious, as far as it tended to impede the muscular, or by whatever other name we choose to call that power by which the blood is propelled by the vessels. Thus it is that little elastic power is bestowed on the veins, which appear to be unassisted in the propulsion of their contents.

The author cannot help thinking that the muscular power of the heart is almost as much overrated as its elastic power has been, when it is maintained, that the circulation is supported by the former power alone. Have those who maintain this position made any calculation of the degree of resistance to be overcome in driving the blood through two capillary systems * at such a rate, that, in any given time, the same quantity shall be delivered by the veins, which is thrown into the arteries? Have they made any estimate of the strength necessary in the different sets of vessels, and, particularly, in the larger arteries, to sustain a power capable of overcoming this resistance?

* It is to be recollected that, in a large and important part of the body—the liver, the blood is, for the second time, distributed through the capillaries, before it returns to the heart,

Whatever power we may suppose the heart to possess, it cannot be greater than the coats of the vessels will bear without rupture *.

Let us turn from hypothesis to simple matter of fact. If the motion of the blood be maintained by the power of the heart alone, it will, of course, cease when this power is destroyed.

The reader has seen, that if a ligature be thrown round the vessels attached to the heart of a newly-dead frog, and the heart be then cut out, the circulation in the web of the hind legs is still found to be vigorous: it cannot be distinguished from that in the web of a healthy frog.

In like manner when a rabbit, about two months old, was killed in the usual way, the chest laid open, a ligature thrown round the aorta, and part of the mesentery brought before the microscope, the blood was found moving in it with great velocity. The author had many times, with the assistance of the microscope, seen the circulation in the healthy rabbit, and could not perceive that the loss of the

* Much has been said of the incompressible nature of the blood, and the consequent effect of each additional quantity thrown into the arteries; but were the blood absolutely incompressible, which we now know not to be the case, what has been said on this head could only be correct, on the supposition of the vessels also being absolutely unyielding.

power of the heart at all affected it for the space of many minutes.

It appears from these experiments, that the motion of the blood in the capillaries, that is, those vessels which are too small to be distinguished by the naked eye, has no direct dependance on the action of the heart. Does it depend on the power of the larger vessels, or on the power of the capillaries themselves? This point also may easily be determined by direct experiment. If it depend on the former, it will be uninfluenced by stimulants and sedatives acting on the capillaries; if on the latter, the velocity of the blood will be greater or less according as they are more or less excited.

The author found from many experiments, to which he will again have occasion to refer, that the velocity of the blood in the capillaries is immediately influenced by the state of these vessels. When they were stimulated by the concentrated rays of the sun, the application of spirits of wine, or gentle friction, the velocity of the blood in them was immediately, and, by the two first, to a great degree, increased †. When the power of the capillaries was destroyed, even in the perfect animal, by

† Introduction to the author's Treatise on Symptomatic Fevers, 4th edit., pp. 15 and 16.

the direct application of an infusion of opium or tobacco to them, the motion of the blood through them immediately ceased*, so little influence has the action of the heart and larger arteries on the circulation in these vessels and in many experiments, where the motion of the blood in the capillaries had become very languid, it was accelerated, and even renewed, when it had ceased, by stimulants applied to them†.

Thus, it appears, that the propulsion of the blood through the capillaries, is the function of these vessels themselves, and has no further dependance on the heart, than that a due supply of blood to them cannot long be obtained after the action of this organ has ceased. The motion of the blood in the capillaries, the reader has seen, does not wholly cease, in internal parts, for several hours after the power of the heart is destroyed‡.

* Experimental Inquiry into the Effects of Opium and Tobacco on the living Animal by the author, published in 1794.

† Introduction to the author's Treatise on Symptomatic Fevers, 4th edit., pp. 15 and 16. The motion of the blood in the capillaries is quite uniform. The author could never observe that the contractions of the heart in the least degree influenced it.

‡ Exper. 67.

The effect of the larger vessels in supporting the motion of the blood, cannot be so easily demonstrated as that of the capillaries; but the reader will find, from many experiments, detailed in various works, and particularly in the introduction to Dr. Hastings's work on the Inflammation of the Mucus Membrane of the Lungs, that they possess a contractile power, which obeys both chemical and mechanical stimulants, and for what purpose they are endowed with such power, if not for the propulsion of the blood, it would be difficult to understand. Whatever may be said of the larger arteries, as the motion of the blood in the capillaries is independent of the power of the heart, that in the larger veins must either depend on their own power, or on the impulse given to the blood by the capillaries,—a position which nobody will maintain. If then the blood is carried on in the capillaries and larger veins by the power of these vessels themselves, and we find, from direct experiment, that the arteries possess a similar power, we can hardly conceive that this power is not employed for the same purpose, in aid of the impulse given by the heart; and did not these direct proofs exist, it is surely more consistent with every thing we know of the animal economy, to suppose that the vessels should assist in the propulsion of their contents, than that these should be driven through them as through inanimate tubes.

Whether the power of the vessels in man be a muscular power or not, a question of subordinate consequence, must chiefly rest on analogy; but there can hardly be a stronger analogy than exists in favour of this opinion, to say nothing of the fibrous appearance observed in some of the larger vessels, and their evident muscularity in some animals. The power of the capillaries obeys the same laws with that of the heart. The capillaries and the heart are excited by the same stimulant, and for the same purpose —the propulsion of the blood. These vessels are affected in the same way as the heart, by other agents, directly applied to them, whether stimulant or sedative *. They bear the same relation to the nervous system, their function being like that of the heart independent of that system †, but capable of being increased or diminished, or even destroyed through it ‡.

The author will not here enter on the nature of the arterial pulse, which has been so fully and ably considered by Dr. Parry, but only observe, that the beating of the arte-

* Treatise on Symptomatic Fevers, 4th edit., pp. 15 and 16, and Experimental Inquiry, 2d edit., p. 133.

† Exper. 11, *et seq.*

‡ Exper. 29, *et seq.*

ries mentioned in the account of the preceding experiments certainly did not wholly, and perhaps not chiefly depend, on the varying diameters of the artery; but very much the author believes on a certain momentary change of place, the consequence of the impetus given to the blood in the large arteries by each contraction of the heart. The author will only add, that, on comparing the whole of the facts, he thinks there is good grounds for believing that the impulse given to the blood, by each contraction of the heart, does for the moment increase the diameters of the larger arteries, although probably not to so great a degree as has been supposed. Dr. Parry's mode of measurement does not appear to have been sufficiently delicate. Dr. Hastings observed, that a ligature placed round the artery was tightened on each contraction of the heart.

The direct influence of the nervous system on the capillaries, explains many of the phenomena both of health and disease. Hence the flushing and paleness of particular parts, especially of the face, produced by affections of the mind, which in many instances cannot depend on the general state of the sanguiferous system. Hence the influence of different states of the nervous system on the various secreting organs, &c.

There is no disease in which the influenee of the nervous system on the capillary vessels is so striking as in Inflammation ; we see it made to recede from one part, and attack another, and modified in all its stages by causes whose operation is confined to the nervous system.

