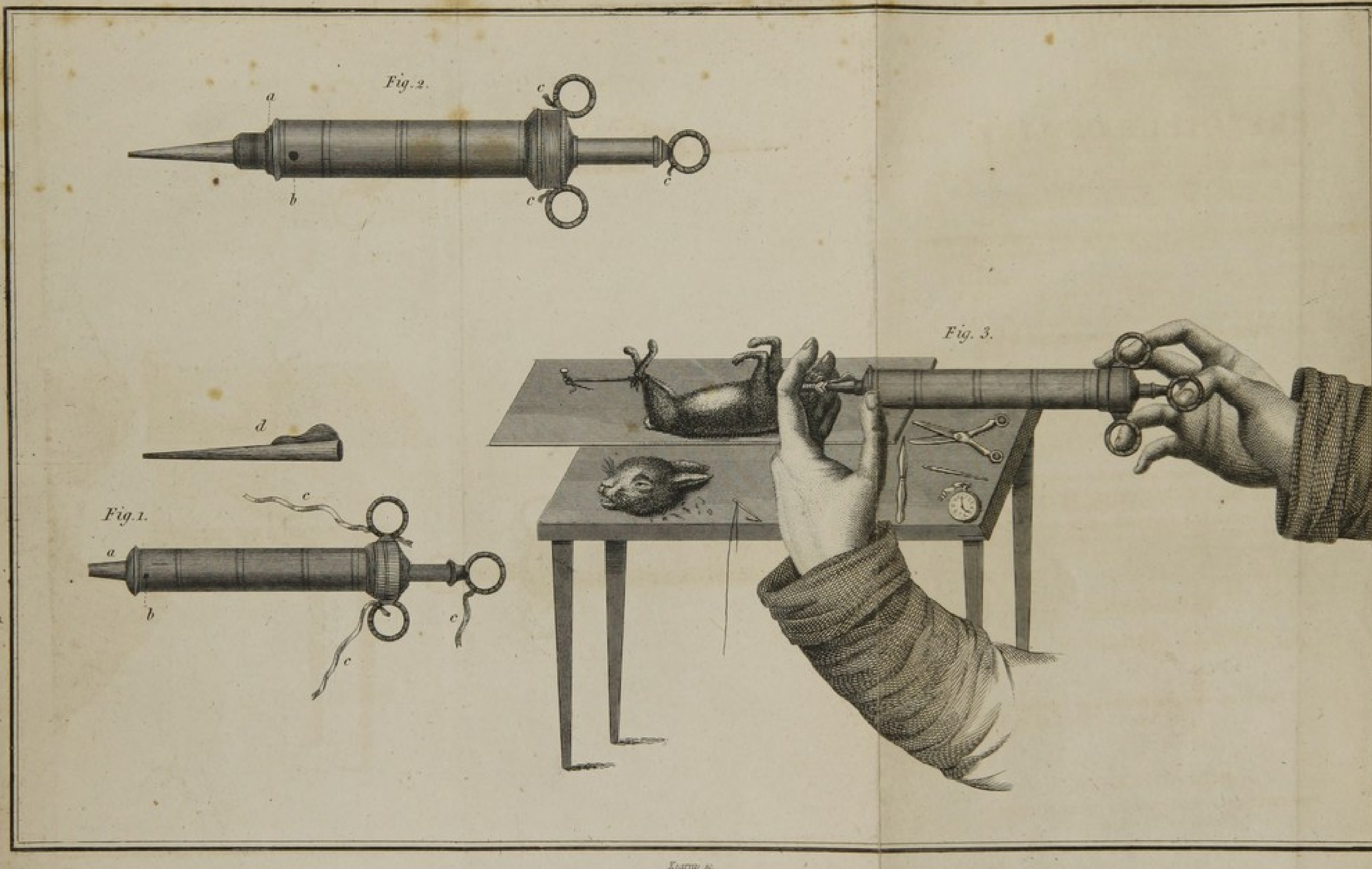

Kearny sc.

EXPERIMENTS

ON THE

PRINCIPLE OF LIFE,

AND PARTICULARLY ON THE

PRINCIPLE OF THE MOTIONS OF THE HEART,

AND

ON THE SEAT OF THIS PRINCIPLE:

INCLUDING

The Report made to the First Class of the Institute, upon the Experiments relative to the Motions of the Heart.

BY M. LE GALLOIS, M. D. P.

Adjunct member of the Society of the Professors of the Faculty of Medicine of Paris, Member of the Philomatic Society, Physician to the Board of Benevolence of the Pantheon-Ward.

TRANSLATED BY

N. C. AND J. G. NANCREDE, M. D.

"Undè anima, atque animi constet natura, videndum."
Lucret. lib. i. v. 132.

PHILADELPHIA:

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WILLIAM FRY, PRINTER.

1813.

ANNEX
Life

TO

BENJAMIN SMITH BARTON, M. D.

ONE OF THE PHYSICIANS TO THE PENNSYLVANIA HOSPITAL; PROFESSOR OF THE THEORY AND PRACTICE OF MEDICINE, &c. IN THE UNIVERSITY OF PENNSYLVANIA; VICE PRESIDENT OF THE AMERICAN PHILOSOPHICAL SOCIETY; PRESIDENT OF THE PHILADELPHIA MEDICAL SOCIETY, &c.,

A GENTLEMAN

NOT MORE EMINENTLY DISTINGUISHED, FOR THE ACCURACY AND DEPTH OF HIS PHILOSOPHICAL AND MEDICAL RESEARCHES, AND THE IMPRESSIVE ELOQUENCE DISPLAYED IN HIS PUBLIC LECTURES, THAN FOR THE URBANITY AND POLITENESS OF HIS MANNERS; ALIKE THE ATTRIBUTES OF THE SCHOLAR, THE PHILOSOPHER AND THE PHYSICIAN,

THIS TRANSLATION

IS RESPECTFULLY INSCRIBED BY
HIS MOST HUMBLE AND MOST OBEDIENT SERVANTS,

THE TRANSLATORS.

PREFACE

OF

THE TRANSLATORS.

IF the labours of men of genius, who devote their talents to the improvement of a science, the most important to humanity, be the property of the human race, it is the duty of those who from profession, or natural fitness, are engaged in the same laudable pursuit, to take possession of them, and impart their benefits to their fellow labourers, throughout every part of the world. And here we are blest, with an invaluable privilege peculiar to the citizens of this happy country,—" the culture and improvement " of the human mind throughout the world have " become theirs: they have had no literary in- " fancy:—their first flights of genius, their first " efforts of science, have placed them on the " eminence obtained by the old world in ages " of study and labour."

Among the recent European publications, calculated to extend the boundaries of science, it is presumed that no attempt conducted with as much sagacity and precision, to as complete success, is more eminently intitled to an English translation, than the work containing the discoveries of Dr. Le Gallois, which in the words of the Commissioners, appointed by the Institute of France, to examine and report on its merit, " is an elegant performance, and certainly the most important that has been pro-produced in Physiology, since the learned experiments of Haller, and one which will constitute an epoch in the history of that science."

It was the Translators' good fortune, while studying medicine in France, to be honoured with the acquaintance and friendship of the Author, and to be present at and to assist in the experiments which led to, and determined his discovery. They could bear testimony, if after the ample and flattering report of the Commissioners, testimony could be wanted, to the fidelity and accuracy of every and the most minute part of the statement of the facts: but such direct experiments, the truth of which it is in the power of every student in Physiology to ascertain, can stand in need of no further authority, than the lucid narration given by the author himself.

Strongly impressed with the importance of the work, its usefulness to medicine, its students and professors, and with a confident hope that its publication will be acceptable to the American physician, they lately brought the volume from France, with a view of rendering it public in their native country. Yet duly aware that the ardour, natural to those engaged in experiments of this nature, might have magnified its real importance, they submitted their views to the Professors of the University, and other learned gentlemen, who being already acquainted with the character of the work, did not hesitate after a re-perusal of it, to say that an English Translation was desirable.

As to the execution and fidelity of the present Translation, although by a continued neglect of their native tongue, during a long residence in France, they may be thought unqualified to present such a work to the public eye, they nevertheless confidently hope for indulgence, and that this attempt will not be construed into presumption, when it is considered that friendship to the author, the zeal ever inseparable from the success attending such a series of minute experiments, (in the greatest part of which they assisted) and the merit of the work itself, made it in some sort a duty. This being, however, no apology for trespassing on the public by an incorrect translation, they submitted their manuscript to some literary physicians in this city, who had the goodness to assist them with their remarks and criticisms, for which, as well as for many other professional favours and services, they take this opportunity of tendering them their sincere and respectful thanks. With due diffidence they add, that should this, their first literary effort, be received with indulgent approbation, they will (on the faith of their correspondents) at a future day give the sequel to these Physiological Experiments referred to in the author's preface.

Philadelphia, October, 1813.

INTRODUCTION.

THE work I now present to the public is composed of Memoirs which I have read at different times before the first class of the Institute, and the society of Professors of the Faculty of Medicine of Paris. The two first sections include that which I submitted last year to the judgment of the class, upon the principle of the power of the heart, and the seat of that principle, and which the class was pleased to receive in terms so flattering and honourable to me. What is said of the heart, being applicable to the other organs of the involuntary functions, the question may be more generally considered as the determination of the seat of that principle which presides over this order of functions. The first section is a summary of the experiments which I had communicated two years before to the faculty of Medicine, the object of which was to inquire, what is the seat of the principle of the inspiratory motions which are submitted to the will. These have not presented less difficulties nor given rise to less disputes, than the involuntary functions, in relation to the principle by which they are animated. It was thought that the principle of action of the one was different from that of the others, as to its seat and even as to its nature. Several systems of physiology were grounded upon this difference. The reader will find in the report of the commissioners of the Institute, a very interesting summary of these theories in what concerns the functions which are independent of the will. I have given in the beginning of this volume, a sketch of those which are relative to the functions dependent on the will.

The first section was only intended to serve as an introduction to the second, and I am sensible that it is too concise; the experiments are little more than pointed out, and what is said of the functions of the brain, would have required much more illustration. But a small supplement will be found to the experimental part, in the first section of the experiments which I have repeated before the commissioners of the Institute, and also in the appendix at the end of this volume. As to the functions of the brain, I will here give some explanations which I have not had occasion to give elsewhere.

I have only considered in this viscus its action upon the inspiratory motions, and that which it exercises upon the internal organs, through the nerves of the eighth pair; because these functions are those which are most easily submitted to direct experiments. But I am far from pretending, that it has not upon the other parts of the body an influence equally important and necessary. I acknowledge, on the contrary, that it is that viscus which determines and regulates every act of the animal functions. For instance, when I move my arm, the principle of this motion emanates from the medulla spinalis and not from the brain, but it is the brain which determines and directs it in the mode appropriated to the purposes for which I make it. Cold-blooded animals furnish an evident proof of this assertion. If a salamander be decapitated at the first vertebra, it may continue to live for several days; but although it moves its body and limbs with as much force as would be necessary to transport it from one place to another, it remains in the same place, and it may be left upon a plate with a little water without fear of its escaping. If we examine all its motions we perceive that they are all irregular and without design. It moves its paws in opposite directions, so that it cannot advance, or if it takes one step forward it soon takes another backward. The same thing is observed in decapitated frogs; they no longer know how to leap, or if they do leap it is only when their hind legs meet with a particular support. If they are laid on their backs, they sometimes agitate themselves seemingly to change their situation, but they remain in the same position, because they no longer know how to make the motions necessary to replace themselves on their belly. But generally speaking, all these animals perform few motions, unless they are touched, and we conceive that it ought to be so, since of all the senses that of touch alone can transmit impressions to them.

Decapitation itself is not necessary to produce these phenomena; they are likewise observed and in a manner still more curious after the simple section of the medulla spinalis, performed at the occiput. In this case the head is alive as well as the rest of the body, as is evinced by the motions of the mouth and of the eyes. And nevertheless, the animal is exactly in the same state as if it had been decapitated, viz. no longer able to govern its motions. A situation truly extraordinary, in which both the head and the body possess life separately, without being able to exercise any action over each other; the head lives as if it were without a body, and the body as if it were without a head.

Reptiles may sometimes continue to govern their motions and to walk after being decapitated; but if we attend to it minutely, we shall find that in all those cases, decapitation has only been partially performed on the cranium, and that the posterior part of the brain has been left united with the body; which shows that the faculty which animals have of regulating their motions, resides in some portion of this part. To find out what this portion is, it would be sufficient to take off successively the anterior portion of the brain, and to continue this operation till the animal had suddenly lost the power of walking. The researches I have made on this subject, have taught me that it has its seat in the medulla oblongata. But to determine it with more precision, larger reptiles are wanting than those I have been able to procure.

Decapitation and the section of the medulla at the occiput, produce phenomena which are perfectly similar in warm-blooded animals. This might be expected from the exact uniformity of the plan by which the nervous power is organized in all vertebral animals, from man down to the reptile; for it is a curious and very important observation of Mr. Cuvier, that the nerves arise and distribute themselves with exact similarity in all those animals. But the warm-blooded are much less suitable than the reptiles for the researches I have just mentioned, because after the section of the medulla they can only be kept alive by the inflation of the lungs, which prevents their being left to themselves in order to study their motions; and because after the partial decapitation the hemorrhage of the cerebral vessels soon destroys the functions of the portion of the brain that has not been taken off, by stopping circulation. It is true, that these inconveniences would be less if new-born animals were selected, but then their powers of locomotion are too weak and too limited. The reptiles on the contrary, need no assistance to supply respiration, without which they can live a long time, and life continues in all the parts of their nervous power for several hours after the entire cessation of the circulation.

I have often been asked, whether warm-blooded animals could walk and run away after decapitation. What I have just said is an answer to that question. Nevertheless, it is to be remarked, that the motions performed by a living trunk without a head, appear generally to be excited by some kind of instinct or will. Guinea pigs, at any age, when they have recovered from the stupor into which decapitation at first threw them, appear strongly to feel the pain occasioned by the wound of the neck; they direct to it both their hind paws alternately, and with eagerness, as if to scratch it. Kittens also make these motions. How does the brain regulate the motions of the body without furnishing its immediate principle? Experiments can furnish but an imperfect answer to this question. Unwilling to adopt all the conjectures which it might suggest, I shall remark, that the brain appears to act upon the medulla spinalis as the latter acts upon the parts which it animates. It is through the nerves that the medulla spinalis transmits its action, and the nerves appear to be formed of the same substance with the white and medullary part of the brain and medulla spinalis. I conceive, therefore, that the white part of the medulla spinalis is composed of nervous filaments, having their origin or termination at one end in the brain, and at the other in every part of the medulla; and that it is in the cinericious part of the medulla that both the spinal nerves and the principle that animates them arise. The anatomical researches of Mr. Gall, appear to me, to give much weight to this opinion.

The action of the brain upon every part of the medulla, does not only produce the effect of determining and regulating the motions, but it appears to increase their energy. The motions are always weaker in the animal decapitated than in that which is not, unless the end of the medulla be immediately touched, for then the motions are very strong and even convulsive. It is true, that the weakness of these motions may also partly be owing to the medulla being always in a morbid state after decapitation. These intimate relations between the brain and the medulla spinalis, will aid in accounting for certain facts which appear at first very difficult to be reconciled with my experiments. Such is the palsy of one whole side of the body produced by causes which affected the brain alone. But though no means of reconciling them should be perceived, it would be no less correct, that an affection limited to the brain may deprive one half of the body of sensation and motion, than that sensation and voluntary motion may be maintained in a decapitated animal. However opposed these facts appear to be, it must be recollected that two facts well ascertained can never destroy each other, and that the apparent contradiction is produced by some intermediate cause, some point of contact that has escaped our observation.

The unity of self, of which we have the consciousness, is another fact which seems to be repugnant to the dissemination of the principle of life through the whole extent of the brain and the medulla spinalis. But it ought to be remarked that the connexion and harmony of all the parts of the nervous power are sufficient to produce the sentiment of this unity, though this power is not concentrated in one point. Suppose, if I may be allowed this vulgar comparison, a number of wheels connected by their cogs, they will form one system only; no one can perform any motion unless they all do the same. But let the connexions be interrupted in one or several places, the result will be a variety of parts capable of motion which will be independent of each other. So if you produce interruptions in the seat of the nervous power, you will thereby establish a variety of centers of sensation entirely distinct. But what is important to be observed is, that those various centers can never be created but by interruptions caused by design or accident, and that each of them always supposes the co-existence of a portion of the seat of the nervous power, which is very different from the opinion by which it is admitted, that in the natural state, each organ has a centre of sensation and a sort of life peculiar to itself. This opinion, rejected by the soundest theories and the best established facts in physiology, had acquired much influence in modern times, when Mr. Cuvier declared himself against it; nothing short of the influence of so justly celebrated a man could arrest its progress.

Another question on which I have dwelt but little, was to know in what manner the nerves transmit the action of the nervous power to those parts where they are distributed. Are they only simple conductors, or is there a secretion produced in them of a nature analogous to that which takes place in the brain or in the medulla spinalis? The researches of Messrs. Reil and Prochaska, had rendered this opinion very probable. Mr. Nysten has since shown, that in the most complete palsies, the irritability is preserved in the paralised limbs as perfectly as in those that are sound. I obtained a like result from an experiment which I have often repeated. It consists in destroying the lumbar portion of the medulla spinalis of a rabbit not ten days old; it should be chosen of that age, that the circulation may not be stopped, and that it may continue to live. Although in this experiment, the hind part is struck with death, and its nerves can no longer receive any influence from the medulla spinalis, it preserves its irritability, and the thighs may for a long time be made to contract by irritating the sciatic nerves. Hence it appears as if a secretion of a particular principle were made through the whole extent of the nerves. This principle, being once produced, continues of itself, and even after the entire cessation of the circulation, in the same manner as that of the brain and of the medulla spinalis. I had thought that it was through the medium of the nervous principle that the brain and the medulla spinalis exercised their action upon the different parts of the body, without a circulation of their principle, but by a sort of a shock upon that of the nerves, nearly as sound is transmitted through the air. To ascertain this conjecture, it was necessary to find a nerve which might easily be isolated through a certain extent, and which commanded some function, the interruption of which would be sudden and very apparent, as soon as the nerve should cease to act. I selected the nerve of the eighth pair in kittens. We shall see by and by, that the ligature or section of those nerves in these animals, produces suddenly all the symptoms of violent suffocation. By detaching them through the greatest part of the neck and by destroying all the vessels which are distributed to them, I expected, if my conjecture was well founded, that as soon as the principle with which they were imbued at the moment of dissection, should be exhausted, the kittens would experience the same suffocation as if these nerves had been tied or divided. But it was in vain I repeated this experiment several times; the result never answered my expectations; respiration never was disordered in any sensible degree, whilst if one or several hours after having detached the nerves, I divided them, suffocation instantly took place. Nevertheless, I do not wholly give up this hypothesis; for the neck of cats is of no great length, and besides it is impossible to detach the par vagum throughout that extent. It is possible that the secretion continuing near the thorax, and the head or the vessels not being destroyed, might extend to the dissected portion.

This is what I had to add upon the functions of the nervous power, and particularly upon that of the brain, to what I have said in my memoirs. The general idea I have of this power is, that its seat constitutes in itself the individual as a living being; all the rest of the organization of an animal only serves to establish a relation between the nervous power and external objects, or to prepare and supply it with materials necessary for its support and nourishment. In the whole range of animals, I perceive only every possible combination of organs capable of sustaining in the nervous power, properties various as these combinations, yet in all intrinsically the same. Among these combinations, those which are the simplest, and in which the conditions, necessary to the maintainance of the nervous power, exist in all the parts, are susceptible of being divided into portions; and life may continue in each portion as in the whole animal, or rather each portion becomes a new animal. On the contrary, those in which these conditions center in certain parts, do not admit the like divisions with the same success: life cannot continue in the segments which are found separated from these parts, longer than the nervous power can subsist of itself without being renewed.

My object has been rather to describe the results than enter into any detail, or to multiply their numbers. All the particulars I have given, appear to me necessary to show the order and succession of the phenomena, and to enable the physiologist to ascertain them. I propose to publish, hereafter, the journals of my experiments, with all the particulars they contain.

I have been more careful in ascertaining the facts than eager to publish them. Nevertheless, I judge it proper to fix their several dates. My researches on the fœtus date from 1806. It was only in 1808, that I communicated my first observations to the society of the professors of the faculty of medicine of Paris. In these, I communicated my first notices on decapitation, and on the functions of the medulla spinalis. At the invitation of Mr. Thouret,* I demonstrated publicly before the same society, on the 2d and 16th of March, 1809, that the principle of the life of the trunk resides in the medulla spinalis; and I afterwards repeated the same experiments, on the 16th of April, before Messrs. Chaussier and Dumeril, whom the society had chosen a committee to examine them, and who made their report on the 27th of the same month.

The subject was far from being exhausted; I soon after began my researches upon the motions of the heart. Mr. Magendie proved, soon after, by some interesting experiments, that it is by acting upon the medulla spinalis that the poison of the Indians, known under the name of *upas tieuté*, kills animals. Mr. Brodie, a member of the royal society of London, was desirous of ascertaining the temperature and the state

* Dean of the faculty of medicine of Paris.

of the secretion in animals kept alive after decapitation. I have repeated the experiments of this author, in what relates to the temperature. The results which he announces did not appear to me as regular as he pretends. Mr. Brodie asserts, that the decapitated animals which are kept alive, cool as rapidly as if they were dead. It is true, that their temperature is considerably reduced; but I have always found in kittens that this reduction of temperature is less than after death. The difference has been in my experiments, from one to three degrees, (centigrade thermometer). It is generally less in rabbits. I found also, that the inflation of the lungs is one of the principal causes of this refrigeration; and that generally speaking, all the circumstances which derange or impede respiration produce this effect. Thus keeping an animal extended on its back will lower its temperature.

My attention is at present engaged in ascertaining whether, under these different circumstances, the formation of the carbonic acid in the lungs is diminished, and with a corresponding alteration of temperature. My friend, Mr. Thillaye junior, has favoured me with his assistance. The experience of this able natural philosopher, his great dexterity and thorough habit of experimenting, render his co-operation singularly valuable. Several causes had interrupted our labours, and among others, some very expensive instruments. Baron Corvisart, first physician to the Emperor, being informed that those instruments were not in the cabinet of natural history of the faculty of medicine, of his own accord, and with his usual munificence, directed them to be sent and placed at my disposal, and he likewise had the goodness to permit me to order the construction of them in the manner I might think most suitable. It is gratifying to me, to take this opportunity of publicly expressing my gratitude.

When these researches shall be concluded, I propose to review and publish my first experiments on fœtuses; particularly those made with a view to determine how long a fœtus may live without breathing, after its communication with its mother has ceased.

Before I close this introduction, I wish in some degree to exculpate the physiologists who make experiments upon living animals, from the reproaches of cruelty, so frequently uttered against them. I do not pretend wholly to justify them. I would only remark, that the most part of those who utter those reproaches may be deserving of the same. For example, do they not go, or have they never gone a hunting? How can the sportsman, who for his own pleasure mutilates so many animals, and often in so cruel a manner, be more humane than the physiologist who is forced to make them perish for his instruction? Whether the rights we assume over those animals be lawful or not, it is certain that few people scruple to destroy, in a variety of ways, such of those animals as cause them the least inconvenience, though ever so trifling; and that we only feed the most part of those that surround us, to sacrifice them to our wants. I can scarcely comprehend that we should be wrong in killing them for our instruction, when we think we are right in destroying them for our food; and especially when, by a refinement in gluttony, we kill them after submitting them to painful operations and long tortures.

I own that it would be barbarous to make animals suffer in vain, if the object of the experiment could be obtained without it. But it is impossible. Experiments upon living animals are one of the greatest lights of physiology. The difference between the dead and the living animal is infinite. If the ablest mechanician is unable to discover all the effect of a machine after having seen it work, how could the most learned anatomist devise, by the study only of the organs, the effect of a machine as prodigiously complicated as the body of an animal. To find out its secrets, it is not enough to observe the simultaneous exercise of all the functions in the animal, while in health; it is above all important to study the effect of the derangement, or the cessation of such or such a function. It is in determining by this analysis what the function of such or such an organ is, as well as its relation with the other functions, that the art of experiments upon living animals consists. But to be able to do it with some degree of precision, it is indispensably necessary to multiply the victims, on account of the variety of circumstances and accidents which may render their result uncertain or inconclusive. I should be tempted to say of physiological experiments, what has been said of charities: *perdenda sunt multa; ut semel ponas benè*. Seneca.

EXPERIMENTS

ON THE

PRINCIPLE OF LIFE.

OF the faculties peculiar to animals, those by which they are eminently characterized are sensation and motion; and it may be said, that the real end of organization in an animal, is to produce and maintain these two faculties. Whatever the internal or external means, the secret or apparent springs, which nature employs; and whatever the actual situation of those means, and of those springs,—as soon as a being feels and moves, it is a living animal endowed with the sense of its own existence. To know in what consists the essence of life, it will be necessary to ascertain what is the precise condition, in the organization of animals, on which sensation and motion depend. In this inquiry, however, two circumstances are to be determined: first, the exact nature of this condition; secondly, the parts to which it is confined, or its seat. For instance, admitting that sensation and motion are produced by a particular principle, the result of organization, we must inquire, what is the nature and the seat of this principle? Both these questions have given rise to various speculations; and to mention only the latter question, though it appears capable of a more ready solution than the former, yet no hypothesis has hitherto been offered fully satisfactory or adapted to all the phenomena.

It might be imagined that the principle of sensation and motion resided in every part of the body, as all seem more or less to participate of these two faculties. But observation having shown that the section of a nerve, in any part of its course, instantly deprives of sensation and motion all the parts to which the inferior portion of the divided nerve is distributed, it necessarily follows that the sentient principle does not reside in the part which receives the impression, and likewise that the power causing motion, exists not in the part which moves; but to discover itsi seat, it is necessary to rec[illegible] to the origin of the nerves. Now as all the nerves arise from the brain and the spinal marrow, the source of life has been supposed to be derived from the brain and spinal marrow. A multitude of facts prove, on the one hand, that the destruction or even certain wounds of the brain, cause sudden death; on the other, that the transverse section of the spinal marrow, in any part of its course, always paralyzes the parts below the section, whilst all those above, continuing their communications with the brain, preserved both sensibility and motion. Moreover, the spinal marrow has been considered by anatomists only as a large nerve arising from the brain, in the same manner as all those which pass through the foramina of the cranium, and which like them are divided at intervals, in order to supply the intervertebral nerves;—in short this spinal marrow was considered as a mere bunch of nerves supplying the trunk, as it was often called. The brain therefore was considered as a center of the nervous power, and consequently as the only seat of the principle of life.

This opinion was carried still further. The unity of *self*, the metaphysical ideas referred to it, and the observation, that certain parts of the brain might be wounded, and even destroyed with impunity, led to the conclusion, that this viscus in its entire capacity was not the seat of this principle, and that there must be a defined spot where all sensation terminated, and whence all motion received its impulse; and this place, designated under the name of *sensorium commune*, or the seat of the soul, was for a long time an object of inquiry among physiologists.

But the object was not obtained by these inquiries; for in proportion as known facts have been examined, and observation has been extended, has the difficulty increased, of reconciling all these facts with the theory which places exclusively in the brain, taken in all its parts, the principle of sensation and animal motion. Thus it could not be conceived, agreeably to this opinion, why reptiles, such as the tortoise, salamander, &c., preserve life for whole months after decapitation; nor why animals of inferior classes present similar or even more extraordinary phenomena. It was still more difficult to understand, why the duration of life in those animals, varies to such a considerable degree according to the mode in which the brain has been removed; why for instance the tortoise, from which Redi had taken this viscus, by opening the cranium, had survived the operation several months, whilst those whose heads he had cut off below the occiput had only survived it a certain number of days (*a*). For the difference is not produced by the hemorrhage as might be supposed. To obviate these difficulties, it has been asserted that the opinion in question was only established upon observations made upon warm-blooded animals, that it was only applicable to them, and that in the cold-blooded ones, the nervous power was subjected to other laws. But a considerable number of facts observed in the warm-blooded themselves, seemed to militate against this explanation.

(*a*) Opere di Frances Redi, 1741. Tom. I. p. 78, and tom. II. p. 194.

It is a well established fact, that birds continue alive and even walk and run after their heads have been cut off. The fact has frequently been quoted of the emperor Commodus, who, whilst ostriches were running in the circus, amused himself by cutting off their heads with arrows, in the form of a crescent. These animals were not prevented from running as before, and only stopt at the end of the course. Several physiologists have obtained a like result, by decapitating turkeys, (*a*) cocks, (*b*) ducks, (*c*) pigeons, (*d*) &c., hence as regards these animals, a particular exception must be made in the laws regulating the nervous power (*e*), and the received theory remains only applicable to man and other mammiferous animals.

In the latter the phenomena appear to correspond with this theory; whether after decapitation

(*a*) Lamétrie, Œuvres Philosoph. 1751, p. 56.

(*b*) Kaauw Boerhaave. impet. faciens. No. 331, p. 262. —Urb. Tosetti, Mém. sur les part. sensi. et irritab. tom. II. p. 194.

(*c*) M. Cuvier, Leçons orales.

(*d*) Woodward, cité par Haller.

(*e*) Haller, Elém. Physiol. tom. IV. p. 355.

or after various wounds of the brain or of the spinal marrow. Nevertheless, some exceptions had been observed at different times. Thus, Desault relates in his journal a case where the spinal marrow had been cut completely across by a gunshot, in which the palsy of the inferior extremities did not take place. A like case is found in the *Selecta medica Francofurtiana* (*a*). Authors assert, that a calf continued to walk a great ways after decapitation (*b*); that a woman walked a few steps (*c*); that a man was able to hold his sword, and brandish it at three different times (*d*); that another man struck his breast with both his hands (*e*), &c. But it has been objected that those facts are very few, and in opposition to daily observation in such cases. It has been further objected that most of them are attested by authors not qualified to be judges; upon which Haller says, in quoting them, that the witnesses must have been philo-

(*a*) Tom. I. p. 4.
(*b*) Riis, quoted by Haller.
(*c*) Rzadskinski, Hist. Nat. Polon. p. 363.
(*d*) Ibid.
(*e*) Struve, Anthrop. Sublimior. 1754, p. 38

sophers to be believed. *Sed hæc ab hominibus philosophicis oportuerat testimonium habere* (*a*). This reflection would not perhaps be misapplied with regard to some facts asserted in the discussion that has arisen on the question, whether life continues after the operation of the guillotine (*b*). It almost always happens that those facts are overloaded with circumstances which weaken their authenticity. How can we believe, for instance, that in a head separated from its trunk, and entirely deprived of circulation, the face became red upon the hangman giving it a slap? Finally, admitting, and it is difficult to withhold assent, that some signs of life have been ascertained after decapitation in mammiferous animals; as those signs were feeble and of very short duration, they may be considered as the

(*a*) Elem. Physiol. tom. IV. p. 393.

(*b*) Let us observe that in this discussion, it is for the most part in the head that signs of life have been sought for; that it is in that part which is least capable of showing any, though it be really alive. Because those who sought it were themselves of opinion, that the brain was the only focus of life, and thence they were not to suspect the existence of life in any other part but the head, after decapitation.

last remains of life, the source of which was exhausted. On the whole, the theory could only be maintained, with regard to mammiferous adults.

But it was not so with the fœtus. We have a number of acephalous fœtuses, both in the human species and other mammiferous animals. How could these fœtuses have lived and grown in their mother's womb without a brain? The answer was, that they were hydrocephalous (*a*), in which water had at last destroyed the brain, as well as all its coverings, and which had continued to live as long as was consistent with the progress of the disease. Perhaps this answer was applicable to some, but it is evident that it could not be applied to those born alive, and with manifest signs, either that the brain had been wanting for a long time, or that it had never existed. Besides it does not explain, why some of these fœtuses perish as soon as they are born, and why others only a few hours or even a few days after? On considering the state of things without pre-

(*a*) Haller, Elem. Physiol. tom. IV. p. 355.—Morgagni. de sed. et causis morbor. epist. XII. art. V. et seq.

judice, it was impossible for the theory to account for these facts.

The experiments of Haller on irritability promised rather than offered a satisfactory solution. Those experiments tended to establish the existence of an internal life, independent of the nervous power. Long before Haller, the functions, of which we have the consciousness and which we command at pleasure, such as the intellectual functions, voluntary motions &c. had been distinguished from those which are exercised without our knowledge, and over which our will has no command, such as circulation, nutrition, secretions &c. The former had been distinguished under the name of external animal functions, &c. and the latter under the name of internal natural functions, &c. But these two orders of functions were equally under the control of the nervous power. The only difference admitted in that respect, was in the mode of the action of that power. According to some, the nerves were organized in such a manner as to render this action more easy and regular, in some than in others (*a*). According to others, the vital or internal functions had their first cause in the cerebellum, and the animal functions had theirs in the cerebrum properly called (*b*). It is evident that this distinction of the functions, explained in either of these ways, only increased the difficulties.

But they were thought to be removed, at least in a great measure, when Haller (*c*) had suggested that the cause of animal motion resides in the muscular fibre itself; that this fibre to perform its contraction needs only a stimulus which determines it; that in the muscle submitted to the will, this stimulus is constantly the nervous power; and that with regard to those which are not under its control it is of a different nature and perfectly independent of this power. It was indeed easy to understand that the internal functions being independent of the nervous power, whilst the animal functions were

(*a*) Borelli, de motu animalium. 1763. p. 89—92.

(*b*) Willis, opera omnia. 1682, tom. I. de cerebri anatome, page 50.

(*c*) Memoires sur les part. sensib. et irrit.

immediately produced by it, the latter might cease, and the nervous power be destroyed, without the former ceasing at the same time. It was even conceivable that they would continue as long as they could dispense with the concurrence of the functions submitted to the nervous power, and especially of respiration, the suppression of which most immediately threatens the life of the whole. Finally, mammiferous fœtuses having no occasion to breathe in their mother's womb, and cold-blooded animals being endowed with the faculty of sustaining a very long privation of air, and of preserving their irritability for a long time, it was imagined that both could live for a very long time without the assistance of the nervous power.

