ON THE

INFLUENCE

OF

PHYSICAL AGENTS ON LIFE,

BY

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Translated from the French,

BY DR. HODGKIN AND DR. FISHER.

TO WHICH ARE ADDED, IN THE

APPENDIX,

SOME OBSERVATIONS ON ELECTRICITY,

BY DR. EDWARDS, M. POUILLET, AND LUKE HOWARD, F.R.S.;

ON ABSORPTION, AND THE USES OF THE SPLEEN,

BY DR. HODGKIN;

ON THE MICROSCOPIC CHARACTERS OF THE ANIMAL TISSUES AND FLUIDS,

BY J. J. LISTER, F.R.S. AND DR. HODGKIN;

AND SOME

NOTES TO THE WORK OF DR. EDWARDS.

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PREFACE.

It does not appear necessary to say much to urge the importance of the investigation of the influence of a variety of physical agents on life, since it constitutes not only a most essential branch of physiology as a science, but is replete with practical points of vital importance and universal application; seeing they are not less connected with the preservation of health, than with the cure of disease. Many of the functions of life are confessedly veiled under an almost impenetrable obscurity. This indeed is so universally admitted, that the idea of reducing them to the rank of those phænomena which come within the province of physics (properly so called) or natural philosophy, and of applying to them those laws which we know to regulate operations in which inorganic or dead matter is concerned, is regarded as hopeless, and many physiologists, without reviving the *autocrateia* of Staël, nevertheless refer to vitality with its *vires conservatrices* as so varying in its powers and operations, as to baffle every attempt to reduce to fixed principles many of those phænomena of which it is an element. We cannot be surprised at this when we consider the almost infinite variety which life presents in the wide range of the animal kingdom, and observe how these varieties are multiplied by those presented by a single species, nay, by a single individual under various circumstances of age, season, and situation. We must not, however, too hastily adopt the idea, that this subject is really one which presents inherent obstacles of insurmountable difficulty. Many subjects which at first appear to be involved in inextricable confusion and perplexity, become clear and intelligible when once the proper clue or explanation is furnished. Some minds are so happily constituted as to have a remarkable readiness in perceiving the relations which connect facts and observations, which to others appear not merely isolated, but absolutely contradictory. This appears to be particularly the case with Dr. Edwards. The labours of his predecessors had accumulated a vast collection of invaluable facts and observations, many of which seemed to be almost annihilated by their standing in direct opposition to others supported by equally valid and respectable authority; the labours of Dr. Edwards have explained many of these discrepances. It may be ill becoming in me to anticipate the judgment of the reader, but I cannot refrain from expressing my admiration of the patient and clear induction with which the Doctor proceeds, step by step, through the great variety of subjects comprised in his work, so as to maintain the unity and connexion of the whole, and of the happy art with which he has both availed himself of the experiments and observations of his predecessors, and supplied the breaks and deficiencies which he met with, by well contrived simple and conclusive experiments of his own.

It is at least presumptive evidence of the merit of the Doctor's work, that different parts of it presented at separate times to the Academy of Sciences of Paris, obtained for their Author, although a foreigner, the honourable distinction of the physiological prize. It is certainly to be regretted, that our philosophical countryman has not himself exhibited his instructive work in an English dress, that our medical literature might have the credit of possessing it as an original rather than as a translation. Translations are generally inferior to original publications. In the present instance I have endeavoured to reduce the weight of this objection by submitting the translation to the Author's perusal, and he has kindly supplied me with some fresh matter, which will be found in the Appendix. Whilst I feel justified in expressing myself as I have done with respect to the original work, to which I have to acknowledge the obligation of much important assistance in practice, I must confess myself very differently circumstanced with regard to the translation.

To suit the convenience of English students, who have in general neither time nor inclination for voluminous reading, Dr. Fisher and myself have laboured, as far as possible, to compress the work without omitting a single experiment or conclusion. This, however, has been no easy task, as Dr. Edwards' own method of exposing the subjects of which he treats is in general too concise to admit of abbreviation, without incurring the risk of producing obscurity.

I have thought it best, in publishing the translation, to omit the copious tables, in which the Author has set forth the individual results of his very numerous experiments, to enable the reader to confirm the conclusions which he has deduced from them. These form a valuable addition to the original work, but as I conceive that the majority of readers will rarely if ever refer to the tables, I have judged that to reprint them would considerably and needlessly increase the price of the book. Those who are engaged in similar researches with Dr. Edwards, and are desirous of referring to the tables, may easily consult them in the original work, since, as they are almost purely numerical, they may be easily understood even by those who are unacquainted with the French language.

The Appendix to the original work, relates to electricity in conjunction with the phænomena of life. It was furnished by Prevost and Dumas, and is principally devoted to their views respecting muscular contractions, on which subject I must confess myself under the necessity of dissenting from those able physiologists. To the Appendix, in the translation, I have made some additions, in order to put the reader in possession of subsequent researches regarding the relations between life and electricity; yet it must be confessed, that this subject is still in a very imperfect state, and calls for further investigation, which would doubtless well repay the labour of conducting it.

Some other points relating to physical phænomena connected with life, are also briefly noticed in the Appendix, viz.: Dutrochet's views respecting endosmosis and exosmosis—those of Dr. Stephens, which have thrown most important light on the chemical changes produced in respiration and circulation, and the labours of other experimenters on the same subject.

I have likewise reprinted, with some additions and alterations, my Thesis on Absorption, a short paper on the Uses of the Spleen, and the microscopic observations of my friend Joseph J. Lister and myself, in relation to the tissues and fluids of animals.

The obvious relation which they bear to the objects of Dr. Edward's work, will, I trust, be a sufficient apology for the introduction of them. The notes which are also given in the Appendix, are few and generally short. For the materials of the Appendix, I am greatly indebted to the kindness of my friends, and it gives me pleasure to embrace this opportunity of acknowledging my obligations in this respect to Sir Astley Cooper, Dr. Stephens, Dr. Marshall Hall, Dr. C. Thompson, my valued friend Luke Howard, C. Woodward and to my learned and accomplished friend A. R. Dusgate.

I cannot conclude this preface without expressing a hope, that the students and younger members of the profession may zealously pursue the investigation of the various interesting subjects which physiology presents, in the philosophical method of which Dr. Edwards has given so striking an example.

CONTENTS.

PART II.

FISHES AND REPTILES.

PART III.

WARM-BLOODED ANIMALS.

PART IV.

MAN AND VERTEBRAL ANIMALS.

APPENDIX.

NOTES.

ERRATA.

Page 9 line 8 from bottom *for* heart *read* hearts.
19 in note *for* preceding *read* succeeding.
24 line 9 from bottom *for* lugs *read* lungs.
245 *for* Chapter XV. *read* Chapter XVII.
331 line 10 from bottom *for* hogs *read* dogs.
334 —— 5 from bottom *for* F. Smith *read* P. Smith.
448 —— 12 *for* contribution *read* contributor.

INTRODUCTION.

THE object of the present work is the examination of the effects of those agents by which we are surrounded, and whose influence is incessantly exerted upon us. They are called physical agents, as being the objects of that part of science which is denominated physics. They are to be distinguished from mechanical agents.

These researches will relate to the AIR in its several conditions of *quantity, motion, or rest, density or rarity;* to WATER in *a liquid state,* and in a state of *vapour;* to TEMPERATURE, as modified both in *degree and duration;* to LIGHT; and to ELECTRICITY.

These agents operate simultaneously, and, in general, imperceptibly, on the animal economy.

The impression produced is the result of their combined influence. Even when the intensity of any one of them is such, that we are enabled to distinguish the cause which is affecting us, it most frequently happens that the sensation alone is attended to, whilst the accompanying changes escape our notice. Hence, the most careful observation of phenomena, as presented by nature, cannot enable us to analyze the result of such combined actions, and to assign to each cause its peculiar effect, whilst those effects, which it is not in the province of sensation to detect, will remain undiscovered. By means of experiments, we may, however, control external circumstances, and vary that of which we wish to appreciate the action; and thence, by observing the correspondence existing between such modification, and the accompanying change which takes place in the animal economy, we may establish the relation of cause and effect. In order to derive advantage from this method, the intensity of the cause must be determined on the one hand, and the degree of effect on the other. In physics we may generally find means of accomplishing the first: the reader will judge how far I have succeeded with the second.

I took, for the subjects of my experiments, various species of animals from all the four vertebrated classes, in order to give greater certainty to particular results, when an agent produced uniform effect on beings so differently constituted.

Moreover, I hoped that the investigation of the very evident modifications, of which certain species are susceptible, might lead to the discovery of similar modifications in species in which they are too little marked to fix the attention in the first instance. I soon found the result to equal my expectation.

In the detail of my researches I have adhered to the order in which they were conducted. I have divided the work into four parts.

The first relates to the Batrachian Reptiles; the second, to the other Cold-blooded Vertebrated Animals; the third, to Warm-blooded Animals; the fourth, to Man, and the other Vertebrated Animals.*

In the outset of these inquiries I soon perceived that the science of electricity was too little advanced to supply me with the requisite means for placing the investigation of

* I also made corresponding experiments with several families of invertebrated animals. M. Adoin, well known by his labours on the anatomy of insects, assisted me in conducting them.

this on a par with that of other agents. The recent discovery of Œrsted, by which the phenomena of electricity and magnetism are connected, forms, in conjunction with those of Ampère, and several other natural philosophers, a new epoch in the annals of this branch of science. The principles which they have established, and the instruments which they have invented for the appreciation of actions hitherto unknown, have furnished Prevost and Dumas with the means of making some very interesting researches on electricity, in connection with the animal economy. To their kindness I am indebted for the concise view of the present state of our knowledge on this subject, which is contained in the Appendix to this work.

Tables are added, exhibiting the individual results of the principal experiments, in order that the reader may be better enabled to judge of the bases on which the conclusions are founded.

The examination of one fact always led me to that of another; hence, the intimate connection between all the phenomena which I have detailed. The importance of the agent decided the point at which my researches were to commence. All the physical agents are indeed indispensable to the maintenance of life; but as the air is that for which there is obviously the most pressing necessity, I began by examining the effects which result from the privation of it. The choice of the animals for experiment followed as a consequence. Those which offered the widest scope for observation, with regard both to the duration of the phenomena, and to the facilities afforded for variation of the experiments, were the first to be examined, I therefore commenced with the family of the batrachians.

They unite many other advantages, which render them peculiarly adapted to afford the first notions of the influence of physical agents. As they participate in the qualities of reptiles and of fishes, the knowledge obtained from the study of them renders it the more easy to pass rapidly to the other cold-blooded vertebrals.

The minutiæ of detail may be collected from the tables whenever the uniformity of the phenomena is obvious, whilst the attention is directed to the particular consideration of those instances which at first sight appear to be exceptions, the examination of which leads to further results.

The higher temperature of the mammalia and of birds, being the physiological fact which forms the strongest contrast between them and reptiles and fishes, I make it the first point to be considered in the study of warm-blooded animals; and, regarding the development of heat as a function abstractedly, I endeavour to determine what are the variations to which it is subject, according to various circumstances with respect to organization on the one hand, and to external agents on the other. The results of this examination furnish the elements which enter into a great number of other phenomena, which are the subjects of subsequent researches.

The commencement of the third part corresponds to the researches in the first, in which I examine the effects of the internal temperature on cold-blooded vertebral animals. I there make no allusion to the facts detailed in the preceding parts, but confine myself in treating of warm-blooded animals to the independent consideration of them.

It is only in the fourth part which relates to man, with the other vertebral animals, that I take an extended view of the phenomena, as well through the medium of the previously detailed facts as of others, which serve as the complement to them, or lead to new considerations. It is this generalization which admits of our entering on the consideration of man. This is the end which I proposed to myself, and to which every thing that I have advanced leads and refers.

The relations of the physical agents to the animal economy are infinite. It was necessary to make a selection. I have confined myself to those direct actions, which the present state of the physical sciences furnishes us with the means of appreciating, and to the examination of their combinations.

In the choice of the circumstances, of which I sought to discover the influence, I have always been guided by the wish to establish principles capable of useful application.

The agents which I have examined, having immediate relation to the nervous system, and to the organs of respiration, circulation, exhalation, and absorption, I have been led to the investigation of a great number of facts connected with hygeia and pathology, of which an idea will at once be formed, when it is considered that I have been particularly occupied with modifications dependent on constitution, and with the changes which constitution undergoes through the operation of external agents.