Such are the observations respecting the powers of circulation which the author judged it proper to premise. He will now endeavour to ascertain how far a review of the facts we possess enables us to advance in explaining the phenomena of this disease; which, both from the nature of the disease itself, and its intimate connexion with nine-tenths of all the diseases we are subject to, may be regarded as the most important object of pathological research: and yet it is that on which, as far as he is capable of judging, there has been a greater display of fallacious reasoning, and a greater disregard of facts, than on any other subject with which he is acquainted.

There is no difficulty, with the aid of the microscope, in perceiving the first step towards a state of inflammation. It is well known, that exposure to the air alone, is sufficient to produce inflammation in the internal membranes of warm-blooded animals. This is also the case in the fin of some kinds of fish. The lampern was the fish the author employed, and in the warm-blooded animal he employed the mesentery of the rabbit.

On bringing either of these membranes before the microscope, we see a network of vessels, many capable of transmitting the globules of blood only one by one where they follow each other in rapid succession. After the part has remained exposed to the air for some time, the globules begin to move through these vessels with less rapidity, and in proportion as this happens, we perceive the diameter of the vessels enlarging, till that which could admit of only one globule now admits of several. As the motion of the globules languishes, and their number increases, their colour becomes conspicuous, which it is not while they pass in smaller number and with greater rapidity. At the same time that these changes take place, we find the number of vessels, capable of transmitting red globules, greatly increased, so that the vessels which, in the healthy state, transmitted only the colourless, are now so much distended as to admit the grosser parts of the blood. From these two causes the part assumes a redder appearance than natural, and also acquires a greater bulk ; and the latter seems further increased by the distension of vessels still too small to transmit the red globules ; for the interstices of the red vessels are now more opaque than before the morbid distension took place, without the appearance of extravasation of any kind.

While these changes, which may be distinctly seen with the assistance of the microscope, are going on, the part to the naked eye becomes inflamed, more opaque, and thicker.

Such then are the changes which take place in the commencement and progress of inflammation. The blood in the capillaries begins to move more slowly; these vessels in the same proportion suffering a degree of morbid distension: and this often goes on till they, by many times, exceed the healthy diameter, and the blood in the most distended vessels ceases to move altogether.

The motion of the blood in the capillaries the reader has seen proved, by direct experiment, to depend on the action of these vessels themselves. When it fails, therefore, we necessarily infer that their action is failing in the same proportion; and this inference is confirmed by their suffering themselves to be morbidly distended by the usual impulse of the blood,—an effect which equally proves their loss of power. It signifies not by what means the power of the capillaries is impaired*, whether by mechanical or chemical injury, whether by a cause operating slowly or suddenly. Any

* The means employed were various, but the effects always the same.

cause impairing their power produces the same effects.

During the foregoing changes, the larger vessels of the part, which are too opaque to permit the motion of the blood to be seen in them, suffer no change that can be detected by the microscope, except that, after the distension of the capillaries has become very great, the vessels immediately preceding them in the course of circulation begin to partake of the distension. Thus when the fins of the lampern were first exposed to the air, the inflammation assumed the appearance of a slight blush, in which it was difficult, with the naked eye, to discover any vessels ; but, after some time, vessels of a considerable size were seen creeping through the inflamed parts. Before this change is observed in the larger vessels, the capillaries are distended to many times their natural size, and the blood in those most distended, has, generally, ceased to move. This, it is evident, cannot go very far, without the latter vessels wholly losing their vitality, and mortification ensuing.

The state of the larger vessels of an inflamed part, with the exception just mentioned, is very different from that of the capillaries, and may be ascertained without the aid of the microscope. The increased pulsation of the larger arteries supplying an inflamed part, sufficiently evinces their increased action : nor is there any difficulty in detecting this increased action. The author has often, in inflammatory affections of the jaws, applied the finger to the external maxillary artery, both where it passes over the bone, and after it assumes the name of labialis, and, in rheumatic affections of the head, to the temporal arteries, and perceived them beating with unusual force. On this increased action of the larger arteries of an inflamed part, the throbbing and general appearance of activity in the part depends, and on it is founded the popular opinion that inflammation consists in an increased action of all the vessels of the inflamed part, an opinion adopted without a moment's reflection on what must necessarily be the consequence of such an increased action. The reader will presently see the effect of this generally-increased action, and its consequences, exhibited by a very simple experiment. The difference between what is called active and passive inflammation, depends on the degree in which the arteries supplying the blood to the debilitated vessels, are excited.

We should, at first view, be inclined to ascribe the increased action of the larger arteries to the impediment opposed to the free transmission of the blood through the debilitated capillaries ; but the following facts point out that it depends little, if at all, on this cause. The communications of the vessels are so numerous and free, that, as the reader will presently see determined by direct experiment, if the passage of the blood is opposed through one channel, it immediately finds another, without occasioning any apparent change in the state of the vessels concerned. The degree in which the larger vessels are excited is rather proportioned to the nervous irritation occasioned by the state of the distended capillaries, than to the degree and extent of the inflammation; for a slight internal inflammation excites the whole sanguiferous system, while a more severe external one has little of this effect; and in habitual inflammation, when the vessels have yielded slowly, and, consequently, without much nervous irritation, there is comparatively little increased excitement of the larger vessels of the part, and often, even in internal parts, none at all of the whole system. From these observations it would appear, that it is to the nervous irritation occasioned by the morbid distension of the capillaries, that we are to ascribe the increased action of the larger arteries of the part. The reader has seen how immediately the action of the vessels is under the influence of the nervous system. The final cause of this increased action is evidently to support the circulation in the debilitated vessels, and excite them to a more vigorous action *.

* See the Introduction to the above-mentioned Treatise on Symptomatic Fevers, 4th edit., pp. 24 and 25.

If the inflammation depend on a debilitated state of the capillaries alone, it follows, that whatever increases the action of these vessels, should relieve the inflammatory symptoms. This may be regarded as an *experimentum crucis* on the subject, for if exciting the capillaries of an inflamed part does not relieve the symptoms, whatever share the debility of these vessels may have in producing the disease, the co-operation of some other cause must be necessary. If, on the contrary, we find that as, on the one hand, whatever debilitates the capillaries, produces inflammation, so, on the other, whatever gives greater activity to them, relieves it, nothing more is required to prove that on their debility the disease depends.

The author wetted the inflamed web of a frog's foot with distilled spirits, at the same time throwing upon it the concentrated rays of the sun, from the reflector of the microscope. The blood in all the vessels, except in those of the most inflamed part, began to move with greater velocity, and, in proportion as this happened, their diameters were diminished, their interstices became less opaque, and the redness of the part was lessened. This experiment was repeated on the lampern, with the same result. By gentle friction, and applying distilled spirits, the motion of the blood in the inflamed part was repeatedly accelerated, and in proportion as this happened the vessels became paler, the deeper red returning as the circulation again became more languid.