We shall hereafter examine whether it be true that the internal functions be independent of the nervous power: let us admit for the present, that they are really so; the explanation which we have just related would be very far from accounting for all the phenomena presented by acephalous or decapitated animals. For it is not only the internal functions that subsist in the present case; part of the animal functions also subsist as the voluntary motions takes place. And to ascribe those movements to irritability would be extending its influence much beyond its real limits. It would even be in direct opposition to the theory of irritability, according to which they can be spontaneously excited by the nervous power alone. Nevertheless the desire of explaining these puzzling phenomena led several eminent authors to adopt this paradox, among others Charles Bonnet (*a*), and Felix Fontana, as we shall soon have occasion to observe.

It is very true that the muscles employed in voluntary motion, preserve the power of contraction after they have ceased either to communicate with the center of nervous power, or to receive any impulse from it; but they never contract spontaneously in those cases; it is always by the immediate application of a stimulus either to their nerves or to their immediate substance. If, for instance, after having cut off a thigh from

(*a*) Considerations sur les corps organisés. 1779. 2d part. p. 106.—Palingénésie, tom. 1, p. 83—92.

a living frog, we irritate either one of the nerves or one of the muscles of that thigh, contraction is produced; in the first case in all the muscles which receive branches from the irritated nerve, in the second only in the irritated muscle. All the other muscles which have not been immediately touched, or the nerves of which have not been irritated, remain at rest. It is quite different in a decapitated frog: here is no necessity in order to excite motion to touch either the muscles or the nerves; it is enough to touch a part of its skin to produce motion, and it even moves itself spontaneously and without irritation. The phenomena presented by the thigh of the frog, are what are commonly called the phenomena of irritability; they are constantly observed for a longer or a shorter time after death. Those observed in the decapitated frog belong to life, and always suppose the existence of the principle that produces the power of feeling and moving. In short, there is such a difference between those phenomena, that we have reason to be surprised they should have been confounded.

The theory of irritability produced no change in the state of the question, and the difficulties which I have mentioned, continued in full force from the time Haller and the authors of his school referred the sources of nervous power to the brain (*a*). Among the experiments of those authors, one was nevertheless well calculated to make them renounce this opinion. I mean that made by the celebrated Fontana. After having decapitated rabbits and guinea-pigs, and prevented the hemorrhage by the ligature of the vessels of the neck, he had maintained the life of those animals for a long space of time by blowing air into their lungs (*b*). This experiment proved clearly that even in the mammiferous adult as well as in reptiles, the life of the trunk does not depend immediately on the brain. After this, only one step remained to be taken; to inquire what was the true source of that life, and to ascertain this source by real experiments. But Fontana did not pursue his experiments further, be-

(*a*) Elemen. Physiolo. tom. IV. p. 393.

(*b*) Traité sur le venin de la vipère sur les poisons Americains et sur quelques autres regetaux. Florence, 1778, tom. I. p. 317.

cause he thought he understood the *rationale*. Strongly prepossessed by the doctrine of Haller, which he extended much further than that great man, he placed the source and the principle of life and of all animal motion in irritability (*a*). The inflation of the lungs was only, in his estimation, one mean of prolonging life in a decapitated animal, only because it contributed to the maintaining of irritability by keeping up the circulation, which was, as he pretended, independent of the nervous power (*b*). Accordingly in speaking of his experiment, he says, that respiration, and the circulation of the fluids in the parts, are adequate to this purpose. So far from his ascribing life, in this case, to the nervous power, his object in decapitating animals, was to cause them to be bitten immediately by vipers, to show that the nerves have no agency in the effect produced by the bite: an evident proof that he considered the source of this power exhausted after decapitation (*c*). It will occasion no surprise

(*a*) Traité sur le venin, &c. Tom. I. p. 81, 90, 93, 289.
(*b*) Ibid. Tom. II. p. 169—171.
(*c*) Ibid. Tom. I. p. 291—9.

after this, to find that he assimilates the bites of the viper inflicted on decapitated animals, with that on a single limb, when separated from the body (*a*).

Considered in this point of view, this experiment therefore left the great question of the exact seat of the principle of life, as undecided as before; and it appeared to be only a confirmation of what was already known. Hence it made on the public no more impression than its author (*b*). At length in proportion as the influence of the nervous power was acknowledged, lately merged in the doctrine of irritability, men of enlightened understanding perceived that only in a new modification of the theory of this power, could a solution of the difficulties be expected. Many united in an opinion that the

(*a*) Traité sur le venin de la vipère, &c. tom. I. p. 317.

(*b*) I had no knowledge of this experiment, when about seven months ago I had communicated mine to the society of the faculty of medicine of Paris, and two months after I had repeated them publicly before the society; M. Majendie, then occupied with his work on the vegetable poisons of the Indians, having had occasion to consult Fontana, found and indicated it to me.

brain could no longer be considered as the exclusive seat of this power. In support of this opinion, well established facts were sufficiently numerous and conclusive. But the moment new boundaries were to be assigned, it happened as is usual on such occasions, when facts are not so directly in point as to assume the character of proof, but readily admit of various interpretations. Each author advanced his own hypothesis, and extended or contracted the limits of the nervous power, according to the light in which he viewed these facts (*a*).

A satisfactory theory was scarcely to be expected from merely combining facts hitherto established. New experiments calculated to throw new light on those facts, and to link them more closely by supplying the deficiences, were still required. We have remarked that one of the greatest difficulties was to reconcile the facts observed in cold-blooded with those observed in the warm-blooded animals in the adult state. We have likewise remarked that fœtuses

(*a*) See among others the works of MM. Platner, Reil, Bichat, Prochaska, Scarpa, Gall.

exhibit a character very analogous to that of cold-blooded animals. In fœtuses therefore the bond was to be sought which would unite the phenomena presented by cold-blooded animals, and the adult mammiferous submitted to the same experiments. There was some hope of finding it on the one hand by studying every circumstance of analogy observed in those experiments between the reptiles and the fœtuses of the mammiferous animals, and on the other by pursuing the inquiry as to those circumstances, in the same fœtuses, as they grow up to the age of adults. Such, at least, is the plan which reflection seems to suggest. It is that which I have pursued. But I must acknowledge, at the same time, that it was chance rather than reflection which furnished me with the idea.

SECTION I.

A FEW years ago a particular case of parturition, which took place within my observation, excited a wish to know how long a fœtus of mature birth may live without breathing, after the communication with the mother has ceased, from whatever cause. In vain I sought for information in authors. I only found contradictory opinions founded some upon facts either inaccurate or too carelessly recorded, others founded on mere speculation. From that time I resolved to consult nature alone, by instituting a series of experiments upon animals. And first, as the separation of the fœtus from its mother is accompanied by a variety of circumstances which may affect the existence of the fœtus, I reduced those cases to three heads: 1st. Where both the fœtus and the mother may be considered as in a state of health. 2d. Where the mother has sustained some injury. 3d. Where the fœtus itself has been the subject.

Among the accidents which may happen to the fœtus, the cause of its death by the preternatural delivery by the feet, was an object of particular inquiry. It is well known, that in cases where midwives are obliged to turn the child, and to deliver it by the feet, if the pelvis is rather narrow, the child generally perishes. Its death is commonly ascribed to the injury of the spinal marrow. It is certain that the violence practised in such cases is considerable, sometimes even to the extent of separating the head, which has remained in the uterus. In examining all the circumstances of the case in question, and submitting them to direct experiments on animals, I did not confine myself to determining the cause of death, when the fœtus had not been decollated; I wished to know besides, in what manner, and by what alteration in the function, decollation caused its death. The hemorrhage no doubt contributes much, but as it is not generally to hemorrhage we ascribe this event, and as we consider the sudden annihilation of all the functions of the brain as the cause, it is evident that the hemorrhage must be prevented, which is always more or less practicable by tying the vessels of the neck, and that the true point in question consisted in determining in what manner the cessation of all cerebral influence produces death in the trunk.

My first experiments were made upon rabbits at the time that the fœtuses, separated from their mothers, can live without breathing; and on the same animals, I continued the investigation of the phenomena accompanying decapitation. I remarked first, that, after the decapitation of a rabbit, life continues in the trunk, and that sensation and voluntary motion continue during a period which is constantly the same, as when a rabbit of the same age is reduced to a state of asphyxia. By producing asphyxia in rabbits of different ages, for instance every five days, from the moment of birth until the age of one month, it is constantly observed, that the duration of sensation and of voluntary motion, in short of all the signs of life, diminishes in proportion to the age of those animals. Thus, in a new born rabbit, sensation and voluntary motion are only extinguished after an asphyxia of fifteen minutes; whilst they are extinguished in two minutes in a rabbit thirty days old. Hence, by decapitating in the same way rabbits every five days, I found that the duration of these phenomena decreased from age to age, according to the same rule as in asphyxia. But there was this difference, between the animal decapitated and that reduced to a state of asphyxia, that the latter makes efforts to breathe; each of these efforts, characterized by the contraction of the diaphragm, and the raising of the ribs, is accompanied with a gaping. These gapings, and those movements of the thorax, which grow weaker and weaker as the asphyxia is protracted, are the last signs of life which are observable; and they always subsist more or less after the cessation of sensation, and of voluntary motion. In the decapitated animal, on the contrary, all the inspiratory motions of the thorax are annihilated at the very instant of decapitation: the head alone continues gaping in the same manner as in asphyxia. If, instead of decapitating the animal, the spinal marrow be separated between the occiput and the first vertebra, the phenomena are the same as after decapitation; that is, all the inspiratory movements of the thorax cease at the instant, and the head preserves the gapings of asphyxia. In short, whether after decapitation or after the section of the spinal marrow near the occiput, gaping is the only remnant of the inspiratory movements; they are the marks of ineffectual efforts made to breathe; a very remarkable phenomenon, which will be of great use to me hereafter, by always considering gaping as the representative sign of inspiratory movements.

From the connection of these facts, I conclude that the decapitated animal is only in a state of asphyxia, and that he is so because he can no longer perform the motions necessary to introduce air into his lungs. Substituting the artificial inflation of the lungs instead of natural respiration, affords a ready means of ascertaining the correctness of this conclusion. Accordingly I made the experiment, and the success was complete. To succeed, it is not even necessary that the lungs should be inflated immediately after decapitation. This was not performed until sensation and voluntary motion had ceased; these will soon be restored, and be strongly characterized; and if the inflation be interrupted, they are again enfeebled, at last disappear entirely, and the animal appears dead; but they re-appear and with the same intensity if the inflation is renewed. I repeated this experiment with the same success on dogs, cats, and guinea-pigs. In a word, a decapitated animal may in this manner be kept perfectly alive, during a time varying according to the age and species, and which in very young rabbits is at least several hours.

It evidently results from these facts that the principle of sensation and voluntary motion does not reside in the brain, according to the usually received opinion, or at least, that the brain is not its exclusive seat. But where then is the seat of this principle? Is it peculiar and circumscribed, or is it disseminated over every part of the body? The following experiments soon convinced me that it resides only in the spinal marrow. Thus if in a decapitated rabbit, which has been most completely revived, and which is kept alive by the inflation of the lungs, the medulla spinalis be destroyed, by pushing an iron rod through the whole length of the vertebral canal, all the phenomena of life instantly disappear, nor is it possible to restore them by any means; only those of irritability are left, which it is known always continue some time after death. If another rabbit be taken, and instead of decapitating it, a simple incision be made in the vertebral canal near the occiput, and an iron wire be introduced through this opening, destroying the spinal marrow, although in this case the brain continues untouched as well as all its nervous communications with the trunk, life will nevertheless instantly and irrevocably disappear in the trunk; it only continues in the head, as is indicated by the gapings.—Finally, if another rabbit be cut in two, each portion as well as the head in the preceding experiment, remains alive for several minutes, varying according to the age of the animal which I shall point out hereafter. If immediately after the division has been made, the whole medulla spinalis is destroyed, in either of these portions, life instantly disappears in it, whilst it continues in the other, and if this portion of the spinal marrow be destroyed, all the parts which receive their nerves from this portion, are struck with death at the same time, while the remainder of this same portion continues alive.

These experiments prove not only that the life of the trunk is dependent upon the medulla spinalis; but they also prove that the life of each part is specially dependent upon the portion of the spinal marrow, from which it receives its nerves. Besides, it is easy to demonstrate that the source of sensation, and of all the voluntary motions of the trunk, exclusively belongs to the spinal marrow, and that none of the viscera of the chest or abdomen have any immediate share in it. For if you open the thorax or abdomen of a rabbit, and tear out the heart, the lungs, the diaphragm, the intestines, all the viscera of these two cavities, it will continue alive after this cruel operation; and besides, if you cut off its head although then reduced to its skeleton, to its spinal marrow and muscles, it is still living; but if partially or entirely you destroy the spinal marrow, it is immediately struck with death which is either partial or general.

It is certain therefore that the life of the trunk, has its immediate principle neither in the brain nor in any of the viscera of the breast or abdomen; but it is no less evident that all those viscera are indispensable to maintain it. Now considering the relations in which they stand, the foregoing facts evidently prove, that as to the cerebrum, the mechanical phenomena of respiration, that is, those movements by which an animal introduces air into its lungs, are produced by the immediate action of that organ. Thus, it is principally in as much as the maintenance of life depends upon respiration, that it is dependent upon the brain; and here arises a great difficulty. The phrenic and all the other nerves of the muscles employed in the mechanical phenomena of respiration, arise from the spinal marrow in the same manner as those of the other muscles of the trunk. How happens it then, that, after decapitation, respiration alone is destroyed, while all the other motions continue? This is in my opinion one of the great mysteries of the nervous power, a mystery that will be discovered sooner or later; and the discovery of which will throw the greatest light upon the mechanism of the functions of this wonderful power (*a*).

But whatever be the organic arrangement through the medium of which the mechanical phenomena of respiration are produced by the brain, this dependency is unequivocal, and it is further ascertained that it is through the spinal marrow that it is exercised. For, as I have already observed, if the spinal marrow be simply divided near the occiput, the animal is exactly in the same state as if its head had been cut off.

It is not upon the whole of the brain that respiration depends, but upon a particular and small part of the *medulla oblongata* situated at a small distance from the occipital foramen, and near the origin of the nerves of the eighth pair,

(*a*) Some facts observed in the course of my experiments, lead me to believe that the accessary nerve of the eighth pair, acts a principal part in this state of dependency in which respiration is found to be upon the brain. This nerve has a singular distribution and course which undoubtedly have a relation to some use which no one has hitherto been able to elucidate.

(*or pneumo-gastric*). For if we open the cranium of a young rabbit, and extract the brain in successive portions, beginning by the anterior part, cutting it into slices, we may then take away the whole cerebrum, then all the cerebellum, and part of the medulla oblongata, without stopping respiration. But it instantly ceases when we cut the origin of the nerves of the eighth pair.

Hence an animal might be decapitated in such a manner, that it should continue to live by its inherent powers, without recourse being had to the artificial inflation of the lungs. For this purpose, our instruments are to be so managed in removing the cranium and the brain, as to spare that particular portion of the medulla oblongata in which the primum mobile of respiration resides, preserving at the same time its continuity with the spinal marrow.

But it is evident that this part only maintains respiration in proportion as it continues in the full possession of its functions, which is admitting that it continues nearly in a healthy state. Now in warm-blooded animals the volume and the number of vessels opened in this operation occasion a hemorrhage which soon impairs the circulation in the stump of the *medulla oblongata;* to which we must add, that in those animals large wounds have an immediate and great influence upon the surrounding parts, which must soon reduce the stump to a state of derangement incompatible with its function. For this reason, the experiment is attended with success, only when made upon very young animals, during a time which does not exceed half an hour, or even less. In other respects, the success is in no ways doubtful.

It is not so with cold-blooded animals. In these the most considerable mutilations have for the most part only limited effects; the hemorrhages they occasion are moderate and of short duration; and the wounds produced by them are easily healed. Another circumstance which is peculiar to them is the great length of time they can live without taking any food. For this reason they have the power of living a long time after decapitation. The observation has been made for upwards of a century, and since Redi, who saw tortoises live upwards of six months after he had taken away their brains, several analogous facts have been noticed (*a*). But, as I have already observed, no one was acquainted with the theory of it. No one knew the residence of the principle of that life, which appeared so surprising, and which was thought to belong only to that class of animals. Nor did we know which were those functions, the preservation of which maintained the existence of this principle. In short, it does not appear that it has been remarked that every species of decapitation does not produce the same effect, and that the duration of life depends, in a great measure, upon the mode in which the animal has been decapitated. Having convinced myself that in those animals life also depends upon the spinal marrow, and in the same manner as in the warm-blooded, it appeared to me certain, in applying to them the result of my experiments upon rab-

(*a*) This observation had perhaps been made upon reptiles before Redi. What is certain is that it had been made upon insects for a long while. It was known in the time of Aristotle, that those animals can live without a head. (Aristotelis opera omnia, 1654. tom. II. p. 131.)

bits, that they could not live a long time after decapitation, unless they preserve the power of breathing; whence I conclude, admitting that this power had also its principle in a limited portion of the spinal marrow, that the condition requisite for their living in this manner was, that in decapitating them, that portion should remain untouched, and that if it were taken off with the head, they would only survive for the time during which they can bear asphyxia. Upon salamanders I tried in a particular manner to ascertain the truth of these conclusions. I have decapitated a great number; several have outlived this operation three or four months, and, if I judge by their excessive leanness at the time of death, they only died of inanition. I have constantly remarked, that the decapitation in these cases was made by removing the cranium above the occipital foramen. On the contrary, when the decapitation took place lower down, and on the superior vertebra, they lived a much less time. Nevertheless I ought to state, that the time they have lived was always longer than that in which they can bear a total privation of air; but that depends upon that portion of air they can breathe through the skin, as I shall prove in another place, and consequently it is so far ascertained, that, even in those cases, they only live a long time because they continue to breathe.

As the inflation of the lungs supplies the place of natural respiration, and the animals decapitated in such a manner that natural respiration may continue, may live until they die of inanition, it would appear, that the inflation of the lungs might keep up life for a long time in a warm-blooded animal, decapitated in any manner. But it is to be remarked, that not only the mechanical phenomena of respiration depend upon the brain, but the peculiar functions of the lungs are also produced by it through the nerves of the eighth pair; and it appears that both depend upon the same part of the brain; for, as we have already seen, the place in the medulla oblongata where the primum mobile of the mechanical phenomena of respiration resides, includes the origin of the nerves of the eighth pair. Now we know that the section of those nerves without any other wound, kills animals more immediately than abstinence. We see therefore, that besides the other causes accelerating death in a warm-blooded animal after decapitation, the maximum of time life may be continued by inflating the lungs, is the same as that which it might live after the section of the nerves of the eighth pair; and that an animal can continue to live after decapitation until it dies of abstinence, unless it continues to breathe without artificial aid. Without entering into any further details, what I have stated appears to me sufficient to establish the reason why the brain is indispensable to the support of life, because it encloses the the primum mobile of respiration. I shall, on another occasion, inquire whether it exercises any other influence on life. I say life, and not its operations, for the latter unquestionably are determined by the brain.

As to the viscera of the abdomen and of the thorax, it is evident that their use is confined to the formation and circulation of the blood. Those of the abdomen serve to prepare the materials fit for repairing the waste which this fluid continually undergoes by the various secretions. The lungs impart to it the arterial character, and the heart distributes it to all the parts. The inflation of the lungs practised on decapitated animals is to be considered as a condition necessary for the formation of arterial blood. But what relation, what connection is there between life and the arterial blood, when once formed, and circulating in the vessels? It is certain that life does not reside in the blood, as has been often stated, and that circulation does not essentially constitute it, since sensation and voluntary motion continue after the heart has been removed, and the general circulation has stopped. But it is not less certain that life, which still subsists when the blood no longer circulates, or has lost its arterial qualities, has only a more or less limited duration. Hence it appears a fair conclusion, that life results from the impressions of the arterial blood on the body. But we have seen, that the brain and the spinal marrow are the sources of sensation, of motion, in short of every thing which constitutes life. It may, therefore, be asserted, that general life, the existence of the individual, is produced by a certain impression of the arterial blood upon the brain and the spinal marrow; an impression which, when once made, has always a duration which is longer or shorter, according to the species and the age of the animal; so that life can only be maintained by the continual renewal of this impression, nearly in the same manner as a body moved by the power of a first impulse cannot continue to move indefinitely, unless this impulse is repeated at intervals. If this be the case, whenever this renewal is interrupted in any portion of the spinal marrow, life, after having continued for a longer or shorter time, yet determined according to the age and species of the animal, in those parts receiving their nerves from this portion of the medulla, must be entirely extinguished. This is what we in fact observe, when we tie the aorta of a rabbit towards the posterior part of the breast, or the anterior part of the belly. Sensation and movement subsist at first in the posterior part, but they weaken by degrees more and more, and soon after wholly disappear.'

SECTION II.

Such are summarily the principal facts which I published in 1809. The inference drawn from them was, that the maintainance of life, in any part of an animal, essentially depended on two conditions;—one, the integrity of the corresponding portion of the spinal marrow, and of its nervous communications;—the other, the circulation of arterial blood in this part; and that consequently, it was possible to keep alive any part of an animal, as long as these two conditions remained in force. For instance, that one might keep alive all the posterior parts alone of the body of an animal, after having effected the death of all the anterior parts, by the destruction of the spinal marrow corresponding with them; or the anterior, after having in the same manner occasioned the death of the posterior; or, in short, the middle part after having destroyed the anterior and posterior parts of the spinal marrow.

It became an object of inquiry whether this inference would be confirmed by direct experiments. My first attempt was on a rabbit twenty days old. Having introduced an iron probe into the vertebral canal of the rabbit, between the last dorsal and the first lumbar vertebra, destroying all the lumbar portion of the spinal marrow, the hind parts were instantly deprived of sensation and movement; but all the rest of the body was perfectly sensible, and respiration went on as before. This state was but of short duration: one minute after, the animal manifested signs of anxiety. Its fore-legs were convulsed. A minute and a half after, respiration was stopped, and was soon after succeded by occasional gapings, accompanied by very feeble efforts in the chest to inspire, and which wholly ceased in three minutes and a half after, when neither sensibility nor any other sign of life existed. This experiment, repeated upon two other rabbits of the same age, was attended with the same result. In one, respiration ceased in a minute, and life in three minutes after; in the other, it stopped in a little more than a minute and a half, and it died in four minutes. I tried to prolong the life of the last by blowing air into the lungs. I began the inflation before sensibility and the gapings had ceased; but these phenomena disappeared quite as soon as if I had done nothing. I have often since repeated the same experiment in similar cases, but always in vain—death is irrevocable.

This unexpected result surprised me the more, by the comparison I made with what I had observed on rabbits of the same age, after decapitation. At the age of twenty days, and much later, the inflation of the lungs will readily keep up life in decapitated rabbits. How happens it then, that they could live after the loss of the whole cerebrum, and that the destruction of only the lumbar portion of the spinal marrow should cause death so quickly, that it was impossible to prolong life one instant? This extraordinary fact could not be explained by any received theory. Moreover, I perceived no means of reconciling it with the conclusions which I had drawn from my former experiments. There must have been some error in the experiments themselves, as the conclusions I had drawn from them were incorrect; or the destruction of the spinal marrow, though partial, produced suddenly in the functions, essential to the maintainance of life, some disorder till then unknown. The experiments had been so often repeated with the same results, that no doubt remained of their accuracy. As to the conclusions I had drawn, they were, strictly speaking, only the general expression of the facts which I had observed; at least, I could view them in no other light. I therefore began to think that the destruction of a portion of the spinal marrow, causes, in the functions which are essential to the maintaining of life, great and essential alterations, which immediately became the object of my inquiry.

I began by ascertaining whether the destruction of the two dorsal and cervical portions of the medulla, performed upon rabbits twenty days old, would produce the same effect as that of the lumbar portion.

I destroyed the dorsal portion of the medulla spinalis, by introducing between the first lumbar and the last dorsal vertebra a probe, which I pushed as far as the last cervical vertebra. The destruction was scarcely finished, when respiration became laborious, slow and accompanied with gapings. All the middle part of the body was dead; the fore and hind parts were living, but sensation was extinguished one minute and a half after; and the gapings as well as the contractions of the diaphragm ceased two minutes after. This experiment, often repeated, always afforded the same result. I again had recourse in this case to the inflation of the lungs, but without any success.

To destroy the cervical portion of the spinal marrow, I introduced an iron probe between the occiput and the first vertebra. The destruction of this portion of the medulla, differs from that of the two others in this, that it annihilates all the inspiratory movements of the thorax and only leaves the gapings, which as I said before are its representative signs. Allowing this operation not to be essential and suddenly mortal, it follows that an animal could only survive it by means of the inflation of the lungs. This I performed with the greatest care but ineffectually. Sensibility and the other signs of life which subsisted in all the posterior parts below the shoulders, disappeared in a minute and a half, and the gapings ceased two minutes after. This experiment was frequently repeated and always with the same result.

The result was, that the destruction of any one of these three portions of the spinal marrow is mortal in rabbits of twenty days; that death is sudden after the destruction of the dorsal and cervical portion; and that it only takes place one minute or one minute and a half later, after the destruction of the lumbar portion. I have met with no exception in this respect, except in regard to the destruction of the lumbar portion of the spinal marrow, few individuals being able to survive it. But there are none who do not die immediately, if, at the time that you destroy the lumbar portion, you also destroy that which corresponds with the two or three last dorsal vertebra.

The question was to ascertain, whether it would be so at every other age. The repetition of the same experiments upon animals of different ages, is calculated to throw great light on many physiological questions. I found that in general, the destruction of the lumbar portion of the spinal marrow does not occasion death in rabbits, before they are ten days old. At fifteen days, a few are found to survive; but the most part perish. When twenty-five and thirty, the same happens as at twenty. When I say that the destruction of the lumbar portion of the spinal marrow does not kill very young rabbits, I do not mean to say they recover. I only mean to say that they do not die suddenly, as those of twenty and more days, but only at a more distant or longer period; a distinction which ought never to be forgotten. Death which takes place suddenly, being evidently due to the immediate action of the spinal marrow upon the powers maintaining life, presents a simple question, which is readily submitted to direct experiments; whilst that death which only happens after a certain lapse of time, is connected with a chain and complication of causes foreign to my present purpose.

The destruction of the dorsal portion of the spinal marrow, is not always mortal in very young rabbits. Many survive it at the age of ten days. But it always kills them at any period beyond fifteen.

When the cervical portion of the spinal marrow is destroyed, most of them die even when the experiment is performed the first day after birth. It is true that for ten days after, the inflation of the lungs may prolong the life of some, but generally it is only for a short time, and the signs of life manifested are weak.

Finally, the simultaneous destruction of the three portions, is certainly mortal at all ages; and the head, which in this case remains alone alive, and continues gaping, is only so for a determined time, which it is impossible to prolong.

All these facts concur to prove, that any one portion of the spinal marrow exercises on life two modes of action which are very distinct. In one, it constitutes life essentially in all the parts which it supplies with nerves. In the other, it contributes to the maintainance of it in all those which receive theirs from the spinal marrow. For instance, when you destroy the lumbar portion, in a rabbit of twenty days, it is according to the first mode of action, that life is instantly destroyed in the hind part; it is according to the second, that it only lasts three minutes in the rest of the body. The first mode of action is uniform in all ages and species. We have just seen that the second varies according to the age in such a manner, that general life more intimately depends on the same portion of spinal marrow, when the animal is a little older than when it is very young. I may add that there is also a difference in this respect, according to the species.

The whole resolves itself into this question: In what consists that kind of action which every portion of the spinal marrow exercises on the life of the other parts? Now my preceding experiments having led me to admit only two of the above mentioned conditions, as being indispensable for the maintenance of life, in any one part of the body, namely, the integrity of the corresponding portion of the spinal marrow, and the continuance of circulation, it was difficult to comprehend how the destruction of one portion of the spinal marrow, could interfere with either of these two conditions.

One consideration appears likely to determine the condition, which respects the integrity of the portion of spinal marrow not destroyed; it is this, that if the destruction of the lumbar portion of the spinal marrow, in a rabbit of twenty days, for instance, impaired the integrity of the rest of the spinal marrow, so far as to cause an almost sudden cessation of its functions, the same effect must be produced at all ages; and we have seen that this was not the case. A direct experiment completely removed all doubts in that respect. This experiment consists in cutting transversely the spinal marrow between the last dorsal and the first lumbar vertebra of a rabbit at least twenty days old. After this operation, sensation and voluntary motion continued to exist in the hinder part. But there is no further connection of sensation, nor of motion, between the anterior and the posterior parts, as soon as the section of the spinal marrow is performed; that is to say, that if you pinch the tail, or one of the hind paws, all the hind part is agitated, but the fore part appears to feel nothing of it, and does not stir. If *vice versa*, you pinch an ear, or one of the fore paws, the anterior parts are agitated, but the posterior remain at rest. In a word, the section of the spinal marrow has evidently established, in the same animal, two centers of sensations very distinct and independent of one another; it might even be said, two centers of will, if the motions made by the hind part when pinched, evince the will in the animal to avoid the body that hurts it. The want of connection which takes place between the anterior and the posterior parts is as perfect as if, instead of simply cutting the spinal marrow, you had cut transversely all the rest of the animal at the same place. Therefore, when fifteen or twenty minutes after the section of the spinal marrow, the animal being perfectly alive, you destroy the posterior segment, that is, the lumbar portion, if you avoid touching with a probe the extremity of the anterior segment, the hind part is strongly convulsed, while this destruction is performed, though the fore part remains motionless and appears not affected by it; which does not prevent life from being wholly extinguished about three minutes after. It is evident that, in this experiment, the destruction of the lumbar portion of the spinal marrow kills the animal, although it does not affect in any way the anterior portions.

It remained to examine the second condition, that is, whether the general circulation is disordered or suppressed by the destruction of the spinal marrow. If it were, it could only be because the motion, or at least the power of the heart, have their source in this spinal marrow. This was very embarrassing, from its militating against the theory, apparently the best established, respecting the causes of the circulation of the blood.

This theory, as is well known, is that of Haller. It is grounded on the principle, that the motions of the heart are independent of the nervous power, and that they have their cause in irritability, a property essentially inherent in all the muscles, but which the heart has in a higher degree than any other. Irritability only gives to the heart a power to contract with a suitable degree of force: there is also required a cause to set this power into action; a stimulus, the presence or absence of which may determine or suppress the contraction. This stimulus is the contact of blood on the internal surface of the heart. When the two auricles are filled with blood, their fibres, irritated by the presence of this fluid, contract themselves and force it into the ventricles, which being in their turn irritated by this same blood, are alike contracted, and force it in the same manner into the arteries. Relaxation succeeding to contraction after the expulsion of the stimulus, the auricles are immediately filled again with new blood poured in by the veins, a second contraction is produced, which again transmitting the stimulus into the ventricles, produces another in these. The same causes being constantly re-produced in the same manner, the alternate motions of the auricles and of the ventricles of the heart, and of course the circulation, continue during life. Such is the theory which has been adopted for upwards of half a century, and which still prevails in books, as well as in schools, in spite of the frequent attacks it received.

Thus directly led, by my experiments, to call in question the correctness of a theory, so superior to any hitherto proposed, for explaining the admirable constancy and harmony of the motions of the heart, I began to examine its principles with more attention than I had hitherto done, and I soon perceived that it was not without reason it had been so briskly attacked at various times.

Apprehensive lest I should swell this memoir, if I should relate and discuss in this place all the facts on which this theory is founded, I shall confine myself to merely pointing out two, which I select in preference, not only because they belong to those which have been considered as the most important, and thus require no digression, but because the proofs which have been adduced may be estimated from the simple experiments. The first of these facts is, that the motions of the heart are not produced by the brain. Haller insisted much upon this fact, and sought to multiply the proofs of it (*a*). It is certain, that the brain being considered by this illustrious author, and by all those who have followed him, as the only cause of nervous power, to prove that the motions of the heart are independent of the brain, was to prove that they are not produced by that power. But it is evident, that this proof vanishes, as soon as it is established that the brain is the only source of nervous power.

The second fact is this: if you take the heart of a living animal and lay it on a table, although it be by this separation free from the action of the nervous power, its diastole and systole still continue, and sometimes even for a considerable length of time after. This fact is well established. But the question is, whether these motions are sufficient to maintain the circulation, and whether they preserve the force requisite for that purpose: it does not appear that to this sufficient attention has been paid. Now, the

(*a*) Elem. Physiol. tom. 1. lib. iv. sect. v.

chain of my experiments and of my ideas, leads me to examine this very question, in order to ascertain whether the circulation is dependent upon the spinal marrow: for if the destruction of the spinal marrow stops this function, it can only be in two ways; either in causing a cessation of the motions of the heart, or by weakening them. But as the motions continue, even after the heart has been removed, it was a fair presumption that they would also continue after the destruction of the spinal marrow; and this is what does happen, as can easily be proved, by opening the cranium of an animal of any age whatever, and by introducing through the opening a probe and thus destroying the brain and all the spinal marrow: if the thorax of the animal be then opened, the motions of the heart will be found still to continue. If then, notwithstanding these motions, the circulation be stopped, it is owing to the want of force to maintain it; and consequently, whatever be the motions of the heart which continue after the total or partial destruction of the spinal marrow, the question to be resolved is, whether this destruction has for its immediate effect the stoppage of the circulation.

This question appears very simple, and it seems that nothing is more easy than to ascertain whether or not the blood circulates in the vessels. But when we attempt the experiment, it is found in certain cases very complicated. All the difficulty lies in finding out by what signs we may know that the circulation is stopped. The absence of hemorrhage, when we separate a large artery, or when we amputate a limb, appears to be one of the most certain. It is so in fact; but, in general, it is only when animals are somewhat advanced in age. When they are very young, and when the foramen ovale is not yet closed, the hemorrhage is a doubtful sign of the state of the circulation. Thus we conceive that, at that age, the amputation of a limb, of a thigh for instance, may cause a hemorrhage more or less profuse, and yet the circulation is not continued: for the motions of the heart which, as we have already seen, always continue a certain time after death, have a certain force; and although this force is not sufficient to keep up circulation, that is to say, to force the blood from the arteries into the veins, it may nevertheless be sufficient to force it out through the opening of a large artery. The venous blood, which, after death, is constantly accumulated in the right cavities of the heart, passing into the left cavities through the foramen ovale will keep up the hemorrhage as long as the motions of the heart preserve any force. It should, however, be observed that, in those cases, though the hemorrhage has taken place from an artery, it only affords venous blood, and consequently of a black colour. In this respect the hemorrhage itself furnishes a very important sign of the state of the circulation.