The greater number of the facts which I have related, were first brought forward in various papers which I have read before the Royal Academy of Sciences of Paris, or presented to that body as subjects for the prize founded for the promotion of experimental physiology.*

* Chap I. The part, *On Asphyxia* was read to the Academy of Sciences in 1817. and printed in the *Annales de Physique et de Chimie* for the same year, Vol. 5.

Chap. II. The first part, *On the Influence of Temperature* was read to the Academy in 1818, and published in the *Annales de Physique et de Chimie* the same year, Vol. 8.

Chap. III. The first part, *On the Influence of Air*, contained in

I owe the acknowledgment of my obligations to my pupil M. Vavasseur, who assisted me in the course of my experiments.

water, was read to the Academy in 1818, and inserted in the *Annales de Physique et de Chimie*, Vol. 10.

Chap. IV. The first part, *On the Vivifying Influence of the Atmosphere*—

Chap. V. First part, *On the Influence of the Atmosphere on Transpiration*—

Chap. VI. First part, *On Absorption and Transpiration in Water*, —were read to the Academy of Sciences in 1819.

These three chapters united to the second part with a short statement of the facts contained in the third, were presented to the *concours* for the prize of experimental physiology in 1819, and were crowned by the Royal Academy of Sciences, together with the work of M. Serres *sur les l'Ostéogénie* in 1820. Baron Cuvier gave an account of these memoirs in the *Analysis of the Transactions of the Royal Academy of Sciences*, published each year.

The 1st and 2d sections of chap. 16. 4th part, are extracted from a paper which I read to the Academy, January 1821, *On Respiration and the Influence of the Seasons on the Animal Economy;* and which, being presented to the *concours*, divided the prize for experimental physiology with M. Dutrochet's paper. *On the Growth and Reproduction of Vegetables.*

The 3d section, *On the Exhalation and Absorption of Azote*, Chap. 16, 4th part, was read to the Academy in 1823, and printed in the *Annales de Physique et de Chimie*, and in Magendie's *Journal de Physiologie;* the 4th section, *On the Production of Carbonic Acid in Respiration;* and the 5th section, *A General View of the Changes in the Air in Respiration*—were read to the Academy in the same year. It will be seen from several parts of this work that I did not originally intend here to treat of the changes of the air in respiration, this subject being designed for one of the parts of another work, *On the Influence of the principal Chemical Agents.* For reasons which I need not relate, I have concluded to publish these researches in this place, where they will serve as a supplement to those which precede them.

PART I.

THE BATRACHIAN REPTILES.

CHAP. I.

ON ASPHYXIA.

The action of Air in respiration, is one of the phenomena with the investigation of which physiology was the first engaged; but it has been one of the last to be studied with advantage. The solution of this question depended on another science which, until latter times, did not furnish the requisite light.

When Priestley had discovered oxygen gas, and its property of converting dark into red blood, and when Lavoisier had laid the foundation of the new chemical theory, Goodwin made the application of it to asphyxia, and demonstrated by accurate and skilfully combined experiments that the exclusion of air, by preventing the conversion of dark into red blood, is the cause of the death of animals. Bichat again took up the subject, and has published a treatise on asphyxia, under the title of "Researches on Life and Death." He took a wide view of the subject; and, by a beautiful train of experiments, endeavoured to determine the triple relation of the nervous, respiratory and circulatory systems.

He drew the conclusion, that venous blood penetrating the brain, causes its functions to cease, and that afterwards, the heart ceases to beat from the same cause.

Legallois likewise treated of Asphyxia in his Researches on the Principle of Life, and made it appear that venous blood, acting on the spinal marrow, causes the movements of the heart to be stopped. It is to be observed, that these physiologists made their experiments almost exclusively on warm-blooded animals. The phenomena presented by cold-blooded animals merited particular attention. Spalanzani took them up in his *Researches on the Relation which the Air bears to organized Beings,* a work equally remarkable for the number and the importance of its facts. The alteration which the air undergoes from the organs, capable of modifying it, was the principal object of his enquiry. The relation between the three great functions, on which Bichat and Legallois have so much insisted, but little arrested his attention. At that time physiology had not made the progress which it has done since the labours of that celebrated experimental philosopher and naturalist; and chemistry had not then perfected the process for the examination of gases. One of the philosophers, who has the most essentially contributed to this improvement, has also published a treatise on the respiration of fishes, which leaves nothing to be desired on this point.*

The phenomena presented by cold-blooded animals are so wonderful, that it would seem impossible to bring them together with those exhibited by the other vertebrated animals. It would not be believed, that they are united by a common chain, if the careful investigation of nature did not discover the uniformity of her laws.

* *Memoire sur la Respiration des Poissons,* by Humboldt and Provençal, in the Memoirs of the Society of Arcueil.

SECT. 1.—*Comparative influence of Air and Water upon the nervous and muscular systems.*

Previous to our examination of the phenomena of asphyxia, we shall first enquire whether the media in which it may take place have not a peculiar influence, independent of that which is exerted over the lungs. Of these media, the most important are air and water. The singular power possessed by reptiles of living a considerable time after the excision of the heart, supplies us with the means of appreciating the respective influence of these media. By the removal of the heart the circulation of the blood, and, as a necessary consequence, respiration, are annihilated. A part of the blood escapes; and that portion which remains may be regarded as a constituent part of the organs. The nervous and muscular system alone are left, and these are inseparably connected.

If, after having cut out the hearts of reptiles, taking care to remove, also, the bulb of the aorta, an equal number be placed in air and in water, deprived of air, the difference in the duration of life, if any difference exist under these two circumstances, will indicate the respective influence of these media on the nervous and muscular systems, independently of that which it may exert on circulation and respiration. This experiment was performed on salamanders, frogs, and toads.

I cut out the heart of four salamanders of the species Triton, removing, also, the bulb of the aorta. I exposed two to the air, and immersed the two others in water of the same temperature, which had been deprived of air by boiling. In about four or five hours the salamanders in the water appeared dead; but that life still existed was rendered evident, when they were moved or pinched. One died in eight hours, the other in nine. Those in the air, however, lived from twenty-four to twenty-six hours. These experiments were afterwards repeated with the same precautions upon six other salamanders, and similar results were obtained. Consequently air, in comparison with water, has a superior vivifying influence upon the system of these animals, independently of its action by means of circulation and respiration.

The heart and bulb of the aorta were removed from twelve frogs (R. esculenta and R. temporaria) six of which were placed in water, deprived of air, and six in air. Those in the water lived two hours, and those in the air three. Their activity which continued to be considerable, after the excision of the heart, decreased far more rapidly in the water than in air, and stimulation produced much less effect. The same experiment succeeds equally well upon toads.

If a frog, thus deprived of its heart, and immersed in water, be drawn out and exposed to the air, at the moment when all signs of life have disappeared, it immediately begins to recover. If it be again plunged in water all appearance of life instantly ceases; and it may thus be made, several times alternately, to lose and recover its motion and sensibility. This confirms, in a striking manner, the vivifying effect of air, and the deleterious influence of water on the nervous system.*

* Nasse has likewise shewn by experiment, that water has the effect of destroying the irritability of muscles, and has pointed out an application of this fact to some points of physiology and pathology. This property of water had already been noticed by Humboldt, and also by Pierson. *Note of the Editor.*

Sect. 2.—*Asphyxia in Water.*

In the preceding cases the functions of the nervous and muscular systems alone remained. In asphyxia, there is, in addition to these, the circulation of blood, which has been deprived of the influence of the air. I next attempted to ascertain the comparative duration of life under these two conditions, in order to discover the effect which the circulation of venous blood produces on the nervous system. With this view, frogs, whose hearts had been removed, and an equal number left entire, were placed in vessels containing water deprived of air. The result, in all cases, exhibited a marked difference, sometimes above twenty hours in favour of the latter. Similar results were obtained with toads and salamanders. The removal of the air in the lungs, by pressure or excision of the lungs themselves occasions no difference in the effects. Hence the circulation, even of venous blood, is favourable to the action of the nervous and muscular systems, though incapable of maintaining life beyond a very limited period.

Sect. 3.—*Strangulation.*

It may be presumed, that the water which, from the experiments in sect. 1., was shewn to exert a deleterious influence upon the nervous system, may have prevented the circulation of venous blood from prolonging life so much as it would have done in a less noxious medium. I strangled six frogs, by tying, very tightly, with a packthread round the neck, a piece of bladder fitted very closely to the head, so as to exclude the air. In fact, the ligature was sufficiently tight to effect this of itself. At first the frogs were paralysed, but they afterwards, to a great degree recovered, and lived from one to five days; while the same number in water were dead in ten or twelve hours. The same experiment upon salamanders was attended with similar results. One of these animals lived twelve days, when the head became gangrenous; it afforded me an opportunity of making observations analagous to those of M. Dumeril, in his interesting experiments on a salamander, which survived decapitation a sufficient length of time for the neck to cicatrize. The phænomena in these cases being complicated with serious injury of the the nervous system, belong to a subsequent section. On comparing asphyxia by submersion with strangulation in the air, we see so marked a difference in the duration of life, as to lead to the inference, either that these animals can live for many days without any other action of the air than that which is exerted on the nervous system, or that that fluid acts also upon the blood through the skin.

SECT. 4.—*Cutaneous Respiration.*

Spallanzani concluded, from his investigation, that when the skin of these animals (frogs and other batrachians) is in contact with the air, carbonic acid is produced; but he operated upon batrachians whose lungs had been cut out. In this case the blood from the wound, in contact with the air, must necessarily produce carbonic acid. To obviate this objection M. Chevillot and myself placed frogs, strangled with bladder and a ligature, as in the preceding experiment, in receivers containing atmospheric air. We took them out alive an hour or two after, and having examined the air of the vessel, we found in it a sensible quantity of carbonic acid. Hence it follows, that the length of time which reptiles, in the state of strangulation, can live in air, must in part be referred to the action of that fluid upon the skin.

I defer, for the present, the consideration of the mode in which the carbonic acid was produced.

Sect. 5.—*Animals inclosed in solid Bodies.*

From the preceding facts and observations, it appears that animals asphyxiated under water perish sooner than the mere circulation of venous blood would cause them to do; while the life of those in air is prolonged by the influence of that fluid exerted through the skin. If, therefore, the animals could be incased in a solid material, which should exert no deleterious influence on the nervous system, the influence of the venous blood would be free from both these complications. Numerous instances are recorded of toads having been found in blocks of stone, and other similar situations, in which they must have remained, without extinction of life, for an incalculable length of time. But in these cases there was probably some crevice, forming a communication between the external air and the cavity containing the animal.* In 1777 Hérissant proved to the Academy of Sciences that toads could live eighteen months in boxes inclosed in plaster; but as, even in this experiment, the animals were surrounded by the air in the boxes, it is not absolutely conclusive.

I took, on the 24th of February 1817, five pasteboard boxes of three and a half inches diameter and two deep, and filled each of them with plaster, in which was imbedded a toad; one of them was found alive on the nineteenth day. The others were left for examination after a longer period. Similar experiments were tried upon salamanders and frogs with the like results; but these last do not live so long as the toads and salamanders.

* We are inclined to think that at least in some of these instances such communications must have been altogether impossible.

The foregoing facts appear still more remarkable, on comparing the duration of life of some of those animals exposed to air, with that of others buried in solid bodies. Four frogs were exposed to the air in a dry bottle. At the same time, an equal number were placed in dry sand of the temperature of the atmosphere. I examined them every twenty-four hours. On the third day all those in the air were dead, except one, while all those buried in the sand, with one exception, were perfectly alive.

The life of the animals inclosed in plaster or sand appears to be preserved by the air having still sufficient access to them to exert its vivifying influence through the skin. The permeability of sand is evident. In order to ascertain how far plaster possessed the same property, I took an open tube, five inches long and five or six lines in diameter; closed one extremity with plaster to the extent of about an inch, and took care to cover it outside. I let it dry and again put plaster over it, in order to close the imperceptible openings which might exist in it. When the whole was sufficiently dry, the tube was filled with mercury, and inverted over the same fluid: it was not long before I perceived that the air penetrated and lowered the mercury. This experiment repeated several times had always the same result, which shews that air freely penetrates plaster.

It might, however, have been the case, that the quantity of air which penetrates the plaster was insufficient to support the life of these animals. I therefore inclosed frogs, salamanders, and toads in plaster, as in the preceding experiment, and placed some under water and others under mercury, to intercept the air, and found that they died almost as soon as when the water is in immediate contact with them.

But it remains to be ascertained why the duration of the life of these animals is longer in the sand or plaster than in the air? Frogs and salamanders waste rapidly in the air, and undergo desiccation. In the proportion that they waste, their motions are performed with increasing difficulty; they move, however, until they have lost the quantity of water necessary to their existence.