Dr. Hastings, in repeating the author's experiments *, in like manner, excited the inflamed capillaries in a frog's foot, by oil of turpentine, and observed the inflammatory symptoms abate in proportion as the capillary vessels lost their increased size, and the motion of the blood was accelerated in them ; and in one instance, of which he gives an account in the 90th page, this process was continued till the inflammation wholly subsided. Excessive heat and cold, in Dr. Hastings's experiments, produced languid motion of the blood, and dilatation of the capillary vessels, exactly in the same proportion as the part became inflamed. When the inflammation was caused by cold, he saw it cured by a moderate and continued application of heat, by which the motion of the blood in these vessels was accelerated, and they were made to resume their natural dimensions. When the inflammation arose from the excessive application of heat, cold produced the same effects. These facts, while they, in a striking manner, confirm the result of the experiments just related, illustrate some of the positions respecting muscular contractility, which have been laid before the

* See the Introduction to Dr. Hastings's Treatise of the Inflammation of the Mucous Membrane of the Lungs.

reader, shewing that both cold and heat, the temperature of the body being the mean, like all other agents, act on it, either as a stimulant or sedative, according to the degree in which they are applied. In a certain degree he has seen all act as a stimulant, in a greater degree as a sedative; the difference between what is called a stimulant and sedative consisting, in the former, distilled spirits or heat for example, being more inclined to act as a stimulant, and the latter, tobacco and cold for example, as a sedative: but there is a quantity of tobacco, and a degree of cold, so small as to act as a stimulant, and a quantity of distilled spirits and a degree of heat, so great as to act as a sedative.

It is evident that the blood cannot be long retained in the debilitated capillaries, and thus, as it were, thrown out of the circulation, without some morbid changes taking place in it. Its vitality must soon cease after its motion is wholly suspended, and the changes, to which dead blood is liable, begin to take place in it. Dr. Hastings observed, that when the debilitated capillaries were stimulated, the blood which passed from them, often contained irregular flocculi, instead of globules, which he compares to the ragged portions separated from the coagulum of arterial blood *.

* Dr. Hastings's Treatise, p. 97.

For the manner in which the various symptoms of inflammation, and means of cure, support the view of the disease, afforded by these experiments, the reader is referred to the Introduction to the author's Treatise on Symptomatic Fevers above mentioned.

Nothing can be more simple than the *modus operandi* of the means of cure in inflammation according to that view of it. All the local measures are such as either relieve the vessels from part of the fluid which distends them beyond their natural capacity, or more directly excite them to a more vigorous action. All the general means are such as influence the impulse of the blood, either reducing it where it is so powerful as still further to distend the debilitated vessels, or increasing it, when it becomes too languid to afford the aid necessary for supporting some motion of the blood in these vessels, and thus preventing gangrene, the effect of its total stoppage.

It appeared to the author that it would tend to throw additional light on what has been said, to subject to the test of direct experiment the principal opinions which prevailed respecting the nature of inflammation previous to that which referred it to a debility of the capillary vessels *. Four only deserve attention: the

* For the origin of this opinion, see Dr. Hastings's work, and the author's Treatise on Symptomatic Fevers. Mr. John

opinion which supposes this disease to arise from a morbid lentor of the blood clogging the minute vessels; that which ascribes it to what has been termed *error loci,* the grosser parts of the blood getting into vessels too small to transmit them; that which supposes a spasm of the extreme vessels to be its cause; and, lastly, that which refers it to a morbidly-increased action of the vessels of the inflamed part.

The reader will readily perceive that the principle of the three first doctrines is the same. In all, obstruction in some of the minute vessels is regarded as the cause of inflammation. It is surprising, therefore, that none of the supporters of these opinions thought of trying whether or not obstruction is capable of producing it. Admitting that the vessels are obstructed, it does not follow that an accumulation of blood will take place in the part. The blood may pass off by communicating branches, or the vessels may resist the distending force.

Allen, a gentleman whose abilities are well known to the scientific public, and Dr. Lubbock, first brought it forward in a connected form in the Medical Society of Edinburgh about the year 1790; and although imperfect traces of it, which often contradict each other, may be found of an earlier date, they appear to have been unknown to them, and are not such as can deprive them of the merit of having been the first to give a distinct and connected explanation of the phenomena of the most important disease to which we are liable.

A small hot wire was suddenly passed through the web of a frog's foot, by which the skin about the hole was shrivelled, and the vessels obstructed, no fluid of any kind being discharged. Here an obstruction was produced surely more than equal to what takes place in many inflammations of small extent, and yet no symptom of inflammation ensued, every part of the web remaining as pale as before the experiment *.

In order to ascertain whether inflammation arises from an increased action of the vessels of the part, it is only necessary to induce such an action, and observe its effects. Having brought the web of a frog's foot before the microscope, the author now and then, during some minutes, observed the velocity of the blood, which continued, as far as he could judge, the same. The foot was then wetted with distilled spirits, and, in a few seconds, the blood in all the vessels was moved with a greatly-increased velocity, which, as the web was constantly kept wet with the distilled spirits, continued, as long as he observed it, ten minutes or a quarter of an hour. But during no part of the time could he perceive the slightest symptom of inflammation, either with or without the microscope. The vessels, instead of appearing redder, and more turgid,

* Introduction to a Treatise on Symptomatic Fevers.

were evidently paler and smaller than before the application of the distilled spirits. The velocity of the circulation was further increased by throwing on the web, the concentrated rays of the sun, from the reflector of the microscope, but still with the same effects *.

The result of the foregoing experiments has since been confirmed by many made by Dr. Hastings, of which an account is given in his Treatise on Inflammation of the Mucous Membrane of the Lungs, to which the author has several times had occasion to refer.

The author, in the preface to his Treatise on simple Fever, has in a cursory way pointed out the manner in which the experiments made with a view to ascertain the nature of inflammation seem to throw some light on that of fever, which appears to be only a state of general inflammation, the whole of the capillaries being debilitated, and the whole of the other parts concerned in supporting the circulation morbidly excited in the first stage ; and the second stage being the necessary effect of this morbid excitement when it has failed to restore activity to the debilitated capillaries.

Here the capillaries are less distended than in inflammation, the impulse of the blood distending them less in proportion as they are

* Introduction to a Treatise on Symptomatic Fevers.

more numerous. Hence the symptoms arising from this distension are comparatively slight: but the general fulness and increased redness and heat of the surfaces, and the general failure of the secreting power, sufficiently indicate its presence, which is confirmed by all the plans of treatment which have been found successful.

When the larger vessels of a part are debilitated and consequently distended without previous distension of the capillaries; the disease, which may be termed congestion or partial plethora, is of a nature very different from inflammation. In this case little or no distension of the capillaries takes place, as appears from their being pale or only slightly redder than natural. The impulse of the blood, from the debilitated state of the larger vessels, being too weak greatly to distend them, they more or less perfectly retain their power, and as long as the larger vessels can afford any supply of blood, preserve the circulation. The reader has seen that they can support the motion of the blood, both in the warm and cold-blooded animal, long after the effect of the powers of the larger vessels has ceased. Such appears to be the state of the vessels of the brain in sanguineous apoplexy, while in inflammation the larger vessels are comparatively little distended, the distension being chiefly in the capillaries. This difference is evident on dissection. After the latter disease, when it has been distinctly formed, a general blush is observed in the parts of the brain affected; while, after the former, a preternatural distension of the larger vessels is conspicuous, while the brain itself is often nearly or altogether of the natural colour. It is an observation of writers on inflammation of the brain, that if stupor supervene on delirium in this disease, it is almost always fatal. The cause of which is evident from what has been said. If, while the capillaries are debilitated, the larger vessels to a considerable degree also lose their power, the circulation in the former must wholly fail.