This sign is inferred from the colour of the blood. Wherever the arterial blood does not become scarlet, and the arterial hemorrhage continues of a dark colour, during the inflation of the lungs, (which I suppose to have been very carefully made) it is a sign that the circulation has ceased. But this very rule is liable to some exceptions, which depend upon the foramen ovale, or upon the relative force of the right ventricle of the heart. When the circulation, without being stopped, is considerably weakened, and only a small quantity of blood passes through the lungs, this small quantity of blood, by being mixed in the left auricle with a much larger quantity, poured in by the right auricle through the foramen ovale, loses almost entirely its scarlet colour, and blood nearly black passes into the aorta. It is therefore, in very young animals only, that those exceptions take place. But they may be observed in guinea-pigs when much older, because in those animals the foramen ovale remains of considerable size till the adult age. As to the exceptions which have their cause in the right ventricle of the heart, I have only found them in new born cats; I intend to publish them in a more particular manner, in a memoir upon the obliteration of the ductus arteriosus.

The inspection of the carotids also furnishes signs intitled to great attention, and which are drawn from the fulness and colour of those arteries. I say the colour, for the transparency of the coats of those arteries in young animals, such as those upon which I performed my experiments, renders it easy to distinguish, by mere inspection, the shades which the colour of the blood may assume, and which is very convenient. When therefore, the carotids are full and round, acquiring a bright scarlet colour on the lungs being inflated, then resuming their dark colour when the inflation is interrupted, and becoming scarlet on its being renewed, there is no doubt that the circulation continues. On the contrary, it is certain that it is stopped when those arteries are empty and flattened, and when, in those cases in which they still contain a little blood, this blood does not change its colour by the inflation of the lungs. I shall say on that subject, that this condition of the carotids is one of the most certain and immediate signs that we can have of the death of an animal; one of the most immediate, since it may be ascertained the instant the circulation is stopped, and while the heart still continues to throb. In a great number of experiments upon asphyxia, I never could recall animals to life, whenever the asphyxia had been protracted till the carotids were empty and flattened, although the throbbings of the heart were sometimes felt through the parietes of the thorax. But when the animals are very young and very small, the carotids being of themselves very small, and possessing at this age a strong contractibility, it is not always easy to ascertain whether they are empty and flattened, or only contracted and straitened, in consequence of the weakened circulation.

All these signs present therefore some degree of uncertainty at an early age: this uncertainty is more particularly remarked in certain species than in others. In dogs, and especially in cats, under the age of five days, it is sometimes very puzzling. Fortunately it scarcely takes place in rabbits, and it may be said in general that the presence, the colour, or the absence of hemorrhage, the fulness, the colour, or the emptiness of the carotids, sufficiently indicate whether circulation is or is not stopped in those animals at any age.

Otherwise, there is never any such uncertainty, unless when the object is to ascertain the instant when the cessation of the circulation takes place after the destruction of the spinal marrow. For when the circulation is easily stopped, the hemorrhages and other circumstances which might at first render it doubtful, soon after indicate it, by their disappearing. They are, in this case, but of short duration, while they continue, or they may be protracted a much longer time, even when the circulation goes on very languidly. Nevertheless, as the precise moment when circulation stops was an important point to be determined, I wished to obtain some other sign, which might be equally applicable, without a possibility of doubt, to all species and ages. In my preceding researches on the decapitation of rabbits, I had observed that the head when separated from the body continued gaping during a time which varied according to the age of those animals, but which was nearly uniform in animals of the same age. I had also observed, in my experiments on the spinal marrow, that after its total destruction, the gapings, the only remaining signs of life, had evidently at the same age the same duration, as in the heads severed from the body, without being able to protract it any longer. It was very evident that there was no circulation in the heads separated from the body, and, consequently, that gaping could only continue while life continued in the brain, after the circulation had ceased. This first occasioned a suspicion, that the destruction of the spinal marrow suddenly stops this function. By a recurrence to those facts, I concluded that it must be with the rest of the body as with the head; that is, that life, and the signs that manifest it, ought equally to have in the trunk a determined duration, according to the age, after the cessation of circulation, and that it might be possible to obtain a pretty certain indication, applicable to all cases, not only of the cessation of the circulation, but also of the period when it had taken place. For that purpose it was sufficient to stop the circulation suddenly and completely, in a certain number of animals of different ages; then to note carefully the duration of the various signs of life, and to draw up a comparative table of the duration of the same phenomena, in animals of the same species and age, in experiments which might be supposed capable to stop the circulation. I had recourse to this process, and it appeared completely to answer my purpose.

The most certain method of suddenly stopping the circulation, is to tie or divide the heart, at the basis of the large vessels. I performed both operations upon rabbits every five days, in the first month after their birth, and carefully noted down the duration of the gaping, and of the sensibility, for every age. The following table contains the result of those experiments. I make no difference between the ligature and the excision of the heart, because it appeared to me, that the effects of both were perfectly alike, when they had been made at the same interval of time from the opening of the thorax; an interval which ought not to exceed one half minute. I also add two other tables, which exhibit the duration of the same phenomena, in the asphyxia by simply opening the thorax, and also in the asphyxia by submersion.

TABLE

Of the duration of the gapings, and of that of the sensibility of rabbits of different ages.

1st. After the extraction of the heart.

AGES.	SENSIBILITY.	GAPINGS.
Days.	Minutes.	Minutes.
1	14	20
5	6	9
10	$3\frac{1}{3}$	4
15	$2\frac{1}{3}$	$2\frac{3}{4}$
20	$1\frac{1}{3}$	$2\frac{2}{3}$
25	$1\frac{1}{4}$	$1\frac{1}{2}$
30	1	$1\frac{1}{3}$

2d. After the opening of the thorax.

AGES.	SENSIBILITY.	GAPINGS.
Days.	Minutes.	Minutes.
1	16	30
5	12	16
10	$5\frac{1}{2}$	$7\frac{1}{2}$
15	$3\frac{3}{4}$	$5\frac{1}{2}$
20	$2\frac{3}{4}$	$3\frac{1}{2}$
25	2	$2\frac{1}{2}$
30	$1\frac{3}{4}$	$2\frac{1}{4}$

3d. In asphyxia by submersion.

AGES.	SENSIBILITY.	GAPINGS.
Days.	Minutes.	Minutes.
1	15	27
5	10	16
10	$4\frac{1}{3}$	$5\frac{1}{2}$
15	3	4
20	$2\frac{3}{4}$	$3\frac{1}{4}$
25	2	$3\frac{3}{4}$
30	$1\frac{1}{2}$	$2\frac{1}{2}$

These results, chiefly those relative to the excision of the heart, are the averages of a certain number of experiments.

I tried sensation by pinching the ears, the paws and the tail; and I noted the extinction, at the instant when these pinchings caused no more motion. But I ought to remark, that it often happens, that some sensibility remains in the anus, when none remains in the parts I have just named.

Upon the excision of the heart, as well as in asphyxia, the gapings are always accompanied by inspiratory motions of the thorax. Commonly, these last a little longer than the gapings. I only put down the gapings in this table, because in many experiments on the spinal marrow, they are the only ones to be observed.

Besides the signs I have just mentioned, I availed myself of a few others in my experiments; but without mentioning them here, I shall pass to the particulars of the experiments themselves. These particulars will be sufficient to show the nature and importance of each of these signs.

As I have already stated, I performed upon rabbits both the total and the partial destruction of the spinal marrow, every five days, from the moment of their birth till they were a month old; which makes seven different ages. These cases are, 1. The section of the spinal marrow. 2. Decapitation. 3. The destruction of the whole spinal marrow. 4. The destruction of all the cerebral portion. 5. That of the dorsal portion. 6. That of the lumbar portion.

The object of the three first cases, is to compare the state of the circulation, after the section of the spinal marrow at the occiput, and after decapitation, with the state of it after the detruction of all the spinal marrow. As to the three other cases, the destruction of the same portion of the spinal marrow, not producing the same effects on life, at the different ages, the object of these three cases is to compare these effects, in relation to the circulation, every five days.

Every case required a repetition of each experiment, in order to ascertain precisely the course of the phenomena it produces. The number of experiments, multiplied by that of the cases, and again by the seven different ages comprised in the first month of the birth, is much too large to allow of a full detail. I shall confine myself to relating one of the experiments for each of the six cases which have been considered, on the first, on the tenth, and on the twentieth day from the birth.

EXPERIMENTS

Made to determine the effects of various wounds of the Spinal Marrow on the circulation of the blood.

Experiments on Rabbits, the day of their birth.

CASE I.

Section of the Spinal Marrow near the Occiput.—Circulation continues.

Spinal marrow cut with a needle, between the occipital bone and the first vertebra. All the inspiratory motions are instantly annihilated and replaced by gapings; the animal is agitated for a little more than one minute, after which sensibility continues over the whole body. Sensibility is extinguished in about sixteen minutes (*a*); twenty minutes after the gapings still continue, and the carotids being black and round, but of a smaller size than in the beginning of the experiment;—pulmonary inflation begun. In less than five seconds, the carotids grow larger

(*a*) The minutes are always counted from the beginning of the experiments, or of the first experiment on the same animal; here it is from the division of the spinal marrow at the occiput.

and very red; a little after the gapings are accelerated and increased. Sensibility is restored at about twenty-one minutes. The carotids became soon of a dark colour, on interrupting the inflation, and red on its renewal. At twenty-five minutes, amputation of one of the feet—hemorrhage of a vermilion colour during the inflation, dark coloured when the inflation ceases. At thirty minutes, same phenomena continue: both carotids tied, each with the corresponding external and internal jugular veins.

CASE II.

Decapitation.—Circulation continues.

On the same rabbit. At thirty-two minutes, decapitation performed above the first cervical vertebra. The head, separated from the body, continues to gape for several minutes.—Pulmonary inflation resumed at thirty-three minutes.—Sensibility preserved in the trunk. At forty minutes, amputation of the other foot; a red or dark coloured hemorrhage, as the inflation is either continued or suspended.

CASE III.

Destruction of all the Spinal Marrow.—Circulation suddenly stopped.

On the same rabbit. At fifty minutes, same state of sensibility and hemorrhage, and the throbbings of the heart continue to be distinct, through the parietes of the thorax—the whole spinal marrow destroyed as far as the tail, by introducing an iron probe through the whole length of the vertebral canal. Instantly the whole body is flaccid, and entirely deprived of sensation and motion. Throbbings of the heart no longer distinct, and do not become so afterwards.—Inflation resumed at fifty-one minutes. No effect. A thigh cut off at fifty-five minutes, does not bleed at all. The other thigh cut at sixty minutes, yields two or three drops of black blood, which appear to come from the femoral vein, which is pretty full; the wound being wiped with a spunge bleeds no more.—Inflation discontinued at seventy minutes. Thorax opened at ninety minutes. Pulmonary veins partly black and partly red.

Same case on another rabbit, without decapitation, by destroying first of all the whole spinal marrow, by the introduction of the probe between the occipital bone and the first vertebra through the whole length of the vertebral canal. The whole trunk flaccid and dead.—Gapings the only signs of life in the head; throbbings of the heart no longer distinct. At four minutes, the carotids being nearly empty, and only containing very little black blood, pulmonary inflation commenced. Towards five minutes, a *small stream of red blood* appears in the carotids, which is insufficient to fill them; does not change its colour when the inflation is interrupted, and disappears towards the end of the seventh minute. Both thighs being cut off, one at six, the other at nine minutes, do not bleed.—The inflation is continued with great care, without success, till the eighteenth minute.—Colour of the pulmonary veins as in the first rabbit.

These experiments repeated upon a number of animals of the same species and age, have always afforded the same results.

CASE IV.

Destruction of the Medulla Cervicalis.—Circulation is suddenly stopped.

Immediate destruction of the medulla cervicalis, from the occiput to the first dorsal vertebra. Gapings: the neck flaccid and dead; the anterior paws no longer sensible; the rest of the body is: the throbbings of the heart are but faintly distinguished. The inflation began at four minutes: the throbbings of the heart are accelerated, and become more distinct. The carotids, which only contained a very small stream of black blood, are filled, and assume a red colour. Soon after, the throbbings of the heart cease to be distinct, and the carotids are gradually emptied. At six minutes they only contain a very small thin stream of red blood, which preserves this colour during the interruption of the inflation. A thigh cut off at six minutes, bleeds a little; the blood is black. This hemorrhage continues a few minutes, and the blood remains black. Sensation ceases at eleven minutes, and the gapings at twelve minutes; the other thigh cut off at fourteen minutes, does not bleed. The inflation is discontinued at sixteen minutes. The pulmonary veins are of a bright brown.

CASE V.

Destruction of the Dorsal portion of the Spinal Marrow.—Circulation continues.

On another rabbit. Immediate destruction of all the dorsal portion of the spinal marrow, by introducing the probe into the vertebral canal between the first lumbar and the last dorsal vertebra. The head, the neck, and the hind part continue alive: the middle of the body is dead. The motions of inspiration subsist, but they are weakened and are only performed by means of the diaphragm. The throbbings of the heart are also weakened; there are no gapings. At five minutes, amputation of a foot—no hemorrhage. At six minutes, amputation of a leg—red hemorrhage. At fifteen minutes the animal continued to live and breathe; and the hemorrhages were red. This was used for another experiment.

This experiment has not always the same result in rabbits of this age. It frequently happens, that the destruction of the dorsal portion of the spinal marrow is immediately followed by all the signs which indicate that the circulation is stopped.

CASE VI.

Destruction of the Lumbar portion of the Spinal Marrow.—Circulation continues.

On another rabbit. Immediate destruction of all the lumbar portion of the spinal marrow, by again introducing the probe, between the first lumbar and the last dorsal vertebra, in a direction towards the tail. All the hind part dead. The rest of the body is and continues alive. Respiration a little deranged at first, then pretty well restored, and performed without gapings. At eight minutes, one of the feet amputated, bleeds red blood. At fifteen minutes respiration continues with sufficient facility; the throbbings of the heart are distinct; the animal carries its head well, and supports itself upon its fore legs.

Experiments on Rabbits ten days old.

I shall only indicate for the future the principal phenomena, and chiefly those which indicate the state of the circulation.

CASE I.

Section of the Marrow at the Occiput.—Circulation continues.

Spinal marrow cut with a needle, between the occipital bone and the first vertebra. Sensation ceases at six minutes, and the gapings at seven. At eight minutes, the carotids being black, and still plump, inflated the lungs. At the fourth stroke of the *piston*, the carotids are of a bright red, and much larger. The gapings re-appear at about eight minutes and a half, and feeling at about nine minutes and a half. At about twelve minutes amputation of one foot—red or black hemorrhage as the inflation is or is not continued. At fourteen minutes, same phenomena: ligature of the carotids and of the jugular veins.

CASE II.

Decapitation.—Circulation continues.

On the same animal. At fifteen minutes, decapitation at the first cervical vertebra. Inflation renewed at sixteen minutes. At eighteen minutes sensibility appears to be more lively than before decapitation; the animal is more strongly agitated. At twenty minutes, amputation of one foot; a red hemorrhage. At twenty-one minutes, the inflation being interrupted for seven minutes, instantly the hemorrhage becomes and remains black; at twenty-eight minutes, sensibility appearing extinguished and the hemorrhage stopped, but the throbbings of the heart being still sufficiently distinct, inflation renewed. Sensibility restored at about twenty nine minutes; the hemorrhage re-appears also and is red during the inflation.

CASE III.

All the Spinal Marrow destroyed.—Circulation ceases.

On the same rabbit. At thirty-three minutes, sensibility being very apparent, the hemorrhage red, and the throbbings of the heart distinct, all the spinal marrow destroyed, the throbbings of the heart are no longer distinct at thirty-three minutes and three quarters, and have not appeared again. The inflation renewed at thirty-four minutes; no effect. Both thighs cut off, the one at thirty-six, the other at forty minutes, do not bleed. The inflation continued with great care, is only given up at forty-two minutes. Pulmonary veins found red on the opening of the breast, made at sixty minutes.

If you destroy immediately all the spinal marrow, without decapitating the animal, the results are the same. As soon as this operation is performed, the throbbings of the heart are no longer felt; the carotids are empty and flat; the amputation of the thigh furnishes no blood; and the gapings, which take place in this case, cease at about three minutes and a half; nor can they be continued by the inflation of the lungs.

CASE IV.

Medulla Cervicalis destroyed.—Circulation stopped.

On another rabbit. Immediate destruction of the cervical portion of the spinal marrow, from the occiput to the first dorsal vertebra. Inflation began at two minutes and a half, the carotids being flat and nearly empty, and the throbbings of the heart no longer distinct, but gapings and sensibility still subsisting; a little red blood slowly returning into the carotids, but not enough to make them plump. Sensibility is extinguished at about three minutes, and the gapings at about three minutes and three quarters: the motions of the heart have not been distinct. Both thighs being cut off, the one at four, the other at ten minutes, yield no blood. The inflation is given up at fifteen minutes.

CASE V.

Medulla Dorsalis destroyed.—Circulation stopped two minutes after.

Upon another rabbit. Immediate destruction of the medulla dorsalis, from the first lumbar vertebra to the first dorsal. Respiration disturbed and only carried on by the diaphragm, but it continued at first. A minute and a half after, a leg amputated—red hemorrhage. At two minutes, respiration is succeeded by slow gapings, accompanied by deep contractions of the diaphragm. Inflation performed four minutes after, the carotids still beating, but containing only a small stream of pale red blood; no effect produced. The carotids continue to empty themselves; sensation ceases at five minutes, the gapings at six, the contractions of the diaphragm at about seven minutes; the inflation given up at about thirteen minutes. The thorax opened about ten minutes after; the pulmonary veins found red, and the foramen ovale closed. In this experiment the signs of life disappeared about two minutes later than they would have done, after the excision of the heart; for which the the circulation was only stopped two minutes after the destruction of the medulla spinalis.

CASE VI.

Lumbar portion of the Medulla Spinalis destroyed.—Circulation continues.

On another rabbit. Immediate destruction of the whole lumbar portion of the medulla spinalis. The motions of the heart are at first rendered irregular and slower, and respiration is disordered. This disorder continues only a short time. At ten minutes, respiration is pretty free, and the motions of the heart observe the same time as before the experiment, only they are weaker and less sensible. Twelve minutes after, one leg being amputated, yielded red blood. Fifteen minutes after, the animal is still in the same state, and is reserved for another experiment.

In general, about the age of ten days, the effects of the destruction of the spinal marrow are very various. The only thing to be depended on at that period, is the sudden cessation of the circulation, by the simultaneous destruction of the three portions of medulla spinalis, and its greater or less degree of weakness in the circulation, consequent on the condition of any one of these portions. This appears to be connected with the influence which each portion has on the circulation, and which increases with the age of the animal. It approaches its maximum at about ten days. Thus the same portion of medulla spinalis which, being destroyed at that age, does not stop the circulation, will constantly stop it, if performed a few days later.

Experiments upon Rabbits, twenty days old.

CASE I.

Section of the Medulla performed at the occiput.—Circulation continues.

Spinal marrow cut off at the occiput with a needle. Sensation disappears three minutes, and the gapings three minutes and three quarters after. Pulmonary inflation commenced at four and a half minutes, the carotids being black and still round, but moderately full and the motions of the heart distinct. In less than five seconds, the carotids are fuller and more red. The gapings reappear at four and three quarters minutes, and sensation at about five minutes. At eight minutes, amputation of one foot; hemorrhage of a red colour during the inflation. At ten minutes, the gapings, sensation and hemorrhage continue: ligature of the carotids and the jugular veins.

CASE II.

Decapitation.—Circulation continues.

Upon the same rabbit. At eleven minutes same phenomena. Decapitation at the first cervical vertebra, the stump of the neck bleeds pretty copiously; black blood. Inflation begun at twelve minutes, sensibility perfectly restored. At sixteen minutes the amputation of one leg produces red hemorrhage.

CASE III.

Whole Medulla Spinalis destroyed.—Circulation stopped.

Upon the same animal. At eighteen minutes, sensation manifest and the motions of the heart distinct, all the medulla spinalis is destroyed; one instant after, the motions of the heart are no more discernible, and have not returned. Inflation resumed at nineteen minutes, and continued till twenty-six,—no effect. A thigh cut off at twenty-eight does not bleed, neither did the other cut off at twenty-four minutes. Pulmonary veins are red.

CASE IV.

Medulla Cervicalis destroyed.—Circulation stopped.

Upon another rabbit. Immediate destruction of the medulla cervicalis; sensation is extinguished one and a quarter minute after. At one minute and a half, the motions of the heart not distinct; one thigh amputated does not bleed;—end of the gapings. At two minutes and a half, pulmonary inflation resumed; the carotid being flat and nearly empty;—slow return of a thin stream of red blood, which disappears soon after, and those arteries are quite white at five minutes. The motions of the heart not distinct; the first amputated thigh has not bled, neither did that amputated at eight minutes. Inflation discontinued at fifteen minutes.—The pulmonary veins are red.

CASE V.

Medulla Dorsalis destroyed.—Circulation stopped.

Immediate destruction of the medulla dorsalis; soon after, the motions of the heart are no longer distinct; sensibility ceases at one minute and a half, and the gapings a little before two minutes. The carotids are flat and empty at two minutes. At four minutes, amputation of one thigh; no hemorrhage. Pulmonary inflation not performed. The breast opened at eighteen minutes; pulmonary veins red.

CASE VI.

Medulla Lumbaris destroyed.—Circulation ceases two minutes after.

Upon another rabbit. Immediate destruction of the medulla lumbaris; respiration disordered but without gaping; motions of the heart irregular, but yet tolerably distinct. The animal supports itself upon its fore feet, and carries its head well. At one minute and a half, totters; and can scarcely support it. At two minutes, falls upon its side, and respiration stopped at once; a few seconds after, gapings accompanied with movements of the thorax are observed. About that time, the motions of the heart are no longer distinct. Sensation ends at three minutes and a half, and the gapings at about four. Pulmonary inflation begun at three minutes and two thirds; no effect. Carotids are flat and empty at five minutes. One leg cut off at one minute and a half, bleeds a little; blood red: the thigh cut at three minutes does not bleed; nor does that cut off at seven minutes. Inflation discontinued at ten minutes. Pulmonary veins red.

I will not here repeat what I have said upon the value of the signs, drawn from the colour or absence of the hemorrhage, of the fulness, colour or vacuity of the carotids, of the facility or impossibility of feeling the motions of the heart through the parietes of the thorax, &c. If we compare what they are, after the section of the medulla at the occiput, and even after decapitation, with what they become after the total or partial destruction of the spinal marrow, there will I think, remain no doubt, that in this last case, whenever life ceases, in those parts of the animal in which the destruction of the medulla had not occasioned death, it is only because this destruction has stopped the general circulation. But I ought to remark, that among the signs calculated to show the state of the circulation, the duration of sensation, and that of the gapings deserve the greatest attention. We have just observed, that in the tenth and twentieth day, as in the first day after birth, these durations coincide with those which take place after the excision of the heart; which is the more striking as they differ in a remarkable manner, especially from the first day after birth, from that produced by asphyxia. (See the above table.) I add that a continuance of sensation, and that of the gapings, are the signs which are most generally applicable to all species and ages. In dogs, and especially in cats, under the age of five days, it often happens that all the other signs are insufficient for determining whether the circulation is or is not stopped, after the destruction of the spinal marrow; the continuance of the gapings alone can decide the question.

The attentive reader has doubtless observed, in the cases I have just recited, that the pulmonary inflation sometimes produces in the carotid arteries a small stream of red blood, even when all the other signs indicate that the circulation is stopped, and when there ought not to exist any but black blood in the pulmonary veins or in the left cavities of the heart. This fact requires explanation.

Whenever we proceed to the destruction of the spinal marrow near the occiput, the inspiratory motions of the thorax being annihilated at the instant when the medulla is disorganized in that part, and before the destruction is so far effected as to stop the circulation, the asphyxia always takes place before the circulation ceases, and consequently the pulmonary veins and the left cavities of the heart contain black blood, in the same way as the right cavities do at the moment when the pulmonary inflation is attempted, after the destruction of the spinal marrow. In this state of things, if red blood issues through the carotids during the inflation, arterial blood must have been formed in the lungs, and that blood must have passed from the pulmonary veins through the left cavities of the heart, and afterwards into the aorta. The question then is: whether this fact indicates the remains of the circulation, or whether the pulmonary inflation can determine the formation of arterial blood after death, when circulation is entirely stopped. For this purpose I took two rabbits twenty days old; in one I destroyed the whole source of the nervous power, by means of a probe introduced through the cranium, which I pushed through the whole length of the vertebral canal. The other I destroyed by asphyxia, by separating the spinal marrow near the occiput. I left them undisturbed for forty-five minutes, at the end of which time I opened the thorax; and, after ascertaining that the pulmonary veins in both were black; that the little blood contained in the left auricle was black also; that all the cavities of the heart were at rest, and even that they appeared no longer irritable, at least by the action of the scalpel; I inflated the lungs. By degrees, the pulmonary veins and afterwards the left auricle assumed a fine red colour, but no motion of the heart was manifested. Hence it is evident, that the formation of arterial blood may take place in the lungs, even when the circulation is entirely stopped by any cause whatever. Let us now suppose that the inflation of the lungs is performed, not as in these two experiments, three quarters of an hour after death, but at the instant when the circulation has stopped, the irritability of the heart still continuing; each inflation, by diminishing the capacity of the lungs, will express, as from a spunge, the arterial blood from the pulmonary veins into the left auricle; and this blood, which all the experiments on asphyxia point out to be the most powerful stimulus of the cavities of the heart, will sufficiently increase the weak contractions of the left ventricle to force it into the carotids; but, as might be expected, only in a very small quantity, insufficient to fill them, or even to give them a round shape; finally, this small stream of red blood is of but short duration, and the carotids are soon left empty, because the weakness of the irritable motions of the heart quickly increases. This fact, therefore, does not imply the the existence of circulation, and is not in opposition to the other signs I have already mentioned.

It will be perceived, from what I have just stated, why I so carefully noted, in my experiments, the colour of the pulmonary veins on opening the thorax. Indeed this colour indicates the state of the circulation at the moment when the pulmonary inflation was discontinued. It is evident that whenever those veins are of a red colour, it is a proof that the circulation was stopped, otherwise they could not have remained red, since the blood from the pulmonary artery and the right cavities of the heart would have continued to pass through them. On the contrary, when they are found black, it is in general a sign that the circulation was not stopped; but this last case is liable to some exceptions, especially in the ages when the foramen ovale is not closed.

It is therefore demonstrated, that the destruction of the spinal marrow suddenly stops the circulation, and that consequently the motions of the heart derive all their power from the medulla spinalis. Those which still subsist, either after this destruction, or after the heart has been freed from the action of the nervous power in any other manner, and which have deceived Haller and the authors of his school, are motions without power, and perfectly analagous to the irritable motions, observed in the other muscles for a longer or a shorter time after death. In the latter, those motions only take place when the muscle or the nerve distributed to it, is directly stimulated; and one stimulus only produces one motion. In the heart, the motions are repeated spontaneously, because the blood it contains is its natural stimulus. It is further demonstrated, that the heart receives the source of its power indiscriminately from every part of the spinal marrow. Of the two modes of action, which each portion of the medulla exercises upon life, one by which it constitutes essentially all the parts which receive their nerves from it, the other by which it contributes to maintain it in the rest of the body, the latter therefore depends upon the powerful influence which it exercises upon the motion of the heart.

Thus we find the apparently strange effects of the partial destruction of the spinal marrow accounted for. And this conclusion I had drawn from my first experiments: that two conditions are sufficient to maintain life in any one portion of an animal, *i. e.* the integrity of the corresponding spinal marrow; and the continuation of the circulation, requires no further confirmation. For it is evident that, if we cannot maintain life in a part of an animal after having occasioned death in the rest of the body, it is solely because one of these two conditions has been destroyed. Whence it must be concluded, that it would in all cases be easily accomplished, if we could find the means of preventing the circulation from stopping, after we have destroyed a portion of the spinal marrow. Now we have this mean. It consists in limiting by ligatures, made on the arteries, the extent of those parts to which the heart distributes the blood.

We have just observed that, generally speaking, when the rabbits have reached or gone beyond the age of twenty days, the destruction even of the lumbar portion of the spinal marrow destroys them in the space of three or four minutes, by stopping the general circulation at the end of one or two minutes. We have seen also, in the recapitulation of my first experiments, that the ligature on the aorta by intercepting the circulation through the whole portion of the spinal marrow, which is posterior to the ligature, destroys sensation and motion in all the parts which receive their nerves from that portion of the medulla, which then is for those parts of no use, and as if it did not exist, or as if it had been destroyed. Hence it might be inferred that, by tying the aorta about the last dorsal vertebra, the general circulation ought to stop one or two minutes after the lumbar medulla should, by the effect of this ligature, have lost its vital action. But on the other hand, the ligature of the aorta, necessarily producing a great change in the general circulation, since the parts which receive the blood from the heart, in the greater circulation, are greatly reduced in number, while the lesser circulation continues the same, it was evident that, on that account, the annihilation of the vital action in the lumbar portion of the medulla spinalis, by the ligature of the aorta, does not offer an exact parallel with that produced by the destruction of that portion of the medulla spinalis. This difference of the results, in these two cases, could only be determined by experiment.

Under these circumstances, I performed anew the ligature of the abdominal aorta, by opening the belly of a rabbit thirty days old. I passed a thread under the aorta, and tied it immediately below the cœliac artery, which nearly corresponds with the begining of the lumbar vertebræ. Motion and sensation disappeared in the hind parts about two minutes and a quarter; but the fore part continued alive. The animal supported itself on its fore feet, carried its head well, and respiration was performed with ease. Fifteen minutes after, it was still in the same state; the flaccidity and absolute insensibility, and in a word the apparent death of all the posterior parts, left no doubt, that the lumbar portion of the medulla spinalis had entirely lost its action, and no longer contributed in any degree to the maintenance of the circulation. Nevertheless, to obtain a direct proof, I destroyed it at this period of fifteen minutes, The animal appeared perfectly sensible, upon the introduction of the probe in the vertebral canal, between the last dorsal and the first lumbar vertebræ; but it manifested no further signs of pain, the moment the instrument had penetrated as far as the first lumbar vertebra; and this destruction, which, at the instant when it is performed, is always accompanied with strong convulsions in the hind part, when the influence of the spinal marrow is unimpaired, did not produce the slightest motion;

a certain proof that all this portion of the medulla spinalis was dead. Accordingly, the animal continued to live for the fifteen ensuing minutes, after which it was submitted to another experiment. It is evident that the ligature of the aorta had given it the power of surviving the destruction of the lumbar portion of the medulla spinalis.

It still remained a question, whether it would be the same with the other portions of the medulla spinalis. I mean, whether the influence of such portions could be destroyed by ligature, without stopping the general circulation. We have seen, that although the different portions of the spinal marrow contribute to the powers of the heart, the influence of the cervical portion appears to be the most considerable, at least in rabbits. The immediate destruction of that portion is constantly and suddenly mortal in those animals, when they are older than ten days; and before this age, they can scarcely outlive it for a very few minutes. It was therefore a very important point to be ascertained, whether it were possible to destroy the cervical portion of the medulla spinalis, in a rabbit of thirty days, without instantly killing it. But the only arteries in the neck, that can be tied, are the carotids, and as these can be supplied by the vertebral, a ligature made on them is not always sufficient to insure the success of the experiment. Considering the conditions which must be complied with, to insure success, it appeared to me that the safest way was to decapitate the animal, the only operation capable of wholly intercepting the circulation in the head, and in a part of the neck.

My conjecture was confirmed by the experiment. Seven times did I destroy the cervical portion of the medulla spinalis, in rabbits of thirty days, after having decapitated them; and yet the circulation was not stopped in any one. The following are the particulars of those experiments.

The spinal marrow divided near the occiput with a needle.—Pulmonary inflation commenced at three minutes and interrupted at four, for the purpose of tying one of the carotids, together with the corresponding internal and external jugular veins;—resumed at five minutes, then discontinued at six, to tie the carotid and the jugular veins on the other side;—resumed again at seven minutes, and interrupted at eight for one minute more, to separate the aspera arteria from the larynx, and to separate the head with scissors at the first cervical vertebra;—at twelve minutes, the animal being perfectly alive, sensible and even performing spontaneous movements, all the cervical portion of the spinal marrow was destroyed. The inflation, which had been discontinued for this operation, was renewed at thirteen minutes; motion and sensation appeared suppressed in the fore feet, but perfect in the thorax and in the hind part, and still continued at twenty-four minutes, that is to say, twelve minutes after the destruction of the cervical portion of the medulla spinalis; when the probe was again introduced into the vertebral canal, and the dorsal portion of the medulla spinalis, was destroyed, as far down as the eighth vertebra of the back. All the signs of life ceased entirely in the hind part, a little before twenty-five minutes and a half, and could not be excited, though the pulmonary inflation, resumed at twenty-five minutes, was continued to thirty-two; a thigh cut off at twenty-seven minutes did not bleed. These particulars clearly show, that the circulation continued after the destruction of the cervical portion of the spinal marrow; but that it suddenly stopped upon the destruction of the anterior two thirds of the dorsal portion.

The other six experiments were made nearly upon the same plan. In all I destroyed the cervical portion of the spinal marrow at once. But in some, instead of destroying at once the dorsal portion of the medulla spinalis, as far down as the eighth vertebra of the back, I only at first destroyed it as far down as the fourth inclusively; then, five minutes after, as low as the eighth; and finally, after five other minutes as low as the first lumbar vertebra; this produced in the results a difference worthy of remark.