The pasteboard-boxes containing toads and salamanders, mentioned in p. 13, were opened at intervals of from six weeks to two months and a half from the commencement of those experiments. The animals were all dead, and in a state of complete desiccation. I observed the same of the frogs which had died in the sand. Hence I concluded, that in both cases death arose from the loss of the fluids by perspiration, and I presumed that the perspiration must be less in the plaster than in the air. This was afterwards proved by exposing some frogs to the air in dry vessels, and burying others in dry sand, and afterwards weighing them, at intervals of two, three, four, and five days, I uniformly observed a greater loss in the air than in the sand. Comparative experiments were also made in air and plaster upon toads, and the difference was much more striking than in the sand. Hence the cause of the greater duration of life in sand or plaster than in air, is from the perspiration being more abundant in the air than in the solid substances.

Under an exhausted receiver, in which the effects of rapid evaporation and absence of air are combined, death, as might be expected, takes place very speedily.

Several experiments which I performed, in conjunction with M. Chevillot, on frogs and salamanders, demonstrate this fact.

CHAPTER II.

ON THE INFLUENCE OF TEMPERATURE.

THE facts detailed in the preceding chapter may be modified by various circumstances, which have not yet been considered. One of the most important of these is temperature.

In the months of July and September, 1816. I made forty-two experiments on the submersion of frogs, in glasses containing two-tenths of a litre * of aerated water inverted over saucers.

The mean temperature of the atmosphere was 15.°6 of the centigrade thermometer, or 60° of Fahrenheit in July, in September it was 14.°1 of the cent. or 58° of Fahr. the temperature of the water was from 17° cent. or 63° Fahr. to 15° cent. or 60° Fahr. The mean duration of life, or sensibility to ordinary stimuli was one hour and thirty-seven minutes in July, and one hour and forty-five minutes in September.

At the same time I made the following experiments, in order that the only appreciable difference might be in the temperature. Spallanzani and some other naturalists had already observed, that frogs immersed in water lived longer in winter than in summer, but they had not investigated the subject.

The temperature of the Seine water being 17° cent. or 63° Fahr. I cooled it by means of ice to 10° cent. or 50′ Fahr.,

* A litre is equal to 1·76 pint, new measure.

and found that, of two frogs immersed in it, one lived 5h. 50′ and the other 6h. 15′. When the water was reduced to 0° cent. or 32° Fahr. eight frogs were introduced into it, and they lived from 6h. 7′ to 8h. 18′. When, instead of cooling the water, its temperature was raised to 22° cent. or 72 Fahr., that of the air being 20° cent. or 68° Fahr., the frogs only lived from 1h. 10′ to 35′; when it was raised to 32° cent. or 90° Fahr. they died in from 32′ to 12′; and when it was raised to 42° cent. or 108° Fahr., they scarcely lived a few seconds, and in no instance exceeded two minutes.

Hence we may observe that as the temperature of the water is reduced, the duration of the life of the frogs immersed under it, is prolonged until at 32° Fahr., or 0° of the centigrade thermometer, it is more than tripled. On the other hand the elevation of the temperature produces a corresponding abbreviation of life, till, at 108° of Fahr. or 42° of the centigrade thermometer, death might almost be said to be immediate. It is worthy of remark that the degree of heat at which frogs cannot survive immersion in water, is about the natural temperature of warm-blooded animals.

The temperature about zero appears then the most favourable to the life of frogs plunged in water, but it must not be supposed that the prolongation of their life was occasioned by their becoming torpid. They are certainly less active at that temperature, but they perform the functions of voluntary motion and enjoy the use of their senses. On the other hand, the elevation of temperature is accompanied by a progressive and corresponding diminution in the duration of life, and a proportional increase of agility.

Analogous experiments on toads and salamanders produced similar results.

In warm climates, animals of this class may perhaps continue to live in water at 42° cent. or 108° Fahr., but since they would have full liberty of respiration, this fact, if proved, would not be an objection to the preceding experiments, which have reference to a state of asphyxia. The influence of temperature with freedom of respiration will be examined in the sequel.

In reply to an objection which may be raised, that the speedy death of the frogs might be occasioned by the rapid transition from the temperature of 15° cent. or 60° Fahr. to that of 42° cent. or 108° Fahr., rather than to the elevation of the latter, it may be observed that the transition was equally rapid in the descending scale.

The considerations, to which the preceding researches conduct us, are by no means so simple as might at first be imagined. The influence of climates and seasons, the mode of life of these animals, the action of the air contained in the water, and the relation which it bears to temperature, and lastly the effect of habit are all accessory circumstances whose complicated elements exert their respective influences.

Sect. 1. — *Influence of the Seasons.*

It will be recollected that in the months of July and September, the frogs lived from an hour to 2h. 27′ in aerated water at 15° cent. or 60° Fahr. and at 17° cent. or 63° Fahr. On the 7th November, ten frogs similarly placed in water kept at the temperature of 17° cent. or 63° Fahr. lived from 2h. 5′ to 5h. 35′. All the circumstances being the same in these cases, except the season, it is to this cause that the difference in the results must be referred.

But in what way does the season produce this effect? Is it by means of temperature, or the intensity of light, or the weight of the atmosphere, or its hygrometric or electric states, or its degree of motion or rest? Is any thing to be referred to changes in habits of the animals themselves?

The influence of light and electricity must be left out of the question, until we can appreciate the degree of their intensity. The pressure of the atmosphere which exerts an influence by affecting perspiration may be disregarded, since the difference of mean pressure in the two seasons in which the experiments were made was very inconsiderable.* The same may be said of the influence of the winds and of the hygrometric state of the air, since batrachians, though powerfully affected by these causes whilst living in the air, are wholly removed from their operation when immersed in water. The only circumstance, therefore, left for consideration is the influence of temperature, and as the water in which the animals were immersed was kept at the same degree in both the series of experiments, it is evident that the temperature of the atmosphere prevailing at the time could exert no influence upon the result. The case, however, is different in regard to the temperature, during a certain space of time previous to the experiment. The shallow waters which frogs inhabit, vary in temperature with the atmosphere and more or less approximate to it. The frogs submitted to the July experiments had been for the preceding months under the influence of a mean temperature of 14°.8 cent. or 58°.6 Fahr., and those made use of in September had experienced during August, the effect of a mean temperature of 15°.5 cent. or 60° Fahr. while the frogs subjected to the November experiments had been for the previous month exposed to a temperature of 7°.3 cent. or 45° Fahr. Hence results the remarkable fact, that these animals were able to live in the latter season twice as long

* The effects of variation in the rapidity of perspiration, and of diminished atmospheric pressure in accelerating perspiration, are shewn in the preceding chapter.

as in summer in water at the same temperature. Admitting this to be the uniform result, it necessarily supposes a considerable change in the constitution of these animals, which thus prolongs the duration of their existence in water. To ascertain the uniformity of this principle was the subject of the following experiments.

On the 23d Nov. 1817, the air and water being at 10° cent. or 50° Fahr. and the mean temperature of the month being nearly the same; five frogs were placed in water at the same degree. They lived from 5h. 10′ to 11h. 40′; the latter period being about double the duration of life of these animals in water at the same degree in summer. On the 22d Dec. the thermometer having been about 0° cent. or 32° Fahr. for twenty days, three frogs were put in water at 10° cent. or 50° Fahr.; they lived from twenty to twenty-four hours. On the 23d Dec. the temperature being still 0° cent. or 32° Fahr. four frogs were placed in water at 0° cent. or 32° Fahr. the same apparatus being employed as in the preceding experiments. They lived from twenty-four to sixty hours.

The experiments just related were frequently repeated with the same results, for two successive seasons, and can leave no doubt on the mind respecting, 1st, the influence of the temperature of the water in which the animals were immersed, and 2d, the influence of the temperature of the air for some days previous to the experiment. When these causes are combined the effect is doubled. Hence, in the last-mentioned experiment the animals were placed in circumstances the most favourable to the prolongation of their life under water. The congelation of water fixes the limit of the descending scale. In a greater degree of cold, the conditions are altered, and belong to the question of asphyxia in solid bodies.

Being desirous of ascertaining whether the influence of previous temperature would extend itself to the case of batrachians immersed in water at a high temperature. I placed, on the 30th October, the mean temperature of the month having been 7° cent. or 45° Fahr., six frogs in water, kept at the temperature of 42° cent. or 107° 6′ Fahr. the degree which proves instantly fatal to batrachians in summer. But they lived about the same time as in the analogous experiments made in the summer, viz. from one to two minutes. I tried the same experiment on the 23d December, the temperature of the month having been near 0° cent. or 32° Fahr. and repeated it upon toads and salamanders with the same results.

CHAPTER III.

ON THE INFLUENCE OF THE AIR CONTAINED IN WATER.

In entering on this subject, it will be necessary to direct our attention to the habits of the frog. Spallanzani, from observations made by himself in the neighbourhood of Pavia, states, that the frogs there leave the water in October, and withdraw to the sand, in which they provide themselves with an aperture, called by the frog-catchers, il respiro della rana, the frog's breathing hole. French naturalists, however, assert that frogs make their winter retreat in the water from October till spring; and M. Bosc, who has paid great attention to the habits of these animals, informs us that he has often found them under water during winter. But do they remain constantly in this situation? or do they come occasionally to the surface for the purpose of respiration? This is a question not easily decided by observation, for however narrowly we might watch them, we could scarcely be certain that they had not come to the surface without being observed. Besides, it has appeared, from one of the preceding experiments, that in winter they have lived in water for two days and a half. From direct observation, therefore, we can derive little assistance in our inquiry, whether frogs can during winter, dispense with respiration. The affirmative side of this question is somewhat supported by the fact, that they are sometimes found alive in water which is covered with ice. But this is not decisive, unless it could also be ascertained how many days had elapsed since the formation of the ice, and whether it were free from any aperture. M. Bosc has informed me, that he has seen, in winter, frogs quit the water for several days in succession, at a certain hour, and take breath for a short time on land.

In the numerous experiments which I made in the winters of 1816 and 1817, on the asphyxia of frogs in a limited quantity of aerated water, they have never lived longer than two days and a half, even at the temperatures most likely to prolong their existence during their submersion. Spallanzani, in one instance, found a frog live eight days under water, varying in temperature from half a degree to a degree above zero, cent. and one to two degrees above 32° Fahr.; he adds, that a more elevated temperature would inevitably occasion death in the course of a day. But during the season of the retirement of these animals, the temperature varies considerably.

It may, perhaps, be supposed that frogs remain torpid during their hibernation. Torpor, however, does not exempt animals from the necessity of respiration; but even admitting the contrary, it has been ascertained by the observations of M. Bosc and myself, that frogs, though less active in cold weather, are not torpid even at the temperature of zero, cent. or 32° Fahr.

Let us now proceed to investigate the influence of the air contained in the water. I am acquainted with one experiment only, which has been made in reference to this subject. Spallanzani placed a frog in water, deprived of air, and another in a similar quantity of aerated water. The former was at the point of death in ten hours, but the latter not until twenty had elapsed. This insulated experiment, however, proves nothing, since the difference in the duration of life might have been occasioned by the different constitution of the individuals.

The influence of the air contained in water, on the life of fishes, has been examined with great care by Spallanzani, Sylvester, Humboldt, and Provençal; and their labours have brought to light some most interesting facts, in reference to the natural history of fishes, but the conclusions are applicable to this class of animals only, their gills being especially designed for receiving the influence of the air contained in water.

To be amphibious in the strictest sense of the word, an animal ought to be capable of performing respiration both in the atmosphere and by means of the air contained in water; a double faculty, hitherto ascribed to no adult reptile, except the proteus and the siren. The axolotl, as Cuvier has shewn, has precisely the organization of the larva of the salamander. If these singular animals, which have been united to the family of batrachians, possess, like tadpoles, the faculty of breathing the air of the atmosphere, as well as that of water, they have also, like them, the double apparatus of lugs and gills. But, with these exceptions, the adult batrachians have only lungs, organs exclusively adapted to atmospheric respiration. There is nothing, therefore, which should lead us to conclude a priori, that they are capable of performing both functions.

By the following experiments I have endeavoured to discover how far they are influenced by the air contained in water.