In other parts, as well as in the brain, we constantly observe, that the distension of the capillaries is attended with acute symptoms, great pain and fever, while that of the larger vessels is generally attended with little of either, being chiefly denoted by a failure in the function of the part affected. The cause of this difference appears from those experiments which prove that the sanguiferous and nervous systems sympathize in their extreme parts in a way they are not found to do in any other*; which we have reason to believe arises from the capillaries supplying to the nervous power the fluids on which it operates in the function

* Exp. 48 and 49, and page 113.

of secretion*, the failure of which must necessarily occasion a degree of derangement in the nervous system, which cannot arise to the same degree from causes chiefly affecting the larger vessels; for however debilitated these vessels may be, unless the circulation in them fail altogether, in which case the death of the part soon ensues, the capillaries, as appears from what has just been said, are still capable of affording a certain supply of fluids to the secreting power.

It has long been observed by physicians, that inflammation of the same organ sometimes excites acute pain and a great degree of fever, and in other cases comparatively little of these symptoms, being chiefly remarkable by the lesion of function it occasions. And this difference has been supposed to depend on the inflammation having its seat sometimes in the membranes and sometimes in the substance of the organ. Thus inflammation of the brain has been divided into two species—phrenitis and phrenismus; namely, inflammation of the membranes and that of the substance of this organ; the latter differing in no essential respect from sanguineous apoplexy—that of the lungs into pleurisy, inflammation of their membranes, and peripneumony of their substance, &c., and the difference of the symptoms in such cases has

* Chap. 5.

been supposed to depend on the nature of the parts affected. Numerous dissections, however, have now proved the fallacy of this explanation. The substance of the organ alone having often been found affected in the most acute, and the membranes alone in the least acute cases *. In

* If the reader will consult the 20th Epistle of Morgagni *De Sedibus et Causis Morborum*, particularly the 9th, 33d, 35th, 39th, 41st, 43d, 47th, 49th and 62d sections of it, and some parts of his 21st Epistle, he will find that the symptoms regarded as peculiar to pleurisy have frequently attended inflammation of the substance of the lungs, and that, when the membranes were not at all affected. When we inspect the bodies of those who die of inflammation of the lungs (says Schroeder Opusc. Med.) they alone are sometimes found inflamed, although the symptoms of pleurisy had been well marked. Petrus Servius opened three hundred people at Rome, who died with the symptoms of pleurisy, in which the lungs were greatly inflamed, the membranes little or not at all. Tissot met with similar cases; and Diemerbroech says, that in two or three cases, in which there had been no acute pain, and where consequently, according to the common opinion, the substance of the lungs alone should have been found affected, the membranes equally partook of the disease. Burserius observes, that dissections are not wanting to prove that inflammation of the membranes has been present without any pain. Sydenham seems to go so far as to believe the substance of the lungs to be very frequently the seat of pleurisy. And Juncker, in his Conspectus Pathologiæ, observes that pleurisy often passes into peripneumony, by which we may understand that the substance of the lungs was found inflamed where the symptoms had been those of pleurisy; for such was the prepossession in favour of this division of inflam-

the former the author believes the capillaries, in the latter the larger vessels are often the chief seat of the disease. He is aware that this will not always be found to be the case, for the capillaries sometimes suffer distension with little or no pain, particularly where the progress of the disease is slow. In general, however, in proportion as the distension is confined to the larger vessels there is less fever and less pain, and when they alone are affected there is little or none of either.

All local diseases producing fever, seem to consist in debility of the capillary vessels of the part affected. Dr. Cullen arranges them all under three heads, Inflammation, Hemorrhagy, and serous discharge. If we examine the symptoms of the two last we shall find, that except these diseases are of a mere passive nature, arising from external violence or extreme relaxation, in which cases they do not

mation of the chest, that when it was found that the appearances on dissection did not correspond with it, it was supposed that the one form of the disease had passed into the other, an opinion which seems to have been sanctioned even by Haller. Yet we find in some of the oldest writers more correct observations. Hippocrates speaks of pleurisy and peripneumony as affections of nearly, if not altogether, the same parts; and Galen observes that the pain in peripneumony is sometimes acute. Many observations to the same effect might be added from authors of the first authority, both with respect to this disease and other inflammatory affections.

excite fever, their symptoms are those of inflammation more or less relieved by discharges, in the one case, the effect of rupture of the vessels, in the other, apparently of distension of their extremities; and it is particularly to be remarked, that it is only in proportion as the symptoms of inflammation prevail, that those of fever attend. It seems then from direct experiment to be a law of the animal economy, that debility and consequent distension of the capillary vessels, and this alone of all local affections, applies to the nervous system, such an irritation as excites to preternatural action the larger vessels of the part, and when of great extent or in vital parts, the whole sanguiferous system.

The intelligent reader will readily perceive how much these observations tend to strengthen the opinion of the nature of simple fever which has just been suggested.

The attention of Mr. Andrew Knight has been peculiarly attracted by the galvanic experiments which have been laid before the reader; and the strong analogy which subsists between animal and vegetable life, in which his brilliant discoveries have obtained for him a name which will last as long as science is respected, has induced him to reflect much on their results. He has favoured the author with many ingenious suggestions relating chiefly to vegetable life. One relating to the subject before us, the author cannot avoid mentioning, although he has not yet attempted to profit by it; the use of galvanism in the worst cases of typhus, in which there is an universal failure of the secreting power and the debility of the nervous system forms so prominent a feature. It may certainly be used with safety, and probably with advantage in this disease. The proper mode of using it, the author conceives to be, by many wires from the positive end of the trough applied to various parts of the head and spine, and many from the negative end to such parts of the surface as shall send the influence through the body as much as possible in the direction of the nerves. Many of the observations which he has had occasion to make in his Treatise on Indigestion on the use of galvanism in this disease and habitual asthma, and which have been confirmed by repeated trials, will probably be found applicable to this and other cases in which it may be employed.

Having taken a cursory view of the nature of the diseases which arise from morbid distension, and consequent failure of circulation in the capillaries and larger vessels of the brain, he is now to consider the effects of a deranged state of this organ itself, and how far the experiments, which have been laid before the reader, throw light on the symptoms arising from this cause.

CHAPTER III.

On Nervous Apoplexy.

The means of distinguishing that species of apoplexy which depends on the state of the circulation in the head, from that which depends on the state of the brain itself, and the proper treatment of the latter are still among the desiderata of medicine. The object of the author in the present chapter is to ascertain how far the preceding experiments throw light on those subjects.