We have just observed that, by destroying the dorsal portion of the medulla spinalis as low as the eighth vertebra at once, the circulation was stopped instantaneously. But it was not so when this portion was successively destroyed at different times. For instance, in the cases I have just related, where the dorsal portion was destroyed by thirds, the circulation was only stopped by the destruction of the whole of this portion of the medulla spinalis. Indeed, it was not entirely so, when this destruction, instead of being performed by thirds, was by fourths or by fifths. What could produce this strange difference? Repeated inquiries have suggested the following explanation.

The destruction of any one portion of the spinal marrow, by producing death in all the parts which receive their nerves from it, considerably weakens the circulation in all those parts; but this weakness is not immediate; it only acquires its maximum a few minutes after. The circulation, which still continued with activity in a part of the neck after decapitation, becomes much weaker; therefore, when the cervical portion of the medulla spinalis has been destroyed, it decreases also considerably in the shoulders, in the fore feet, and in a part of the thorax, when we proceed to the destruction of the dorsal portion, at the three or four first vertebræ of the back, and so on. These successive destructions, though they do not produce the effect of a complete ligature of the arteries, nevertheless have the effect of an incomplete ligature. Now from all I have just stated upon the ligature of the arteries, connected with the destruction of the medulla spinalis, as the extent of the spinal marrow necessary to maintain the circulation is smaller in proportion as the circulation extends to fewer parts, it may be readily conceived that if, by the ligature of the vessels, or by amputation, the destruction of a certain portion of the spinal marrow is rendered practicable, without stopping the circulation, this operation, by weakening the circulation in all the parts corresponding with the portion thus destroyed, will render the destruction of another portion likewise possible. By the same mechanism, this renders the same operation practicable upon another portion, and so on, till, by these successive destructions, the portion of spinal marrow left untouched, can no longer be reduced, unless the circulation, thus gradually brought to the last degree of weakness, is entirely stopped. To this effect, produced by the partial destruction of the spinal marrow on the circulation in the corresponding parts, must be added another, analogous on the general circulation.

It is, that the heart being more and more weakened by those destructions, the circulation is proportionably concentrated: it only preserves some degree of activity in the parts near the heart, and languishes in all those at a little distance.

This explanation will elucidate many of the difficulties, to be met with in the experiments made on the medulla spinalis. Among these difficulties, those which embarrassed me the most were the differences, sometimes considerable, which I have observed, when I wished to determine with precision, the exact length of spinal marrow necessary to the maintenance of circulation, for every age in every species. I proceeded, as if groping in the dark. After having destroyed a certain length of the spinal marrow, whether respiration continued, or it was necessary to inflate the lungs, I waited some minutes to observe the effect of this injury. If the circulation was not stopped by it, I destroyed another portion; then again I waited a few minutes, to see the effect, and so on to the last partial destruction, when the circulation appeared stopped. Then I considered the sum of all those successive destructions, as the length of the medulla spinalis which must be destroyed, to stop the circulation in an animal of the species and age of that which had been the subject of the experiment. This effect was really produced, when such a length was destroyed at one operation. But when, instead of destroying it at once, or at four or five different times, I attempted to do it at two different operations, I was astonished to see the circulation stopped by the first, though the destruction of the medulla spinalis had not exceeded one half of the length, which was thought necessary to produce that effect. And vice versa, when I had begun by a portion, the destruction of which had been found sufficient to stop the circulation, if by chance or design I happened to destroy afterwards the same portion at several times, I often found that the circulation was not stopped, unless I destroyed another portion, and that sometimes considerable. In a word, with each experiment there was a difference in the result, and which, in most cases, was too striking to be considered as purely accidental.

After many fruitless efforts to elucidate this dark question, I determined to abandon it, not without regret at having sacrificed so many animals, and lost so much time. I changed my plan, and, instead of trying to determine for each age the precise length of the medulla spinalis, the destruction of which stopped the circulation, my inquiries were confined to the effects of the three portions, the cervical, dorsal, and lumbar, when destroyed separately and at different ages. I have already given the results; they indicate only in a general manner, that the extent of spinal marrow, strictly required to maintain the circulation, is in proportion to the age of the animal. I no longer thought of the difficulties I had experienced, in the prosecution of my first plan, or rather I had lost all hopes of any elucidation, when I was led to study the effects of the ligature upon arteries, and to compare these effects with those produced by the destruction of the spinal marrow. From that moment all these difficulties vanished.

Generally speaking, whenever the circulation has been much weakened by any cause, in a part somewhat important of the body, there is reason to believe that the general circulation will not be stopped, at least immediately, by the destruction of a like portion of medulla spinalis, which, if this circumstance had not taken place would have been sufficient to stop it. I will state an instance of it. I have sometimes observed that, by cutting through the spinal marrow near the occiput, and then waiting several minutes, before I destroyed the cervical portion, this last operation did not stop the circulation even in rabbits of thirty days, in which it is always stopped, as we have seen above, when it is performed immediately. But it is easily known in these cases, that the circulation has been stopped, or considerably weakened in the head; by the gaping which continued at first, now ceasing, or becoming very rare and very weak; by the sensibility being extinguished in the eye and incapable of being excited; by the carotids being distended near the thorax, and easily changing colour; by the interruption or renewal of the inflation of the lungs, contracted and almost empty, and nearly of the same colour near the head. Nevertheless, these cases are very rare, and it is correct to say, that the surest way to preserve life in rabbits of that age, after the destruction of the cervical portion of the medulla spinalis, is to begin by cutting off their heads.

These facts, by showing that there is no portion of the medulla spinalis, which cannot be supplied by another, by means of certain operations, prove in a satisfactory manner, that it is from every part of this spinal marrow, that the heart receives the principle of its power. We observe that the quantity, the contingent of power, which each portion of the spinal marrow furnishes to that organ, is at least equal to what it would strictly require, to keep up the circulation, in the parts alone corresponding with that portion. Hence it might be inferred, that by cutting off portions of an animal at both ends, suitable ligatures being made on the blood-vessels, the trunk might be so reduced, as always to admit the possibility of life's being maintained in it. I had no doubt of the correctness of this conclusion; strictly adhering to the method adopted in the prosecution of the inquiries, of drawing no conclusions but what appeared indubitably to flow from my experiment, and seeking afterwards by direct experiment, a confirmation of those conclusions, I determined to know, whether it would be possible to keep up life in a simple stump of an animal. I was not entirely at liberty in the choice of this stump, because it was necessary that the heart and the lungs, should remain annexed to it, and in such a manner that the circulation and the inflation of the lungs, might be carried on without difficulty. These conditions can only be found in the breast. The thorax of a rabbit of thirty days, I accordingly proposed to keep alive after having separated it from the rest of the animal, by cutting off the anterior and posterior parts. My first essays were unsuccessful; I met with no difficulty in keeping up life after cutting off one of the two extremities of the animal, either the head or the hind part; but when

I cut off both ends, and the breast was left alone, every sign of life was soon and irrevocably extinguished. Eight times successively, did I fail in this experiment; I still persisted with unremitting perseverance, being fully persuaded of the possibility of success. What contributed to support my hope was, that by examining with attention all the circumstances of each experiment, I almost always discovered the cause of its failure. The three principal ones were, 1st. The passage of air into the blood vessels, a fatal accident, and unfortunately a very frequent one in experiments of this kind. 2d. The passage of air into the cavity of the breast under the diaphragm, separated from the vertebral column. 3d. The decapitation being made too near the breast, which produced too copious a hemorrhage, especially through the vertebral arteries which cannot be tied, at the same time that it favoured much the passage of the air into the vessels. Finally, by varying the mode of operating, and by paying a much greater attention to every part of the experiment, my hope was completely realized; and I succeeded in maintaining life for upwards of three quarters of an hour, in the detached and isolated breast of a rabbit thirty days old. I have several times since obtained the same success. I have even obtained it by processes which had at first appeared to me unfavourable. Nevertheless, the following is that which has appeared to me to succeed the best.

The belly of the animal is first opened; a ligature is passed round the aorta, immediately below the cœliac trunk, another is passed round the vena cava near the liver, a simple knot is tied in each of these ligatures, but is left somewhat loose. This being done, the trachea and both carotids are laid bare; each of these arteries is tied, conjointly with the external and internal jugular veins; the trachea is opened for the inflation of the lungs; the spinal marrow is divided with a needle, near the occiput, and the inflation is begun, without waiting till the asphyxia has extinguished sensation; after it has been continued for three or four minutes, the animal being fully alive, the trachea is separated forward from the larynx, then the head is cut off with a pair of scissors, at the first vertebræ of the neck; and immediately resuming the inflation, which is to be continued for three or four minutes; after which the knots are to be tightened which had been previously prepared upon the aorta and the inferior vena cava; the inflation is to be then renewed, and again interrupted at the end of three or four minutes, to cut off the hind part, which is performed by taking away the intestinal tube from the beginning of the duodenum, then by dividing with scissors the soft parts surrounding the vertebral column, and then the column itself, immediately below the ligatures made on the aorta and the vena cava. In this manner, there remains only the breast, the stomach, and the liver, which might also be removed if care be taken to prevent hemorrhage. The operation being finished, all that remains is to continue the inflation of the lungs, as long as the breast shows any signs of life. The most apparent of those signs are the motions and the sensation preserved in the fore feet, and the small writhing motions observed in the thorax, when the skin is severely pinched and when the posterior extremity of the dorsal portion of the medulla spinalis is touched. In some cases, after carrying the experiment thus far, I have destroyed the remainder of the cervical portion of the medulla spinalis and part of the dorsal; in those cases, although life only existed in the two posterior thirds of the breast, I could still prolong it.

It is beyond a doubt, that if both the lungs and the heart could continue to perform their functions with any other portion, as they do with that of the breast, life might likewise be maintained in them. It is therefore demonstrated by a direct experiment, that the medulla spinalis of any portion whatever, may at once animate all the parts of that portion, and supply the heart with the power it requires to maintain the circulation in it; and that if life cannot be prolonged in any one portion taken at random, it is only because the anatomical arrangement of the organs prevents it. But if the place of the heart could be supplied by injection—and if, for the regular continuance of this injection, there could be furnished a quantity of arterial blood, whether natural, or artificially formed, supposing such a formation possible,—then life might be indefinitely maintained in any portion; and consequently, after decapitation, even in the head itself, without destroying any of the functions peculiar to the brain. Not only life might thus be kept up both in the head and in any other portion separated from the body of an animal, but it might also be re-produced after its entire extinction. It might be restored likewise to the whole body, and thereby a complete resurrection be performed in the full extent of the word. But this requires some little explanation.

From all I have stated in this work, life is produced by an impression of the arterial blood, made upon the brain and the medulla spinalis, or to a principle resulting from this impression. Therefore it is the cessation of this impression, the extinction of this principle, which constitutes death; and consequently to make life succeed death, or, in other words, to effect a resurrection, it is necessary to renew this principle. Now, this renewal is impracticable; since, on the one hand, it cannot take place if the heart does not preserve sufficient power to propel the blood as far as the spinal marrow; and since, on the other, all the energy of this organ is dependent on this very principle, which by this hypothesis is supposed to be extinct. Therefore it is this reciprocity of action, now fully demonstrated, between the heart and the medulla spinalis, which establishes the impossibility of resurrection in the actual state of things. But if there existed any means of supplying the natural circulation, which cannot be restored, it is certain that a dead body may be resuscitated sometime after death;—a time which would be limited by several circumstances, and variable according to the species, the age of the animal, the causes of its death, the seasons, &c. The partial resurrections which may be performed at pleasure, leave no doubt in that respect. Thus, if with this view you repeat an experiment stated above, which had already been made by Stenon, and which consists in tying the aorta opposite to the first lumbar vertebra, we have seen that a little while after sensation and motion have entirely disappeared in the hind part, yet the circulation and life have continued in the anterior part. But if, after waiting a time treble and even quadruple of that at the end of which all the signs of life have disappeared, you untie the aorta, sensation and motion return by degrees in the dead parts as circulation is re-established. In the same manner, by tying all the arteries that go to the head, you might reduce this part to a state of death; and all the intellectual functions peculiar to the animal which is the subject of the experiment would be not only weakened, disordered, or suspended, as in the case of asphyxia or syncope, but wholly annihilated, whilst the rest of the body would be full of life. These functions would be resumed afterwards, upon the arteries being untied. It is sufficiently evident, without my dwelling any longer upon this subject, why those partial resurrections are the only ones within the power of the physiologist, and at the same time the only ones he can admit in the common course of things.

RECAPITULATORY OBSERVATIONS.

I SHALL conclude by a recapitulation of the principal facts above stated.

The principle of the sensation and motion of the trunk, has its seat in the medulla spinalis, and not in the brain; but the primum mobile of respiration resides in that part of the medulla oblongata which gives rise to the nerves of the eighth pair.

From this double disposition, the section of the medulla spinalis near the occiput, and decapitation, annihilate the inspiratory motions, without causing a cessation of life in the trunk, which only dies by asphyxia and at the end of the same time as if respiration had been prevented in any other way, admitting that the hemorrhage had been stopped.

In obviating asphyxia by the inflation of the lungs, the existence of an animal may be protracted for a time, the maximum of which in this case is the same as after the section of the nerves of the eighth pair.

If, instead of being perfomed near the occiput, decapitation is performed upon the cranium, so as to spare the part in which the primum mobile of respiration resides, and to leave it in continuity with the spinal marrow, the animal may live and breathe by its own power, and without any assistance, until it dies of inanition. It is the maximum of its existence in this other case; but, from well known causes, cold-blooded animals are the only ones that can reach it.

Not only the life of the trunk is in general dependent upon the spinal marrow, but that of each part depends in a special manner upon the portion of the medulla from which it receives its nerves; so that, by destroying a certain extent of the medulla spinalis, we only deprive those parts of life which receive their nerves from the medulla destroyed; all those which receive theirs from the medulla not destroyed, continue alive for a longer or a shorter time.

If, instead of destroying the medulla, transverse sections be made, the parts which correspond to each segment of the medulla possess sensation and voluntary motion, but without any harmony, and in a manner as independent of one another as if the whole body of the animal had been cut transversely in the same places; in a word, there is, in this case, as many distinct centers of sensation as there have been segments cut in the medulla.

That life may continue in any part of the body, besides the integrity of the corresponding medulla, the circulation constitutes an essential condition. If you interrupt the circulation in a part, death constantly follows; but even when this last effect is produced in the most unequivocal manner, life soon returns, if you can succeed in restoring the circulation in this part, and particularly in the medulla spinalis.

Death never happens, either in a part, or the whole body, as soon as the circulation has been interrupted, but only some time after. This time, which is determined in the animals of the same species and same age, is the longer in the warm-blooded as they are younger. Thus when the circulation is suddenly stopped in rabbits, whether by ligatures or by taking away the heart, sensation is only extinguished at the end of fourteen minutes, when they are new born; two minutes and a half, when they are fifteen days old; and one minute, when they are thirty. In cold-blooded animals, it is only extinguished some hours after. The length of time which animals survive this experiment, so characterises the cessation of the circulation, that it is perfectly distinct from that which takes place in death from any other cause. For instance, it is always shorter in an animal, of whatever species or age, than when the death of the animal is occasioned by asphyxia.

Since in every part of the body, life essentially depends upon the integrity of the corresponding spinal marrow, and the continuance of the circulation; and that, according to the Hallerian theory of irritability, the motions of the heart, and consequently circulation, are independent of the nervous power; it would seem that we might at pleasure, continue the life of any one portion of an animal, after having produced the death of all the other parts, by destroying the corresponding portion of spinal marrow: but this is not the case. After the destruction of a certain extent of medulla spinalis, produced in any one part of the vetebral column, life only continues in the part the spinal marrow of which is left untouched a limited time, which is longer or shorter according to the age of the animal. Now, the duration of life in this case happens to be the same as if the heart had been removed from an animal of the same species and age. All the other phenomena which are then observed, such as the vacuity of the carotids, absence of the hemorrhage after the amputation of the limbs, &c., concur to prove that the destruction of the medulla has instanstaneously deprived the heart of the energy necessary for maintaining the circulation, without stopping at first its motions, which are no more than the motions of irritability.

By assimilating these motions, without power, to those which take place during life, the authors of the Hallerian school have fallen into error.

In all species and at all ages the destruction of any one portion of the medulla spinalis, has always the effect of weakening the energy of the heart; but the portion which must be destroyed, to bring this weakness below the degree necessary for maintaining the circulation, varies in the different species, and is longer in the same species as the animal is younger.

If ligatures be made either on the aorta or any other large arteries, previous to the destruction of the medulla spinalis, the results are different; and the destruction of the same portion of the spinal marrow, which, without these ligatures, would have suddenly stopped the circulation, will be insufficient to produce this effect. Generally, by diminishing with ligatures the extent of the parts to which the heart is to distribute blood, we lessen the degree of force which this organ requires to perform its function, and we shorten in proportion the extent of the medulla, which is indispensable to maintain the circulation.

The destruction of a portion of the spinal marrow insufficient to stop the general circulation, always weakens it in the parts corresponding to the portion which has been destroyed, and, to a certain degree, acts as a ligature. Furthermore, the energy of the heart being weakened by this operation, the general circulation is concentrated, and only preserves some slight activity in the parts nearest to the heart, which likewise produces a similar effect. Hence it happens, that when the spinal marrow is destroyed succesively in small portions, and at intervals, a much greater length may be destroyed without stopping the circulation, than would be sufficient to produce that effect, if it were destroyed at once.

Whether by this method, or by ligatures made upon the arteries, there is no one portion of the medulla spinalis which cannot be prevented from co-operating in maintaining the circulation without destroying this function, there is none which may not become sufficient to maintain it; and we find that, at all ages, any one portion supplies the heart with a power sufficient to maintain the circulation in all the parts corresponding to that portion. Upon this is founded the possibility of preserving life in an isolated portion taken from the middle of an animal's body. But in whatever manner these experiments are conducted, whenever it is carried so far as to suppress the action of the spinal marrow, through its whole length, the circulation is irrevocably stopped.

CONCLUSIONS.

Among the numerous consequences flowing from these facts, I shall confine myself to stating the following:

Life is produced by an impression of the arterial blood made upon the brain, and the medulla spinalis, or by a principle resulting from this impression.

This impression being once produced, this principle once formed, has always a specific duration; but it is variable, according to the species and age of the animal. Consequently there is no one mean of killing an animal instantaneously, or rather there is no other mean than the simultaneous destruction of the brain, and of all the medulla spinalis.

The prolongation of life depends upon the continual renewal of this impression, nearly in the same manner as one body, moved by a first impulse, cannot continue to move indefinitely unless the same impulse is repeated at intervals.

This property of the principle in question, namely, to survive wounds, and a very considerable destruction of the rest of the body, provided its peculiar seat has not been injured, affords a ready criterion for determining in what part of the nervous power the primum mobile of such a function resides. For whenever by destroying a certain portion either of the brain or of the spinal marrow, you cause the cessation of a function suddenly, and before the known period when it would cease naturally, you may be assured that this function depends upon the part that has been destroyed. It was in this manner that I discovered that the primum mobile of respiration had its seat in that part of the medulla oblongata which gives rise to the nerves of the eighth pair; and it is by pursuing this mode, that one might to a certain degree discover the use of certain parts of the brain, so much the object of speculation, but hitherto only defined in systems produced by a lively imagination. These inquiries would be attended with so much the more success, as the animals might be selected of such ages and species, as are capable of surviving a longer time the cessation of the circulation.

It is this impression, this principle formed in the brain and the spinal marrow, which, under the name of nervous power, and through the medium of the nerves, animates all the rest of the body, and presides over all its functions.

The heart derives all its powers from this principle, as do all the other parts the sensation and motion with which they are endowed, with this difference, that the heart derives its power from every point of the spinal marrow, without exception, whilst every part of the body is only animated by a portion of that medulla (by that which supplies its nerves); a difference which may serve to explain the intensity of the power of the heart, and its uninterrupted continuance, from the moment of conception, till the hour of death.

The action of this principle upon the heart, and, consequently, the activity of the circulation, is not the same in all species; and, in the same species, it is more considerable in proportion as the animal is nearer the time of its birth; if we admit that it is so much greater, as a smaller portion of medulla spinalis is sufficient to maintain the circulation. This circumstance has many applications in physiology and pathology of the first period of life.

From the great sympathetic nerve, the heart receives its principal nervous filaments; and it is only through that nerve, that it can receive its energy from every point of the spinal marrow. The great sympathetic must, therefore, have its origin in this medulla; and thence the numerous questions that have been raised on the origin of this nerve, namely, whether it proceeds from the brain, or from the spinal marrow; or, as Bichat pretends, whether those different portions are only branches communicating from the ganglions, which this author considers as so many smaller brains, forming a distinct nervous system, independent of the brain and the spinal marrow (*a*). All these questions, hitherto inexplicable in anatomy, are completely determined by experiments; and it is at the same time demonstrated, that the ganglions cannot be considered as *smaller brains*.

From the same principle, we can no longer admit the assertion of Bichat, though pretty generally adopted, that there is in the same individual two distinct lives, one animal, the other organic; that the brain is the only center of animal life; and that the heart, independent of the brain and of the nervous power, is the center of organic life.

It must, however, be observed, that there is a real and a very important distinction to be made between the organs that receive their nerves from the great sympathetic, and those which receive theirs immediately from the medulla ob-

(*a*) This opinion on the use of the ganglions appears to have been first produced by Winslow; and several authors, among others Winterl, Johnstone, Unzer, Lecat, Pfeffinger, Prochaska, &c. had reproduced it before Bichat.

longata and spinal marrow. The former receive their principle of action from the whole nervous power: their functions are not submitted to the will; they are exercised in every instant of life, and, at most, suffer only remissions. The latter, on the contrary, have their principle of action in a limited portion of the nervous power: their functions are submitted to the will; they are temporary, and can only be repeated after complete intermissions of longer or shorter duration. This distinction comprises nearly the same organs as that of the two lives; but it is evident that it rests upon a basis entirely different, since the organs of organic life, which, in the system of the two lives, is considered as independent of the brain and of the spinal marrow, are precisely those which receive the most powerful influence from it. Numerous anatomical, physiological, and pathological facts, can only be conceived and accounted for by this distinction. For instance, it is known that certain pains in the bowels cause debility, prostration of strength, and great disorder throughout the animal economy. This fact, which is unaccountable in the system of the two lives, is readily understood, from the moment we reflect that the intestines derive their principle of action from all parts of the nervous power, through the great sympathetic, from which they receive their nerves; and that, consequently, their affections ought to re-act immediately upon every part of this same power.

Death being only the extinction of the principle formed in the brain and the spinal marrow by the action of arterial blood, it can only be partial when the extinction is so; it is general, when the extinction takes place through the the whole extent of the brain and the medulla spinalis.

Partial death, in any region it may happen, admits of a real resurrection, whenever the portion of medulla spinalis that has been left alive, can supply the heart with sufficient power to restore the circulation in the dead portion. If general death is irrevocable, it is not because the reproduction of the principle in question cannot be effected through the whole extent of the medulla spinalis, full as well as in a portion, at the end of a shorter or longer period after its entire extinction; but it is, because the heart, having lost all its power by the very effect of the extinction of this principle, without any means of recovering it, the circulation has ceased for ever. In short, the extinction of the principle of the medulla spinalis, and the spontaneous cessation of the circulation, are two things inseparable and mutually announcing each other.

Among the certain signs of death, we must therefore reckon all those which prove, that the circulation has ceased. For this reason the vacuity of the carotids is an infallible one, even when the contractions of the heart are still felt distinctly through the parietes of the thorax. Hence it follows that it is very far from being true, as has been asserted, that the last term of life extends to the abolition of irritability in this organ.*

* Haller Elem. Physiolo. tom. VIII. lib. XXX. p. 123.

GENERAL RESULTS.

Such are the principal results of a laborious investigation, in which I became insensibly engaged without foreseeing the consequent extent and difficulties. From my first experiment, the sole object of which was to determine the time that a fœtus can live without breathing, after the communication with the mother has ceased, to that where I succeeded in maintaining life in a portion extracted from the middle of the body of a rabbit, I have been irresistibly led from one experiment to another; the first experiment requiring a second to elucidate it, and this another, and so on successively. There are none which I have not repeated several times. In physiological inquiries, it becomes an indispensable necessity often to repeat and review the same experiment; a necessity which is grounded on the one hand upon the complication of the phenomena which they present, on the other on this, that many causes may produce their failure, and render the labours of this kind so long and arduous. But of all those in which I engaged, there are none which I have repeated with more care, nor on which I have meditated longer, than those relative to the seat where the principle of the power of the heart resided. Haller's theory, notwithstanding the imperfection imputed to it, appeared to me still so well established, and all the modifications proposed appearing unsatisfactory to me, that it was only from the maturest and the most attentive scrutiny of the facts which overthrow its foundation that my conviction could be shaken. Although it is now full two years, since I discovered and intimated that the principle of the power of the heart resided in the spinal marrow, I now for the first time present the proofs to the public.

I do not pretend, however, that Haller's theory is erroneous in all its parts. It is only so, when it deprives the nervous power of all active participation in the motions of the heart, which it exclusively ascribes to muscular irritability (*a*).

(*a*) I ought to remark that under the name of Hallerian theory, I do not only mean that, which this great

But in other respects, as already stated in this work, I have had many opportunities of ascertaining the truth of this other part of the same theory, that the blood, and the arterial blood particularly, is the stimulus, the presence of which determines the contractions of the heart.

man has consigned in his immortal work on physiology, Lib. iv. sect. v.; but also that of the authors of his school. It is worthy of remark, that Haller never did presume formally to deny the influence of the nervous power on the heart, and that he even seems to admit it, though indeed in a problematic manner, and inconsistently with the facts he brings forward to prove that these motions do not depend upon the brain. In short, he only appears to admit it as a conscientious obligation, if I may thus express myself, and because he could not otherwise dispose of the nerves of the heart. For which reason he has almost reduced it to nothing, in the last edition of the four first volumes of his physiology, (vide L'Auctarium, page 72, last paragraph, in which it is evident we must read *potest* instead of *nequit*, page 73, line first.)

The authors of his school have been much less reserved; they have maintained in formal terms, that the motions of the heart do in no way depend upon the nervous power. See among others, a dissertation of Fontana, page 234 of the third volume of the memoirs on the sensible and irritable parts of the animal body.—And the treatise on the poison of the viper, &c. Florence, 1781. tom. II. p. 169. 171.

I have only treated in this work of the action of the medulla spinalis on the heart; it is not that the medulla oblongata does not exercise one also, but it is less considerable, and will engage my attention on a future occasion.

U

SECTION III.

When it is once fully demonstrated that the life of the trunk has its principle in the spinal marrow, and that to protract it, it is only necessary to supply the want of natural respiration by the artificial inflation of the lungs, the first question which presents itself, is to determine how long it can be maintained by that process.

It would seem that the best way of deciding this question, would be to try to maintain life the longest time possible in a certain number of individual animals. But if we should content ourselves with this process, which is purely empirical, we should only obtain an imperfect solution. For the death of a decapitated animal may be produced or accelerated by several causes; some of which depend on the imperfection or failure of the means employed to maintain life; others on the accidents necessarily connected with so considerable and so serious a wound as that made by decapitation. Now, all these causes are more or less foreign to the question.

The particular object of inquiry, when our attention is directed to the time which an animal can live after decapitation, is to ascertain how far the trunk can dispense with the action of the brain; or what amounts to the same thing, at what period and in what manner death occurs from the mere cessation of this action. It is therefore of this last and principal cause, that we must study the kind and the degree of influence, abstracted from every other.

The brain can exercise no action upon the trunk but through the medium of the spinal marrow, and the nerves of the eighth pair (pneumo gastric); and it is evident that, after decapitation, this twofold mode of action is annihilated. We have seen that its want may be supplied, at least for some time, by the inflation of the lungs; but this inflation, in reality, acts only in place of the mechanical phenomena of respiration; and we have also seen that it is through the spinal marrow that the brain presides over these phenomena. By inflating the lungs of a decapitated animal, therefore, we only obviate the effects produced by the cessation of the influence, which the brain exercised through the medium of the spinal marrow on respiration; but there is nothing to show that we remedy at the same time against the cessation of the influence, exercised through the nerves of the eighth pair, so as to enable us to prolong life indefinitely.

To ascertain this, it was necessary to study the immediate effects of the cessation of this last kind of influence, considered by themselves and independent of any other connection, as they take place after the division or ligature of the nerves of the eighth pair. Previous to this investigation which now engages my attention, I had already had occasion, as I shall state by and by, to perform the section of these nerves. Upon repeating this experiment afterwards, with the view of applying the result to my present object, I had three things to examine: 1st. How long animals can survive the section of the pneumo-gastric nerve. 2d. What is the cause of their death. 3d. Whether the time during which life may be maintained in decapitated animals, and whether the cause of their death (as the opening of bodies seems to indicate) have any affinity with what is observed, after the section of the pneumo-gastric nerves.

The experiment in question is one of the most ancient that have been made upon animals, and one of those most frequently repeated. Before I proceed further, I think it my duty to indicate the principal authors who have performed it, as well as the different views in which they have presented their results.

Rufus of Ephesus (*a*), a Greek physician who lived under Trajan about the beginning of the second century of the christian era, mentions the compression or the ligature of the nerves of the par vagum. It is true, he does not otherwise designate those nerves, than as the

(*a*) Appellationes Part. Hum. Corp. Græcè. Parisiis, 1554, p. 32.

nerves in the neighbourhood of the carotids; which made some authors, and among others Daniel Leclerc (*a*), think that Rufus only meant the recurrent nerves (inferior laryngeal.) But Morgagni (*b*) has shown that Daniel Leclerc had misunderstood the passage of Rufus, and that the recurrent nerves were not known in the time of this author (*c*).

(*a*) Histoire de la Médecine, 1723, p. 657.

(*b*) De Sedibus et Causis Morborum. Epist. XIX. Art. 23.

(*c*) This is Ruffus's passage, καρωτίδας δὲ τὰς διὰ τῦ τραχήλυ κοίλας ὠνόμαζον πάλαι, ὅτι πιεζόντων καρώ δ' εις καὶ ἄφωνοι ἐγίνοντο. ὤφθη δὲ νῦν τὸ πάθημα οὐ τῶν ἀρτηριῶν, ἀλλὰ νεύρων αἰσθητικῶν πεφυκότων πλησίον· ὥστε εἰ ἐθέλοις μεταθεῖναι τοὔνομα, οὐκ ἂν ἁμαρτάνοις.

We see by this passage, that the ancients had given the name carotids to the arteries of the neck, because they believed the compression of those vessels produced a soporific state and aphony; and that it was known in the time of Rufus, that it was not the compression of these arteries, but that of the surrounding nerves, which produced those effects: which supposes that these nerves are so situated in regard to the carotids, that those vessels cannot be compressed, unless the nerves in question be exposed to be so at the same time. Now, it is evident that this cannot in any way apply to the recurrent nerves, but that it is fully applicable to the

Besides, the only effects which Rufus ascribes to the compressions of the pneumo-gastric nerves, are drowsiness and loss of voice.

Galen (*a*) makes mention of the same experiment, as having performed it both by ligature and section; and he points out no other effects, or rather he reduces the two above mentioned to one, the loss of voice.

Next to Galen, Piccolhomini (*b*) appears to

eighth pair, which are not only situated near the carotids, but so contiguous, that we cannot avoid compressing or tying them together at the same time, unless particular attention be paid. It is, likewise, under the name of nerves *adjacent* or *contiguous* to the carotids, that Galen designates the nerves of the eighth pair, in speaking of the effects of their compression in book 2. chapter 6. de Hippocr. et Platon. decretis, and in the first book, chapter 6. *de locis affectis;* and what leaves no doubt upon the subject is, that in this treatise, he compares the effects of the section and of the ligature of those nerves *contiguous to the carotids*, to those of the section of the ligature of the recurrent nerve.

(*a*) Galeni Opera. Venetiis, apud Juntas, 1576, de Hippocr. et Plat. decretis, lib. II. cap. VI. p. 239. et de locis affectis, lib. I. cap. VI. p. 6, verso.

(*b*) Anatomicæ Prælectiones Archang. Piccolhomini Romæ, 1586, p. 272.

be one of the first who took up the subject. It is not, however, certain that he performed this experiment; his expressions would induce the belief that he spoke of it rather from conjecture than from observation. Nevertheless what he says is very remarkable; he intimates not only that the experiment is mortal, but he offers an opinion upon the cause of that death, which afterwards adopted by celebrated men whose systems it favoured, and combated by others with whom it clashed, was alternately defended or attacked for two centuries. He pretends that it is by stopping the motion of the heart that this experiment kills animals (*a*).

(*a*) Riolan is incorrect, when he sometimes ascribes to Bauhin* the opinion of Piccolhomini; at others he makes him share it.† Bauhin quotes Piccolhomini, but it is to refute him; and he is supported by the authority of Galen, in asserting that the nerves have nothing to do with the functions of the heart, and that this organ contains in itself the principle of its motion.‡

* Jo. Riolani Opera Anatomica. Lutetiæ Parisiorum, 1649, p. 414.

† Idem, p. 227.

‡ Caspari Bauhini Theatrum Anatomicum. 1621, p. 219.

Riolan, who admitted no nerves in the heart (*a*), did not fail to attack this opinion (*b*). By repeating the experiment, he found that animals continued to live and even to run as before. Plempius (*c*) was of Riolan's opinion, and found in the experiment in question a proof that the heart contains within itself the principle of its motions; but it does not appear that he has performed it.

Willis (*d*) repeated it. He was particularly interested in studying its results, as he had pronounced that the principle of the internal functions resided in the cerebellum, and as he thought that it was chiefly from the nerves of the eighth pair, that the heart receives that of its motions, the effects produced by the section of the nerves of the eighth pair appeared to be the touchstone of his doctrine. In fact, he found that this experiment was in his favour, as it so

(*a*) Opera Anatom. p. 227.

(*b*) Ibidem, p. 414.

(*c*) Fundamenta Medicinæ. Lovanii, 1644, p. 112.

(*d*) Opera Omnia, edente Blasio. 1682, tom. I. Nervorum Descriptio, p. 86.

far disordered the motions of the heart as sooner or later to kill animals; and he pretended, that if death was not sudden, it was because the nervous power could yet exercise some influence upon the heart through the *recurrent nerves*, and the great sympathetic. It was likewise to the disorder of the motions of the heart that Lower (*a*) and Boyle (*b*) ascribed the death of animals which they submitted to this experiment.