SECT. 1. — *On the Effects of limited quantities of Water.*

Several glasses, holding about five ounces and a half, and filled with water, deprived of air by boiling, and then cooled to the temperature of the atmosphere, were inverted over saucers containing about the same quantity of similar water. An equal number of similar glasses were filled with aerated water at the same temperature. At the same time a frog was introduced into each of these vessels, and the duration of their respective lives carefully noticed. The result was in favour of the frogs placed in the aerated water, but it was not very decisive, showing only that the small quantity of water made use of did not contain so much air as to produce marked and uniform differences. We might safely, therefore, conclude, that though Humboldt and Provençal have shewn that boiling in open vessels is not sufficient entirely to deprive water of the air contained in it, and that the small quantity which remains is capable of exerting a marked influence upon fishes; yet, that in this instance, no sensible influence could have been produced from this cause, in consequence of which I judged it unnecessary to have recourse to the method which they employed for entirely banishing air from the water which they used.

I next endeavoured to render the difference more striking, by increasing the quantity of aerated water.

On the 10th of November, the air being at 11°cent. or 52° Fahr., and the water at 13° cent. or 55° 4′ Fahr. six glasses similar to those used in the preceding experiments, were filled with aerated water, and inverted over the aperture perforated in the shelf of a pneumatic trough, containing ninety-eight pints and a half of Seine water. A frog was placed in each glass. At the same time, the same number of frogs was put into similar glasses of boiled water of the same temperature, and inverted over saucers. The latter lived from three hours and forty minutes to five hours and thirty minutes; while those in the aerated water lived from six hours and forty-three minutes to ten hours and forty minutes. The result of these experiments, though satisfactorily shewing that aerated water has a decided influence in prolonging the life of these animals, is yet far from proving that it is capable of doing so to an indefinite extent.

Sect. 2. — *Stagnant Water renewed at intervals.*

Although the want of organs specially constituted to act in the air contained in water, rendered it improbable that frogs could live in water like fishes, I thought I ought to leave nothing untried, in order to determine the influence of aerated water upon their existence in that liquid.

On the 4th of December, the temperature of the room being 6° cent. or 43° Fahr., a male frog of the species rana temporaria, was secured at the bottom of a glass vessel, holding seventeen pints and a half of Arcueil water, by means of a transverse wire grating. The next day the water was drawn off with a syphon till only a sufficient quantity was left to keep the animal covered, when the vessel was replenished with fresh water. This was repeated daily; the frog not merely survived for eight days, the longest period for which Spallanzani had been able to keep a frog alive, in water at 1° or 0° 5., but continued to live to the 25th of February, that is, for more than two months and a half, during which period the temperature had varied from 0° cent. or 32° Fahr. to 11° cent. or 51° Fahr. An accidental neglect to renew the water occasioned the death of the animal.

This experiment shows the remarkable fact, that frogs are really amphibious, since they can not only breathe the air of the atmosphere, but can also live exclusively by means of the air contained in water.

Tadpoles, which are possessed of gills as well as lungs, can also live in water without coming to the surface; a fact which I proved by an experiment conducted in the same manner as that which has just been detailed. They cannot, however, live on land previous to the full development of their limbs.

Sect. 3.— *Action of aerated water upon the skin.*

Let us now inquire what is the organ through the medium of which the vivifying influence of the air contained in the water is exerted upon these animals. We shall first examine what foundation there may be for supposing that the water enters their lungs, which would, in this case, perform the functions of gills. Inspiration in frogs is performed by a kind of deglutition, and is accompanied by very evident movements of the throat, and of the soft parts under the lower jaw. When the animal is breathing in the atmosphere, their movements are repeated from forty to one hundred times in a minute. If it be plunged in water they immediately cease, and whatever be the length of time during which the submersion is continued, it is very seldom that any movement of deglutition can be observed. In the numerous experiments which I have made upon the asphyxia of frogs in aerated water, I have observed these movements in only a very few instances, and Spallanzani never perceived them. Humboldt observed that the frequency of the inspiration of a frog in a limited quantity of atmospheric air was diminished by the introduction of azote, and that the rarity of inspiration was proportioned to the quantity of azote introduced; but neither azote, hydrogen, nor carbonic acid, has so strong a tendency to suspend inspiration as water.

These experiments might be deemed sufficient to prove that it is not through the medium of the lungs that these animals receive the influence of the air contained in water.

The importance of the fact, however, induced me to make a careful examination of the lungs of frogs, which had been previously immersed in water for a considerable time, and in no instance could I detect any water in them. This is likewise confirmed by the experience of Spallanzani.

The air contained in water, therefore, does not act upon the lungs of the frogs which are immersed in it. Its action must consequently be referred to the skin, the only other organ in contact with the fluid.

The question whether this action on the skin is analogous to that on the gills of fishes, and the investigation of the changes which these organs effect in the air, belong to a subsequent part of this work. It will be sufficient here to state, that during the time in which the life of the animal was maintained by aerated water, the arteries in the webs of the feet evidently contained florid blood.

Sect. 4.—*Running water.*

In the foregoing experiment, the water in which the frog was placed was at rest. Would life have been equally maintained in running water? This query would certainly appear an idle one, had not Spallanzani been led to conclude from his experiments, that the animals died sooner when submerged in running water, than in that which was at rest in vessels kept in his laboratory.

On the 6th of November, a frog in a net to which a weight was attached, was sunk to the bottom of the Seine, at a part where there was about ten feet of water, and was retained in that situation. On the eleventh, the net was drawn up, and the frog being found alive and well, was again similarly sunk. He was afterwards examined on the seventeenth, when he was found equally lively. At the very same season, frogs placed in vessels holding five ounces and a half of water which was left unchanged, survived only a few hours.

Water salamanders as well as frogs may have life supported by the contact of aerated water with the skin. A crested salamander and a green salamander of Latreille, were confined by means of transverse septa of wire at the bottom of vessels, each containing four quarts of Arcueil water changed daily; they lived about two months, and the latter died from neglect to change the water, on the same day that the frog mentioned above, suffered from the same cause. Both the above mentioned species of salamander bear submersion in water, at the temperature of zero, without becoming torpid.

As conclusions deduced from experiments on frogs and water reptiles might not apply to the brown toad, which is altogether a land animal, an individual of this species was put into a net on the 6th of November, 1817, and sunk in the Seine. On the 17th he was still living; but he had made his escape when the net was again examined a month after. At the season of this experiment, brown toads as well as frogs survived only a few hours when confined under the surface of limited qualities of unchanged water.

Sect. 5. —*Limits of this Mode of Life.*

The faculty of living by means of air dissolved in water being shewn to belong to the three genera of batrachians, which were made the subject of the preceding experiments, it is important to know the conditions which influence this mode of life. Are these animals capable of it at all seasons? and what is the influence of temperature? It might be supposed that when frogs quit their water retreats, they are no longer able to live under water, since at this season their constitution undergoes a remarkable change. They are in a state of the most lively excitation, and certain parts of their bodies become visibly altered: for example, the thumb of the male acquires a black colour, and a considerable increase of size. It is the period at which the species is propagated. In order to ascertain whether, at this time, frogs continued to retain the power of living under water, one of these animals was tied by the leg, and secured at the bottom of a vessel, containing forty-nine pints of Arcueil water. He lived twenty days, during which time the water had been changed every twenty-four hours, and its temperature had never exceeded 10° cent. or 50° Fahr. Frogs, then, may live under water for a long time after they are wont to quit it in the spring. Experiment further proved that they possess the same faculty in autumn.

But is this mode of existence subjected to no limits? Is it only necessary to attend to the quality of the aerated water? Does temperature, which is productive of so great an influence when the water is limited in quantity, exercise none when the quantity is unlimited?

The frog before mentioned, immersed in aerated water, which was changed every twenty-four hours, died on the twentieth day, the temperature not being elevated above 10° cent. or 50° Fahr. This was in the spring of 1816.

In October, 1817, a frog lived under water, in an earthen vessel, containing forty-nine pints, for eleven days. During this interval the temperature varied from 9° cent. or 48° Fahr. to 12° cent. or 53° 6′ Fahr., and it was at the latter temperature when the animal died. These experiments induced me to attempt others, in order to determine whether so slight an elevation of temperature could affect the existence of these animals in aerated water, which was frequently changed.

On the 12th of April I put a frog in a tub containing fifty-six litres (seven gallons and a half) of Seine water, at 12° cent. or 53° 6′ Fahr., and kept it at the bottom, by means of a packthread attached to a weight. I found it dead the next day. I repeated the same experiment for several successive days with the same result. The temperature of the water had risen in this interval to 14° cent. or 57° Fahr. I repeated these experiments on toads and salamanders, with the same result.

In these experiments, the animals were kept in vessels containing water which was renewed every twenty-four hours. But would they experience the same fatal effect from this slight elevation of temperature, if they were kept under the water of ponds and rivers, so as not to be allowed to come to the surface to breathe? To solve this question I tried the following experiment:—

On the 12th of April I put seven frogs and two toads into an osier basket, which was immersed in the Seine; the temperature of the river, at the surface, was 12° cent. or 53° 6′ Fahr. On the 20th of the same month I drew them out, and of the seven frogs, four were dead, the two toads were still alive. The temperature continued at 12° cent. or 53° 6′ Fahr. The running water was, therefore, much more favourable to the life of the frogs than the water in the vessels. Could this be attributed to a difference of temperature at the surface and bottom? To decide this, I filled a bottle with water, and corked it, I then sunk it where I had placed the basket, at the depth of five feet and a half. I drew it out twenty-four hours afterwards, and found the temperature of the water which it contained exactly the same as that at the surface. The same experiment, repeated several times in this month, gave the same result.

Of the two toads, one died on the 5th of May, the water at 16° cent. or 61° Fahr., the other on the 19th, the water at 17° cent. or 62° 6′ Fahr. On the 13th of June one of the seven frogs was still living. During this interval of above two months, the temperature varied from 12° cent. or 53° 6′ Fahr. to 22° cent. or 70° Fahr. In the first week more than half the frogs died, between 12° cent. or 53°. 6′ Fahr. to 14° cent. or 57° Fahr.; one only resisted the temperature of 22° cent. or 70° Fahr.

Sect. 6.—*Combined Action of Water, Air, and Temperature.*

In the life of frogs under water, there are then at least three conditions having a powerful influence on their existence:— 1. the presence of air in the water; 2. the quantity, or the change of water; and 3. its temperature.

The relation of these three causes deserves particular notice. We have examined the first with great attention, and have proved that the air in the water could maintain the life of batrachians immersed in that liquid. But how does the temperature act in this case? Since the air is the principal condition for prolonging their existence, one might suppose that the elevation of temperature acts by diminishing the quantity of air.

But Humboldt and Provençal, in their work on the Respiration of Fishes, have proved that the Seine water contained the same quantity of air, in the various analyses which they made of it, from the month of September to that of February. Now, its temperature varies in that interval, at least from 0° cent. or 32° Fahr. to 16° or 17° cent. or 61° or 62° Fahr., which last is higher than that at which the greater number of the frogs above mentioned died. Since it is the temperature, and not the quantity of air which varies, it is to the former that we must attribute the variation in the effects.

The experiments related in the last chapter perfectly accord with those which have been just mentioned. By the former it was shewn, that when frogs are immersed in five ounces and a half of aerated water, the duration of their life is inversely proportional to the elevation of temperature from 0° to 42° cent. or 32° to 107°. 6 Fahr., at which point they die, almost suddenly; and that through the whole range of this scale a small number of degrees is sufficient to produce a great difference in the duration of their life. It has now been shewn, that the air contained in the water has a contrary effect to the elevation of temperature. When they are immersed in about two gallons, changed every day, a temperature between 0° cent. or 32° Fahr., and 10° cent. or 50° Fahr. is not sufficiently high to counterbalance the vivifying effect of the air; but when it rises to 10° or 12° cent. or 50° to 53° 6′ Fahr., the former overcomes the latter, and the animals die, unless the quantity of air is increased. Now the quantity of air may be increased by furnishing, in a given time, a greater quantity of aerated water; this was the cause of some of the frogs in running water resisting the temperature which would be fatal to them in the vessels with water changed only once in twenty-four hours. But the influence of the change of the water is very inconsiderable beyond certain limits; for, as is well known, water contains but a small part of its bulk of air; and according to Humboldt, that of the Seine only $\frac{1}{38}$. These animals, then, have but one means of resisting the effects of temperature, and that is, by coming to the surface to breathe the air of the atmosphere, without which most frogs would die, in a temperature as low as 12° or 14° cent. 53°6. Fahr. or 57° Fahr.

The small quantity of air contained in water under 10° cent. or 50° Fahr., which is sufficient to support the life of batrachians in that liquid, produces an extraordinary effect upon their mode of existence. The extreme activity of frogs is well known, and there is a striking contrast in this respect between them and toads; but keeping them under aerated water destroys this characteristic. It does even more; they become so sluggish in their movements as to resemble tortoises. The slightest noise, which in their state of liberty excites a panic among them, at that time makes no impression. Light, which, on other occasions calls them so easily to the surface, no longer induces them to rise, when the temperature is sufficiently low. They have, however, the faculties of sense and motion; but in air of the same temperature they are extremely lively.