As it appears, as far as the author is capable of judging, from what has been said, that the leading features of sanguineous apoplexy depend on the fact, that the power of the heart and blood-vessels is independent of the nervous system, in consequence of which that of the brain may be overwhelmed by a compressing force without directly affecting the powers of circulation *; so he thinks it will appear from what he is about to say, that the leading features of nervous apoplexy depend on the fact, that the power of the heart and blood-vessels, though independent of the nervous

* Exp. 18.

system, may be influenced even to its total destruction through this system *.

He will in the first place consider the consequence of such an impression made on the nervous system as greatly lessens the power of the heart and blood-vessels †. If the organization of the brain be suddenly deranged, the reader has seen, instant debility, and if the cause be powerful, a speedy destruction, of all the functions ensue ‡. In proportion as the cause is less violent, a longer period intervenes between the debility occasioned by the first impression of the offending cause, and the ceasing of the functions of life ; and when the cause is still slighter, the functions, instead of ceasing, gradually regain their healthy state. Whatever be the result, on the first impression of the offending cause, the sensibility is impaired ; the heart acts more frequently and feebly, and often irregularly ; and the circulating system suffers a similar loss of power in every part of the body. The sphincters of the rectum and bladder do not merely cease to be excited by any voluntary effort as in sanguineous apoplexy, but from the impaired excitability of the

* Compare the Experiments related in Chap. I. of Part II., with Exp. 19, 20, 21, 23, 25, 29, 30, 31, 32.

† See the Experiments related in the second Chapter of Part II.

‡ Exp. 20, 21.

muscular system, the power on which the degree of contraction constituting their state of rest depends, is more or less impaired.

This state is, if the offending cause has not been extreme, succeeded by some improvement in the symptoms, the heart and blood-vessels in some degree recover from the shock they received *. The former begins to beat with less frequency, and with more force and regularity, and the latter to convey the blood with greater velocity, and in a more uniform stream †. In proportion as this change takes place, the various functions, as the author has very frequently observed in animals, improve; but if the offending cause has been severe, the heart soon begins again to beat more languidly, and with it all the functions gradually fail. If the injury done to the nervous system be of such a nature as particularly to debilitate the vessels of the injured part, during that interval, in which the vigour of the circulation is in some degree restored, the vessels of this part yielding to the increased impulse of the blood, the symptoms of inflammation are thus added to those arising from the original injury.

Such appear from the result of the experiments detailed in the preceding Inquiry, to be he consequences of an injury of the brain or

* Exp. 19, 29. † Exp. 19, 29.

spinal marrow, capable of suddenly and to a considerable degree, deranging their organization. The reader will perceive that if the view of the subject just taken be correct, the nervous is a much more complicated disease than the sanguineous apoplexy. In the latter, the powers of the nervous system are impaired, but those of the sanguiferous system are, in the commencement of the disease, entire, and only become affected through the failure of the functions of respiration and secretion. In nervous apoplexy, not only the powers of circulation suffer directly from the injury done to the nervous system, thus producing a combination of diseased states of both systems, but the debility of the heart and blood-vessels have a secondary effect on the nervous system. The action of the brain and spinal marrow fail from defective circulation, and a state of these organs, analogous to what takes place in fainting, is superadded to that produced by the cause of the disease. It is not surprising, therefore, that this species of apoplexy sometimes proves instantly fatal; which sanguineous apoplexy, affecting the powers of circulation, only through the failure of other functions, cannot do, except it exists in such a degree as to produce instantaneous and total insensibility, which seldom if ever happens.

The principles of the treatment in the former case also, are much more complicated. In sanguineous apoplexy, we have but one object in view, to relieve the brain from pressure. In nervous apoplexy, while we endeavour to counteract the effects of the offending cause on the brain, it is necessary to support the circulation; the failure of which, to a certain degree, must immediately prove fatal. This ought to be done, however, in such a way as tends least to occasion morbid distension of the vessels of the head, to which the cause of the disease often renders them particularly liable; and which will produce either sanguineous apoplexy or inflammation of the brain, according as the distension takes place in the larger or smaller vessels. From this view of the subject we may readily understand why abstraction of blood often proves fatal in nervous apoplexy, and yet much of the stimulant effect cannot be borne.

The simplest cases of nervous apoplexy, and those most nearly approaching to the state of the animals in the experiments just referred to *, are cases from mechanical injury of the brain. When a blow on the head fractures the skull, and occasions part of the bone to press on the brain without doing further injury to this organ, the case resembles in its nature the

* Exp. 19, &c.

sanguineous apoplexy. When the compressing power is removed, the apoplectic symptoms disappear; but when the blow has produced what surgeons call concussion of the brain, the case is only a slighter degree of the state in which the rabbits and frogs were found after the brain had been crushed.

No writer, perhaps, has detailed the symptoms of concussion of the brain with greater correctness than Mr. Abernethy, in the third part of his Surgical and Physiological Essays. It is impossible not to remark how accurately his account of these symptoms corresponds with the results of the experiments which have been laid before the reader:—"The whole "train of symptoms," he observes, "following "a concussion of the brain, may, I think, be "properly divided into three stages. The "first is, that state of insensibility and de-"rangement of the bodily powers which im-"mediately succeeds the accident. While it "lasts, the patient scarcely feels any injury "that may be inflicted on him; his breathing "is difficult, but in general without stertor, "his pulse intermitting, and his extremities "cold. But such a state cannot last long; it "goes off gradually, and is succeeded by an-"other, which I consider as the second stage of "concussion. In this, the pulse and respira-"ration become better, and, though not regu-

" larly performed, are sufficient to maintain " life, and to diffuse warmth over the extreme " parts of the body. The feeling of the pa- " tient is now so far restored, that he is sen- " sible if his skin is pricked, but he lies stupid " and inattentive to slight external impres- " sions. As the effects of concussion diminish, " he becomes capable of replying to questions " put to him in a loud tone of voice, especially " when they refer to his chief suffering at the " time, as pain in the head, &c. ; otherwise he " answers incoherently, and as if his attention " was occupied by something else. As long as " the stupor remains, the inflammation of the " brain seems to be moderate, but as the former " abates, the latter seldom fails to increase; " and this constitutes the third stage, which is " the most important of the series of effects " proceeding from concussion. These several " stages vary considerably in their degree and " duration, but more or less of each will be " found to take place in every instance where " the brain has been violently shaken."

The chief difference between the symptoms of concussion and nervous apoplexy arising from internal causes, is, that in the latter there is not so uniform a tendency to inflammation; which, in the cases referred to by Mr. Abernethy, is evidently the effect of the injury done to the vessels by the blow, which we have reason to believe causes them to suffer mórbid distension as soon as a certain vigour of circulation is restored. It is this renewed vigour of circulation after the immediate effect of the blow has subsided, so remarkable in the experiments just referred to, that again gives some energy to the brain, and explains Mr. Abernethy's observation, that the tendency to inflammation comes on, as the stupor abates.