These various experiments and hypotheses having given importance and celebrity to the division of the nerves of the eighth pair, several other physiologists were desirous of ascertaining its effects. Of this number were Chirac (*c*), Bohn (*d*), Duverney (*e*), Vieussens (*f*),

(*a*) Tractatus de Corde. 1708, p. 90.

(*b*) Birch's History of the Royal Society. Vol. I. p. 504.

(*c*) Mentioned by Senac, Treatise on the Heart, 2d edition, vol. II. p. 120.

(*d*) Circulus Anatom. Physiol. Lipsiæ, 1697, p. 104.

(*e*) Quoted by Senac, *loco citato*.

(*f*) Treatise on the Heart. Toulouse, 1715, p. 122.

Schrader (*a*), Valsalva (*b*), Morgagni (*c*), Baglivi (*d*), Courten (*e*), Berger (*f*), Ens (*g*), Senac (*h*), Heuermann (*i*), Haller (*k*), Brunn (*l*), Molinelli (*m*).

Of these authors, some adopt, others reject the opinion of Willis. The principal reason which gave currency to the latter was, that if the motions of the heart specially depended upon the brain through the nerves of the eighth

(*a*) Mentioned by Morgagni in his edition of the works of Valsalva. Venice, 1740, Epist. XIII. art. 30.

(*b*) Ibidem, art. 28, et seq.

(*c*) Ibidem.

(*d*) Georg. Baglivi Opera Omnia. Lugduni, 1710. Dissertatio de Observ. Anatom. Pract. No. VII. et VIII. p. 676-7.

(*e*) Mentioned by Haller, Elem. Physiol. tom. I. p. 462.

(*f*) Physiologia Medica. Francofurti, 1737, p. 63.

(*g*) De Causâ vices cordis altern. No. 4.

(*h*) Traité du Cœur, tom. II. p. 122.

(*i*) Mentioned by Haller, Elem. Physiol. tom. I. p. 462.

(*k*) Mémoires sur les parties sensibles et irritables. tom. I. p. 224-8.

(*l*) Commentarii de rebus in Scient. Nat. et Medic. Lipsiæ, tom. IV. p. 432-8.

(*m*) Ibidem, tom. V. p. 301.

pair, death must be in all cases either sudden, or very soon after the section of those nerves, whilst it only occurs after a shorter or a longer lapse of time, and sometimes several days;—and the explanation given by Willis appeared inadmissible, because the section of the great sympathetics, connected by many of the above mentioned authors, with that of the nerves of the eighth pair, had not accelerated death sensibly, or, at least, not so much as might be expected if this explanation had been correct. But, in fact, it had happened several times, that the animals died immediately upon the ligature or the section of the nerves of the eighth pair being performed. This fact had been observed by Piccolhomini (*a*), by Bohn (*b*), by Varignon, in a case he reports to the Academy of Sciences in 1706, (note) (*c*), by Berger (*d*), by Ens (*d*), by Schrader (*d*), by Molinelli (*d*), and, as it appears, by Senac (*e*).

(*a*) Loco citato. Admitting that he has made the experiment.

(*b*) Loco citato.

(*c*) Hist. de l'Académie de Sciences. An. 1706, p. 23.

(*d*) Loco citato.

(*e*) Loco citato. p. 123.

Neither Morgagni (*a*), nor Haller (*b*) who was particularly interested in elucidating them, could give a satisfactory account. The embarrassment of Haller especially was the greater, as he had met with a like case (*c*). He had seen a dog in his hands expire immediately after the ligature of the par vagum.

In this contrariety of inquiries and opinions, the attention was not solely directed to the motions of the heart. Other phenomena were observed, and new causes of death were inferred from them. It appears that Willis himself ascribed death partly to the animal's inability to eat (*d*). Baglivi, also, is disposed to believe that in some cases at least, they perished through inanition. Valsalva remarked, that to frequent efforts to vomit were added a disorder in the digestion, and even that the aliments could not readily reach the

(*a*) De Sedibus. et caus. Morb. Epist. 19. Art. 23. And in his edition of the works of Valsalva, Epist. 13. Art. 30.

(*b*) Elem. Physiol. tom. I. p. 463.

(*c*) Mémoires sur les parties Sensibles et Irritables, tom. I. p. 249, Exp. 181.

(*d*) Loco citato.

stomach, and were stopped in the œsophagus. He observed, besides, that previous to death, animals vomited a bloody froth; and that, after death, their lungs were found red and filled with extravasated blood. He suspected that the efforts to vomit occasioned the rupture of some of the pulmonary vessels, and that the hemorrhage was the cause of death. Vieussens and Senac likewise observed the red colour and the swelling of the lungs, but they ascribe this state to an inflammatory engorgement, rather than to an effusion of blood; and they thought that this engorgement might cause death by stopping circulation.

The phenomena of dyspnœa had not escaped Haller's notice, more than that of most other authors. But the gastric symptoms appear to have fixed his attention in a special manner; and, as he makes an express mention in every one of his experiments on the abolition of the digestive power, and the corruption of the matter contained in the stomach, without saying any thing of the state of the lungs, which he appears not to have examined, it is beyond all doubt, that he placed the principal cause of death in the stomach.

Besides the authors just mentioned, some others have also performed the section of the eighth pair, but with views very different from what engaged my attention.

Thus Petit (*a*) made it together with that of the great sympathetic, to determine the action of the latter upon the eyes, and thereby ascertain its origin; Fontana (*b*), Cruikshank (*c*), Haighton (*d*), Meyer (*e*), with a view of proving the regeneration of the nerves, also performed it. All these authors found that the animals perished, and they noted the principal symptoms which preceded death, but they did not seek its causes. However Cruikshank observed, as

(*a*) Mémoires de l'Académie des Sciences, an. 1727.

(*b*) Traité sur le venin de la Vipère, tom. II. p. 177.

(*c*) Journal général de Médecine, par M. Sédillot, vol. II. du Supplem. p. 80, et suiv.

(*d*) Ibid. p. 95 et suiv.

(*e*) Cité par M. Dupuytren.

well as some of the preceding authors, that an engorgement of blood was formed in the lungs.

Such were the principal remarks which had been made upon the effects of the section of the nerves of the eighth pair, previous to the re-organization of medical schools in France.

At this period, Bichat repeated this experiment. He found that respiration becomes very laborious, and continues to be so till death. It appears further, that it is particularly to this symptom that he ascribes the death, for he makes no mention of any other; and yet, by one of those contradictions which are not unfrequent in this author, he concludes from this very experiment that the brain has no actual and direct influence upon the lungs (*a*).

Mr. Dupuytren repeated this experiment some time after. His memoir (*b*) is remarkable

(*a*) Recherch. Phys. sur la Vie et la Mort, 2d partie, art. 10. sect. 1.

(*b*) Inséré dans la Biblioth. Médic., tom. 17, p. 1.

for a precision and a spirit of analysis not to be found in the authors who had preceded him. His special object was to determine the kind of influence which the brain exercises over the lungs through the nerves in question. The result of his inquiries was, that the animals upon whom the section was made, constantly die of asphyxia. He found the proof of it not only in the dyspnœa which takes place, but also in the colour of the arterial blood, which becomes blacker and blacker, as in asphyxia. This asphyxia might be supposed to arise in two different ways:—either the asmospheric air, though penetrating freely into the breast, can no longer be combined with the blood that passes through the lungs, or convert it into arterial blood; or its introduction into the lungs is prevented, and not reaching the pulmonary vesicles, it can no longer be brought in contact with the blood. We perceive that in both cases the effect is the same, since no more arterial blood can be formed. Mr. Dupuytren declared for the first of these two modes of asphyxia. He concluded therefore, first, that all animals on which the section has been made of the two pneumo-gastric nerves die of asphyxia; second, that they die because the atmospheric air, though continuing to penetrate freely into the lungs and, coming in contact with the blood, can no longer be combined with this fluid, as this combination can only be made by the influence of the vital principle, and through the medium of the nerves.

This second part of Mr. Dupuytren's opinion, was liable to great objections; for it is an ancient as well as a daily observation, that extravasated blood, when in contact with the air, assumes a fine arterial colour. Besides, if the asphyxia was due to the alleged cause, it must be sudden and complete, and animals must perish as quickly by the section of the two pneumo-gastric nerves as they do by submersion or strangulation. Now this is what Mr. Dupuytren himself had not observed. Mr. Dumas (*a*), Dean of the faculty of Montpellier, not satisfied with these observations, had recourse to direct experiments, which

(*a*) Journal général de Medicine, par M. Sedillot. Tom. 33, p. 353.

he made upon dogs; and he found that, by blowing air into the lungs of those animals, after having divided the par vagum, arterial blood was formed of as fine a red colour as before. He thence concluded that this operation does not at all prevent the combination of the air with the blood, which goes through the lungs; but that it produces the second of the two modes of asphyxia, which I have mentioned; that is, that it renders the introduction of the air into the lungs difficult, so that an external force is necessary to propel it into the pulmonary vesicles. But he did not indicate the cause which thus prevented the air from penetrating into the lungs.

About the same time Mr. Blainville took up the same question (*a*). He concluded from his experiments that the blood combines itself with air, full as well after as before the section of the nerves, and that air does not cease to enter freely

(*a*) Propositions extracted from an essay on respiration;—Inaugural dissertation inserted in the collection of Theses of the Faculty of Medicine of Paris, An. 1808, No. 114.

into the lungs; and rejecting all idea of asphyxia, he appeared, with Haller and some other physiologists, to admit that the principal cause of death depended upon the abolition of the digestive power, and the alteration of the matters contained in the stomach. Nevertheless he had taken the precaution o ascertain the state of the lungs after death, which Messrs. Dupuytren and Dumas had neglected to do. He had remarked that in the rabbits submitted to his experiments the bronchiæ were more or less filled with mucocity, at times streaked with blood, and that the lungs were covered with large brown spots. But it appears that he had only considered those spots as superficial (*a*).

In this state of things, Mr. Provençal (*b*) applied himself to ascertain whether there was real asphyxia. He had recourse for this to means entirely chemical. Considering that whenever an animal is more or less asphyxiated, he consumes in a given time less oxygen gas, that he forms

(*a*) Ibid. p. 20, et seq.

(*b*) Bulletin des Sciences Medicales. Tom. V. p. 361.

less carbonic acid, that his temperature is lower than when he is not; Mr. Provençal examined what those animals offered in these three different views, in whom he had divided the par vagum, and found that they were in a perfect state of asphyxia; which became greater and greater in proportion as they approached their end. Besides, he, like Mr. Blainville, took care to examine the lungs, which he found red, and engorged with blood in dogs, but without any preternatural appearance in rabbits and guinea-pigs. His experiments appeared to settle only the fact, and not the mode of asphyxia: nevertheless he appeared to admit the second part of Mr. Dupuytren's opinion; but with this restriction, that the division of the par vagum only prevents, to a certain degree and not entirely, the combination of oxygen with the blood.

By resuming the opinions of the various authors which I have just mentioned upon the cause of death, after the ligature or the division of the par vagum, we see that this cause has been successively placed in three different organs; to wit, the heart, the stomach, and the lungs; organs which, in fact, all receive, more or less, small filaments from the par vagum. It has been objected with propriety, that death would be prompter than it commonly is, if it were caused by the immediate suspension of the motions of the heart; and much slower if it only depended upon the abolition of the digestive power. As to the lungs, in investigating to what alteration, either of their substance or of their functions, the cause may be attributed, it is evident that the quantity of blood extravasated or engorged in those organs, is not sufficient to occasion death by the hemorrhage; and admitting that the engorgement be inflammatory, it is not probable that this engorgement causes the death of animals by stopping circulation.

The asphyxia accorded best with the principal phenomena of the experiment; but although its existence had been proved by direct ones, the difficulty of understanding the manner in which it was produced, had given rise to doubts upon the very ground of the subject; and some authors had rejected the fact, merely because they could not understand the *mode*.

Not long after Mr. Dupuytren's experiments were published, I had occasion to make use of the division of the pneumo-gastric nerves to produce asphyxia, whatever was in other respects this mode of asphyxia. I was then intent upon ascertaining, how long animals of the same species but of different ages can without dying bear asphyxia, when simply produced by the interception of the air, or the suspension of the inspiratory motions. After ascertaining the rule by which this time decreases from the moment of birth to the age of adults, I was desirous to know whether the periods at which animals of different ages die after the division of the par vagum, would be in conformity with this rule. The first animal which I submitted to this experiment, was a small dog two days old. I knew by my own experiments, that a new born dog will bear asphyxia about seven times longer than an adult; and I had learnt by those various authors who have divided the par vagum of an adult dog, that it only dies after one or two days, and sometimes even much later. I had reason, therefore, to hope that my small dog would survive for a considerable number of days. But it turned out quite differently. As soon as I had divided the nerves of that small animal, he made the greatest efforts to breathe. I clearly saw that little or no air entered the thorax. He struggled in a convulsive manner. The conflict lasted but two or three minutes, after which his body was flaccid and his head hanging down; the power of sensation still continued, and he made every now and then new efforts to breathe; but sensibility was gradually extinguished, and, in less than half an hour he showed no signs of life. This result astonished me. It was not long before I repeated the experiment upon a dog of the same age. But the issue was the same. The examination of the bodies of these two dogs had produced no satisfaction. I was still seeking the cause of this strange phenomenon, when one day, teazed by the shrill cries of a little dog which I held to tie its carotids for a particular experiment, I had recourse, to quiet it, to Galen's method, and divided the two recurrent nerves which first presented to my view. He immediately made great efforts to breathe, and, after having manifested the same phenomena as those whose par vagum I had divided, he died in my hands in less than half an hour. In whatever mode the division of the recurrent nerves had caused the death of this small dog, there was no doubt that the death of the two first dogs had been produced by the same cause. We know, indeed, that by dividing in the neck the two nerves of the eighth pair, we necessarily cut the recurrents, which are branches supplied by the former as they enter the thorax.

It remained to know, why the section of the recurrent nerve produces death so quickly; as these nerves are distributed to the larynx, it could only be in this organ that the cause was to be sought. I suspected that it only consisted in a sudden and considerable diminution of the opening of the glottis. One way to confirm this suspicion, was to make a large opening in the trachea below the larynx, after having divided either the recurrent nerves or those of the eighth pair. The air having ready access by this opening into the lungs, without passing through the glottis, all the symptoms of suffocation which I had observed in my three little dogs, would no longer take place, if my conjecture was well grounded. I did not fail to submit to this confirmation the first dog I could find; it was three days old. The division of the recurrent nerves was performed, and complete asphyxia succeeded as in the preceding cases. Sensibility was about to be extinguished, and he was only making unfrequent efforts to breathe, when I made an opening in the trachea. Upon its first inspiration, the air rushed into the thorax by this opening, the carotids from black soon became of a fine red colour, and the animal recovered without any other assistance. I have likewise made an opening in the trachea of other small dogs, in which I had divided the nerves of the eighth pair; the effect has been the same, only respiration was left a little deeper than after the section of the recurrents.

I was desirous afterwards of ascertaining whether the same phenomena took place in the other species of animals. I have therefore divided sometimes the par vagum, at others the recurrents of cats, rabbits, and guinea-pigs in the first days after birth. I found that cats die in the same manner, and, perhaps, still quicker than dogs. The division of the recurrents does not obstruct the glottis so completely in guinea-pigs as in rabbits; the former only die about an hour after, and the rabbits at the end of a few hours. But although the glottis continues to afford more or less passage to the air, in the animals of these two species, after the division of the recurrents, the dyspnœa which ensues appears to be the principal cause of their death when the par vagum is divided; for they live about the same time after the division of these last nerves, as after that of the recurrents; and on the contrary, they live longer after the division of the par vagum, with an opening in the trachea, than after that of the recurrents without a like opening. I had divided the par vagum of a little guinea-pig only a few hours old: he died an hour after. I immediately took another of the same litter, as a standard of comparison, in which I only divided the two recurrents. Fifty minutes after this operation, the dyspnœa becoming, by degrees, intolerable, it fell on its side, and appeared to be dying. I made an opening in the trachea; respiration was re-established, and he recovered quickly. Eighteen hours after it had pretty well recovered, when I performed the division of the par vagum, which he only survived three hours and a half.

After determining the influence of the recurrent nerves upon the effects of the division of the par vagum in these four species of animals about the time of their birth, I applied myself to investigate what becomes of this influence, in proportion as these animals grow older. I shall not here enter into all the particulars of this inquiry, but will only mention, that the division of the recurrent nerves produces a suffocation, which is less considerable in proportion as the animals grow older; thus in dogs and cats two or three weeks old, this operation produces a dyspnœa, which, though slighter than in the first days after birth, is sufficient to cause death at the end of a few hours. At the age of three months, and even sooner, it does not sufficiently disorder dogs as to cause their death; cats are more affected, and if they be ever so little agitated or forced to walk, they fall as if suffocated. If, in a cat of this age, you add the effect of the recurrent nerves upon the glottis, and that of the par vagum upon the viscera of the thorax,—a double effect, always produced by dividing these nerves in the neck, then the dyspnœa is of the most violent kind; and there is no way of preventing threatened death, but by making an opening in the trachea. After it is made, respiration is performed without great efforts, though it is less frequent than in health, and it becomes so more and more. As often as you stop this opening with the finger, the animal falls into convulsions, as in the beginning of a complete asphyxia.

It is exactly so with rabbits and guinea-pigs; the dyspnœa, produced in them by the division of the recurrent, is less severe in proportion as they are older; but it is always greater in guinea-pigs than in rabbits. For instance, the latter are much less disordered by it when one month old, than the guinea-pigs at five months old. These may still die in consequence of it, in the space of twenty-four hours.

The reason of these differences is easily conceived. It is produced by the opening of the glottis in animals of the same age, being, in proportion to the capacity of the lungs, larger in one species than in the other, and larger still in the adult than at the time of birth in those of the same species, as Professor Richerand had already ascertained in the human species (*a*). Now, if we admit that the figure of the glottis be nearly alike in these various animals, the areas of the like figures being to each other as the squares of similar dimensions, we find that a contraction of the same extent in the opening of the glottis, ought to intercept the passage of the air in very different degrees.

This etiology of suffocation, produced by the division of the recurrent nerves, is in uniformity with that which I had published after my first experiments. It admits that the effect of this operation is to lessen the opening of the glottis. This appeared to me sufficiently proved by all the circumstances of suffocation, and particularly by the method which occasions its cessation. But some respectable anatomists had

(*a*) Nouveaux Elemens de Physiologie, 3d ed. Tom. II. p. 436.

doubted it. Some asserted that the cartilages which compose the larynx, possess too little mobility in regard to each other, to allow any contraction worthy of notice, much less a degree sufficient to produce suffocation. Others said, that the property of the division of a nerve being to paralyze the parts to which this nerve is distributed, and the palsy being always accompanied with relaxation, the section of the recurrent nerves ought to relax, and consequently enlarge instead of contracting the glottis.

To clear up these doubts, I performed the following experiments, before the society of Professors of the Faculty of Medicine of Paris.

I took rabbits about two months old, in whom I separated the larynx from the os hyoides, and adjacent parts, without wounding either its own muscles or the recurrent nerves; after which I inclined it sufficiently towards the breast, to show plainly the opening of the glottis. This opening was evidently round, or at most slightly oval and perpendicular, (the larynx being supposed in its proper situation, and the animal on its feet) especially during the inspirations. This situation well ascertained, I divided the two nerves of the eighth pair at the middle of the neck; immediately the two arytænoid cartilages approached near to each other, and to the thyroid; the opening of the glottis was diminished, and, instead of a hole nearly round, only presented a permanent and perpendicular fissure. In other rabbits of the same age, the arytænoid cartilages and the glottis, before the section of the same nerves, had motions corresponding with those of respiration. At each inspiration the glottis enlarged and assumed a round shape; afterwards, during the expiration, it grew narrower by the approximation of the arytænoid cartilages to each other and towards the thyroid, and thus successively. But after the division both of the nerves of the eighth pair and the recurrents, it remained immoveable and contracted to a fissure. We are to observe, that these motions of the glottis only take place, or, at least, are only observable when respiration is somewhat laborious. When it is free, the glottis remains pretty widely open, with very little variation.

These comparative situations of the glottis, before and after the division of the nerves in question, in animals in whom this operation never causes threatened suffocation, even at the moment of their birth, sufficiently indicated what must take place in those upon whom it produces the effect. I have repeated this experiment which I had made upon rabbits, on three new born dogs and four cats. In these seven animals the opening of the larynx was attended by motions which had a regular correspondence with those of respiration. Upon each inspiration this opening enlarged itself, and towards the end of expiration it grew so narrow as to appear closed, and continued so till the moment when a new inspiration was begun. By cutting either the par vagum or the recurrent on one side, the opening of the larynx lessened one half of its size immediately, and the arytænoid cartilage of the same side was left motionless; that on the other side preserved its motions. When the two nerves of the eighth pair or both recurrents were divided, the two cartilages were motionless and contiguous on their internal edges; the ligaments of the glottis were also drawn together on their sharp edges, and the glottis appeared entirely closed. Every effort of inspiration made by the animals closed it more instead of opening it; and this was produced by the pressure of external air, which still increased the closure of those ligaments, on account of their oblique position, and the sort of *cul-de-sac* formed by their anterior surface. The expiration, on the contrary, was very easy. I wholly separated the larynx, together with a certain extent of the trachea, and introduced the end of a syringe into the latter: the air propelled by the syringe, issued freely through the larynx; but when the piston, drawn back in a contrary direction, inspired the air by the glottis, I felt, in moving it, a resistance equal to that I should have felt, if I had put my finger on the end of the syringe.

Therefore, it is clearly and really, by paralysing the arytænoid muscles, and thus relaxing the ligaments of the glottis, that the division of the recurrent nerves produces suffocation.

From all I have just stated, it results that in experiments upon the division of the eighth pair, the effects of this operation upon the viscera of the thorax and abdomen, to which the nerves are distributed, are more or less complicated with the effect of the division of the recurrent nerves upon the larynx; and that according to the age and species of animals, this complication may be so great as to become the immediate cause of death, which in this case happens more or less suddenly, but long before the time when it would have taken place in consequence of the division of the eighth pair, free from this complication.

These facts lead us therefore to a very simple explanation of those cases of sudden death, which happened after the division of the eighth pair; by which, as I have said above, some authors had been so much puzzled and others so gratified. In fact, of the authors whom I have mentioned as having observed those sudden deaths, those who have had the attention to point out the species and age of those animals on whom they made their experiments, inform us, they were dogs and cats, and that they were newly born.

Here therefore appears a new effect of the division of the recurrent nerves, and consequently of that of the par vagum, which I know not to have been observed by any of the numerous authors who have performed either of those two operations. It is known that Galen, to whom is ascribed, or rather who ascribed to himself, the discovery of the recurrent nerves, is also the first who has performed their division. The only effect he observed from them was aphony: the animal on which he performed was judiciously selected to show this effect; it was a pig (*a*). This experiment was afterwards repeated by Vesalius (*b*). It was also by Colombus (*c*), by Riolan (*d*), by Bidloo (*e*), by Muralto (*f*), by Chirac (*g*), by Drelincourt (*h*), by George Martin (*i*),

(*a*) De locis affectis. Lib. I. cap. 6. De præcognit. ad posthumum. p. 216.

(*b*) De hum. corporis fabrica. Basileæ. 1555. p. 823.

(*c*) De re anatomicâ. Parisiis, 1562. p. 473 et 477.

(*d*) Encheiridium anatom. Parisiis, 1658. p. 243.—Opera anatomi. p. 414.

(*e*) Exercitationes anatom. chirurg. Lugd. Batav. 1708. p. 2.

(*f*) (*g*) (*k*) (*l*) Cités par Haller, Elem. Phys. tom. III. p. 409.

by Courten (*k*), by Emmet (*l*). Portal (*m*), and Dupuytren (*n*), have also performed it. Aphony alone arrested the attention of all these authors, who confined themselves to the studying its various circumstances (*o*). Thus they examined how far the voice is weakened by the division of one nerve, how far it is extinguished by that of the two nerves, in what cases and how long before the animal can recover from it. All these questions being foreign to my object, I shall pay no attention to them. But I ought to caution my readers, when they peruse these authors, to observe if the nerves have been tied or cut. The ligature may produce results that may

(*h*) Experimenta anatom. Ludg. Batav. 1681. p. 11.

(*i*) Essais et observ. de Médecine de la Sociétié d'Edimbourg. Paris, 1742. tom. II. p. 138.

(*m*) Lettre de Collomb sur un cours de physiologie, fait par M. Portal en 1771.

(*n*) Mémoire cité plus haut.

(*o*) The cause to which Martin ascribes aphony is remarkable. He thinks that the effect of the section of the recurrents is to enlarge the glottis. It was upon a pig, five or six weeks old, that he made his experiments. Since the operation, says he, the animal breathed as if the glottis had been too widely opened; he died at the end of six or seven weeks, being still in a state of aphony.

appear contradictory, as it has not been made tight enough wholly to intercept the action of the nervous power, or as it has been sufficiently so to produce that effect without disorganizing the nerve, or in short as it has been so far so as to disorganize it. In the first case, aphony is more or less incomplete, in whatever degree it may exist; in the second, it ceases as soon as the ligature is removed; it continues in the third, after the removal of the ligatures, as if the nerves had been divided. This remark is applicable to the ligature of the par vagum, and to that of the other nerves. Though the effects of the ligature, when carried to the degree which constitutes the two last cases just mentioned, be nearly the same with those of the division, nevertheless, to avoid all doubt, I have always had recourse to the division in my experiments either on the recurrent or the par vagum.

But the authors I have quoted, have indiscriminately employed both the ligature and the division; and if, on reciting their experiments, I have mentioned most commonly the division, it was with a view to avoid unnecessary details.

From all that precedes, it results that to estimate correctly the effects of the division of the par vagum upon the viscera of the thorax, it is necessary first to know those of the division of the recurrents; and that in most cases, it is proper to begin destroying the effects of the latter, by making a large opening in the trachea, taking away the piece. Not that this opening is free from inconvenience; it occasions inflammation, and consequent swelling in the surrounding parts, especially in the membrane that lines the trachea; extraneous bodies may also be introduced into it; finally, the muscles and the skin often obstruct it. But I know of no other means that can supply the want of this opening. All that can be done, in cases where the inconveniences cannot be prevented, is to account for them in the result.

Let us suppose then, that by performing the division of the par vagum, we have ascertained, that no effect capable of affecting respiration shall be produced upon the larynx; the question is to seek in this case, what is the cause of death. I have said before, that, in this experiment, I had no other object in view at first, than to know if the periods at which the animals of different ages die, accorded with the lapse of time in which animals of the same species and the same age perish by asphyxia. The comparison was easy to be made; for the lapse of time in which animals may support asphyxia, though variable according to the age, is nearly fixed for every age, and only admits a very small latitude in individuals of the same species. I therefore performed the division of the eighth pair upon different species of animals, and particularly upon rabbits, from the moment of their birth, to the age of one or two months. I found nothing certain nor settled, in the time in which animals of the same age perished in consequence of it; and I remarked, in the different ages, nothing to be compared to this decreasing law, according to which animals bear a shorter asphyxia, in proportion as they are older. Thus I have seen new born rabbits, &c. die after the division of the par vagum, full as quickly as others aged two months; and frequently the latter survive as long as those much younger. This led me to think that they do not die of asphyxia, or that if they do, it is complicated by some circumstances, variable according to the individuals. This was the opinion I had adopted, when my experiments upon decapitation brought me back to resume those on the division of the eighth pair; with a view to discover, if possible, what was the true, or, at least, the principal cause of death.

I shall not here state minutely all the phenomena produced by these operations; they have been observed and described by the authors above mentioned. I am only to attend to the results. Therefore when we examine these phenomena with attention, we find that the gastric viscera, the lungs and the heart are affected. The gastric viscera, because the animals are more or less distressed with nausea and even with vomitings, in the species capable of vomiting;—the lungs, because there is always a considerable dyspnœa, the intensity of which only increases till death;—the heart, because in general the carotids lose their fulness and tension.

The heart, the lungs, and the stomach are organs of so great importance, and the disorder of their functions so far exposes the existence of the animal, that one alone would be sufficient to cause death. It is possible, therefore, that each of those organs, considered separately, should be so violently affected by the division of the nerves of the par vagum as to occasion it; and I will say, that this appears to me very probable. Nevertheless it cannot be thence concluded that the immediate cause of death resides in all and in each of these organs. For, on the one hand, they may not be affected in the same degree; and on the other, admitting that they should be, their functions, though indispensable to the support of life, are so in a manner more or less immediate; I mean that the cessation of the functions of each of those organs, though necessarily mortal, is not so in the same time; and that consequently the disorder of one organ causing death before that of another has had time to produce the same effect, it is only to the first that this effect must be attributed. Let us suppose, for instance, that in an adult rabbit the functions of the heart, of the lungs, and of the stomach cease entirely, and at the same time; in this case death will be almost sudden, and it will take place precisely within the same time as if the functions of the heart had ceased. It is evident that we shall not be able to ascribe it to the cessation of the functions of the stomach, since an adult rabbit only dies after three weeks complete abstinence; nor to that of the functions of the lungs, for although the length of time during which rabbits outlive it, is very short, it is at least twice as long as that which they outlive the cessation of circulation. If, on the contrary, the functions of the heart should continue undiminished, and those of the lungs and stomach were alone annihilated, death in this case would also be very prompt, but less so than in the first case; it would happen in the same space of time as after a complete asphyxia, nor could we attribute to it the cessation of the functions of the stomach. If the disorder of those organs was proportionably more violent in one than in the other, and that in neither of them was it sufficient wholly to suspend their functions, the effects would no longer be the same, and death would no longer be attributed to that organ, the functions of which admit the shortest interruption; but it would depend upon the organ, the disorder of which would be the most considerable; or rather the cause of death would be in a compound ratio of the disorder of the organ and the importance of its functions. This is what takes place after the division of the nerves of the par vagum. In this experiment the heart, the lungs, and the stomach, are affected in different degrees, and none of these organs are sufficiently so as entirely to suspend its functions, except the stomach in certain cases. To inquire how the division of those nerves occasions death in animals, is therefore to inquire which among the impaired functions are those which are sufficiently so as to produce death before the disorder of the others has had time to produce the same effect.

The principal sign by which the heart is known to be disordered after the division of the par vagum is, as I have said before, a decrease of the fulness and tension of the arterial system, which is very easily distinguished in the carotids. It is very probable, that the motions of this organ also suffer disorders, both as to their frequency and regularity, but it is difficult to ascertain it, and not to confound the confusion caused by pain and fear, during the experiment, which is renewed by the agitation excited whenever the hand is laid upon the breast of the animal to feel the motions of the heart, with that which is only owing to the section of the nerves. Notwithstanding I have never observed that those disorders were as considerable as Willis and Lower have said, at least in the beginning of the experiment. Towards the end, when death is near, the pulsations of the heart are very unfrequent, and irregular, but many causes may contribute to make them so. In short, the disorder of the heart would, no doubt, in time produce unfavourable effects, and it ought to aggravate the other symptoms, but nothing shows that they may be considered as the immediate cause of death. I shall endeavour in another instance to determine, by direct experiments, the kind of influence the brain exercises upon circulation through the intermedium of the par vagum.

The disorder of the stomach is, in general, much more severe than that of the heart; for the functions of the former of these organs suffer a much greater disorder than those of the latter. I even think that in certain cases, of all the functions injured by the division of the par vagum, those of the stomach are so in the highest degree. At least, this is what takes place in some species. In guinea-pigs, for instance, digestion appears to be not only weakened or deranged, but entirely abolished. I had divided the par vagum on the right side, in a female guinea-pig about eighteen months old. Respiration still continuing pretty free, and the anxiety being moderate, the animal continued to eat. But in proportion as it eat its belly increased in size. It became so enlarged, that its size was almost equal to the length of its body. It died in four days and four hours after the division of the nerves. The stomach occupied almost all the capacity of the belly, it was distended by a large quantity of food, which was found nearly in the same state as that in which it had been swallowed.

It is evident that in this experiment, the stomach had wholly lost its power of digesting and that of propelling the food into the intestines. This effect does not always take place after the division of one single nerve, but we can scarcely doubt that the division of both nerves will ever produce it, especially when we consider in this last case, how guinea-pigs are distressed by nausea and efforts to vomit. Now, after the division of the two nerves, guinea-pigs of the age of that we allude to, die in the space of three or four hours, and sometimes still sooner. Their death cannot therefore be ascribed to the abolition of the digestive power, which they are able to survive upwards of four days, even when it is most complete. I say upwards of four days; for it appears that in the case I have just mentioned, the abolition of the digestive power was the occasional cause of death, and that the proximate cause, was the result of the enormous distensions of the stomach, which had rendered respiration very laborious, and had, besides, determined a certain state of phlogosis in the membranes of this viscus, as well as in the omentum and the peritonæum. It is highly presumable, that had it not been for this complication, the animal would have lived the same length of time, as during a complete abstinence of nine or ten days.

As death cannot be attributed to the state of the stomach, even in animals, the digestion of which is destroyed, much less can it be so in those with which, like the rabbits, the gastric symptoms are less violent. I shall add that I have never met with that corruption, that putrid degeneracy of aliments, contained in the stomach, which several respectable authors have considered as the cause of death. I had expected, that this effect would be more observable and easy to distinguish in animals still sucking; and by dividing the par vagum at different ages, I had bestowed particular attention on those which took no other food than the mother's milk. But on examining comparatively with this view, animals who had died of this operation, and those who died in any other manner, the milk contained in the stomach of both, ever presented the same appearance. Besides, admitting that the aliments are corrupted in the stomach of animals, in whom we had divided the eighth pair, could it be thence concluded, that this corruption is the immediate cause of so quick a death, as that which most frequently takes place in this experiment? Is it not known, that in certain disorders of the stomach, the aliments undergo a variety of very considerable alterations, which do not prevent the individuals labouring under those disorders, from prolonging their life for a sufficient time? Finally, I shall add that the stomach itself, if I except a slight phlogosis, has presented to my view nothing particular, and this state was only observable in a small number of cases.