CHAPTER IV.

ON THE VIVIFYING ACTION OF THE ATMOSPHERE.

SECT. 1.—*Influence of Cutaneous Respiration.*

IN order to appreciate the influence of the atmosphere on the skin, it is necessary to suspend the action of the lungs, by intercepting the passage of the air to those organs. As the mouth of these animals, when they breathe, is necessarily shut, in order to introduce the air into the lungs by an act of deglutition, it has been thought that this mode of respiration could be suspended by keeping the mouth open. In order to determine, whether I could avail myself of this circumstance for the object which I had in view, I placed a piece of stick in the mouth of a frog to serve as a gag: it projected a little on each side; and was fastened at its extremities by a thread which passed under the axillæ. I tried this experiment on six frogs, which were placed under a glass, in a saucer; the edges of the glass were slightly raised to allow change of air, and a little water was also introduced into the saucer to supply the animal with the necessary degree of moisture. The temperature was then 24° cent. or 75° Fahr. In this state, five died the following day; the sixth lived seven days. The state of constraint occasioned by the stick which kept the mouth open, and the slight compression of the limb by the thread could certainly not explain this rapidly fatal result. Respiration was evidently checked, but it was not entirely suspended. The movements of deglutition, although less frequent, still took place; the flanks at intervals contracted. These indications of respiration were sufficient to destroy my confidence in the experiment for the accuracy of which, a perfect suspension of the communication between the lungs and the atmosphere was absolutely necessary.

A ligature passed behind the head can be sufficiently tightened to completely intercept the passage of the air. I in this manner applied a ligature to six frogs, and took particular care to use the most rigid compression, and tied the ligature several times, so as altogether to exclude the atmospheric air. The temperature was 12° cent. or 53°.6 Fahr. in the room, and 6° cent. or 43° Fahr. out of doors. I placed the animals on wet sand. They lived a considerable time, one of them for twenty days. These animals would have died in the space of from one to three days, if I had placed them in five ounces and a half of water, as I proved at the same season in this and the preceding years. The influence of the atmosphere on the skin must then have been considerable, in order to obviate the effects of strangulation for so long a time.

It may be here mentioned that the more rapid termination of life in the former experiment in which respiration was only imperfectly suspended, than in the present, is fully explained by the higher temperature to which they were exposed. The important influence of this circumstance has already been shown.

The violent operation, however, inflicted in this last experiment must have tended to shorten life; and consequently to set limits to the beneficial influence of the atmosphere upon their skin. I, therefore, determined upon other more effective means of accomplishing my purpose; this was no less than the absolute removal of the lungs which may be done by a very slight incision, and with the loss of very little blood. I performed this extirpation in the middle of December, 1818, on three frogs of moderate size. They did not appear to suffer much, and presented, after the operation, the same activity as those which had not been touched. I placed them upon moist sand. The temperature of the room was 7° cent. or 45° Fahr., and it rose to 12° cent. or 53°.6′ Fahr. on the 17th Jan. 1819. Two died at this time, having lived thirty-three days, and the third on the 24th, having lived forty.

If we now call to mind the long duration of the life of these animals under aerated water which was continually renewed, and which acts only on the skin, we shall be inclined to query, since air dissolved in water serves so well to maintain their life without the aid of their lungs, ought not they to find still greater resources in the atmosphere itself, if we only furnish them with sufficient moisture? To answer this in the affirmative would be a mere assumption. The comparative influence of the atmosphere and of aerated water is so little understood, that we cannot say why fishes live better in aerated water than in air. Yet the knowledge of this would be of considerable physiological interest.

I wished to determine whether the operation itself, in the preceding experiment, did not tend to shorten life. With this view, on the 4th March 1819, I cut out the lungs of six frogs, and closed the incision by a suture. They were placed in a basket with six other frogs, which had not been mutilated, and immersed in the water of the Seine, which was then at 4° cent. or 39°.6, but in the space of a week, it progressively rose to 9° cent. or 48° Fahr. The greater number of the frogs without lungs died before the others; but at the end of the experiment, one of the frogs deprived of lungs was found alive, with the only survivor of those which possessed them. The season being unfavourable to the life of these animals under water, terminated the experiment on the 15th of March.

These frogs were in every respect similarly circumstanced, with the single exception of the operation; I therefore, felt myself warranted to conclude, from the result of this experiment, that since the greater part of the frogs which had undergone the operation died before those which had not, the operation must have also contributed to terminate the lives of those which were placed in the atmosphere, in the preceding experiment. Hence it may not have shewn the utmost duration of life in the batrachians, maintained by the influence of the air exerted on the skin alone, but for the present we admit the limit which it has given us, and proceed to further considerations respecting the action of the air.

Sect. 2.—*Influence of Pulmonary Respiration.*

We have now to resolve the converse of the question considered in the last section, viz. would these animals live if permitted to breathe by the lungs alone, the atmosphere being altogether excluded from contact with the skin?

A frog was placed in a glass containing five ounces and a half of water. A wooden cover at the surface of the water prevented him from coming out, and an opening which was made in it gave him liberty to breathe the atmospheric air. The liquid, which he dirtied in a few hours, was changed every day. The temperature was 12° cent. or 53° 6′ Fahr. and it was as high as 24° cent. or 75° Fahr. at the latter period of the experiment. This frog lived three months and a half, with no other nourishment than the small quantity of water in which it was immersed. In this situation, the animal has no other direct communication with the atmosphere than by the lungs. Through the medium of the water, he can, it is true, receive the influence of the small quantity of air contained in this liquid; but we have seen in former experiments that where these animals were immersed in the same quantity of aerated water without being allowed to breathe at the surface, this quantity of air did not sensibly prolong their existence. Still, however, there is some room to doubt, whether this small quantity, which under other circumstances, might be safely overlooked, may not be useful in this instance and contribute to aid the action of the lungs.

The application of a coating to the surface of the body, naturally suggests itself as a ready method of cutting off the influence of the air on the skin; but the moisture and continued secretion of the skin, renders it nearly or quite impracticable. The removal of the skin does not get rid of the difficulty, because none of the batrachians long survive this severe operation, which is rather surprising, when we consider how much mutilation they are capable of enduring.

Oil would answer the purpose of excluding the air if it were free from objection in other respects. If it be substituted for water in the glasses with floating covers, as in the preceding experiments, the frogs die in a short space of time. The experiment was tried on ten frogs, six of them lived seven or eight hours, the other four died the following day. The temperature was at 21° cent. or 70° Fahr. as in the experiments with water. It was also found that this substance has a deleterious action on the skin. Some frogs were placed in glasses containing five ounces and a half of oil, and others in the same quantity of water, and not allowed to breathe. Those in the oil made extraordinary movements, and even many attempts at vomiting; however they lived equally long in both liquids. If, in these two cases, instead of suppressing respiration it be left free, the difference becomes considerable. Water, which contains or absorbs a little air, has a tendency the reverse of that of the oil, and pulmonary respiration with this feeble assistance in the one case, and slight obstacle in the other, is found sufficient or insufficient to support life. If then we could confine these animals, in their relation to the atmosphere, to pulmonary respiration alone, they would be as it were, on the limits of life and death.

This consideration induced me to inquire if there were not other animals of the same family, to the support of whose life pulmonary respiration would not be sufficient, notwithstanding the influence of the small quantity of air contained in the water. Tree-frogs are animals of this family; they differ from common frogs and toads in having a little cushion at the end of their toes, which enables them to climb perpendicularly on trees, and even on smooth and flat walls. The species submitted to the experiment is that which is the most common in France. I made use of the same apparatus as in the preceding experiment, with the addition of a small but loose net, fixed over the opening in the floating cover. The frog putting its head under the net, breathed in the atmosphere without being able to escape from the water which surrounded him. Eight of these animals in succession were submitted to this experiment in the space of five days; the temperature varied from 17° cent. or 62° Fahr. to 20° cent. or 68° Fahr.; there was in each glass only about five ounces and a half of water, which was changed several times a day. They did not live, however, beyond three or four days.

Hence it is evident that pulmonary respiration is not sufficient to support the life of tree-frogs without being accompanied by the atmospheric influence upon the skin. The case is the same with the rana obstetricans, on which the experiment was also tried, and we may conclude that the observation applies to all the batrachians.

I put seventeen frogs into a vessel containing seven pints of Seine water permitting them to breathe at the surface; the temperature was the same as in the preceding experiments. Four days after, seven of them died. I repeated this experiment on twenty frogs placed in the same circumstances, adopting the precaution of changing the water every day; nine died in the space of three days; while others, which were placed in glasses with five ounces and a half of water, all lived. The difference depended on the depth of the water. In the glasses, being supported by the bottom, they breathe *ad libitum*, but in vessels containing seven pints, and having a foot in depth, although they may support themselves for some little time at the surface, yet, after having expelled a certain quantity of air from the lungs, their specific gravity being increased, sends them to the bottom, and they rise and sink alternately, till these intermissions of respiration, uncompensated by the action of well aerated water on the skin, puts an end to their existence. We may therefore conclude, that frogs would die in deep waters, if they could not occasionally come to the bank, or find support from time to time on other bodies.

CHAPTER V.

THE INFLUENCE OF THE ATMOSPHERE ON PERSPIRATION.

The first very perceptible change which animals experience when placed in the atmosphere, consists in a diminution of weight, from a vapour which is exhaled from, or a liquid which transudes through their skin, or escapes from the pulmonary surface, and is known under the name of sensible and insensible perspiration. It is this loss of weight that we are now to appreciate, as well as its variations, according to certain circumstances.

SECT. 1.—*Loss by Perspiration in equal and successive Periods.*

We shall first inquire, What is the relative quantity of perspiration in equal and successive periods? Is it variable or uniform? Or, if variable, does it increase or diminish according to any fixed law?

It was very necessary to make this preliminary enquiry, in order to ascertain the rate of the loss by perspiration, influenced only by changes depending on the animal itself, and consequently avoid confounding these variations with those which depend on external agents.

With a view to determine the relation of the losses of weight sustained in equal times, I weighed a frog from hour to hour in air, which appeared calm, the temperature was carefully noted, and remained sensibly the same during the course of the experiment. In comparing the successive losses of weight, a remarkable fluctuation was observed. The variations were very considerable, in some cases amounting to double or triple quantities in equal times; they were usually alternate, without, however, presenting equality in their increments and decrements. Repeated experiments proved that this phenomenon was not confined to an individual case, but appeared even in the different genera of the family which were examined. This irregularity not depending on any error in the mode of experimenting, supposes the action of various influential causes, which do not remain constant in the course of the experiment. This induced me to give a longer period to the duration of the experiments, and in weighing the animals at intervals of two hours, I found a marked tendency to diminution in the quantities lost in equal times. On comparing them afterwards at intervals of three hours, the tendency becomes indubitable; three hours in most cases proved sufficient to render the diminution constant; but in a few instances, intervals of nine hours were necessary to arrive at such a result.

This difference, doubtless, depends on a change in the state of the animal. Now the most remarkable change in its state is the progressive diminution of the mass of its fluids; and in proportion as this is reduced by previous perspiration, ought the subsequent losses from this cause to be less considerable. In observing the degree of rapidity with which the loss by perspiration takes place, it deserves particular notice, that in the intervals of time employed in the experiments just related, the loss in the first period was often great in proportion to that in the subsequent periods, and that in these succeeding intervals its rapidity progressively lessened. Taking the animal at the point of saturation at the commencement of the experiment, it may be said that it loses by perspiration less and less in proportion as it removes from this point. Hence it is obvious, that for a number of experiments to agree in these results, attention must be paid to the condition of the animals in respect of saturation. If we compare the weight of the animals, and the perspiration, without reference to their state as to saturation, we shall obtain not only very different, but even contradictory results. We shall have to return to this subject in the sequel.

Sect. 2.—*Effect of Rest and of Motion in the Air.*

The fluctuations in the amount of loss by perspiration, as observed from hour to hour, did not arise from any circumstance dependent on the life of the animal, nor even on its peculiar organization, since they are found to take place in pieces of charcoal soaked in water, and exposed to the influence of spontaneous evaporation, under the same circumstances, with respect to the atmosphere, as the frogs. We must, therefore, have recourse to external agents, to account for the variation. It is well known that the atmosphere, even when it appears to us perfectly calm, is really sufficiently agitated to exercise a perceptible influence on evaporation. We are, then, naturally led to examine into the extent of that influence on the perspiration of animals. For this purpose I hung some frogs in the draft of an open window, and placed an equal number in the same room at another window which was shut. The animals exposed to the open window lost at least the double, and, according to the intensity of the wind, the triple, and quadruple of what was lost by those which were placed in the interior of the room. It was also found, that on suspending these animals in vessels, with a wide mouth, to allow the perspiration to dissipate itself freely in the atmosphere, the hourly fluctuations either ceased altogether, or were very inconsiderable.