In nervous apoplexy, from internal causes, the sensibility is often as much impaired as in the sanguineous apoplexy. When this is the case the danger is very urgent; but, as in concussion of the brain, it frequently is much less so, compared with the severity of the other symptoms and the degree of danger, because here the sanguiferous, as well as the nervous system, suffers. In sanguineous apoplexy, the derangement of function being confined to the nervous system, the danger is nearly proportioned to the degree of insensibility; but in the case before us, symptoms of the greatest danger often occur, although the patient is not wholly insensible, and not unfrequently while he is affected with a degree of irritability. The foregoing symptoms, with the state of the pulse, afford the best means of distinguishing these species of apoplexy. In the sanguineous, we have seen, the pulse is strong, regular, and generally less frequent than natural; in the nervous, it is weak, frequent, irregular, and sometimes fluttering.

Such are the distinguishing symptoms of well-formed sanguineous and nervous apoplexy; and were these diseases always so formed, it would be easy to distinguish them. But we have to lament that this is by no means the case, as indeed from what has been said might, *à priori*, have been supposed. For it must often happen in apoplexy, from distension of the vessels, that the brain will sustain some further injury than that of mere uniform compression. It is not improbable that the circumstance of the compressing force, acting partially, may sometimes alone be sufficient to produce this effect; and powerful causes, injuring the organization of the brain, must often be of such a nature as at the same time to occasion debility, and consequently more or less distension of its vessels. To these circumstances, and to the difficulty of distinguishing apoplexy arising from mere distension of the vessels, from that arising from an extravasation of blood or serum, it appears to the author that all the difficulties respecting, distinguishing, and prognosticating the event in the different species of this disease are to be ascribed.

It is the tendency to distension of the vessels of the brain that renders a very stimulating plan of treatment, a doubtful practice, even in the most decided cases of nervous apoplexy. Were it not for this, the state of the sanguiferous and nervous systems in these cases would equally call for such a plan. But it would seem that the more debilitated the brain is, the more readily it feels the effects of any morbid distension of its vessels. Thus our practice in such cases is confined on all hands. Irreparable injury may be done by the free use either of stimulants or evacuants.

The mode of treatment which has appeared to the author the most successful, is a gently stimulating plan, combined for the purpose of preventing morbid distension in the head, with medicines moderately determining the fluids to the surface of the body, and keeping the bowels free without occasioning a great discharge from them; with occasional abstractions of blood from the head, when the insensibility seems inclined to increase. It appears from what has been said, that the increase of this symptom is the best indication of the vessels of the head becoming more distended.

Profuse sweating not relieving the symptoms, which is a frequent occurrence in severe cases of nervous apoplexy, seems always to indicate great danger, and to arise from a general relaxation of the extreme vessels, caused by the sedative effect of the disease on the brain, and is analogous to the effect on the capillaries, which the reader has seen produced by crushing the brain, or the application of tobacco to this organ *. In cases arising from injuries of the head, Mr. Abernethy thinks that the great tendency to inflammation altogether forbids the stimulating plan. I have already alluded to the circumstance which often makes the indications of cure in this respect different in concussion of the brain and nervous apoplexy arising from internal causes, namely, the greater tendency to inflammation in the former, arising from the local effects of the injury.

The foregoing view of the nature of the different species of apoplexy, not the result of preconceived opinions, but of facts open to the examination of every one who chooses to repeat the experiments, and so strikingly confirmed by the observations of Mr. Abernethy and other writers on the effects of injuries of the brain, may tend, perhaps, to render the practice in this varied disease more determinate. It seems, by affording a more correct view of the nature of the symptoms of the sanguineous and nervous apoplexy, than could have been obtained without a knowledge of the relation which subsists between the sanguiferous and

* Exp. 29, 39, 31, &c.

nervous systems, to point out with more precision than has yet been done, the symptoms essential to each, and consequently the modes of practice suited to the various cases in which they separately occur, or are blended together. The author has entered no farther on these modes of practice than was necessary to point out the general principles on which they are founded.

Inflammation of considerable extent, or of a vital part, the reader has seen, excites increased action of the sanguiferous system. He will more readily understand here, than he would have done in the Chapter on Inflammation, why in certain inflammations the action of this system, instead of being increased, is diminished. The author has just had occasion to observe, that distension of the vessels of the brain seems often, merely from the partial action of the distending power, so to injure this organ as to give rise to more or less of the symptoms of nervous apoplexy. A similar injury of the brain we might *à priori* suppose, must sometimes happen in that species of the distension of the vessels which produces inflammation of this organ; so that although in this disease the pulse is often strong, and the heat great, as in most other internal inflammations, it sometimes happens that the heat is but little increased, and the pulse small, frequent, and fluttering, more or less of the sedative effect having been produced in the brain, the danger, for reasons just pointed out in speaking of the nature of nervous apoplexy, being very great.

A similar state of the circulation is observed in other inflammations, which occasion very great nervous irritation. Thus in inflammation of the stomach and bowels, the heat is often little increased, and the pulse is feeble; the brain and spinal marrow being so injured by the irritation of the inflamed state of these important organs, as to weaken the action of the heart and blood-vessels, and thus cause a greater or less degree of syncope to be combined with the original disease. The author has seen the powers of circulation so enfeebled by violent inflammation of the alimentary canal, that within twelve hours after the attack it was impossible to obtain four ounces of blood, although large veins in both arms and both legs, and one of the temporal arteries, were opened; no blood having been taken previously, and the patient, at the time of the attack, having been strong and in good health. He died within twenty-four hours of the commencement of his disease. On inspecting the body, the whole of the alimentary canal was found inflamed, and a small spot on the stomach, of a purple colour, without any other morbid appearance. In all such cases, however, the pulse, though feeble, is still hard. The vessels still firmly embrace their contents. The peculiar irritation of the nervous system which attends inflammation, still excites, throughout the whole sanguiferous system, that effort to support the circulation in the debilitated vessels of the inflamed parts.

We see other causes of powerful nervous irritation, producing great debility of the sanguiferous system. Throwing a solution of opium or tobacco into the cavity of the abdomen for example, immediately enfeebles the power of the heart.

The reader may readily understand from what has been said, why inflammation of important organs often and sometimes very suddenly proves fatal, without the inflammation running its usual course, the derangement of the nervous system being such as to destroy the powers of circulation. This evidently happened in the case just mentioned. He may also see, why the pulse, in such cases, rises after blood-letting, which lessens the offending cause. The author believes that in some other cases in which the pulse rises after blood-letting, this effect may be explained in the same way. On the same principle also, as far as he can judge, we must explain the sudden debility, and subsequent loss of power in the circulating system, which ensues on mortification of any of the vital organs.

CHAPTER IV.

On Affections of the Spinal Marrow.

THE experiments in which different portions of the spinal marrow were destroyed * seem to throw considerable light on the nature of the diseases of this organ. The reader has seen that the destruction of any part of it not only, as is generally known, renders paralytic; that is, deprives of their only stimulant, the muscles of voluntary motion which correspond to that part, and to all parts of the spinal marrow lying below it †; but, by lessening the supply of nervous power to the great chain of gan-

* Exp. 58, 59, 60.