Of all the symptoms produced by the division of the par vagum, those in which respiration is concerned, are at once the most steady and the most remarkable; for this reason, they have been observed by most authors who have repeated this experiment. These symptoms manifest themselves as soon as the nerves have been divided, and their severity increases more and more. Thus, respiration is slow, and performed principally by the elevation of the first ribs, and in proportion as it becomes more laborious, every inspiratory power is called into action. The animal keeps itself quiet, (especially rabbits and guinea-pigs,) and seems only desirous of inspiring as much air as it can. The colour of the arterial blood at first but little changed, loses its brightness by degrees, and assumes darker shades. The animal is found by the touch to become colder. Nevertheless, respiration is never wholly stopped immediately after the division of the nerves, as digestion appears to be, at least in certain cases; and it is scarcely to be doubted, that if the dyspnœa made no progress, and continued such as it is in the beginning of the experiment, the animal could live a considerable time, and would die of inanition rather than of asphyxia. If the immediate cause of death resides in the lungs, this cause should possess the property of acquiring gradually, such an intensity, as to render respiration more laborious, and to produce complete asphyxia in the end. Therefore, in all the animals that have died of the division of the par vagum, we constantly find that the lungs are more voluminous than in the natural state, and that they are gorged with blood. The sanguine ingorgement gives them a brown and red colour, which is not commonly uniform, but which is spread in large spaces. The pulmonary vesiculæ are so depressed by it, that if you separate those spaces from the portions which are left more or less free from it, and throw them into water, they sink to the bottom. Further, we most frequently meet in the bronchiæ, with a frothy fluid, sometimes of a reddish colour, in sufficient quantity to fill up the pulmonary vesiculæ, and the greatest part of the bronchiæ, which swells the lungs in the spaces which are not gorged with blood. This fluid is produced by a serous extravasation which is formed in the aerial tubes, and which are converted into froth by the respiratory motions, by mixing it with the inspired air. This fluid is found in great quantity in rabbits and guinea-pigs; it is often seen to issue out of their mouth and nostrils in the last moments of life. After death it flows through the incisions made in the lungs; and it is often sufficient to make an opening in the trachea, and to compress the belly and the breast, to make it flow through this opening. The effect of the sanguine engorgement and frothy effusion, is evidently to prevent the air from penetrating into the pulmonary vesiculæ;

and the inspection of these two states of the lungs, leaves no doubt that if they should occur immediately after the division of the par vagum, in the degree observed after death, the asphyxia would be complete from the very first instant. But they are only formed and increased gradually, as it is easy to ascertain by killing animals at different periods after the division of the par vagum, to examine their lungs.

The engorgement of blood, and the frothy effusion, are somewhat in an inverse ratio of one to the other. When the effusion happens early, it suffocates the animal before the engorgement has had time to make much progress, and death happens sooner. When, on the contrary, this effusion is formed slowly, and in a small quantity, the animal dies later, and only when its lungs are almost entirely gorged with blood. The time which both of these states of the lungs takes for their formation, is very variable, and appears to belong to individual circumstances, rather than to the age of animals of the same species; hence the time which the animals survive this experiment ought to vary in the same way; and it does in fact vary much, as I have said before. This explains the reason why this time does not accord with that during which animals of the same species and age may support sudden and complete asphyxia.

It remains to inquire in what way the division of the par vagum produces both these effects in the lungs. It is probable that it is in a manner analogous to what takes place in the other parts, the nerves of which are divided. It is known that they fall into a state of palsy and flaccidity, not much unlike what takes place after death. Without doubt there also happens in the lungs a loss of tone, a sort of palsy. At least this is what appears to indicate the remarkable weakness observed in the texture of this viscus, which is easily torn, especially in the places which are gorged with blood. The experiment of Hales confirms this opinion. This author (*a*) found that by introducing blood into the pulmonary artery, through a tube adapted to this artery,

(*a*) Hæmastatique sauvages translation. Geneva 1744, eleventh experiment, p. 61-6.

held vertically and only two feet high, the lungs were distended and become very red, and that serosity is poured into the pulmonary vesiculæ through the arterial coats. Hales observes, with reason, that this ready transudation of the serosity is caused by the relaxation and atony that exist after death.

It has been seen that among the authors who were engaged in the division of the nerves of the eighth pair, several had perceived the sanguine engorgement of the lungs, and that some had even indicated it as a cause of death; but at the time when these latter authors wrote, the true theory of respiration was not yet known; it was not to asphyxia they had referred this cause, but to a hemorrhage, or a pulmonary inflammation carried to a mortal degree. As to the effusion of a fluid into the bronchiæ, I do not know that any other, besides Mr. Blainville, has made mention of it; and it is recollected that this learned man had not thoroughly investigated its effects on respiration, any more than those of the sanguine engorgement of the lungs.

In a memoir which I had the honour of presenting to the first class of the Institute in the year 1809, on the experiment in question, I ascribed the death of animals to the closure of the glottis; or, when the glottis remained sufficiently open, to the two states of the lungs just mentioned; the class appointed a committee to examine the facts. I shall here produce the results of the experiments which I then repeated before the commissaries, and which Messrs. Dumeril and Blainville were good enough to attend. To shorten this account I shall only relate the experiments upon the par vagum, and must omit those which I made at the same time on the recurrent nerves.

The division of the eighth pair of nerves was performed upon a dog fifteen days old. Respiration immediately became very laborious. The animal opened its mouth very wide, and made great motions of the thorax for respiration. The carotids being uncovered were of a brown colour. At the expiration of five minutes, the body having lost its vigour and the head being in a drooping posture, a large opening was made in the trachea: respiration soon ceased to be laborious; the carotids assumed a beautiful purple colour, and the strength returned. This fact, which argues against the mode of asphyxia adopted by Dupuytren, proves at the same time against Dumas's opinion, that air can penetrate into the lungs freely enough, and without the help of inflation during the first moments of the experiment.

The same nerves were divided in two guinea-pigs about a year old, and in three rabbits about two months old. A fourth rabbit of the same breed, was strangled by means of a tight ligature round the trachea, with a view of comparing its lungs with those of the three others.

Having terminated these experiments, the animals were placed in a lower room, and remained there for twenty-four hours, when we examined the bodies. We expected to find them all dead by that time; and they were so in fact.

The lungs of the dog were very red and filled with blood, but less so than they are commonly found in this experiment No portion of them sunk to the bottom in water. The cold of a low and damp room ought to have contributed to destroy this little animal, which was yet accustomed to the heat of its mother, before the division of the par vagum had been performed long enough to produce its entire effect upon the lungs. For cold alone is sufficient to kill very young animals in a very short time.

In the lungs of the two guinea-pigs, an engorgement of blood was observed very distinctly, and distributed in large spots. The bronchiæ of one of these animals, was filled up with a reddish and frothy fluid. Those of the other only contained a very small quantity.

A fluid entirely similar was abundantly found in the bronchiæ of one of the three rabbits; a slight pressure upon the abdomen and thorax was sufficient to make it flow out, by an opening made in the trachea. One of the two others presented only a small quantity of this fluid. The third appeared not to contain any. But in this last animal, we found a serous effusion and some hydatids in the two cavities of the thorax. In these three animals, the lungs were swelled with blood, and of a brownish red colour in some places, between which smaller ones had preserved the natural pale rose colour of the lungs. On separating and throwing into water the engorged masses, they sunk to the bottom. Nothing similar was observed in the rabbit that had been strangled. Its lungs were filled with air throughout, and of an uniform pale rose colour; they were likewise flaccid and had diminished in bulk, while the lungs of the other three, as well as those of the dog and the two guinea-pigs, were more or less tumid.

I must here remark in respect to the sanguine engorgement of the lungs, that it is not only after the division of the par vagum that it is met with; it is also observed in several other cases, and principally in the most of those, whose death has been the result of a long continued asphyxia. But in all these cases, it does not present precisely the same appearances and the lungs are not tumified in the same manner, as after the division of the eighth pair. The effusion of a serous fluid in the bronchiæ, is also observed in other cases. It comes on more particulary in the affections of the chest, complicated with debility and atony, and is the most frequent termination of peripneumonia notha, which is so often fatal to old people. Their bronchiæ fills up progressively, the rattle comes on, and they die stifled (*a*).

Let us recapitulate the principle facts, relative to the division of the par vagum.

It most commonly occurs, that the division of a single nerve is not mortal. That of the two nerves is constantly so.

The division of the two nerves affects at the same time the larynx, the heart, the alimentary canal and the lungs. The affection of the larynx is *propagated* by the recurrent nerves, in such a manner as that the division of the nerves is

(*a*) Cullen's Practice.

sufficient to produce it. This affection consists not only in an alteration of the voice, but also in a diminution of the aperture of the glottis. Both of these effects are owing to the paralysis of the arytænoid muscles, which permit the arytænoid cartilages to incline towards the glottis, which relaxes its ligaments, and brings them nearer to one another at the same time; and all these parts remain motionless in that state.

The diminution of the aperture of the glottis, varies according to the species, and still more so, according to the age. In certain species, such as dogs, and especially cats, it is so considerable, that those animals are strangled nearly as readily as if the trachea had been tied. As these animals grow up the danger diminishes; and when they have attained a certain age they are but slightly affected by it; at least, this is what happens in dogs.

Hence, we may conclude, that of all the symptoms produced by the division of the par vagum, the most severe, and those which cause death most promptly, are in certain cases those which are connected with the larynx. In general, whenever the difficulty of breathing increases immediately after the operation, it is to be presumed, that its principal cause exists in the larynx. For example, the violence with which dyspnœa makes its sudden appearance in horses even of an adult age, and the quickness of their death, proves that, in those animals, the glottis undergoes a considerable contraction. A large opening made in the trachea, furnishes at once the remedy and etiology of all those cases. The aperture of the glottis is, therefore, never in the living animal such as we find it in the dead body; and the arytænoid cartilages require to be supported by their muscles, as the superior eye-lid requires to be supported by its own muscle.

The affection of the heart is difficult to be determined; but whatever may be the effects it can finally produce, it does not prevent the continuing of the circulation; and other functions are disordered in a mortal degree, before these effects have acquired all their force.

The affection of the stomach is in general more violent. It is so in different degrees according to the species, and even according to the individuals of the same species. But we do not find in this viscus any *morbid* state well determined, except sometimes a slight state of phlogosis. It does not appear that the aliments contained in it, undergo any particular putrefaction; and even if this should take place, it is very doubtful whether this corruption, any more than the entire destruction of the functions of the stomach, would be the immediate cause of death. Finally, death comes on at a period, and with such a series of symptoms, as does not permit us to place its cause in the stomach.

These symptoms are those which depend upon an affection of the lungs; they are the most remarkable and the most constant which we observe in the experiment in question. Respiration is deep and laborious, and becomes so more and more. It sometimes takes place with a frothy noise, which can be heard in the thorax. The arterial blood assumes a darker hue and the animal loses its heat. After death we find the lungs swelled with air, partly filled with blood and partly with a serous and often frothy fluid, and inspection of them clearly shows that the exterior air could no more penetrate, or if it did, it was in a very small quantity. The formation, not sudden, but progressive, and more or less rapid, of the sanguine engorgement, and of the serous effusion in the lungs, accounts for the ever increasing progress of dyspnœa.

From all these facts, we may conclude that the division of the nerves of the par vagum kills animals by producing asphyxia, and that asphyxia may take place in three different manners: 1. By the diminution of the aperture of the glottis. 2. By the sanguine engorgement of the lungs. 3. By the effusion of a serous fluid in the bronchiæ. According to the species, age, and constitution of the animals, death may be occasioned by one alone of these three modes of asphyxia, or by two, or even by varied combinations of the three.

Such is the most satisfactory solution that I could obtain, of one of the questions that I had proposed to myself in the beginning of this memoir, viz. What is the cause of death, after the division of the par vagum? As to this other question, How long can animals survive it? the same solution indicates that this time cannot be always the same, because the causes producing asphyxia attain their maximum only in a variable manner, and which the most often depends upon circumstances entirely relating to the individual. In fact, out of thirty-one rabbits from one to forty days, upon whom I divided the par vagum, death occurred between six hours and a quarter and eighteen hours and a half.

To apply these results to decapitated animals, it became necessary to ascertain whether the time during which life can be maintained in these animals, and whether the state of their lungs after death, bear any relation with what is observed after the division of the par vagum. This is the question which I proposed. Nor is it an easy one to resolve. The reason of it is, that even when decapitation has been performed in the most successful manner, and when every thing announces that the experiment will be attended with most success; pulmonary inflation being long protracted, produces, in a great number of cases, accidents, which become mortal long before the period when these animals would have perished merely by the cessation of the influence of the brain. The most frequent of these are the passage of inflated air into the blood-vessels of the lungs, and the passage of the same air in the substance of the lungs, or into the cavity of the thorax and into that of the abdomen. The first of these circumstances kills the animals by preventing circulation; the others destroy the effect of pulmonary inflation, and increase the difficulty to such a degree that it soon becomes impossible to be continued. It is sometimes only at the expiration of two or three hours of inflation that either of these accidents takes place. Hence it is a very tedious, and even disgusting circumstance, to be obliged to recommence so often experiments of such length, to be able to conduct some of them to a happy issue, in such a manner that the animal should die without his death being produced by any accident or any other circumstance, except by the cessation of the influence of the brain. The longest space of time during which I have been able to maintain life in decapitated rabbits, was from five hours to five and a half; and even in this I could only succeed three times. It was in summer, the temperature of the atmosphere being at twenty-five degrees (centigrade thermometer.) The rabbits were twelve days old. It appears to me that the space of time during which I have been able to maintain life in them is nearly the same, as the shortest period in which the individuals of the same species survive after the division of the par vagum, and which is, as I have already mentioned, six hours and a quarter, to leave no doubt that life may be maintained as long, and even longer in rabbits after decapitation, if this operation did not place them in a situation much more critical than the simple division of the par vagum. But besides the hemorrhage more or less copious, which always takes place, the cutting instrument carried into the seat of the nervous power produces a commotion which they sometimes have a great difficulty to recover from, and which weakens all their functions. It results from this, that, in general, they are in a state of atony very discernable (*a*). This state of atony is equally remarkable in the lungs, by the ease and quickness with which the serous effusion which I have mentioned is formed. When life has been supported during a certain time in a decapitated rabbit, his lungs are always found swelled and filled with a frothy fluid. I have sometimes seen the effusion of this fluid carried to such a degree as to render inflation impossible in less than an hour. It comes on more rapidly after the division of the par vagum, and I have always considered it as the principal cause of death, whenever it did not depend upon some manifest accident. There is also formed in the lungs a sanguine engorgement characterised by spots of a brownish red colour, and which is more considerable in proportion as life has been supported

(*a*) This commotion also takes place in reptiles. It has been remarked very often, that salamanders, as soon as they have been decapitated, are in a state of torpor and stupor which has the appearance of threatening death. Yet they recover, by degrees, well enough to live whole months after.

for a longer space of time, and as the serous effusion has been formed less rapidly.

The rabbits kept alive after decapitation, therefore, have their lungs apparently in the same state, as after the division of the nerves of the eighth pair; and consequently whatever may be done to protract their existence, they must perish of asphyxia as in this last case, and at the furthest in the same time. This is the maximum of their existence, but in a great many cases it is not possible to make them reach it; for which I have given sufficient reasons (*a*).

(*a*) There is a reason with which I was not acquainted, when I was employed in these researches. I supposed that pulmonary inflation might completely supply the place of natural respiration. But I have since found, and I have proved in a memoir, which I had the honour of recently presenting to the first class of the Institute, that it supplies the place of that function very imperfectly. In fact, if in a rabbit, sound and healthy in other respects, we substitute pulmonary inflation to natural respiration, and that no other passage for air into the lungs, except that through the syringe, should be left open, the animal becomes almost as cold as if he were dead; and by continuing this operation for a certain time, I have only considered the questions which have been discussed throughout this work physiologically; but applications to pathology are obvious. I shall confine myself to pointing out some of them.

There are many observations, of destructions of considerable portions of the brain, which have been attended with death only after a certain space of time. Thus, we have often seen, either in battle or in cases of suicide, balls go through the brain, and notwithstanding, the patients have

we may actually make him perish by cold. I was far from suspecting that pulmonary inflation, by the aid of which we produce such surprising effects, could be attended with such serious inconveniences. Now, since notwithstanding these inconveniences, I have supported life during five hours and a half in decapitated rabbits, we may easily conceive that if they had not taken place, the animals might have been kept alive during a much longer space of time, but notwithstanding, never beyond the time they live after the division of the eighth pair. It appears that inflation contributes to produce, or at least to accelerate that frothy effusion, which in general we find more frequently and more abundantly after decapitation, than after the division of the eighth pair. For very often a similar one is formed in the sound animals which we inflate.

Would these things happen if an animal was made to breathe through the tube of a syringe that was not manipulated by the erring hand of man?

survived some time. In sanguine apoplexy, it frequently happens that life is prolonged for a considerable time after the blood effused in the substance of the brain has destroyed the intellectual functions, and the greatest part of the senses. In all these cases, whatever may be the *disorganization* existing in the brain, life continues, as long as this disorganization does not extend to that spot of the medulla oblongata which gives rise to the nerves of the eighth pair. On the contrary, whenever by any exterior or interior cause, this same part is all at once destroyed, or affected in such a manner as to prevent the exercise of its functions, respiration is stopped at the very instant, and the patient dies as quickly as if he had been strangled. Death may even appear to have been instanstaneous, owing to the torpor and stupor which are suddenly added to asphyxia, and which are the effects of the commotion which the affection of the brain produces in the nervous power.

When the origin of the nerves of the eighth pair is affected in a manner somewhat less serious, and its functions are not suspended but only altered, symptoms appear very similar to those which take place after the divison of the eighth pair. This is what is observed in a great many cases of apoplexy, which begin with obstinate vomitings, and which, on that account, might be taken for an indigestion. At the same time, there is a difficulty in respiration, the voice is altered or even more or less difficult. These symptoms indicate a mortal apoplexy when they precede or accompany the other signs of the disease. Sometimes before the apoplectic fit, the patients have been subject at several different times to obstinate coughs, which assumed the appearance of catarrhal affections. But it may happen, especially in childhood, where sanguine affections in the brain are rare, that the cause which acts upon the medulla oblongata is easier to be removed, and, for instance, it is produced by an engorgement of the vessels of that part. In this case, whatever may be the violence of the symptoms just mentioned, they will admit of a pretty speedy cure. Instances of this case are not rare. I have recently seen a remarkable case in a child of eight years old, a daughter of Mr. Benizy, engraver, rue de Harlay, number twenty-one.

This child had, for about fifteen days, laboured under a severe cough, when one morning, after a light breakfast, she was attacked with considerable vomiting, which lasted upwards of two hours. At the same time her respiration became difficult, her voice became weak, and entirely died away; finally she fainted. I saw her three hours after the attack of vomiting. She had done vomiting, but was still senseless, could not articulate a word, her respiration continued to be laborious, there was froth at the nostrils; the eyes were fixed, and nearly insensible; the jaws not fixed; and deglutition could still be carried on, though with difficulty. All the right side of the body was insensible and paralysed. The left side possessed sensation; the arm and leg of that side were agitated with convulsive motions. I advised the application of leeches to the throat, a blister to the nape of the neck, and a vomit. These remedies employed, immediately produced all the effect which could be expected from them. It was then two o'clock in the evening; at five, the reollection began to return, the eyes had recovered their mobility, the paralysis and the convulsions had ceased. In the night there were several attacks of spontaneous vomitings, there also came on, in the same night, an hemorrhage from the nose. The next morning, the patient was well, exclusive of some fatigue. It was the first time in her life that she had ever felt a fit of this kind. She had no symptoms of worms, nor were there any affections peculiar to dentition. She has ever since enjoyed good health.

I shall conclude by saying a few words upon acephali. The principal questions to which these fœtuses have given rise, are, to ascertain in what manner they can live and grow in the womb of their mother; and why they perish at different periods after birth, some continuing to live several hours, and even several days, and others only a few moments. These questions no longer present any difficulties. The brain, whatever may be its other functions, and whatever may be the empire it exercises upon the actions of life, has no other immediate action upon the support of life, except by respiration, of which it contains the primum mobile. For we have seen that its action upon circulation and digestion does not interest life, either in so coniderable or immediate a manner. Now so long as a fœtus is contained in its mother's womb, it has no need of breathing, and consequently the action of the brain on the mechanical phenomena of respiration through the diaphramatic and intercostal nerves, and that upon the lungs through the par vagum, becomes useless. I add that it can even live without its action upon the abdominal viscera, for there appears to be no digestion before birth. The brain, therefore, is not necessary to its life, and it may be entirely deprived of it, without ceasing to grow. It is in its medulla spinalis that it finds the principle of its existence and growth. But as soon as it is born, as soon as its mother no longer breathes for it, it must breathe for itself. If the brain is entirely wanting as far as the origin of the nerves of the eighth pair, it cannot perform any inspiratory motion, and it can only live the space of time during which, at that age, it can support asphyxia, from the moment that it ceased to communicate with its mother. But whatever be the other parts of this viscera which are wanting, if the origin of the nerves of the eighth pair subsists, it may breathe, and will, in fact, breathe a longer or shorter time, according as this portion of the medulla oblongata is more or less perfect, and according as it is more or less protected from external agency. In those cases of adult animals in whom the brain has been found ossified, the medulla oblongata never was observed to be so.

I am aware that cases are recorded of fœtuses not only acephali, but in whom there did not exist any medulla spinalis. But besides that these cases are very few in comparison with those of simple acephali, it would be very important to ascertain whether these fœtuses were born dead or alive, and this is what authors have not always mentioned. I know but of two instances in which we are assured that they have been born alive, without either brain or medulla spinalis (*a*). It is with these fœtuses as with those which have been asserted to have been born, some without a heart, and others without any appearance of umbilical cord, and which are equally

(*a*) Hist. de l'Acad. de Sciences, an. 1711, Obs. Anat. 3, et an 1712, Obs. Anat. 6.

as unaccountable by physiology. To admit such extraordinary facts, it would require new and well authenticated observations. As to the fœtuses still born and without medulla spinalis, it is easily conceived that some diseases, and among others hydrorachitis, could have destroyed this medulla in the mother's womb, and that death had been the result of it.

A

REPORT

MADE TO THE

Class of Physical and Mathematical Sciences

OF THE

Imperial Institute of France,

On the two first paragraphs of the preceding work.

REPORT, &c.

THE perpetual secretary of the class of physical sciences certifies that the following is an extract from the procès verbal of the sitting of Monday, September 9th, 1811.

The class having intrusted us, Mr. De Humboldt, Mr. Hallé, and myself, to prepare a report upon the memoir read at the sitting of the third of June last, by Dr. Le Gallois, respecting the principle of the powers of the heart, and the seat of this principle, we shall proceed to give the class an account of it, which may be as long as the memoir itself, because it requires details and elucidations, without which it would be difficult to estimate all the merit of this excellent work.

It was not till after the discovery of the circulation of the blood, which Harvey completed and published in the beginning of the seventeenth century, that physiologists applied their attention to the cause and mechanism of the motions of the heart, which produced afterwards so many different systems.

We shall not mention those of Descartes (*a*), nor those of Sylvius, of Le Boe (*b*), of Borelli (*c*); they are too absurd, and can only serve to prove how unfortunate were the first attempts made to explain one of the most important functions of the animal economy. We shall begin with the opinion of Willis, that is to say, by the distinction which he was the first to establish between the nerves destined to voluntary motions, and those which preside over the functions independent of the will. He placed the origin of the latter in the cerebellum, and that of the nerves of voluntary motions in the cerebrum.

(*a*) L'homme de René Descartes, et la formation du fœtus avec les remarques de Louis Laforgue, Paris 1677. p. 4 et 106.

(*b*) Francisci Deleboe, Sylvii, opera medica. Genevæ, 1681, p. 5. 27, 28, 33, 475.

(*c*) Joh. Alph. Borelli de motu animalium. Hagæ Comitum, 1743, p. 89, 92.

He pretended that if the motions of the heart, as well as the other vital functions do not undergo any interruption, it is because the action of the cerebellum continues without ceasing; and that on the contrary, the motions subjected to the will require repose, because the action of the brain is not continual (*a*). This distinction of Willis was pretty generally admitted, till about the middle of the last century. It was especially on account of this system, that the division of the nerves of the eighth pair, from which almost all the cardiac nerves were supposed to arise, was performed in different countries. The design was to prove by this assertion, that the heart receives all its motions from the brain; and it was said that the animals died of it, only because it destroyed the communications between these two organs. But besides that they die much later than they would if they perished from this cause, it has been well ascertained of late by several learned men, and es-

(*a*) Tho. Willis opera omnia, edente Ger. Balsio. Amstelodami, 1682. Tom. I. de cerebri anatome. Cap. XV. p. 50.

pecially by Dr. Le Gallois, in a memoir, the insertion of which has been ordered by the class among those of the foreign litterati (*a*), that death is produced in those cases by an entirely different cause. Indeed it has some times happened that animals have died almost suddenly after the division of the nerves in question; nor have the advocates of Willis failed to lay a great stress upon these experiments, of which their adversaries could not give any satisfactory explanation. But Dr. Le Gallois has demonstrated, in the memoir which we have just quoted, that this sudden death only takes place in certain species of animals, and then only when these animals were very young; and further, that it is the effect of a more or less complete asphyxia produced by the closure of the glottis. We can therefore find nothing, even in these facts, which affords any proof in favour of the opinion of Willis; and we may add, that the eighth pair does not arise from the cerebellum, and that it is not to this

(*a*) This memoir is comprised in the third paragraph above.

pair that the greatest part of the nerves of the heart belong.

Boerhaave was of Willis's opinion; but besides the nervous action, he admits two other causes of these motions, and of their harmony; to wit, the action of the blood of the coronary arteries on the fibres of the heart, and that of the venous blood on the internal surfaces of the cavities (*cavités cardiaques*) of the heart.

It was the union of these three causes that occasioned the systole of the heart, and it was the simultaneous interruption of their action by the effect even of the systole which produced the diastole, during which these causes resumed their action (*a*). But this etiology, excepting what respects the stimulus of the blood, on the internal surfaces of the heart, was contradicted by facts;—which did not prevent it from pre-

(*a*) Her. Boerhaave Instit. Medicæ. §. 409. Vanswieten in aphorismos, &c. Lugduni Batav. 1745. Tom. II. p. 18.

vailing in the schools, together with another not less celebrated error.

We advert to Stahl and his *soul*, or Archeus, which either regulating all the motions of the living body, and submitting them to the will, or making them independent of it, as they may simply be useful or absolutely necessary for life, presides over all those of the heart, and through the nerves secure their duration and continuity; —a species of physiological reverie, which is repugnant to the true principles of the science.

After all, where did the Stahlians place the residence of this simple and indivisible being?—In the brain, without doubt; but then how happens it that an animal can live and the motions of its heart continue after decapitation? Will they assign the heart itself for its seat?—But all animals, and especially the cold-blooded ones, outlive a longer or a shorter time the excision of this organ (*a*).

(*a*) See for the exposition and refutation of this system, Haller's Elem. Physiol. tom. I. p. 480—8. et tom. IV. p. 517—34.

Other authors, such as Abraham Ens (*a*), Stœhelin (*b*) &c. have also endeavoured to account for the motions of the heart; but their systems, almost as soon forgotten as they were conceived, do not deserve our notice.

Those of Boerhaave and Stahl were nearly the only ones which prevailed, when, in 1752, Haller published his experiments on irritability. These experiments, as well as those which his followers published afterwards, tend to prove that the contractile property belongs exclusively to the muscular fibre. This property, which Haller designates sometimes under the name of *vis incita*, at others, after Glisson, under that of *irritability*, is the source of all the motions which take place in the animal; but it cannot produce them unless some cause, some stimulus determines it to act. Thus every muscular motion always supposes two things, the irritability which produces the contraction of the muscle, and a

(*a*) Dissertatio physiol. de causâ vices cordis alternas producente. Lugd. Batav. 1745.

(*b*) Dissertatio de pulsibus. Basileæ, 1749.

stimulus which determines the irritability to commence its action. The irritability is every where the same; its force alone varies in the several muscles, but does not obey the same stimulus in all the muscles. The nervous power is the natural stimulus of all those which are submitted to the will; and it is by exciting or suspending the action of this power upon the irritability of certain muscles, that the will causes such part to act, or sets it at rest. It is not so with the involuntary muscles. These acknowledge stimuli of various sorts, which are appropriated to their functions, and wholly abstracted from the nervous power. The blood is the natural stimulus of the irritability of the heart; it is the alimentary substances that stimulate that of the intestinal canal, &c. In these principles we readily find the explanation of the principal circumstances which are observed in the motions of the heart. Thus those motions are not submitted to the will, because they are independent of the nervous power; they continue without interruption during life, because the irritability by which they are produced belongs essentially to the fibres of the heart, and because the blood by which they are directed is constantly brought back to this organ by the veins in proportion as it is taken away by the arteries. The systole and diastole alternately and regularly succeed each other, because the stimulus of the blood always produces the systole whether in the auricles, or in the ventricles, and because the systole, by destroying the stimulus, is the cause of the diastole, which brings back the systole, by allowing a new influx of blood.

Such is summarily the celebrated theory of the Hallerian irritability; this theory had not been, as all the others we have mentioned, contrived in the closet; it was grounded upon experiments made by Haller himself and his most distinguished disciples, who ranked first among the anatomists and physicians of the last century. These experiments which were repeated throughout Europe, met almost every where with partisans; but there were also found a certain number of censors of great reputation. The principal point of this difference of opinion, the object of disputes to the present day, consists in ascertaining whether the motions of the heart are really independent of the nervous power.

The facts upon which the Hallerian school has maintained the affirmative may be reduced to three heads: First, If you cut off all communication between the heart and the brain, *the only source of nervous power*, by the division of the nerves which are distributed to the heart, by that of the medulla spinalis in the neck, or even by decapitation, the motions of the heart continue as before. Second, If you take out the heart of a living animal and lay it on a table, this organ continues to pulsate, and sometimes for a very long time, (M. De Humbolt shewed us that it pulsated more strongly and for a longer time, when it was held suspended.) Third, Convulsions are always produced even sometimes after death, in the muscles which belong to voluntary motions, by irritating the nerves of those muscles, either in a mechanical, or in any other way. The irritation of the cardiac nerves on the contrary, causes no alteration in the motions of the heart, nor does it re-excite them when they have ceased; it is so with the irritation of the medulla oblongata and spinalis, which occasions strong convulsions through all the body, and produces no effect upon the heart.

These facts are correct, except perhaps those of the third head, upon which there is some difference of opinion. But in admitting them, the adversaries of irritability have demanded, how happens it, if the nervous power does not act upon the heart, that this organ should receive nerves? And why is it found so eminently submitted to the empire of the passions? Haller has never given clear answers to their objections, but every thing proves that he was aware of all their force. When we read with attention all that he has said upon the motions of the heart, in his memoirs upon irritability (*a*), and especially in his great work on physiology (*b*), we are struck with the contradictions with which it abounds, and which renders its perusal fatiguing. His great object is, every where, to prove that the motions of the heart are independent of

(*a*) Mémoires sur la nature sensible et irritable des parties, etc. Lausanne, 1756.—Opera minora. tom. I.

(*b*) Elem. Physiol. lib. IV. sect. V. et lib. XI. sect. III.

the nervous power; every fact, experiment and observation which he cites, tends to this object. Nevertheless, he seems to admit in several places, that the nerves have an action upon the heart; it is true that it is with an air of doubt that he admits it, confining himself to say, that it is possible, that it is not improbable, that the heart borrows a moving power from the nerves (*a*). These contradictions, with which he was charged by several justly celebrated authors, among others by Messrs. Prochaska (*b*), Behrends (*c*), and Ernest Platner (*d*) &c., evidently proceed from the impossibility of reconciling the result of his experiments, with the intervention of the nervous power in the motions of the heart; and that by rejecting this intervention, he could not account either for the use of the cardiac nerves or for the influence of the passions on the heart. For this is the real cause

(*a*) Ibid. lib. IV. sect. V. p. 493, et alibi passim.

(*b*) Opera Minora. Viennæ, 1800, tom. II. p. 90.

(*c*) Tom. III. p. IV. of Ludwig's collection, entitled Scriptores Nevrolog. Minores Selecti. Lipsiæ, 1791-5. IV. tom. in 4to.

(*d*) Tom. II. p. 269, of the same collection.

of difficulty in the controversy in question. Those who, like Fontana, have formally rejected all intervention of the nervous power, have been forced to admit, that the nerves destined in every other part to carry life, sensation and motion, had no known use in the heart (*a*).

Such consequences, evidently betrayed the insufficiency of the theory of Haller. Hence, several of his followers have acknowledged the necessity of modifications, and of admitting the nervous power, as one of the conditions upon which irritability depends. From that time, they have been able to account for the use of the nerves of the heart, and the power of the passions upon this organ. But, from the moment they wished to account for the motions of the heart not being stopped, by the entire suppression of all communication between the brain and the heart, they were obliged to abandon the opinion generally received, by which the

(*a*) Mémoires sur les parties sensibl. et irritab. tom. III. p. 234. See also Caldani, ibid. p. 471; et le traité sur le venin de la vipère. tom. II. p. 169,171.

brain is considered as the center and the only source of the nervous power; and they admitted *without direct proofs*, that this power is produced in the whole extent of the *nervous system*, and even in the smallest nerves, and that it can exist independently of the brain, for a certain time, in the nerves of every part. Among the authors of this last opinion, the learned professor Prochaska is one of those who have best described it (*a*). But when he comes to apply it to the motions of the heart, and endeavours to explain why they are independent of the will, and are submitted to the power of the passions, his opinion is not completely decisive. It is to the ganglions he has recourse, and then he hesitates as to the functions he is to ascribe to them. Sometimes he considers them as knots, as ligatures tight enough to intercept all communication between the heart and the *sensorium commune*, in the calm and quiet state, but not sufficiently so, as to prevent the *sensorium*

(*a*) Commentatio de functionibus systematis nervosi, published in 1784, in the third Fascicule of the Annotationes Acad. of this author, and re-published in his *Opera minor*. Vienna, 1800.

from re-acting with more or less energy upon the heart, in the agitation of the passions (*a*). Sometimes, he seems to think that the interception is complete and continued; that it is through the nerves of the eighth pair, that the effect of the passions is felt upon the heart (*b*); and he seems to adopt the opinion of Winslow (*c*), revived by Winterl (*d*), by Johnstone (*e*), by Unzer (*f*), Lecat (*g*), Peffinger (*h*) &c., namely, that the ganglions are so many little brains. He admits at the same time, that the nerves of sensation are distinct from those of motion, so that the heart cannot contract itself, unless the impression of the *stimulus* upon its cavities is transmitted to the ganglions through

(*a*) Opera minor. tom. II. p. 165.