Sect. 3.—*Respiration in Air of extreme Humidity.*

We now come to examine the results arising from the hygrometric state of the atmosphere; and in the first place to consider the question, whether perspiration can take place in air saturated with moisture?

To arrive at the solution of this question, it was of course necessary to remove, as much as possible, the influence of the motion of the air, and all other disturbing causes. With this view the animal was suspended in a glass vessel, inverted over water; which vessel had been ascertained, by experiment, to be sufficiently large to obviate any effect from the alteration of the air by respiration on the duration of its life.

The experiments were often repeated, the intervals of weighing were varied considerably, and a diminution of weight was uniformly observed. It is true, that the chemical changes in the air, occasioned by respiration, would occasion a diminution of weight, in case of this loss not being repaired; but particular experiments on the extent of the respiration of these animals, proved that the slight deduction which this cause requires, leaves a greater loss, which can only be attributed to perspiration. It is true, that these animals have a temperature of their own, though it differs in general, but very little indeed, from that of the bodies which surround them; and this may have a slight influence on perspiration in damp air. But it is the fact rather than its cause which I am here seeking, and we may conclude, that air saturated with moisture does not prevent perspiration, though it reduces it to its minimum, relatively to all the other causes which we have hitherto examined.

Sect. 4.—*Perspiration in dry Air.*

The effects of air as dry as could be procured were afterwards compared with those of air saturated with moisture. Several causes prevented the air of the vessel from attaining the point of extreme dryness; in the first place, the necessity of commencing the experiment on perspiration at the same time with the drying of the air in a close vessel, in order to obviate the passing of the animal through the mercury into a vessel containing air previously dried; which circumstance might occasion such an increase of weight as to destroy the effect of the experiments; add to this, the perspiration of the animal, which, in air perfectly dry, changes the hygrometric state of this fluid.

An hygrometer placed in the vessel with the animal, and a good quantity of quick lime, marked the degree of dryness of the air. On the whole, the effects of calm air progressively dried during the course of the experiment, was very remarkable. In the same space of time, all other circumstances being the same, the perspiration in dry air was from five to ten times greater than in extreme humidity, according to the degree of dryness and the duration of the experiment. If we compare the influence of the hygrometric state of the air with that resulting from its motion, we shall find, that the agitation of the air, provided it is not at the point of extreme moisture, may increase the perspiration, as considerably as a drier air in a state of rest.

Sect. 5.—*Effects of Temperature.*

In order to appreciate the effects of *mere* temperature, it was of course necessary to reduce to a minimum the influence of the two preceding causes. Hence, the experiments made with a view to this object, were performed in a still atmosphere saturated with moisture.

I compared the influence of temperature between 0° and 40° cent. or 32° and 104° Fahr., which are the limits compatible with life, and nearly those of the atmosphere itself. The general tendency of a rise of temperature was to equalise the losses in equal times, or in other words to diminish the decrements in the quantities lost.

As to the relative influence of different degrees of temperature upon the quantity of perspiration itself, it is much less than would have been anticipated. During five hours the quantity perspired at 20° cent. or 68° Fahr., was scarcely twice what it was at 0° cent. or 32° Fahr.; that at 40° cent. or 104° Fahr. is seven times greater than that at 0° cent. or 32° Fahr.; which resembles the effects obtained from a dry and still, compared with a humid atmosphere.

CHAPTER VI.

ABSORPTION AND PERSPIRATION.

The present question is, how is the weight of the body influenced by the contact of water with its external surface? To render this as sensible as possible in the case of frogs, they were first placed in air, until they had undergone evident loss by perspiration, with the expectation, that if they absorbed water this absorption would be more strongly marked, according as they were removed from the point of saturation, which was found to be the fact. These animals, having previously lost a considerable portion of their weight by perspiration, and being afterwards put in water of the same temperature as the air, increased in weight, while the absorption of the fluid was rendered evident, by the sensible diminution of its quantity in the vessel in which the animals were placed.

But to what extent does this absorption take place? what is its rate of progress and what its limit? what ensues when this limit is reached, if the animal be still kept in contact with the water, a condition to which all these animals may be exposed, and of which it is important to know the influence? It results from the experiments which I made, that if perspiration in the atmosphere be not carried too far, water will be absorbed, until the loss incurred thereby shall be repaired. It does not, however, always cease at that point; it may, indeed, go far beyond it, before it arrives at the point of saturation. The quantities absorbed in equal times, like those lost by perspiration in the atmosphere, diminish progressively, provided the temperature is not very high. This diminution is likewise more rapid, according as the animals approach the point of saturation. It appears, also, that the time required to repair by absorption the loss occasioned by perspiration, is shorter than the time during which the same loss is incurred.

A question still remains. When the body has arrived at the point of saturation, does its weight remain stationary, or undergo any further variation? In seeking for the solution of this question, I found that after the body has arrived at its point of saturation, there are alternate stages of diminution and of increase, but the increments do not pass beyond the point of saturation, at which the diminution commences. This circumstance is explained by the fact, that in addition to the aqueous fluid exhaled from the skin, a portion of solid matter is also excreted. The loss occasioned by these excretions are at first compensated by the absorption of the water; but after some time a real and progressive diminution is observed to take place.

It is evident, from what has already been stated, that when one of these animals is placed in water, the weight of his body will increase or diminish according as either of the opposing functions of absorption and transudation predominates over the other. It is interesting to determine what is the influence of temperature upon the relations of these functions. It appeared from experiments, that at 0°. cent. or 32° Fahrenheit, the absorption predominates over the loss of weight; while at 30° cent. or 86° Fahr. the losses are greater than the increase by absorption. It was also observed that elevation of temperature in water had a marked tendency to augment the animal excretions;

from which we seem authorized to conclude that an analogous effect would be produced upon perspiration in the air. On the other hand, the motion of the air, exercising little, if any chemical agency, would have less influence than temperature upon the excretion of animal materials, and consequently contribute more to the production of the aqueous portion of perspirable matter. The effects of dryness and moisture would also seem to have less influence than temperature on the loss of the animal matters.

PART II.

FISHES AND REPTILES.

CHAPTER I.

TADPOLES.

In treating of the family of the Batrachians, the first stage of their lives, during which they have a peculiar form and distinct functions, has been slightly passed over. And since their mode of life at this period, in many respects, resembles that of fishes, I have reserved the examination of it until I should come to treat of this class of cold-blooded animals. The most important peculiarity of tadpoles is not that which depends on their external conformation, the absence of limbs, and the presence of a tail; but that which results from their possessing two kinds of respiratory organs, lungs and gills. Tadpoles unite, in regard to respiration, the functions of reptiles with those of fishes; their use of them varies not only according to their development, but also according to their physical conditions, under the influence of which we are now about to consider them. The tadpole has, in common with the adult animal, the power of supporting life through the medium of the skin, by means of the air contained in water. It has already been shewn that the limits of temperature in which the adult animals are able to exist, are 32° and 50° F. or 0°. and 10° cent., and that beyond the higher limit, the greater part were obliged to have recourse to atmospheric respiration; but tadpoles having an additional organ, by which they are enabled to avail themselves, in a higher degree, of the vivifying influence of the air contained in water, ought, one would imagine, to support, under water, a much greater elevation of temperature, without having recourse to the external air. That this is actually the case, is shewnby experiments, in which they were kept a long time in vessels with the water occasionally changed, and in running water, at the temperature of 25° cent. or 77° Fahr.

The most important point in our enquiries respecting tadpoles, is the influence which physical agents may exert on their transformation. The action of these agents on the form of animals, is one of the most curious questions in physiology. One of the conditions which is best known, is the necessity of aliment for the development of forms. This is the reason that when we wish to hasten the metamorphosis of tadpoles, we take care to mix with the water in which they are kept a small quantity of nutritious substances, and to change the liquid, that the decomposition of these materials may not prove fatal to them. On the other hand, their transformation is retarded, when the supply of nourishment is scanty. Temperature is another condition, the influence of which is generally known. We are aware that tadpoles change in warm seasons: but it is a fact not so generally known, that in our climate a great many are not changed the same year. This happens to those which are produced late in the summer. The subsequent temperature not being sufficiently high, they pass the winter in the state of larva, and do not quit it until the return of warm weather. These are the only influences which have been hitherto ascertained with regard to the developement of these animals. There is another which I have endeavoured to determine, and to which I have been led by my experiments on the adults: it is, the effect which atmospheric, compared with aquatic respiration, exercises on the form of these animals in their earliest age. The difference which these two modes of respiration occasioned in the activity of the adult animal, induced me to conceive, that limiting the tadpole to aquatic respiration would tend to continue its original form. With this view I procured a tin box, divided into twelve compartments, each of which was numbered and pierced with holes, so that the water might readily pass through the box. A tadpole, (which had been previously weighed) was put into each compartment, and the box was then placed in the river Seine, some feet below the surface. A larger numbei was at the same time put into an earthen vessel, taining about four gallons of Seine water, which was hanged every day. These tadpoles were at liberty to rise to the surface and respire air, and they soon went through their metamorphosis. Of the twelve placed in the box under water ten preserved their form, without any progress in their transformation, although some had doubled, and others trebled their weight. It should be observed, that at the time when the experiment was begun, the tadpoles had acquired the size at which the change is about to take place. Two only were transformed, and this very much later than those which, in the earthen vessel, had the liberty of respiration in air. The want of atmospheric respiration appeared here to have a marked influence, but we had not the means of accurately informing ourselves of one very influential circumstance, that is, the supply of nourishment. In the river the water is renewed incessantly; and frequently vegetable and animal substances must necessarily be more abundant in it, than in the water of a vessel which is changed only every twenty-four hours. Notwithstanding this difference in favour of the tadpoles deprived of atmospheric respiration, it had influence upon two only; the other ten underwent no change.

It would appear to result from these facts, that the young animals, with double respiration, would retain their original form under water, if their nutriment were not too abundant, and the temperature were not too high; and that the difference of atmospheric respiration alone, joined to these circumstances, would determine the transformation.

This conclusion, at first, appeared to me to be strictly correct; but there was an element which I had not taken into account; namely, the absence of light: for the tadpoles, which were in the tin box, were deprived of light as well as of atmospheric air. For the present, we will rest satisfied with the conclusion that, under these two privations, tadpoles are retarded in their transformation; but we shall return to the subject in another part of this work, in which the influence of light is considered.

There are three remarkable animals, which have a strong affinity to tadpoles, and have been considered as belonging to the family of batrachians, these are the axolotl, the siren, and the proteus. We are indebted to Cuvier for some valuable investigations respecting the structure of these animals. According to him, the axolotl has the anatomical characters of the larva of the salamander; and the siren and proteus are species of different genera from each other. In the proteus, the lungs, he says, are little more than rudimentary. These animals are all furnished with a double respiratory apparatus, lungs and gills; but the pulmonary organ of the proteus is, as we have said, in an imperfect state. It is possible, that the result of the preceding researches might be applicable to these animals. It would be desirable to ascertain the effect of the united influence of increased nourishment, elevation of temperature, aerial respiration, and the presence of light, on the axolotl, and the siren; and to examine whether the exercise of the lungs, by a frequent use of atmospheric respiration at the surface of the water, would not tend to suppress the branchiæ, as happens to the young batrachians, when temperature and nutrition are favorable. It may be remarked, that the proteus has been always found placed in those conditions as to temperature, darkness, and respiration, in which the branchiæ remain. In fact, it inhabits the subterranean waters of the lakes of Carniola, in which it cannot perform atmospheric respiration, and where the temperature is, perhaps, sufficiently low to preserve the branchiæ. The last point of view under which it remains to examine tadpoles, relates to their existence in air: but this subject being intimately connected with the life of fishes in the air, will be examined when I treat of that class of animals.

CHAPTER II.

FISHES.

The labours of Spallanzani, of Sylvestre, and of Humboldt and Provençal, have made us more accurately acquainted with the physiology of fishes than with that of any other cold-blooded animals. I shall not attempt to detail the result of their researches, most of which are foreign to my present subject. I have only to consider fishes in relation to those points which bear on the phenomena already presented to us by the batrachian reptiles.