† It appears from what has been said, that although both the muscles corresponding to the part of the spinal marrow destroyed, and those corresponding to all parts below it, equally cease to move, it is from different causes; the former, because their nervous power is destroyed; the latter, because their nervous power is no longer subject to the sensorial power. Whether in the former case the power of the muscles themselves is impaired, will depend on the rapidity with which the offending cause has operated. See Part II., Chap. II.

glions, influences the state of the thoracic and abdominal viscera and the temperature of the animal, consequences of which we have not hitherto been aware *.

Even in early stages of diseased spine, affections of the stomach and lungs frequently attend, and the patient often complains of a sense of cold. The celebrated Mr. Pott remarks of this disease, " loss of appetite, a hard dry " cough, laborious respiration, &c., appear " pretty early, and in such a manner as to de- " mand attention." And in another place he observes, that there is " an unusual sense of " coldness of the thighs, not accountable for " from the weather." Similar observations are made by every writer on diseased spine. How well they correspond with the foregoing views need not be pointed out.

It appears from experiment 62, in which the spinal marrow was simply divided, compared with experiments 58, 59, 60, in which portions of it were destroyed, that we may judge of the extent of the injury done to this organ, in diseases of the spine, by the state of the stomach and lungs. Any thing, which so affects the spinal marrow as to interrupt the communication between the brain and other parts, will of course prevent the influence of the will from

* Exp. 58, 59, 60.

reaching them, however small a part of the spinal marrow may be injured. But if a considerable part is injured, along with loss of power in the limbs, the patient will experience symptoms of indigestion and oppressed breathing proportioned to the importance and extent of the part whose function is destroyed. The author has already had occasion to explain why the lungs are particularly affected by injury of the dorsal, and the intestines by that of the lumbar portion of the spinal marrow*.

From what is said of Asthma and Indigestion in the author's treatise on the latter disease, the reader will see reason to believe, that the foregoing symptoms may be relieved by the use of galvanism. This observation in the foregoing editions of the present Inquiry, induced Mr. Earle to try the effects of galvanism in such cases at St. Bartholomew's Hospital. He was so good as to address to the author the following letter, detailing the results.

"*George Street, August* 14, 1822.

"My dear Sir,

"I have much pleasure in transmitting to you the following account of the trials made with galvanism at St. Bartholomew's Hospital. The first case is that in which you witnessed its first application.

* See Chap. IX.

" Elizabeth Pepperall, aged 17, of fair complexion, and light hair, was admitted into St. Bartholomew's Hospital in August, 1821, in consequence of an affection of the spine, which had existed for about a year and a half. At the time of her admission, it appeared, that almost all the dorsal and lumbar vertebræ were affected. She had nearly lost all power over her lower extremities and pelvic viscera; and she complained of very severe cramps at the pit of the stomach, and acute pain in the course of the costal nerves, which was much increased by pressure on the ribs, or any attempt at a deep inspiration. Her general health was much deranged; her pulse was very rapid, with, occasionally, severe palpitation of the heart, and constant dyspnœa. Her digestive powers were greatly impaired; she had no appetite, and could only digest a small portion of stale bread, and some milk and water. Even this meal was always followed by uneasy sensations at her stomach, and an increase of head-ach, from which she was hardly ever free. Her bowels were obstinately costive, and the urine was scanty, and deposited large quantities of lithate of ammonia.

" She was placed on one of my invalid beds, which enabled her to remain in a state of uninterrupted rest; and after the repeated application of leeches, issues were made on either side of the dorsal spine, and subsequently in the lumbar region. The issues were kept actively open, and the strictest attention was paid to her general health. The spine very gradually became less sensible, and the power over the pelvic viscera and lower extremities slowly returned ; still, however, her stomach was incapable of digesting any other food than bread and milk and water, her head-ach remained nearly unabated, and her breathing was habitually difficult. She was in this state when you saw her, and the galvanism was first administered (December 19.)

" A trough containing plates of about three inches was employed. The positive wire was applied to the nape of the neck, the negative a little below the pit of the stomach. No sensation was at first produced by twenty plates ; but after the sensation was excited, she could not endure more than twelve. The first sensation she experienced, caused her to take involuntarily a sudden and deep inspiration. The galvanism was applied for about a quarter of an hour, at the end of which time, her breathing became much freer than it had been for many months. Of this she repeatedly expressed herself perfectly certain, at the same time she felt considerable uneasiness at the stomach. She was slightly hysterical, in consequence of the agitation she had experienced, but her breathing was tranquil during the whole evening.

"With a view to remove the tenderness in the epigastrium, leeches were applied to the region of the stomach, and the whole plan of treatment adapted to the second stage of Indigestion was resorted to. When the tenderness had somewhat abated, the galvanism was repeated with more decided relief to the breathing, and without causing much uneasiness at the stomach.

"After several applications of it, the relief she experienced in her breathing, lasted for two or three days, and at length it was only necessary to repeat it occasionally. The effect of its administration was uniformly the same; a most sensible and speedy relief from a state of anxious breathing to perfect ease and repose. Its beneficial effects were not, however, confined to the respiration; the powers of her stomach greatly improved, and she was able to digest a small quantity of meat or the yolk of an egg without pain. As her stomach improved, she lost the distressing head-ach, which had so constantly attended, as at one time to lead me to apprehend the existence of disease in the brain, having met with other cases in which scrofulous affection had existed in the brain and spine at the same time, Her progress from this time was uniform, and far more rapid than it had been before ; and in about two months, the catamenia, which had been suspended from the commencement of the disease, returned.

" The patient was sufficiently recovered to leave the hospital, and return to her friends at Dartmouth early in July; at which time she was able to walk with very little assistance, and without experiencing the least pain in her back. On reviewing the circumstances of this case, I have not the least hesitation in stating my decided opinion of the great benefit which was derived from the employment of galvanism, not only in affording temporary relief to the breathing, but in improving the secretions, and thus materially contributing to the ultimate recovery of the patient. I feel particularly happy that the patient was in a public hospital, and that the means were employed in the presence of many intelligent medical friends and pupils, who were all equally satisfied with myself of the essential and permanent benefit which she derived from the administration of galvanism.

" It was employed in two other similar cases in the same hospital, those of Ann Baillies, and Maria May, in which it produced similar good effects, except that in one of these, the improvement of the general health, although not less than in the other cases, did not appear to have the same beneficial effect on the disease of the spine. It was tried in another case of spine disease, which was attended with fits of spasmodic asthma. These, as I was taught to expect, from the observations you have published on this subject, it failed to relieve. It is remarkable, that in the case of Ann Baillies, in which the pulse was from 140 to 150, and very weak, the use of the galvanism always rendered it stronger, and brought it down from thirty to forty beats in the minute.