(*b*) Ibidem p. 167.

(*c*) Exposition anatom. Traité des nerfs. sect. 364.

(*d*) Nova inflam. theoria. Viennæ, 1767. cap. 5. p. 154.

(*e*) Essay on the use of the ganglions. 1771.

(*f*) Quoted by Prochaska, opera minor. tom. II. p. 169.

(*g*) Traité de l'existence, de la nature et des propriétés du fluide nerveux. Berlin, 1765, p. 225.

(*h*) De structura nervorum, Argentorati. 1782, sect. I. §. 34, sur la fin. Inserted in the collection of Ludwig, vol. I.

the nerves of sensation, and thence reflected to the fibres through the nerves of motion (*a*).

But besides that the whole of this opinion is, from the confession of the author himself, a simple conjecture, it supposes in the first place, that circulation should continue after the destruction of the medulla spinalis; and, secondly, that the heart should cease to pulsate, at the instant when its communications with the ganglions and the plexuses are interrupted: now, these suppositions are disproved by the facts.

Such fruitless endeavours to modify the theory of irritability, by the intervention of the nervous power, have only increased the zeal of some authors to maintain this theory in its primitive purity; and as the use of the nerves of the heart was one of the most difficult parts of this theory, Sœmmering, one of the most profound anatomists of Germany, and Behrends, one of his most distinguished disciples, maintained in 1792, that the heart has no nerves,

(*a*) Opera minor. tom. II. p. 169.

and that all those which appear to be distributed to this organ, lose themselves in the coats of the coronary arteries, without one single filament being received by its own fibres (*a*): an opinion which, so far from removing all the difficulties, would only render the influence of the passions upon the motions of the heart still more inexplicable. These two authors pretend that the cardiac nerves serve to keep up and increase the irritability of the coronary arteries;—but the existence of irritability in the arteries is still dubious, and if it were demonstrated, it would be very strange that it should depend upon the nervous power in the arteries, and that it should be entirely independent of it in the heart, the most irritable of all organs.

In other respects, science could only gain by the doubts proposed by Behrends, doubts unattended with proofs on the cardiac nerves, since they determined the learned Scarpa in his turn to enter the lists, and they have been the means of

(*a*) Behrends, dissertatio quâ demonstratur cor nervis carere. Moguntiæ, 1792. Inserted in the third volume of the collections of Ludwig.

producing his beautiful work upon the nerves of the heart (*a*). Scarpa proves in this work, that the nerves are as numerous and are distributed in the same manner in the heart as in the other muscles. Like Prochaska, he admits that sensibility and irritability are essentially united, and that the nervous power is generated through the whole extent of the nerves; but he does not admit that the ganglions are so many little brains (*b*); he appears to think that the nervous power, such as it exists throughout the nerves, is of itself sufficient for the exercise of the various functions; and that it only wants a *stimulus* to be brought into action. It is from the brain that the *stimulus* of the muscles submitted to the will proceeds; and in the usual state the blood is the *stimulus* of the heart; but in the strong emotions of the mind, the brain becomes also the *stimulus* of this organ (*c*).

(*a*) Tabulæ nevrologicæ ad illustrandum historiam anatomicam cardiacorum nervorum, etc. Ticini, 1794.

(*b*) Ibid. sect. 30.

(*c*) Ibid. sect. 22. 24, 25, 26, 27. 29.

In conformity with this opinion, the heart should pulsate in the same manner, and with the same force after decapitation, and after the destruction of the medulla spinalis, and after it has been extirpated. Scarpa himself compares the pulsations which take place in apoplexy, to those observed when the heart has no further communication with the brain nor the medulla spinalis (*a*), but we shall see by and by that this is far from being the case. In other respects we ought not to omit a very important remark of this author, which it is surprising should not have been made before. It respects the passive state of the heart, when the medulla spinalis and cardiac nerves are irritated. Scarpa observes that this passive state, about which so much has been said, and which is considered as a demonstrative proof that the motions of the heart do not depend upon the nerves, proves only that the nerves of the heart are not of the same order as those of the voluntary muscles, and that the nervous power is not exercised in the same manner (*b*). This reflec-

(*a*) Tabulæ nevrologicæ, &c. sect. 25.

(*b*) Ibid. sect. 20.

tion is without doubt very judicious, and it is through an error of experimental logic that we have been surprised not to obtain the same effects from the irritation of two orders of nerves which were entirely different.

The work of Scarpa did not alter Dr. Sœmmering's opinion (*a*); nor did it prevent Bichat from denying that the nervous power has any share in producing the motions of the heart (*b*). This latter author, while he acknowledges an animal and an organic life distinct from each other, admits a nervous system for each of these two lives. The system of the ganglions, which like the above mentioned authors, he considers as little brains belonging to organic life, and the cerebral system to animal life (*c*). To be consistent with himself, Bichat ought to have admitted, like Prochaska, that the heart being

(*a*) Th. Sœmmering de corporis humani fabricâ. Trajecti ad Mænum 1796. Tom. III. p. 30, 43, 46, 50, et ibid. 1800. Tom. V. p. 43.

(*b*) Recherches physiol. sur la vie et la mort. Paris, an. 8-1800. Part. II. art. 11, § 1.

(*c*) Ibid. Part. I. art. 6. § 4.

the center of organic life (*a*), receives from the ganglions the principle of its motions; but he did not; he was thrown into this inconsistency chiefly by galvanic experiments, having in vain tried to produce contractions in the heart by galvanizing the cardiac nerves; experiments upon which Sœmmering and Behrends had also endeavoured to establish their opinion. However, these experiments might have succeeded, as was ascertained by one of us in 1797 (*b*) and three years before by Fowler (*c*).

Such is the succinct but faithful account of the principal systems by the aid of which it has been attempted, from the discovery of the circulation down to this day, to account for the motions of the heart. On a general retrospect view of these systems, we remark, that in all those

(*a*) Ibid. art. 1. § 2.

(*b*) M. De Humboldt, expériences sur l'irritation de la fibre nerveuse et musculaire, published in 1797, and translated into French two years after. Tom. I. chap. 9.

(*c*) Experiments on animal electricity, 1794. By Richard Fowler.

maintained before Haller (*a*), the nervous power is always considered, sometimes in one relation, at others in another, as one of the conditions which are essential to the production of the motions of the heart, and it has ever been in the brain only that they placed its seat. The cardiac nerves therefore had a determined use in all those systems, and it was easily comprehended in what manner the heart was submitted to the power of the passions; but it was impossible to explain why circulation continues in the acephali, or why, in experiments made upon animals, the interception of all communication between the brain and the heart does not stop the motions of the latter. Ever since the days of Haller, irritability has been the basis of all systems. When we consider this property as being essential to the fibre and as independent of the nervous power, circulation in the acephali and the various phenomena observed in the experiments just mentioned had nothing puzzling; but the use of the nerves of the heart, and the influence of

(*a*) And also in Ens, de Stœhelin, and others of whom we have not spoken.

the passions upon this organ became inexplicable. The necessity of removing these difficulties produced two sects among the partisans of irritability. Some who were the zealous advocates of pure irritability, called in to their assistance the most improbable hypotheses, and all their efforts only serve to prove how difficult the cause they have embraced is to defend. Others had introduced the nervous power in irritability which they considered as one of the functions of this power; but whether on account of its seat, or the state of the nervous power, they were obliged to admit conditions which, by their own confession, are far from being demonstrated, respecting which they do not agree among themselves, and which, in the application they make of them to the motions of the heart, either do not entirely remove the old difficulties, or give rise to new ones.

It is easy to perceive the cause of so little progress having been made in elucidating this great and extensive question. If we examine all that has been said on this subject since the days of Haller, we find that nearly the same facts, the same experiments, and the same reasonings have ever been brought forward on both sides. The only new experiments are those applications of galvanism to stimulate the cardiac nerves. And these are only apparently new, for in the time of Haller electricity had been employed in the same view (*a*). It is evident that science had nothing to hope from following the paths which had been trodden by so many celebrated men, for nearly sixty years. New roads were to be opened; new methods were to be contrived, in order to interrogate nature; but above all it was necessary to introduce in physiological experiments that precision and strict logic to which other physical sciences have in our days been indebted for the very great progress they have made;—and this is what was performed by the author of the memoir of which we here give an account.

Dr. Le Gallois's intention was not to investigate the causes of the motions of the heart; he

(*a*) See, among others, Mém. sur les parties sensib. et irritab. Tom. III. p. 214.

was satisfied with the theory of Haller, when experiments, undertaken with quite different views, led him to so extraordinary a result, that he could no longer understand his own experiments, unless he should ascertain whether and how the nervous power intervenes in the functions of the heart? The better to understand his work, we shall relate on what occasion, and by what chain of facts and reasoning, he found himself engaged in this inquiry.

A particular obstetric case, a few years ago, made him desirous of knowing how long a full grown fœtus may live without breathing, from the moment when, from any one cause, it has ceased to communicate with its mother? This question which is curious in itself, and very interesting for the practice of midwifery and medical jurisprudence, had scarcely been touched by authors: Dr. Le Gallois undertook to resolve it by direct experiments made upon animals; and in order that his answer might apply to the greatest number possible of cases, he placed the fœtuses of the animals in the various states, which resembled the principal accidents which may happen in the human fœtus, at the same time that it ceases to communicate with its mother. Among these circumstances, one which happens but too often, is the separation of the head in the artificial delivery by the feet. The author wished to know what becomes of the fœtus in this case: whether it perishes at the very instant of decollation, and of what kind of death it dies. He found that the trunk continues alive, and that, by preventing the hemorrhage by the ligature of the vessels of the neck, it only dies after the same length of time, and death is only attended with the same phenomena, as if without the separation of the head respiration had been completely suppressed; and what completed his demonstration that the decapitated animal is in fact only asphyxiated is, that its existence can be protracted ad libitum by supplying the want of natural respiration by the inflation of the lungs.

Dr. Le Gallois concluded from these facts, that decollation does nothing more than arrest inspiratory motions, and that consequently the principal of all these motions resides in the brain; but that the principle of the life of the trunk is in the trunk itself. In investigating afterwards, where the immediate seat of each of these two principles resides, he discovered that the principle of the inspiratory motions, is seated in that part of the medulla oblongata which gives rise to the nerves of the eighth pair; and that the principle of the life of the trunk, has its source in the medulla spinalis. It is not by the whole of this medulla that every part of the body is animated, but only by that portion from which it receives its nerves; so that by destroying only one portion of the medulla spinalis, those parts of the the body which correspond to that portion, are alone affected with death. Further, by intercepting the circulation of the blood in a portion of the medulla spinalis, life is weakened and soon wholly extinguished in all the parts which receive their nerves from that portion of the medulla. There are two modes therefore of causing the cessation of life in any particular part of the body of an animal: the one by destroying the medulla from which this part receives its nerves, the other by intercepting in it the circulation of the blood.

It thence resulted, that the support of life in any one part of the body depended essentially upon two conditions; namely, the integrity of the corresponding portion of the medulla spinalis, and the circulation of the blood; and consequently that it would be possible to keep up life in any one part of an animal, as long as these two conditions can be made to exist; that we may, for example, keep alive the anterior part alone, after having occasioned death in the posterior, by the destruction of the corresponding portion of the medulla spinalis; or the posterior, after having caused the death of the anterior.

Dr. Le Gallois, whose method has constantly been, to seek in direct experiments the confirmation of the consequences which he had drawn from preceding ones, was desirous to know whether it would be really possible, thus to keep alive any one portion of an animal alone, after having caused the death of the rest of the body. He first submitted a rabbit twenty days old to these inquiries, by destroying in that animal, all the lumbar portion of the medulla spinalis. This operation, in no ways wounding immediately the rest of the medulla spinalis, and circulation not being, according to the theory of Haller, to be affected by it, there was every reason to expect by reasoning from the preceding experiments, that the animal would have outlived it a considerable space of time; and that it would not have died, except in consequence of the symptoms which were to be produced by so severe a wound: but respiration stopped between one and two minutes after, and in less than four minutes it gave no signs of life. The same experiment, repeated several times, was always attended with the same result, nor could it be prevented; and it was established that a rabbit, twenty days old, cannot outlive the loss of the lumbar portion of the medulla spinalis, which was the more surprising, as the rabbits of that age will continue to live after decapitation, that is, after the entire loss of the brain. This fact, the author could not reconcile with his preceding experiments, and it is this which led him to the discovery, that the principle of the power of the heart resides in the medulla spinalis.

Dr. Le Gallois first ascertained, that the destruction of each of these two portions of the medulla spinalis, (viz. the dorsal and cervical) was as mortal in rabbits of twenty days old, as that of the lumbar portion, and even in a shorter time, to wit, about two minutes; he then found that the same experiments repeated upon rabbits of different ages did not give the same results; in general, the destruction of the lumbar portion of the medulla spinalis, is not suddenly mortal in these animals before they have attained the age of ten days; several outlive it still at the age of fifteen. Beyond twenty days the effect is the same as at twenty. Very young rabbits may likewise continue to live after the destruction either of the dorsal portion of the medulla or of the cervical; but for a shorter time, and less frequently after the destruction of the latter, than after that of the dorsal. None can outlive either beyond the age of fifteen days.

In all these partial destructions, even when death is sudden, it is never instantaneous, except in the parts which receive their nerves from the medulla destroyed; but it only takes place in the rest of the body after a certain but determined time, which no means can protract. This length of time, which is the same in all animals of the same species and age, is longer in proportion as the animals are younger. For instance, when the cervical portion of the medulla is destroyed in young rabbits, life is destroyed at the instant in the whole neck; but it continues in the head, and this is known by the gapings it excites. It continues likewise in the posterior parts from the shoulders, as is fully testified by the sensation and voluntary motion that are preserved. In the first day after birth, the gapings last about twenty minutes, the sensibility and motion of the rest of the body fifteen minutes. At the age of fifteen days, the duration of the gapings does not exceed three minutes, nor that of sensibility or motion two minutes and a half. Lastly, at the age of thirty days, the gapings cease at between one and one minute and a half, and sensibility at one minute. After the destruction of the dorsal portion of the medulla spinalis, it is the breast, and not the neck, that is affected with death; in other respects there are the same phenomena of the same duration. If the three portions of the medulla are destroyed at the same time, the gapings, the only signs of life then existing, still have, at the different ages, the duration which we have just indicated.

The author, who had so often performed decapitation upon rabbits of different ages, had constantly remarked, that the head when separated from the body, continues to gape during a time determined for every age. This length of time was evidently the same as after the destruction of the medulla spinalis. Now it is evident that after decapitation, there can be no circulation in the head, and that the gapings which take place in this case, only continue while life exists in the brain, after the entire cessation of circulation. This was the first notice that Dr. Le Gallois had, that when the partial destruction of the medulla spinalis has occasioned life to cease in all the rest of the body, it is because it suddenly stops the circulation. In order to ascertain it, he cut out the heart at the origin of the large vessels in rabbits, every five days, from the moment of their birth to the age of one month; and having noted with care, the duration of the different signs of life from the moment when the circulation had been stopped by that means, he found that those periods of duration were precisely the same, as those he had observed after the destruction of the medulla spinalis; he might have considered this approximation as sufficient to decide the question, but he wished to ascertain in a more direct manner, if the circulation be really stopped at the very instant when the medulla spinalis is destroyed. The absence of the hemorrhage and the emptiness of the arteries were the most evident signs of it that he could deduce; and he found in fact, that as soon as the operation is performed, the carotids are empty, and the amputation of the limbs furnishes no blood, though performed very near the trunk and before life is extinct in the parts, the medulla of which had not been destroyed. In a word, all the signs which may serve to show the state of the circulation, demonstrated that whenever the destruction of any one portion of the medulla spinalis causes death suddenly in the rest of the body, it is occasioned by the arresting of that function. This latter effect takes place, not because the motions of the heart cease at once, but because they lose all their energy, so as not to have power to propel the blood as far as the carotids.

It thence results, that it is from the medulla spinalis, wholly and exclusively, that the heart receives the principle of its energy, since the destruction of any one of these three portions may stop the circulation. It further results, that every part of the spinal marrow, exercises on life two very distinct modes of action: one by which it constitutes it in every part which receives nerves from it; the other by which it constitutes it in every part of the body, by contributing to furnish to all the organs, which receive filaments from the great sympathetic, and particularly to the heart, the principle of power and of life, without which they cannot perform their functions.

It is clearly perceived, therefore, that in order to keep the anterior or posterior parts of an animal alone alive, after producing death in the rest of the body, by the destruction of the medulla corresponding with it, one should be able to prevent this destruction from stopping circulation. Now, this is very easily accomplished by diminishing the sum of power which the heart is to dispense, to keep up circulation in proportion as that of the power which it receives from the medulla spinalis is diminished. For this purpose it will be sufficient to decrease, by ligatures made upon the arteries, the extent of the parts where the heart is to distribute blood. We have observed, for instance, that the destruction of the lumbar portion of the medulla spinalis is quickly mortal in rabbits of twenty or more days old; but it does not kill them, if, before producing it, the abdominal portion of the aorta has been tied between the cæliac and anterior mesenteric arteries. The application of this principle to other parts of the body, leads to a case apparently very singular, which is, that in order to keep up life in rabbits of a certain age, after having destroyed their cervical medulla, their heads must first be cut off; if this medulla be first destroyed without decapitation, they are irrevocably dead. But our astonishment will cease when we reflect, that by decapitation, we withdraw the whole head from the influence of circulation, and that by this operation, the heart requiring less power to continue its function, it may be weakened by the destruction of the cervical medulla, without ceasing to perform it.

It is likewise easily conceivable, that any other operation capable of suspending or considerably retarding circulation in a certain extent of the body of an animal, ought to produce a similar effect, and also furnish the means of attacking with impunity such portion of medulla spinalis, the destruction of which would have been mortal, had it not been for this previous operation. This is what is obtained by the very effect of the destruction of the medulla. This destruction produces two effects upon circulation; by one, general circulation is weakened, by depriving the heart of the contingent of power which it receives from the medulla destroyed; by the other, without wholly stopping circulation in those parts affected with death, it lessens it in a very high degree, which is equivalent in a certain measure to the ligatures made upon the arteries of those parts. But this effect is sensibly felt only a few minutes after the destruction of the medulla. Thence it happens that the destruction of a first portion of medulla spinalis furnishes the means of destroying a second; this latter a third, and so on. For instance, when by decapitating a rabbit we are enabled to destroy the cervical medulla, the destruction of this medulla furnishes in some minutes the means of destroying one fourth part of the dorsal medulla; and by continuing thus to operate at intervals upon similar lengths of this same medulla, we shall be able to accomplish the whole destruction without stopping circulation, which is then supported by the lumbar portion of the medulla spinalis alone.

From what we have just stated, it may be concluded that, in rabbits, any one portion of the medulla spinalis supplies the heart with sufficient power to maintain circulation in all the parts which correspond to that portion; and that consequently, by cutting a rabbit transversely and into portions, it would be possible to keep alive separately and indefinitely each portion, if both the lungs and the heart, which are necessary for the formation and circulation of the arterial blood, could be a part of them. But they can only be a part of the chest; and it is fully in our power to maintain life in the chest alone, and when separated, after having cut off the anterior and posterior parts, and prevented the hemorrhage by suitable ligatures; and this is practicable upon rabbits of thirty days and upwards.

Such are the principal results of the researches of Dr. Le Gallois. These results, all produced by one another, and which furnish one another a mutual support, are grounded upon direct experiments, made with a precision till now unknown in physiology.

We shall now state such of these experiments as the author has repeated in our presence. We have bestowed on these repetitions, three sittings, each of several hours; and to avoid every precipitation, and allow ourselves time to weigh the facts at leisure, there has been an interval of one week between each sitting.

EXPERIMENTS

Repeated before the Commission of the Institute.

We shall divide them into two paragraphs. The first will comprise those which tend to prove that the primum mobile of all inspiratory motions reside in that part of the medulla oblongata which gives rise to the nerves of the eighth pair. In the second we shall describe those, the object of which is to show that the powers of the heart have their origin in the medulla spinalis.

SECTION I.

Experiments relative to the Principles of Inspiratory Motion.

The author took a rabbit five or six days old; he separated the larynx from the os hyoides, and laid the glottis bare, that we might observe its movements; after which he opened the cranium, then took out the cerebrum and afterwards the cerebellum. After this double extraction, the inspirations continued; they were characterised each by four movements, which took place at the same time; namely, a gaping, the opening of the glottis, the elevation of the ribs, and the contraction of the diaphragm. These four motions having been well ascertained, and being to last for a certain time, according to the age of the animal, the author extracted the medulla oblongata;—and at the same instant these motions ceased all at once. We found that the extracted portion of medulla oblongata extended as far as the great occipital foramen, and that in it was included the origin of the nerves of the eighth pair.

The same experiment was repeated upon another rabbit of the same age, with this difference, that after the extraction of the cerebrum and the cerebellum, instead of taking away at first, as great an extent of the medulla oblongata, it was extracted successively by slices about three millimetres in thickness, (a little more than the twelfth part of a French inch.) The four inspiratory motions continued after the extraction of the three first slices; but they were suddenly stopt after that of the fourth.

It was ascertained that the third slice ended in the posterior part and near the *pons varolii*, and that the fourth included the origin of the nervesof the eighth pair.

The same experiment, repeated upon several other rabbits, was constantly attended with the same results.

He proceeded in the same manner upon a cat five weeks old, only he separated the two recurrent nerves before he had extracted by slices the medulla oblongata. Upon this the glottis closed, and it remained motionless in that state; but the three other motions, namely, gaping, elevation of the ribs and contraction of the diaphragm, continued, nor did they stop till the moment he took off from the medulla oblongata the origin of the nerves of the eighth pair.

It is evident that if instead of destroying this part, in which the primum mobile of all inspiratory motions reside, one should confine oneself to preventing it from communicating with the organs which perform those motions, the same effect would be produced; that is to say, that those motions, the organs of which would no longer communicate with the part in question, would be suppressed. This is what we have just observed in the cat, in which the section of the recurrent nerves suppressed the motion of the glottis without stopping the three other motions. To suspend these in the same manner, it is sufficient to notice through what channel their organs communicate with the medulla oblongata. Now, it is clear that it is through the intercostal nerves, and consequently through the medulla spinalis, that the medulla oblongata acts upon the muscles which raise the ribs, and that it is through the phrenic nerves and consequently again by the medulla spinalis, that it acts upon the diaphragm. When we divide the medulla spinalis upon the last cervical vertebra, and above the origin of the phrenic nerves, we must suppress the motions of the ribs, not those of the diaphragm; —and when we divide this medulla between the occiput and the origin of the phrenic nerves, we must cause a sudden cessation of all the movements of the ribs and of the diaphragm at once. This is, in fact, what takes place. The author took a rabbit about ten days old, and after attentively examining the motions of the thorax, he divided the medulla spinalis upon the seventh cervical vertebra; at that instant the motions which depend upon the raising of the ribs were stopped; but the contractions of the diaphragm continued: he again divided the medulla spinalis at the first cervical vertebra, and the contraction of the diaphragm immediately ceased. Finally, he cut the eighth pair at about the middle of the neck, and the motions of the glottis were suppressed. Thus, of the four inspiratory motions, there were left only the gapings, which proved that the medulla oblongata still preserved the power of producing them all, and that it only exercised it without effect in regard to the three others, because it no longer communicated with their organs. Here we are to observe, that several authors, among others Arnemann, had remarked, before Dr. Le Gallois, that the section of the medulla spinalis did not stop the motions of the diaphragm, unless it were performed between the occiput and the origin of the phrenic nerves. But these authors consider the brain as the only source of life and of all the motions of the body. They thought, in conformity with this opinion, that the section of the medulla spinalis paralyzed instantaneously every part of the body which received its nerves from that medulla below the section, and that consequently when the section was formed near the occiput, the diaphragm ceased its contractions, because it partook of the palsy of all the parts below the section. But Dr. Le Gallois has demonstrated that the section of the medulla, performed at the first or the last cervical vertebræ, only stops the inspiratory motions, and that it allows the existence of sensation and voluntary motion through the whole body. This distinction is important: no one had made it before him.

It is not only in warm-blooded animals, that these experiments are observed with the results we have just stated. To prove that these results depend upon the general laws of the animal economy, and that the nervous power is distributed and governed in a uniform manner in all animals which possess vertebræ, the author took a frog, and after remarking that, in these animals, which possess neither ribs nor a diaphragm, there are only two kinds of inspiratory motions; namely, those of the glottis, which is opened in the form of a lozenge, and those of throat, which is raised and lowered alternately; he took off one half of the cerebrum,—both motions continued: he then destroyed about one half of what was left of that viscus,—the same motion continued: finally, he carried the destruction of the brain as far as near the great occipital foramen, and both motions were instantaneously stopped for ever. The medulla spinalis of another frog was cut off upon the third vertebra,—the inspiratory motions continued. It was afterwards separated, between the occiput and the first vertebra of a third frog, and at that instant, the motions of the throat which correspond with those of the diaphragm ceased. After these two last experiments, the frogs were, and have continued alive both in the *head* and in the rest of the body, but they were unable to govern themselves, and were, in this respect, in the same case with the first, the brain of which had been destroyed.

SECTION II.

Experiments relative to the Principle of the Power of the Heart.

THE author first proved that life always continues a certain time, even in warm-blooded animals, after the total cessation of circulation; and that this time is determined according to the age. For this purpose he opened the thorax, and extracted the heart of a rabbit five or six days old, and he did the same on another, ten days old. In the first the gapings ceased seven minutes after, and sensibility four, from the excision of the heart. In the second, the gapings lasted four minutes, and sensibility but three. The cervical and a small portion of the dorsal medulla, were then destroyed on another rabbit of the same litter as the last, and the pulmonary inflation was immediately performed; but notwithstanding this assistance, the gapings ceased three minutes and a half after, and sensibility a little after two and a half minutes; a duration which, as may be observed, coincides within one half a minute with those observed after the excision of the heart.

To prove that, in this experiment, it is really by arresting circulation that the destruction of a part of the medulla causes the cessation of life in the rest of the body, the author took a rabbit of the same age with the two last: he first divided the medulla near the occiput. After this section, the carotids were black, but round and full, and the amputation of a leg furnished black blood. The pulmonary inflation having been performed, the carotids soon grew of a fine vermillion colour, and the hemorrhage assumed the same. These signs leave no doubt that the circulation had continued after the division of the medulla, near the occiput. The author destroyed on this rabbit, the same portion of medulla as on the preceding, and immediately the carotids appeared flaccid, and soon after empty and flat. Both thighs, amputated in less than two minutes after the destruction of the medulla, did not furnish one drop of blood.

The destruction of the cervical medulla, performed on several other rabbits, from twenty to thirty days old, furnished exactly the same results; *i. e.* the carotids emptied themselves, very soon after the amputation of the limbs furnished no blood, and, notwithstanding the pulmonary inflation performed with the utmost care, all the signs of life had almost only the same duration with those observed in the cases of the excision of the heart, according to the tables which Dr. Le Gallois has given in his memoir for the different ages.

After the destruction of the medulla dorsalis, the same results occurred, as it regarded the vacuity of the carotids, the absence of the hemorrhage, and the duration of life.

The destruction of the lumbar medulla on rabbits aged four or five weeks, gave similar results, with this simple difference, that circulation did not stop instantaneously, as after the destruction either of the cervical or the dorsal medulla, but only about two minutes, and in one case about four minutes after; which proves that the action of the lumbar portion of the medulla on the heart, though very real and very great, is not as immediate as that of each of the two other portions.

After proving, by these experiments, that circulation depends upon all the portions of the medulla spinalis, the author convinced us that there are none of those portions which cannot be destroyed with impunity, by restraining gradually the extent of the parts, to which the heart distributes blood. He took a rabbit six weeks old, and after opening its abdomen, he tied the aorta, between the cæliac and anterior mesenteric artery, after which he destroyed all the lumbar medulla. This rabbit was still perfectly alive, supporting itself on its fore paws, and supporting its head well more than half an hour afterwards, when the commission adjourned; whilst another rabbit, nearly of the same age, whose lumbar medulla had been destroyed without the aorta being tied, as a point of comparison, died in less than two minutes.

Dr. Le Gallois afterwards performed the experiment of destroying the cervical medulla, the action of which upon the heart is more immediate, and much greater still than that of the lumbar medulla; he performed it upon rabbits five or six weeks old, without stopping circulation. He first decapitated the animal with the usual precautions; he then performed the pulmonary inflation during five minutes; after which he destroyed all the cervical medulla; he resumed the pulmonary inflation immediately afterwards, and the animal was left fully alive, and continued so as long as was thought fit to continue the inflation. The same experiment was repeated, with the same success, upon two other rabbits of the same age. It was further performed upon one of these, five minutes after having destroyed the cervical medulla, the author destroyed about the anterior third of the medulla dorsalis, then five minutes after the second third, and five minutes after the last third. Circulation and life continued after the destruction of the two first thirds, and only ceased after that of the last third. During this experiment, the inflation was only interrupted during the period necessary each time to destroy the medulla.

These experiments led Dr. Le Gallois to another much more difficult, the object of which is to prove that, by confining by ligatures the circulation to those parts only corresponding with any one portion of the medulla, this portion of medulla supplies the breast with sufficient energy to keep up the circulation of those parts. He detruncated, by cutting off both extremities, a rabbit thirty days old; at one end, at the first lumbar vertebra, and at the other at the second cervical vertebra; then by inflation supported life in the chest of this rabbit thus separated. We shall not describe the mode of operation, because the author has explained it minutely in his memoir, we shall merely state that the experiment completely succeeded, though an artery, that could not be tied, caused a pretty copious hemorrhage, and occasioned fears as to its success.

Finally, Dr. Le Gallois produced the partial death of the hinder parts, in a rabbit about twelve days old, by tying the aorta between the cæliac and anterior mesenteric arteries. Twelve minutes after, death appearing to be complete, he united the aorta, and life was re-established, by degrees, throughout the whole of the hind parts, so far that the animal could walk with ease. This partial resuscitation proves that a general one might likewise be produced, if it were possible to re-establish the circulation after the extinction of life throughout the whole of the spinal marrow. But the experiments of the author demonstrate much better than had been done before, why the resuscitation of the whole body is impossible.

The author also made, in our presence, experiments on guinea-pigs, from whence it results that in these animals, the power of the heart depends likewise upon the medulla spinalis; only a greater extent, than in rabbits of the same age, must be destroyed to stop the circulation.

We shall terminate this account of the experiments which Dr. Le Gallois made in our presence, by those made on cold-blooded animals, the results of which are entirely opposed to those obtained, and to which so much value was attached by the most zealous partisans of Haller, and amongst others, Fontana (*a*).

The author first opened the cranium, and then the thorax of a frog, and laid the heart bare; then he firmly tied the animal (*b*), and whilst one of us observed the motions of the heart with a watch, marking seconds, he destroyed the cerebrum, and all the medulla spinalis, by means of a probe, introduced through the opening of the cranium; the motions of the heart were instantaneously stopped; they were only resumed a few seconds after, and no longer observed the same harmony: they were more frequent than before the destruction of the medulla. The same experiment, made upon five frogs, was uniformly attended with the same results. The motions of the heart were not suspended in all, during the same number of seconds, but the suspension was al-

(*a*) Mémoires sur les parties sensib. et irritab. tom. III. p. 231.—Traité sur le venin de la vipère, etc. Florence, 1781. Tom. II. p. 169,171.

(*b*) Ibid. p. 233, of the work first cited; and page 171 of the second.

ways extremely well marked, as well as the change in the time of the pulsations. We shall add, that the amputation of the thighs in the frogs, whose medulla had just been destroyed, yielded no blood, and that the salamanders being decapitated after a like operation, did not bleed either, whilst in both these cases there was a hemorrhage, when the medulla spinalis was untouched.

These experiments appear to us completely to confirm all the consequences which the author had drawn from them, and with which he concludes his memoir. To confine ourselves to the principal points, we consider as fully demonstrated:

1st. That the principle of all inspiratory motions, resides nearly in that part of the medulla oblongata, which gives rise to the nerves of the eighth pair.

2d. That the principle which animates every part of the body, resides in that part of the medulla spinalis, from which the nerves of that part originate.

3d. That it is likewise from the medulla spinalis, that the heart receives the principle of its life, and of its power; but *in the whole medulla*, and not in a circumscribed portion of it alone.

4th. That the great sympathetic has its origin in the medulla spinalis, and that the peculiar character of this nerve is to place every one of the parts to which it is distributed, under the immediate influence of the whole nervous power.

These results readily resolve all the difficulties that have arisen since the days of Haller, on the causes of the motions of the heart. It will be recollected, that the principal consist in explaining, 1st. Why the heart receives nerves. 2d. Why it is submitted to the power of the passions. 3d. Why it is not to the will. 4th. Why circulation continues in the acephali, and in decapitated animals. It will be recollected also, that no explanation has hitherto been able to reconcile these points, or at least could only do it by hypotheses which, as we have before observed, give rise to other difficulties. But it is now very readily conceived why the heart receives nerves, and why it is found so peculiarly submitted to the power of the passions, since it is animated by all the medulla spinalis. It does not obey the will, because all the organs which are under the influence of the whole of the nervous power are not submitted to it. Finally, circulation continues in the acephali, and in decapitated animals, because the motions of the heart do not depend upon the brain, or only secondarily. It is our duty to remark, that this last point, on which Dr. Le Gallois has diffused so much light, presents nothing but confusion and error in the authors both in the ancient and new Hallerian school; not one of them has distinguished the motions of the heart which take place after decapitation, from those which are observed after the excision of that organ or after the destruction of the medulla spinalis; and they thought that both would be equally adequate to the maintenance of circulation. But these motions are essentially different from each other. The latter have no power to maintain circulation; they are perfectly similar to the weak motions which may be excited in the other muscles for some time after death. Dr. Le Gallois designates them under the name of motions of irritability (*mouvements d'irritabilité*), without attaching at present any other sense to that expression, than to express what takes place in the dead body (phénomènes cadaveriques).