I shall first examine the influence of temperature.

Sect. 1.—*Influence of Temperature on the Life of Fishes, in Water deprived of Air.*

To arrive at a correct result, we must firrst reduce the circumstances of the experiment to their greatest simplicity. For this reason I shall commence by enquiring into the effect of temperature on fishes in water deprived of air. Comparative experiments were made on individuals of the same species, and with as close a resemblance as possible, at temperatures varying from 0° cent. or 32° F. to 40° cent. or 104° Fahr. The result was, that at the higher limit death was speedy as with the batrachians, and the duration of life progressively augmented in proportion as the temperature was diminished to the lower limit. It is here seen that the effect of temperature (excluding all other influences) is altogether analogous to what has been observed in the batrachians; that the limits of the shortest and longest duration of life in the batrachians and fishes placed in water deprived of air, are alike in both; and that in the same range of temperature, from 0° cent. or 32° F. to 40° cent. or 104° Fahr., the duration of their life goes on augmenting or diminishing, according as the temperature falls or rises between these extremes.

In regard to the differences which fishes of the same species present at the same degrees of temperature between these limits, size has a marked influence, the smallest as well as the youngest are those which are the least capable of bearing an elevation of temperature. However different may be the duration of the life of small fishes at low temperatures, at 40° cent. or 104 Fahr. it is almost uniform in all. They scarcely ever live more than two minutes; but the larger fishes are able to survive several minutes longer.

Sect. 2.—*Influence of the Temperature of Aerated Water, in limited Quantities, in close Vessels.*

On varying, in a series of experiments, the temperature and quantities of aerated water, it appears,

1st, That the duration of life goes on increasing with an increase of the quantity of aerated water, the temperature remaining the same.

2. That the same result takes place when, the quantity of water remaining the same, we lower the temperature.

3. That the duration of life remains the same, when, within certain limits, we increase or diminish, at the same time, both the temperature, and the quantity of aerated water.

Sect. 3. — *Influence of Temperature, and limited Quantities of Aerated Water, in contact with the Atmosphere.*

Sylvestre has ascertained that a limited quantity of aerated water, in which a fish is placed, absorbs the air in contact with its surface. It evidently follows from this fact, that the life of the animal, in a limited quantity of water, will, *cæteris paribus,* be the longer, the more fully the absorption of air by the water compensates for that which the animal consumes in the water. Add to this, that the fish, when free, is able to derive directly from the atmosphere fresh supplies of air, according to its wants.

Let us now see the influence of temperature under these circumstances. Take for example a bleak (cyprinus alburnus). If we put it into a vessel with a large mouth, containing five ounces and a half of aerated water at 20° cent. or 68° F. in summer, it dies within a few hours: but when the temperature is lowered to 10° or 12° cent., or 50° or 53° F., and is kept at that degree, the animal lives until its secretions are so abundant as to corrupt the water. If, to remedy this inconvenience, we merely renew the water every twenty-four hours, the animal lives in it almost indefinitely.

This is exactly what we have seen to take place with the batrachians. Between 0° cent. or 32 Fahr. and 10° or 12° cent. or 50° or 53° Fahr. they live an indefinite time in aerated water, provided it be renewed sufficiently often; but they die for the most part as soon as the temperature rises above this limit.

Let us now examine the general result of these facts, notwithstanding the different conditions in which the animals are placed. The more the temperature is raised beyond certain limits, the greater is the degree of the influence of the air required for the support of life. This influence, without reference to other causes, will be great in proportion to the quantity of this fluid. Here, however, there are limits depending on the organization of the animal.

Sect. 4. — *Respiration in the Air.*

As yet we have only considered the respiration of fishes in water: their respiration in air deserves particular attention. When a fish, in a given quantity of aerated water, has reduced the proportion of air until its respiration has become difficult, it rises to the surface and takes in air from the atmosphere. In order to show that atmospheric respiration has an influence on the life of fishes, Sylvestre placed a diaphragm at the surface of the water, to prevent the fishes from taking air directly from the atmosphere. He observed that in this case fishes die sooner than when they had access to the atmosphere, which proves that they can breathe air, and that this mode of respiration tends to prolong their life in water.

Sect. 5. — *Life of Fishes in the Air.*

We now proceed to the examination of a new circumstance in respect to the life of these animals; viz. their existencein the atmosphere. This, as regards the influence of physical agents, is the most obscure point in the life of fishes. It is also a condition in which they present phenomena which do not appear in any way to accord with those presented by animals breathing air. When we take a fish out of the water, we see it, according to its species, die in a few minutes, or in a few hours. It is not then surprising that fishes should have been considered incapable of living by atmospheric respiration, and that this should have been attributed to the greater density of the air existing as atmosphere, compared with that contained in solution in water. Air does, undoubtedly, act differently according to its density, on living beings. It is also true that the greater part of vertebrated animals quickly perish from the opposite transition, by passing from the atmosphere into aerated water; but in this case it is evident, that they die because they have not sufficient air; and we might suppose that fishes die in the atmosphere because they have too much. Having already stated the proofs by which Sylvestre has shewn the influence which the respiration of air exerts in prolonging the life of fishes in water, I may proceed with these animals as with others in the examination of the changes which they undergo by exposure to the atmosphere.

A chub, (cyprinus jeses) and a gudgeon, (cyprinus gobio) were first wiped, then weighed, and exposed to the air. Their gills continued to beat until death. The surface of their bodies gradually dried, and at the time of their death they were stiff, and dry. On weighing them again, I found that they had lost by perspiration, the one, one-fifteenth, and the other, one-fourteenth of its weight. This result is nearly the mean of experiments made on other species.

Having in our researches on the batrachians seen the influence of loss by perspiration from exposure to air, we shall now apply it to the case of fishes. To simplify the examination of this subject, let us here consider, as we have done in our researches on the batrachians, the losses by perspiration, as solely at the expence of the water contained in the animal. Capacity of saturation with water implies the quantity of this liquid which an animal is able to contain, between the point of greatest repletion, or saturation, and that of the greatest inanition, compared with the weight of its body. The means of carrying the body to the point of saturation when it is capable of absorbing water, is to place it in that fluid, until the increase in weight has arrived at its maximum. This is exactly the condition in which fishes are found in their natural state, and on removing them from the water in which they live, we may regard them as saturated with this liquid, provided they are in a state to absorb it. Now we shall take for the measure of their capacity of saturation with water, as we have hitherto done in regard to the batrachians, the loss which they experience by perspiration before death; and we see it is sufficient to ensure the death of fishes, that they lose the fourteenth or fifteenth part of their weight. If this loss appear too inconsiderable for us to ascribe the death of these animals to it, let us compare this result with those which we obtained in our researches on the batrachians. They were not given in the preceding chapter, that they might be reserved for this occasion. It has been shewn that the point of saturation with water, in the case of batrachians, depends on the state of their nutrition, and that it may vary within very considerable limits. Now the losses which they undergo by perspiration vary in the same manner. In conditions favourable to nutrition, their capacity of saturation may equal the third of their weight; but, in unfavourable conditions, it is so small, that the least appreciable loss is sufficient to cause death. On applying these results to fishes, whose capacity for water is small compared to that of batrachians, we shall see that the loss which they experience by evaporation is enough to cause their death in air. But the phenomena relative to this subject are not always so simple; they may be very complicated: one might be led to believe that atmospheric respiration would keep fishes alive if we could devise means for obviating their loss of weight by evaporation. With this view, a fish which had been wiped and then weighed, was suspended in a limited quantity of aerated water, so that it had its head and gills above the surface; it died in nine hours and twenty-one minutes. On then weighing it again, it appears that it had not sensibly diminished in weight, but on the contrary had slightly increased. This result would appear to be independent of the cause we have before assigned for the death of fishes, where the whole body is exposed to the action of the atmosphere. But before enquiring into the influence of a new cause which may be added to the first, let us more attentively examine the complicated case in which fishes are found in the circumstances of the experiment last related. The body is plunged in water, but the head and gills are exposed. On one hand absorption takes place in the water, on the other, perspiration in the air. The absorption by the body plunged in water is proved by the slight increase of weight which takes place during the experiment, and the loss by perspiration from the part exposed to the air is demonstrated by the preceding experiments. Now it is evident, that the organ of respiration, which is exposed to the atmosphere, cannot continue its functions unless the losses by perspiration are repaired. It is true the rest of the body absorbs, and that, on the whole, it does not lose any of its weight; but this condition is not sufficient for the continuance of respiration. It is also necessary that the distribution of the fluid absorbed by the trunk, should be such, that the gills and muscles which move them should receive a proportion of it capable of repairing the loss which those organs experience by perspiration. Presuming it possible that this equilibrium might not take place, I made the following experiment to enquire into the relations of partial and simultaneous perspiration and absorption. I placed some fishes in the opposite position to that of the fish employed in the last experiment, that is, with the head and gills in water of the same quality and quantity, and the trunk, suspended in the air by a thread passed through the end of the tail. They lived in this state many days. I weighed them after that interval, and discovered that there was evidently, in this case, a slight increase of weight. But the drying of the surface of the part of the trunk exposed to the air was as marked as in the case where these animals were entirely exposed to the atmosphere, and where they died after a considerable diminution in weight. It is therefore evident that the fluid absorbed by the gills was not distributed to the rest of the body in a proportion sufficient to repair, in all parts of the trunk, the loss which it had sustained by perspiration in air.

The following fact, relative to the physical conditions of fishes in air, is important in the consideration of the principal causes of their death when so placed. Some fishes, when exposed to the air, soon cease to move their gills, although they continue to live pretty long afterwards; but they die much sooner than those of the same species whose gills beat to the last. Suspecting that this difference in the duration of life proceeded from the interception of the air, I remedied it by raising the gills by a small peg placed beneath them. The branchiæ were thus exposed to the air. This change of condition, in relation to the atmosphere, proved sufficient to protract life as long as in those cases in which the respiratory movements were continued spontaneously. The effect of thus raising the gills is so considerable, that if the gills of a fish, out of water, have quickly ceased to beat, we may, by its means, restore, for a while, their spontaneous action, and even do so for several times in succession. We see, therefore, that the life of fishes in the atmosphere, depends on several conditions; of which the principal are, temperature, the capacity of saturation with water, the corresponding loss by perspiration from the trunk and gills, the quickness of this loss, the action of the muscles which move the gills, and the use which they make of their muscles to avail themselves of the action of the air upon the gills. In short, they come under the general law, relative to the influence of the atmosphere on the life of vertebrated animals. As fishes seem to form an exception to this law, I have thought it necessary to shew that they are so only in appearance. What has been here stated relative to the life of fishes in the atmosphere, is equally applicable to tadpoles, placed in the same circumstances. They die from the quantity of water which they lose by perspiration, and although their capacity of saturation is, at least, equal to that of frogs, since it varies between one-third and one-fourth of their weight, yet, as their size is very small, and their perspiration rapid, on account of the delicacy of their skin, they soon lose that proportion of water, and in the experiments which I made, I found that they did not live more than four hours.

CHAPTER III.

LIZARDS, SERPENTS, AND TORTOISES.