" From observing the good effects of galvanism on the secretions of the stomach, I was induced to make a trial of it in a case of deafness, accompanied with a total want of secretion of cerumen in the right ear. Its first application produced a watery secretion, which by perseverance gradually assumed the taste and all the other characters of cerumen. The hearing was greatly improved in both ears, but how far this was to be ascribed to the restoration of the secretion is rendered doubtful, in consequence of a tumour having at the same time been removed from the tympanum of the left ear by the repeated application of caustic.

The foregoing facts you are perfectly welcome to make any use of, should you think them deserving of notice, and I am,

" My dear Sir,

" Very sincerely, yours,

" Henry Earle."

It appears from the preceding statement, that in disease of the spinal marrow, galvanism is not only capable of performing the office of the diseased part of this organ, by which the vital functions are restored to a state of health, and the patient's sufferings greatly mitigated; but that it also, as might *à priori,* be expected by thus improving the general health, indirectly contributes to the cure of the spinal disease. In one of the cases mentioned by Mr. Earle, it failed to relieve the spinal disease, this being of such a nature, which must occasionally happen, as not to be influenced by the improvement of the general health. With regard to the last case mentioned by Mr. Earle, in which the secretion of cerumen was restored by galvanism, this, it is evident, from what has been said, can only happen when the fault consists in a defect of nervous influence, and not in a diseased state of the vessels.

CHAPTER V.

On Asthma and Dyspepsia.

The experiments related in the preceding Inquiry seem to point out more precisely than former observations have done, what we are to expect from the use of galvanism in the cure of disease. The author cannot help hoping that in those diseases in which the sensorial functions are entire, and the vessels healthy, and merely the function of secretion, which seems immediately to depend on the nervous power, is in fault, galvanism will often prove a valuable means of relief.

As soon as this view of the subject presented itself, he was led to inquire, what diseases depend on a failure of nervous power. The effect on the stomach and lungs, of dividing the eighth pair of nerves *, answered the question respecting two of the most important diseases of this class. The reader has seen, that withdrawing a considerable part of the nervous power from the stomach and lungs deranges the digestive powers, and produces great difficulty of breathing.

When the effect of depriving the lungs of a considerable part of their nervous power is carefully attended to, it will be found in all respects similar to a common disease, which may be called habitual asthma; in which the lungs are more or less clogged with phlegm and the breathing is constantly oppressed, better and worse at different times, but never free, and these symptoms often continue to increase in defiance of every means we can employ, till the

* Exp. 48, 49.

patient is permanently unfitted for all the active duties of life.

The animal, in the experiments referred to, is not affected with the croaking noise and violent agitation which generally characterize fits of spasmodic asthma, this state we cannot induce artificially, except by means which lessen the aperture of the wind-pipe *. It is the state of breathing observed in habitual asthma under which it labours.

The reader has seen from repeated trials, that both the oppressed breathing and the collection of phlegm, occasioned by the division of the eighth pair of nerves, may be prevented by causing the galvanic influence to pass through the lungs †; and with respect to the stomach, that after its function has been destroyed by depriving it of part of its nervous power, it may be restored by the galvanic influence. That galvanism may be employed with safety in the human body we know from numberless instances, in which it has been applied to it in every possible way.

Such are the circumstances which led the author to expect relief from galvanism in indigestion and habitual asthma. The first trials were made about ten years ago, and an account was given of them in the Philosophical Trans-

* See Chap. V. † Exp. 74, 75, 76, 77.

actions of 1817. The results were such as in many instances to exceed the author's expectations; and to have rendered the practice pretty general in this country, although the precautions necessary to render it most successful are not always, the author believes he may say, very seldom, attended to. What these are he has explained at length in his Treatise on Indigestion. It would too much extend the present volume to enter on them here. For the account of his mode of applying this remedy *, he also begs leave to refer the reader to that Treatise.

In it the author has, in other respects, as well as the employment of galvanism, applied the results of the foregoing Inquiry to explain the nature and improve the treatment of Indigestion and its consequences; and he has been gratified by reports from practitioners in various parts of the kingdom confirming the advantages derived from the plans of treatment thus suggested ; which with the circumstance of the Treatise having undergone five editions in little more than four years, encourages him to hope that the experiments in question have not been made in vain.

* Many of the observations on Galvanism here referred to, were republished from the paper just mentioned, which the Royal Society did the author the honour of publishing in the Philosophical Transactions of 1817.

CHAPTER VI.

On Suspended Animation.

Inflating the lungs, in suspended animation from drowning, or other causes obstructing the breathing, seems to act in two ways. It gives to the blood of the smaller vessels of the lungs the arterial properties by which they are excited to action; and acting through the blood of these vessels, it communicates to that of the larger vessels, and of the heart itself, more or less of the same properties, independently of the blood already changed being moved on towards this organ; for M. le Gallois has shewn, that after the circulation has permanently ceased, the blood may be changed, by inflating the lungs, not only in the trunks of the pulmonary veins and the heart itself, but also in the great arteries. By these means the circulation in the lungs is often restored, but it is evident from the experiments which have been laid before the reader, that the function of these organs must be very imperfect till they receive the due supply of nervous power. Now this cannot happen till the re-established circulation has renewed the vigour of the brain and spinal marrow, for which a considerable time is required. We have reason to believe, therefore, that could the due degree of this power be restored to the lungs, at the same time that they are exposed to the influence of the air, recovery might, in many cases, be effected, where inflation of the lungs alone fails.

The reader has seen that galvanism can supply the place of the nervous power in the lungs, enabling them perfectly to perform their functions after the latter is withdrawn. The author would, therefore, propose, that to the instruments used in the recovery of suffocated persons, an apparatus, properly adapted for sending the galvanic influence through the lungs in the direction of their nerves, should be added. It would be improper here to employ, for any considerable length of time, a stronger power than experience has taught us can be used without bad effects in health. The power should not, perhaps, in the present state of our knowledge, exceed that of fifteen, or at most, twenty, four-inch square double plates of zinc and copper, the fluid being one part of muriatic acid and twenty of water *.

* The author mentions plates of this size, because he has most frequently witnessed their effects; but he has reason to believe, that a greater number of much smaller plates will better answer medical purposes.

Little advantage is to be expected from galvanism applied to any other secreting organ, because the revival of the patient depends little, if at all, on the action of any other. Employed as a general stimulant to the brain and spinal marrow, it may be of use by rousing the dormant powers of the system. They are all capable of being excited through these organs. In this way it can only indirectly assist the lungs, and that chiefly in proportion to the degree in which general circulation is restored. It is probable, that as a general stimulant, a greater power of galvanism may be used with safety, because it may, with this view, be applied interruptedly.

WHEN we compare together the whole of the foregoing statements respecting the effects of galvanism in the cure of diseases, may we not hope, that if in so few years such has been the result of its employment on the principles above laid down, a more extensive experience will still extend the advantages derived from it? The author has repeatedly seen its use more successful than any other means in obstinate general debility, in which transmission through the stomach and lungs has still appeared to him the best means of applying it. Certain cases of fever he has already had occasion to mention. It is probable, it will be found useful in all cases of deficient nervous energy, accompanied with little or no inflammatory tendency.

THE END.

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