The last task now devolves upon us, which is, to indicate what is the sole property of Dr. Le Gallois in the work which is the object of this report, and what share others might claim in it.

We can affirm, without fear of contradiction, that all this work belongs to him; it is enough to read his memoir with attention to be convinced of it. Chance furnished him with the idea of his first experiment, and that produced all the others; each of them having been suggested, and as it were, directed by the preceding one. Following them step by step, we find that his own method was his only guide, and by which alone he was governed. It is an unexampled instance in physiology, to find a work so considerable, every part of which is so connected, and so dependent upon each other, that to arrive at the perfect explanation of a fact, it is necessary to re-ascend to all those by which the author has arrived at it; and that no one deduction can be denied, without denying all the preceding ones, or raising a doubt with respect to those which follow.

It might have been expected, that, in researches so numerous, and which, by the importance of the questions they embrace, have arrested the attention of a great number of learned men, the author would frequently have been induced, even by following his own method, to repeat experiments already known. Nevertheless, out of all those recorded in his memoir, we have only remarked two that had been performed before him. One by Fontana, the other by Stenon. The first (*a*) consists in inflating and keeping alive an animal after decapitation. Fontana had only performed this experiment with a view of conveying oxygen to the venous

(*a*) Traité sur le venin de la vipère, etc. tom. I. p. 317.

blood, and it is readily perceived that it was foreign to our object. As it was unconnected, and could not serve as a proof to any one point of doctrine, little attention was paid to it, and it was left confounded with many other facts, from which a glimpse had been perceived, that even warm-blooded animals can survive decapitation, without knowing in other respects what was the real source of their life in that state. Hence it remained nearly unknown, except in a few English and German schools, and Dr. Le Gallois himself was entirely ignorant of it, when he communicated his first researches upon the functions of the medulla spinalis to the Faculty of Medicine of Paris. In other respects, this experiment was only one of the means which Dr. Le Gallois used to demonstrate two of his principal discoveries, namely, *that the principle of the inspiratory motions resides in the medulla oblongata, and that of the life of the trunk in the spinal marrow.*

The experiment of Stenon is that by which a ligature is applied to the abdominal portion of the aorta, and afterwards loosened to demonstrate that the interception of the circulation paralyzes the part in which it takes place, and that on the return of the blood, life is renewed. This experiment is well known, and has often been repeated. The authors who made it, intended to prove,—some, that the contraction of the muscles depends upon the action of the blood upon their fibres; others, that in every part sensibility depends upon the circulation; and in both questions it equally served to prove the affirmative and the negative, according to the mode in which it was performed. Thus when the aorta descendens itself was tied, sensation and motion quickly disappeared in the hinder parts (*a*). But when the ligature was made further on, only upon one of the crural arteries, although in this case circulation was entirely intercepted in the corresponding limb, sensation and motion were preserved for a long time (*b*). In this contradiction of results, each author was satisfied with those which favoured his opinion, and he thought

(*a*) Lorry, journal de méd. an. 1757, p. 15. Haller Mém. sur le mouvement du sang. p. 203, exp. 52.

(*b*) Schwenke, hœmatol. p. 8. Les expériences 57 et 58 de Haller, loc. cit. p. 205.

himself so much the more justified, as the true cause of this contradiction was not known.

But in the hands of Dr. Le Gallois, this very experiment appears under a very different aspect, and assumes a decided character. It is clearly perceived, that if sensation or motion only cease in the posterior limbs when the ligature has been made upon the aorta, it is because it is in this case only that the circulation is intercepted in the portion of the medulla spinalis, which gives rise to the nerves of those limbs.

Such, among the experiments of Dr. Le Gallois, are those alone which in our judgment might perhaps be claimed. But besides that the mode in which they make a part of his work, renders them his property, it appears to us that the novel point of view in which he has considered them, the precision in the details, and the clearness in the results, which he has substituted to the doubt and obscurity hitherto presented, have given a new character to these experiments.

We shall conclude with a few words respecting an opinion of M. Prochaska, which might be thought similar to that demonstrated by Dr. Le Gallois on the functions of the spinal marrow. This author places the *sensorium commune* both in the brain and in the medulla spinalis at the same time (*a*); but it must be remarked, that he is of opinion that the nervous power is generated throughout the whole extent of the nervous system, so that each part finds in its nerves, taken separately, the principle of its life and of its motions (*b*). He only considers the *sensorium*, as a central point where the nerves of sensation and of motion meet and communicate; and which establishes the relations of the nervous parts of the body (*c*); whereas, Dr. Le Gallois has demonstrated, that the spinal marrow is not only a means of communication between the different parts, but that it furnishes the principle of life and of energy which animates the whole body. And what

(*a*) Opera minora. tom. II. p. 51. Marherr, Hartley, &c. were of the same opinion before him.

(*b*) Ibid. p. 82.

(*c*) Ibid p. 151.

proves that Prochaska, in expressing his opinion (which, in other respects, he only gives as a probable (*a*) thing,) was very far from suspecting the true functions of the medulla spinalis, is that he only considers this medulla as a large bunch of nerves, *crassus funis nerveus* (*b*).

In short, it appears to us that it may be said of the different authors, who had some notions of the subject treated by Dr. Le Gallois, what La Place so judiciously remarks on a similar occasion: "we may meet with a few truths, "but they are almost always mingled with so "many errors, and their discovery belongs to "him alone, who by separating them from the "mixture, succeeds in establishing them upon "a solid foundation by calculation and obser-"vations (*c*)."

The opinion of your commissioners is, that the work of Dr. Le Gallois is one of the most

(*a*) Opera minora. p. 153.

(*b*) Ibid. p. 48.

(*c*) Mém. sur l'adhésion des corps à la surface des fluides dans la biblioth. britan. tom. XXXIV. p. 33.

beautiful, and certainly the most important that has been produced in physiology, since the learned experiments of Haller;—that this work will constitute an epoch in this science, on which it must diffuse quite a new light; that its author, so modest, so laborious, so praiseworthy, deserves that this class (of the Institute) should grant him its special benevolence, and also the encouragement which may be in its power. They should not forget adding, that the memoir they have just reviewed, is worthy of occupying a distinguished place in the *recueil des savans étrangers*, if the publication of the important discoveries which it contains, could be deferred till the period, perhaps too remote, of the publication of that collection.

(Signed) DE HUMBOLDT,
HALLE,
PERCY.*

The institute approves of the report and adopts its conclusions.

* From the original, it would appear that M. Percy is the author of the above report. T.

Resolved, Further, that this report shall be printed in the history *of this class*, and that the committee of the class shall make arrangements with Dr. Le Gallois, for defraying the expences occasioned by the experiment which he has already made, and for the further continuance of them.

A true copy.

(Signed) G. CUVIER

Perpetual Secretary.

SUPPLEMENT,

TO SERVE AS A

FURTHER ILLUSTRATION

OF

OMISSIONS IN THE FOREGOING STATEMENTS OF EXPERIMENTS.

ONE of the circumstances which have been found most prejudicial to the progress of experimental physiology, is the slight attention, and I may almost say the absolute neglect, on the part of those who made them, in regard to the choice of animals. They were taken as they happened to be found, without distinction of species or of age, and the results of various experiments performed in this way, were compared as if they had all been made upon animals of the same species and age. I pursued a very different plan; although I performed mine on several species, I have more particularly confined myself to one, which I have selected as the basis of all my researches. I preferred rabbits, because these animals are easily managed in experiments; it is easy to procure a great number, and by raising them I might be perfectly sure of their age; whilst it is very difficult to keep dogs and cats in sufficient number, and the age of those which are procured abroad is mostly uncertain. Therefore I constantly made my first attempts upon rabbits, and upon them I made all the previous trials, through which I was obliged to go to ascertain the results. In this way, all my experiments may be compared with each other. The results being once obtained and fully ascertained, I had only to prove them on other species; and this is what I have done on dogs, cats, and guinea-pigs. To avoid all confusion, I have almost always spoken of rabbits in the two first sections. I advise those who wish to repeat my experiments, to begin by rendering themselves familiar with them, upon the same animals.

Attention should be paid to choosing them of an age appropriate to the intended experiments. Whenever in any experiment respiration or circulation is to be stopped, and that we may be enabled to observe the result of the different phenomena of life in either case, the age of the animals should not exceed ten days, that that these phenomena may last longer, and that more leisure may be obtained to observe them. Particular attention must be paid to this, when we wish to find in what part of the medulla oblongata, the primum mobile of respiration resides; or when we wish to compare the signs of life, in the two portions of a rabbit which has been divided transversely. It is further necessary that the animal should be very young, even when we wish to make only a transverse section of the medulla, with a view to ascertain the independency in which the parts posterior to the section are to those which are anterior. In this experiment, when the animals are a little advanced in age, and the section has been performed near the lumbar region, the palsy takes place in a very few minutes in the posterior parts, although life subsists in the posterior segment of the medulla; of which there is no doubt, since the circulation continues, and since it stops as soon as this segment is destroyed. It appears that the palsy is produced by the circulation being very much weakened in the medulla, perhaps from the section of the superior and inferior spinal arteries (arteres spinales). What seems to render this probable is, that it happens later if the medulla is separated nearer to the neck, and that in very young animals in which the circulation is very active, the palsy does not take place, or become observable, except for a long time after.

The section of the medulla between the occiput and the first vertebra, frequently produces in rabbits mortal syncope. It is a singular fact, of which I shall in another place state the various circumstances; the safest way to avoid this accident, is to separate the medulla between the first and the second cervical vertebræ.

When we wish to observe the effects produced by the destruction of the whole or of part of the spinal marrow, care must be taken that such destruction be complete, which is not always easy, particularly in dogs and cats. It often happens that the instrument slides between the vertebral canal and the membranes lining it, and only bruises the medulla. That which is used is an iron probe, the diameter of which is proportioned to that of the vertebral canal, and consequently larger as the animal is older. I endeavour to introduce it within the membranes, and push it through the whole extent intended to be destroyed,—then I withdraw it; and I repeat these two motions several times, but with caution, lest I should introduce air into the vessels by lacerating them too rashly. Those parts which are most convenient for the introduction of the probe, and easiest to be distinguished in the living animal, are at the occiput, or between the two first cervical vertebræ, and between the last dorsal and the first lumbar vertebræ. The latter is easily known, when the skin has been divided longitudinally upon the spine and the ribs have been laid bare; it is the *intervertebral* space which is situated immediately below the last rib. Whatever portion of medulla I wish to destroy, it is always through one of these two places that I introduce the probe. To destroy all the medulla, I introduce it through the first and then push it as far as the tail. When we only wish to destroy one of the three portions, the destruction of the lumbar portion presents no difficulty; it is sufficient to introduce the probe between the last dorsal vertebra and the first lumbar, and to push it on even to the tail. But that of the cervical and dorsal portions requires some preliminary caution, and can only be done with any degree of precision, when you know previously the usual lengths of those portions in an animal of the species and age of that on which you perform the experiment. The following are the usual length of those of the rabbits:

Ages.	Common Lengths of the Cervical Medulla.		Common Lengths of the Dorsal Medulla.	
Days.	Millim.	Lines.	Millim.	Lines.
1	17	(7½)	33	(14½)
5	18	(8)	36	(16)
10	21	(9½)	44	(19½)
15	24	(10½)	47	(21)
20	27	(12)	51	(22½)
25	29	(13)	56	(25)
30	34	(15)	65	(29)

Measure with a compass the length of the portion to be destroyed, carry it on the probe, and mark it on it with a piece of thread, then push the probe up to the thread, into the vertebral canal, beginning at the occiput, for the cervical medulla, and between the last dorsal and the first lumbar, for the destruction of the dorsal; then lay the nail of the index finger of the hand holding the probe on the thread to prevent its slipping, and, after the operation, compare the measure taken by the compass to be certain that it has not slipped. The experiment being terminated, the vertebral canal should be opened, to ascertain that the destruction of the medulla has been complete; scissors are sufficient for this purpose, in animals one month old, and even more.

I always prefer one of the two hind members for amputation, to try whether there will be any hemorrhage; I amputate with scissors in the middle of the foot, of the leg, or of the thigh, according to the degree of force which the circulation appears to preserve; when I suppose it to be stopped, I begin by the amputation of the thigh. One of the operations which require the most habit in the above mentioned experiments, and that upon which the whole success of most of these experiments depend, is the inflation of the lungs (*a*). Whenever the brain can no longer act upon the inspiratory organs, whether the medulla oblongata has been disorganized, or the medulla spinalis divided or destroyed near its origin, if any other operation has been performed, the effects of which are intended to be observed, it is indispensable to blow air into the lungs, to try to prolong the life of the animal; otherwise it would be doubtful whether its death was produced by the operation or by the asphyxia. It is often necessary to have recourse to this method, though the brain and the origin of the medulla spinalis remain entire; this hap-

(*a*) This operation has been erroneously designated by the name of Hook's experiment. Long before this Englishman, Vesalius* had made use of it, to protract life in animals whose breast he had opened with a view to observe the motions of the heart. Among the authors who have afterwards repeated it with various views, Goodwin† has, in a particular manner, the merit of having presented it, as the most powerful remedy against asphyxia; and it is upon this that I think my experiments will leave no doubt.

* De humani corporis fabricâ. Basileæ. 1555, p. 824.

† La connexion de la vie avec la respiration, traduit de l'Anglais par M. Hallé. Paris, 1798.

pens when the animal has been much weakened, and has not preserved vigour enough to breathe. In this case the circulation still continues; but the asphyxia would soon make it cease. On this subject I would remark, that the weakest degree of action in the medulla spinalis that may be compatible with life, is that which maintains some remains of circulation. It is true that the degree necessary for the last inspirations of a dying animal is very nearly the same, yet it is always a little stronger. In adult animals, the difference of these two degrees is not always easily distinguished; but it is very evident in very young animals. Therefore, when these are asphyxiated by intercepting the air, the inspiratory efforts always end several minutes before the circulation, and they may always be recalled to life long after respiration has entirely ceased.

The principal conditions to be effected in the inflation of the lungs are, to introduce into the lungs a quantity of air proportioned to their capacity, or rather to that which they receive in a natural state; to renew this air by every inflation, and to perform a number of them nearly equal to that of the natural inspiration, in a given time. The success depends, in a great measure, upon the instrument that is used; I choose a common pewter syringe, which has a hole at the bottom of the cylinder, and which is to be a little larger than the orifice of the pipe; and besides the ring which terminates the handle of the piston, it has two more on the top of the body of the cylinder, one on each side. This is the only peculiarity. It is to be used thus: take it with the right hand, passing the index and ring finger into the rings of the cylinder, and the thumb into that of the piston; introduce the pipe in the opening previously made in the trachea, close to and behind the larynx; place the animal on its back, hold it by the head and neck, or if it has been decapitated by the neck and trachea, with the left hand, which is placed behind the index finger, which is brought back upon the trachea to fix the pipe, and retain the inflated air; then set the piston in motion, by alternately moving the thumb to and from the two other fingers (*a*). In these alternate motions,

(*a*) See the plate.

that the air may be regularly propelled into the lungs, thrown out and renewed, it is necessary to stop the hole which is at the bottom of the cylinder, with the thumb of the left hand, during two successive motions of the piston, one rejecting, the other extracting; and to unstop the hole by removing the thumb in the two subsequent motions. Thus if, when the body of the syringe contains the quantity of air to be introduced into the lungs, the hole is stopped, and the piston pushed, this air passes into the chest; and if, whilst you continue to keep the hole stopped, the piston is withdrawn, the same air returns into the body of the syringe. These are the two first motions, or inspiration and expiration. Next to this, if you unstop the hole by removing the thumb, and you push the piston to the bottom of the syringe, this same air entirely escapes through the hole, meeting with less resistance than through the pipe; and if the hole remaining open, you draw the piston, fresh air enters. These are the two subsequent motions which expel and renew the air in the body of the syringe (*a*).

It is impossible to say what is the precise quantity of air that is suitable for each inflation: for if the quantity of a natural inspiration is so difficult to be determined in man, it is much more so in animals. All that can be done in this respect, is, to make a near guess. I have three syringes of different sizes, which are sufficient for all my experiments; and I employ any one of them, according to the age and the size of the animal. The following are their dimensions (*b*):

	Length measured outside.			Internal diameter.	
	millim.	inch.	line.	millim.	line.
The smallest,	77	(2	10)	18	(8)
The middle sized,	81	(3	)	23	(10½)
The largest,	92	(3	5)	37	(16½)

(*a*) The instrument used by Goodwin was also a syringe, but, from an error, difficult to be explained, the hole intended for the renewal of the air, instead of being at the bottom of the cylinder, was about the superior third. In this way, the air could be renewed but very imperfectly.

(*b*) The new French measures proceed in a decimal order; for instance, the millimètre is the tenth part of

The small one answers for rabbits not older than twenty days; and it might even be used when older, if its capacity was not lessened by the whole volume of the piston. When rabbits are but a few days old, I confine the motion of the piston to two or three-twelfths of an inch, and increase it by degrees with the age of the animal.

The small pewter syringes have the inconvenience, that the pipe is frequently too large for the trachea of new born rabbits, and especially for that of guinea-pigs. This may be remedied by a silver pipe fitted upon the pewter one. This pipe, which is very thin at the end, ought to

the centimètre; the latter is the tenth part of the decimètre, the decimètre is the tenth part of the mètre, and so on. The numbers which are annexed to the following names of the French measures, express the number of English inches or Troy grains, to which they are equivalent.

Millimètre	0,039371 Eng. inch.
Centimètre	0,39371
Decimètre	3,9371
Mètre	39,371

The mètre is equal to the forty-millionth part of the whole circumference of the earth. T.

have a conical shape; and, in general, the pipes of all syringes intended for inflation, ought to be conical, and grow larger pretty quickly, that by pushing them in a suitable manner into the trachea they may completely fill it.

I use the middle sized one for rabbits from the age of twenty days to that of two months and beyond, and I likewise vary the motion of the piston; this syringe is always sufficient for adult guinea-pigs.

I only have recourse to the third for larger rabbits, or for those younger animals having more capacious lungs, such as dogs. Care must be taken that the piston perfectly fills the body of the syringe, and yet that its motion should be performed easily and without effort, otherwise inflation will be laborious, and cannot be continued for any length of time. Besides, the jirks occasioned by uneven motions would produce some disorder in the lungs.

As to the number of inflations to be made in a minute, it cannot entirely be assimilated to that of natural inspirations in rabbits and guinea-pigs, which are generally upwards of eighty per minute. There would be danger, in thus hurrying the inflations, that the vessels of the lungs would be ruptured, and the inflated air extravasated. I generally produce fifty per minute.

The decapitation required in many experiments may be performed in several ways, which may be all reduced to tying the vessels of the neck before the head is separated, and to beginning the pulmonary inflation before the animal is killed by asphyxia. It must be recollected, that the asphxyxia begins when the medulla spinalis has been separated between the head and the origin of the phrenic nerves, and that the pulmonary inflation must be resorted to so much the more promptly, as the animal is older. The safest way is, to be governed in that respect by the gapings; there is every reason to expect, that the inflation will succeed, when it is performed before they have ceased. If any circumstance should prevent their being observed in an experiment, the period of their cessation may be calculated from the table of page 84. The process I have described, page 116-17, and 128-29, is specially suitable for rabbits advanced in age. They may be simplified for those which are less than a fortnight old, and which do not require so prompt a recourse to the pulmonary inflation. For these I use the following: The animal being placed on its belly, I hold him by the head with my left hand; I stretch the skin of the nape of the neck between my thumb and index finger of the same hand. I find out, with the index finger of the right hand, through the skin, what is the length of the interval between the first and second cervical vertebræ, and thrust into it a large sewing needle, which I take in the same hand, and with it I cut through the medulla. Then I turn the animal upon its back, and keep it so, still holding it with my left hand by the head, and by fastening to a nail driven on the table, the loop of a piece of twine previously tyed to its hind feet;—I take a scalpel with the right hand, and stretching the skin and the soft parts with my left thumb and index finger, I lay bare the trachea, and the vessels of the neck; I tie both carotids, and the external and internal jugular veins, by means of a common sewing needle with thread (*a*); I slide the scalpel under the larynx, to loosen it from the os hyoides; this being done, I leave the scalpel, and take scissors, with which I separate the neck near the occiput; and it is not till then I begin the inflation of the lungs. It often happens that a guggling noise is heard in the breast, as soon as decapitation has been performed. It is a sign that the air has penetrated the vessels; and the experiment has failed. If there be any difficulty in finding the first cervical vertebra through the skin, they may be laid bare by a longitudinal incision. I prefer the needle to the scalpel, to divide the medulla spinalis, because it causes less hemorrhage.

In all the experiments, attention must be paid to the choice of the animals, which should be sound and in good health. If sick, and especially if cold has rendered them languid, the result

(*a*) Needles, slightly crooked, would be more convenient; but I have given up using the common surgeon's needles, which are too sharp in their edges, because I have frequently cut through the arteries with them will not be the same, especially in what regards the duration of the phenomena. Cold modifies and protracts the phenomena of asphyxia, in a very remarkable manner, in very young animals. This is a curious fact, capable of important applications to the human fœtus; and connected with the theory of hibernation in certain animals. I barely mentioned it to the society of the Faculty of Medicine (*a*); I shall pursue the inquiry hereafter. If the eighth pair of nerves be divided in dogs newly born, that are benumbed with cold, the temperature of the atmosphere being ten degrees (Reaumur*), they may live a whole day in that state, without there being any necessity for making an opening into the trachea. This is owing to their glottis not closing so exactly as in cats, and the very small quantity of air, for which it may still leave a passage, being sufficient for the maintenance of so weak an existence.

(*a*) Bulletin de la Faculté de médecine de Paris, 1812. No. 1.

* Ten degrees of Reaumur's thermometer are equal to fifty-five of Farenheit's.

When the eighth pair is divided in guinea-pigs and an opening is made into the trachea, this canal being very narrow in these animals, it is very difficult to prevent its being stopped. Continual attention should be paid to this.

I have said, that the degree of fulness of the carotids was a sign equally certain and convenient, to judge of the state of the circulation; and that their vacuity always indicates that this function had ceased. But it sometimes happened, that these arteries still contain a small stream of blood, and that they are more or less round, though the circulation be stopped. To prove the correctness of this fact, it is enough to lay bare one of the carotids through a certain extent, and to press it with the end of the finger, moving from the breast towards the head. If, when the finger is taken off, it remains white and flat, as if only a little blood comes from the head, there is no doubt that the circulation is stopped; for when it is going on, even in the smallest degree, the blood often returns in the carotids thus emptied, upon the finger being taken off; it returns from the heart, and by thus trying it several times the result proves always the same.

When the circulation has been weakened by the destruction of a portion of the medulla spinalis, or by any other cause, the degree of pressure necessary to flatten the carotids at any spot, sufficiently indicates that degree of weakness. In health, if you press this artery with a probe, it requires a certain strength to flatten it, and that only takes place in the spot pressed; if it be raised by a probe passed underneath, it still retains a cylindrical form, unless raised very high. But when the circulation is weakened, one moderate pressure is enough to depress this artery, not only in the place compressed, but at a greater or less distance on both sides, before and behind; and by raising it with the probe, it is flattened upon this instrument, and beyond on each side. The degree of weakness of the circulation may thus be estimated and compared, in the different cases, by the facility and extent of the flattening of the carotids.

END OF THE EXPERIMENTS.

NOTE

RESPECTING THE TEETH OF RABBITS AND GUINEA-PIGS.

I HAVE ascertained, by repeated observations, made on rabbits and guinea-pigs of every age, that these animals have no milk teeth, and that they preserve, during all their lives, those which appear before or after birth. In the young animals, the teeth are slightly conical, or truncated pyramids, so that in proportion as the edge is worn down, the part that advances from the socket, being gradually larger, and this continuing till the animal acquires nearly its full growth, they assume a prismatic form. This fact indicates pretty clearly the final cause of the shedding of teeth in the species which are liable to it. It is at present fully proved, that teeth are substances formed by excretion, and not growing by intussusception, or continuing for ever in the state they were in when issuing from the socket. In this state of things, those which fill the alveolar arch of a young animal, and which are fitted to the dimension of its jaws would no longer be so in the same animal when adult; more especially in the carnivorous, the teeth of which do not wear out nor grow from the moment of their being fully out. To remedy the inconveniences of stationary teeth in jaws which continue to grow in all directions, nature employs two methods; namely, the replacing of the first teeth, and the appearance of a second set at a more advanced period. But it is evident, that in animals, such as the rabbit and the guinea-pig, the teeth of which continually grow and become larger as they wear out at the edge, the teeth and jaws were to be left in the same proportion at every age, and that the shedding was useless; and it does not, in fact, take place. We may account from the same principle for the nails and several other bodies of this nature, which, like the teeth, being substances formed by excretion, are not shed to be afterwards replaced. I have always observed that rabbits have six molar teeth on each side of the upper jaw, and not five only, as in the lower. The sixth and posterior one is very small, and it is no doubt for this reason it had escaped the notice of zoologists.

NOTE

RESPECTING THE DURATION OF GESTATION IN GUINEA-PIGS.

GUINEA-PIGS have been so long since naturalized and multiplied in Europe, that it must appear strange that no author has been acquainted with the true duration of gestation in these animals. Buffon says it lasts three weeks; the new dictionary of natural history repeated the same opinion; others have assigned different durations, but they were equally erroneous. The cause of this uncertainty is due to the difficulty of ascertaining the moment of copulation, it being difficult for the male to effect it. It often takes him a fortnight, and sometimes more. During all this time, his apparent ardour and all his efforts are baffled by a singular disposition of the vagina of the female. This disposition consists in this, that the exterior orifice is *glued* and completely closed. The male must unglue it before copulation takes place; it is then glued again three days after, and likewise glued again after parturition. It was by separating the females from the males upon perceiving the separation of the parts, that I found the duration of gestation to be sixty-five days. But this happy privilege of being for ever a virgin after numerous parturition does not belong exclusively to the female of the guinea-pig; it is also the privilege of an ancient inhabitant of Europe—the mouse.

NOTE

RESPECTING THE RELAXATION OF THE SYMPHYSIS OF THE PELVIS IN GUINEA-PIGS AT THE PERIOD OF PARTURITION.

It is known, that in the animated discussions which have arisen concerning the section of the symphysis pubis in certain cases of difficult parturition, the partisans of this operation have grounded the principal hope of success on the swelling and relaxation of all the symphysis of the pelvis towards the end of gestation. They considered this swelling as a mode employed by nature to increase the diameters of the pelvis, an indication to increase them still more by the artificial separation of the symphysis, and the possibility of obtaining a sufficient distance between the two ossa pubis, on account of the hinge-like motion, which may be promoted by the sacro-iliac symphysis thus infiltrated and softened. But while their opponents denied this swelling and its consequences, it does not appear that any one has ever discovered a case in which nature herself performs a true and complete separation of the ossa pubis in order to render the delivery possible. This is nevertheless what is observed in one species of animals—the guinea-pig.

When we compare the pelvis of the female of a guinea-pig, with the head of a full grown fœtus, we are convinced, on the first view, that it would be utterly impossible that its head should come through the pelvis, and consequently that the delivery should take place, if the pelvis constantly preserved the state and dimensions observable at any other time than at that of gestation. Without going at present into any further details concerning the respective dimensions of the head of the fœtus and the pelvis of the female of that species, suffice it to remark, that parturition depends especially upon the transverse diameter of both. Now, the transverse diameter of the head of a fœtus of middling size full grown, but dried, is nine twelfths of an inch (French measure); whilst that of the pelvis of a female of middling size, measured between the acetabula on the bare and dried bones, is not quite five twelfths of an inch. If we take in the account the soft parts lining the cavity of the pelvis, we shall find that when alive its diameter is about one half of the head of a fœtus; and nevertheless guinea-pigs are delivered with much ease. It thence necessarily follows, that nature must have provided some means of removing the difficulty produced by this enormous disproportion. And this is what does in fact take place.

I indicated in 1809 (see preceding note), that the duration of gestation in these animals, was sixty-five days. About three weeks before delivery, the symphysis pubis is seen to acquire more thickness and a slight mobility; these are continually increasing. Eight or ten days before delivery, the two ossa pubis begin to separate from one another. This separation increases slowly at first, and only begins to go on rapidly for the three or four days which precede the delivery. It is such at the moment of parturition that it readily admits the middle finger, and even sometimes both the middle and fore finger together.

The delivery being at an end, the bones of the pubis soon close. Twelve hours after, the distance of the separation has lessened more than one half; and twenty-four hours after, they are in contact at their anterior extremity; and in less than three days, they are perfectly so through the whole extent of their symphysis, which then only presents a slight thickness and mobility. A few days after, nothing is to be seen but a very slight mobility, which disappears sooner or later. But when the females are old or sick, the re-union is made more slowly.

I have measured the separation of the pubes in three females killed at the period of their delivery. In two, which were at the sixty-fourth day of gestation, this separation was about five twelfth parts of an inch; and in a third, which was at the sixty-fifth day, the separation was a little less than six. In these three females the *sacro-iliac* symphysis possessed great mobility but without any remarkable separation. This mobility of the *sacro-iliac* symphysis, without which the separation of the ossa pubis could only be very limited, allows besides, a posterior motion of the os sacrum; and as it is only the posterior extremity of the os sacrum which corresponds with the symphysis of the ossa pubis, it is perceived on the one hand that the head of the fœtus, by pressing upon this extremity, acts upon the sacro-iliac symphysis as it would upon the end of a pretty long lever; and on the other hand that a light motion of the sacrum or of the ossa innominata in the two symphysis, is adequate to produce a sufficient separation between the posterior extremity of the os sacrum and the symphysis of the ossa pubis.

From this statement, it appears that the pelvis of the female of the guinea-pig, is considerably increased in all its diameter at the moment of parturition. Nothing less than such a mechanism was required, that so small an animal might bring forth fœtuses at least as large as those of the rabbit, and which are besides nearly in an adult state. For little guinea-pigs are seen to run almost as soon as they are born; both their eye-lids and ears are opened; all their teeth are out, and they can chew grass on the day of their birth: scarcely do they want to suck, and they might in a warmer climate than ours entirely do without their mother. In short, that which proves perhaps better than any thing else how far they are grown at the moment of their birth is, that they then suffer from asphyxia, in the same manner as other animals do at nearly an adult age. From my experiments, the asphyxia which rabbits can bear is about seven times longer at the moment of birth than at the adult age; it is nearly the same with dogs and cats; whereas the new born guinea-pig can only bear one which is scarcely double of that supported by the adult. Thus the duration of gestation, which in general is shorter in proportion as the animals are smaller, is twice as long, and even a little more, in the guinea-pig than in the rabbit. But these are not the only anomalies which we meet with in these extraordinary animals; I shall point out others hereafter.

THE END.

EXPLANATION

OF

THE PLATE.

Fig. I. Represents the smallest of the three syringes mentioned in page 308.

The tube *a* is truncated and lined with tow for the insertion of a silver one, represented by *d*.

ccc, Are ribbons destined to be bound round the rings of the syringe, so that the metal should not hurt the fingers of the operator.

Fig. II. Represents the middling-sized syringe; the tube is screwed at *a*, which gives the facility of changing it when it is found to be too small, and to substitute another which fills up entirely the trachea of the animal. The tube of the large syringe can also be taken off.

ccc, Knot made at the two ends of the ribbon, bound round each ring. In those two figures, *b* indicates the hole made at the bottom of the body of the pump, to renew the air.

Fig. III. Represents a decapitated rabbit, kept alive by pulmonary inflation, performed with the small syringe to which the silver tube is adapted. See page 306 & 312. The thumb of the left hand stops up the hole of the syringe, so that the operator may perform with ease. This hole must correspond to the middle of the interval, which separates the two rings from the body of the pump. The animal is placed on a board, and has its two hind paws tied to a nail driven in the board. Its head is seen laying on the table, still continuing to gape; near it lays the needle threaded which was used to tie the vessels; at the end of the table are a scalpel, a pair of scissors, a large needle to divide the spinal marrow, and a watch, that the operator ought always to have before his eyes, to indicate the length of time the experiment lasts, and particularly the period when he is to commence pulmonary inflation, and the time he may desist without danger. If to these instruments we add a small piece of iron wire, to clear the tube of the blood and mucus which sometimes obstruct it, a few iron probes of various sizes to destroy the spinal marrow, a compass and a foot-rule to mark on the probe the length of the portion of spinal marrow that is to be destroyed (see page 302), we hall have every thing that is necessary to repeat all the experiments mentioned in the course of this work.

ERRATA.

Page 48, line 24, read the phrase thus,—And if in this half only a part of the spinal marrow be destroyed, all the parts which receive their nerves from this part are instantaneously struck with death while the remainder of the half continues alive.

Page 50,	line 3,	for *breast*,	read *thorax*.
65,	22,	*vertebra*,	*vertebræ*.
95,	13,	*feeling*,	*sensibility*.
103,	8,	*carotid*,	*carotids*.
114,	3,	*two minutes and a quarter*,	read *in about two minutes and a quarter after*.
128,	24,	*vertebræ*,	read *vertebra*.
129,	14,	*breast*,	*chest*.
129,	19,	*breast*,	*thorax*.
165,	29,	*physiology*,	*in the physiology*.
204,	20,	*takes*,	*take*.
205,	8,	*in the*,	*on the*.
205,	note, 1,	*sauvages*,	*Sauvages's*.
266,	14,	*such portion*,	*such a portion*.

Pages 131, 132, and 148, for *resurrection*, read *resuscitation*.