The cold-blooded animals which remain to be examined are the families of lizards, serpents, and tortoises; in other words, the saurian, the ophidian, and the chelonian reptiles. The species employed in my experiments were the grey lizard, the ring-adder, and the rat-tailed and the mud tortoises, which served as types of their different families. The external covering of all these cold-blooded animals like that of the batrachians, receives a vivifying influence from the contact of the atmosphere, and thus concurs with the pulmonary respiration to support their existence, as connected with the influence of the air. The isolated influence of pulmonary respiration in lizards, serpents, and tortoises, presents the same differences as in the batrachians, i. e. in summer it is sufficient with some, and insufficient with others for the continuance of life. The families to which pulmonary respiration is in general sufficient are serpents, and tortoises. In lizards, on the contrary, it does not, in summer, suffice to maintain life. The same experiments were made on these animals, as on the tree-frog, and the *rana obstetricans,* and with the same result: but it was much more remarkable, in as much as their skin being scaly, would certainly not induce us to presume that the action of the air on that organ was so necessary for the preservation of their life. If we enquire into the general cause of these differences in the batrachians and other reptiles, we find it in the varied proportions of the lungs. I have proved that among the species submitted to this kind of experiment, those in whom pulmonary respiration is sufficient are the frog, and the brown toad, and that of Rœsel; these are precisely the species in which the lungs are proportionally the largest. Now, as it has been shown by multiplied experiments that pulmonary respiration alone was scarcely sufficient in summer to maintain the life of these animals, and that it required only slight obstructions to occasion their death, it follows that inferiority in the extent of the lungs, in other species, would produce the same effect when they are limited to pulmonary respiration. We see the same circumstance giving rise to the same result in other reptiles. Tortoises and serpents are similarly circumstanced with the frog and common toad: pulmonary respiration alone appears sufficient for them, but lizards die in summer in a few hours if we confine them to pulmonary respiration, and suppress the vivifying action of the atmosphere on the skin. There is a marked difference in the proportionate extent of their lungs, and those of serpents and tortoises. We see then that, as respects the action of the atmosphere, the general results are the same with all cold-blooded animals. The modifications of the vivifying action of the atmosphere on the external surface of the body, all reduce themselves, on taking the phenomena in a general point of view, to the physical conditions of the external covering. The same may be said of the physical agents which we have examined with reference to perspiration. We shall therefore consider the influence of the external covering as respects its porosity and thickness, in relation both to the vivifying influence of the atmosphere, and to perspiration. We have seen that the batrachians can live in solid coverings surrounding them on all sides, provided these coverings are so porous as to admit a sufficient quantity of air. I showed that these animals lived a long time in plaster exposed to the air, notwithstanding the thickness of the covering; but in pursuing these researches, I afterwards discovered that the quantity of air which they receive through plaster, is only under certain circumstances, sufficient for the maintenance of life. It is evident that through these coverings the proportion of air which they receive in a given time, is less than when the skin is exposed. For this reason they cannot live under running aerated water, when enclosed in solid bodies, although they do so very well without such a covering. In the same way, lizards, serpents, and tortoises, in the experiments which I have made on this subject, were unable, on account of the thickness of their natural coverings, to live under running aerated water. The same cause has an equal influence on perspiration. We have seen, in the first chapter, that when the animals are surrounded by a solid covering, they perspire much less than when the bare skin is exposed to the air. In like manner lizards, serpents, and tortoises, on account of the scales with which they are covered, perspire much less than the batrachians. From these differences dependent on the coverings of the body, arises the variety which we observe in the duration of the life of these animals when deprived of nourishment. This diversity depends on the rapidity or slowness of perspiration, as is proved by the numerous and varied experiments which I have made on the duration of the life of batrachians, under different circumstances with respect to perspiration, among which the effect of solid coverings was the most remarkable. The influence of temperature on the duration of life in lizards, serpents, and tortoises, is analogous to that which I have already shown to be the case with batrachians and fishes.

PART III.

WARM-BLOODED ANIMALS.

CHAPTER I.

ON THE HEAT OF YOUNG ANIMALS.

It is a general opinion, inferred from the circulation being more rapid and the nutritive function more active in young animals, that their temperature is likewise more elevated than that of adults. But this opinion not being founded upon direct observation, I turned my attention to it at the commencement of my researches on animal heat. By means of a thermometer placed under the axilla, and the bulb applied so as to be on all sides in contact with the animal, I ascertained the temperature of some new-born puppies whilst in the act of sucking, and found it to be nearly equal to that of the mother, about a degree or two lower; but as this difference is not constant, and is observable among adults also, it may be altogether disregarded. We are therefore warranted in concluding that the temperature of the new-born animal, when placed near its mother, is not superior to that of adults.

But if, at the temperature between 10° and 20° cent. or 50° and 68° Fahr., a new-born puppy be removed and kept an hour or two from its mother, its temperature falls considerably, and continues falling until, in the course of three or four hours, it stops a very few degrees above that of the surrounding air.

This effect cannot be occasioned by the want of food for so short a time; and even though it were, the difference in this respect between young and adult animals would be no less remarkable. But the temperature begins to fall as soon as the separation takes place, and the diminution is not in the least retarded by furnishing the young animal with milk from time to time. The same phenomenon takes place with kittens and rabbits.

It might be supposed that this difference is accountable from the difference in the natural coverings; as rabbits, for example, are born almost naked, and certainly cool more rapidly than puppies and kittens, but, on the other hand: these, although well covered with hair, will cool down to the same degree, though more slowly, so that this circumstance can have but a secondary effect. Besides, the substitution of an artificial covering is found only to retard, not to prevent the lowering of the temperature to the same degree. We must therefore admit that, in the young animal, less heat is produced in a given time than in the adult.

If we examine the change which the temperature undergoes in the process of life, we shall find at first but little alteration; after a while the diminution will take place more slowly; then the limit to its descent will be gradually higher and higher in the scale, till, at the end of about a fortnight, it will maintain itself at a degree nearly equal to that of the adult animal.

This remarkable change which takes place in the young of the mammalia, with respect to their temperature, makes them pass from the state of cold-blooded to that of warm-blooded animals.

The phenomena above mentioned are not, however, common to the young of all the mammalia. The heat of young guinea-pigs, born when the temperature of the air is between 10° and 20° cent. or 50° and 68° Fahr. in the above experiments, will be found to be nearly as great as that of adults, and if they be separated under the same circumstances, it is not diminished. The same is true of many other animals of this class. The young of mammalia appear to be distinguished into two groups in relation to animal heat. Some are born, as it were, cold-blooded; others warm-blooded. Corresponding with this difference, is a distinction deducible from the state of the eyes. Some are born with the eyes closed, others with the eyes open. The temperature of the former, according to the foregoing experiments, rises successively, and at the end of a fortnight, (which is the period when the eyes open), it is nearly equal to that of adults. Thus the state of the eyes, through having no immediate connection with the production of heat, may yet coincide with an internal structure influencing that function, and certainly furnishes signs which serve to indicate a remarkable change in this respect, since at the period of the opening of their eyes, all young mammalia have nearly the same temperature as adults.

Birds are known to have in general a temperature two or three degrees above the mammalia. Wishing to know if this was the case in the early period of life, I procured some young sparrows about a week old. They were well fed and collected in their nest. I took them out one by one and examined their temperature. It was between 35° and 36° cent. or 98° and 100 Fahr. which is sensibly less than that of adults. As their nest sheltered them, and they contributed by mutual contact to keep each other warm, I separated them, and although the air was mild (17° cent. or 62° 6′ Fahr.) they cooled rapidly. In an hour they fell from 36° to 19° cent. or from 100° to 66° Fahr.

Another series of experiments was made when the air was 22° cent. 71° 6′ Fahr. Even at this high temperature sparrows of the same age cooled rapidly to within one degree of the atmospheric temperature. It is true that these birds are hatched without feathers, but feathers are merely a covering; although they may *retain*, they cannot *produce* heat. It is from within alone that animal heat can originate, however outward coverings may contribute to retard its dissipation. Now, if it is true that birds produce heat more than any other warm-blooded animals, the nakedness of their bodies ought not to prevent them from maintaining their temperature, especially when the external air is warm, since man, and other mammalia with bare skins, have this faculty. In a question so important as this, we should not be satisfied with such reasoning, however probable, but endeavour to ascertain the truth by more direct experiments.

I stripped an adult sparrow of its feathers, cutting them so as completely to expose its skin; at the same time I exposed to the air, then at the temperature of 18° cent. or 64° Fahr. young birds of the same species, taken from their nest, where they had a suitable degree of warmth, which the feathers that they had begun to acquire, tended to preserve. Notwithstanding the advantage which this gave them, they cooled down to within one or two degrees of the external air, whilst the adult bird, though quite naked, preserved the temperature which he had before the experiment, being 20° cent. or 36° Fahr., above that of the atmosphere; his internal source of heat, unaided by covering or muscular exertion, being sufficient to counterbalance all his losses.

Not to pass over any thing calculated to throw a doubt on the conclusion which would naturally be drawn from this experiment, viz. that the source of heat was less powerful in the young than in the adult animal, we must take into account the circumstance of the smaller size of the former. It is evident, that a small body, *cæteris paribus,* will cool faster than a large one; but in the case of its producing heat, and of its developing it in sufficient quantity, it will repair its loss, and retain its temperature, whatever be its size. Now this is just the case of adult warm-blooded animals. The greatest difference in their size does not affect their temperature. The wren preserves its warmth as well as the eagle, when the external temperature is not at an extreme point. On the other hand, young hawks, already covered with a thick down, and almost as large as pigeons, in an atmosphere at 17° cent. or 62° 6′ Fahr. suffered a diminution of 14° or 15° cent. or 25° or 26° Fahr. All circumstances then unite in proving that young birds produce less heat than adults. They require, however, a degree of heat nearly equal to that of their parents, and we have seen that not only the mild warmth of spring, but the strong heat of summer is, of itself, insufficient. This want is supplied by the shelter of their nest, their mutual contact, and the assiduous care of their parents, who are employed in imparting to them the warmth of their own bodies during all the time that is not occupied in obtaining food. These subsidiary aids become less necessary with the growth of the young animals. Before they have acquired all their plumage, and when they are still unable to take their food unassisted, they begin to develope sufficient heat to maintain in spring and summer, the degree which characterises warm-blooded animals.

The phenomena, indicated by the foregoing experiments are not, however, common to all young birds. Some, as soon as they are hatched, can maintain an elevated temperature, if exposed to the air in a favourable reason. They come into the world in a more advanced state than other birds. When just hatched they can eat and run, and it is when other birds can perform these functions that they also develope the same degree of heat.

The young birds which are able to run about and preserve their own temperature, are only convered with a tolerably thick down, and not with feathers, which is another proof that the difference in the temperature of young animals and adults does not essentially depend on the covering with which their bodies are provided.

We have seen that the mammalia born with closed eyes, and birds hatched without feathers, produce so little heat as to be, in relation to the air, in the state of cold-blooded animals. We have followed the changes which they undergo as they advance in life, and have pointed out the epoch at which they acquire the power of preserving a high temperature when exposed without shelter to the action of the air. Let us now examine the circumstances in which they enjoy this faculty. They usually come into the world in summer, when the external temperature is favourable to them;—but suppose an alteration in this respect, will young animals preserve their heat as well as adults? To solve this question, I obtained, in spring, a winter-temperature, by immersing vessels in a mixture of salt and ice. They were in all respects alike; and the air in these continued steady, at 4° c. or 39° F. In this cold atmosphere I placed some young magpies, and left them for a short time; in twenty minutes one of them lost 14°c. or 25°F. The others were examined at different intervals, the longest not exceeding seventy minutes; they had cooled 14°c. or 25°F. and 16°c. or 29°F.; this was a considerable and rapid loss of heat, which the animals could scarcely survive. An adult of the same species, placed in the same circumstances, sunk only 3° c. or 5° F., a loss not incompatible with a state of health.

The cause of this difference must be looked for in the animals themselves, the external circumstances being the same in both cases. By lengthening the duration of the cooling process in the adult bird, I more than compensated for the slight advantage which it might derive from its size and feathers. We can scarcely ascribe the inequality in the cooling of these birds to any other cause than a difference in the power of producing heat. The blackbird, the jay, the oriole, and the starling, were exposed to the same artificial cold, at an age at which their temperature had risen and become stationary, with the same result as in the preceding case. The rapid progress which they make in acquiring the power of producing heat is wonderful. But a few days after the preceding experiment, the young birds cooled much less when they were exposed to the same degree of cold, although their appearance was very little, if at all, altered. This influence of age is not confined to birds; I have proved its existence with the mammalia also. Young guinea pigs at birth are able to walk and run, and take the same food as their mother. They do not require to be warmed by her, and appear to possess an equally steady and constant temperature, when the season is not severe; but they have not the same power of maintaining their temperature against cold. These points were proved by means similar to those employed with birds, and it was made equally evident that the difference depended on an inferior power of producing heat.

On the whole, therefore, we are warranted in the general conclusion, that the power of producing heat in warm-blooded animals is at its minimum at birth, and increases successively until adult age.

CHAPTER II.

ON THE HEAT OF ADULT ANIMALS.

Among warm-blooded animals are to be found a small number, which from their undergoing a considerable loss of temperature, accompanied by a state of torpor during the winter months, have been denominated *hibernating animals.* The species which, in our climate, are universally allowed to merit this appellation are, the bat, the hedgehog, the dormouse, the fat dormouse, the garden dormouse, and the marmot. There are other species which some naturalists suppose to be similarly circumstanced: of these I shall speak hereafter. The animals above-mentioned, have all the characteristics of *mammalia.* They belong to various genera and families, and are not distinguished from other animals of the same class by any peculiarity of structure. It is during their hibernation only that they are in any manner to be distinguished from other mammalia. At this period they appear to be converted, for the time, into cold-blooded animals; their temperature is scarcely above that of the surrounding atmosphere; they are torpid like reptiles; their respiratory movements are irregular, feeble, and at long intervals; and, what is very remarkable, this state continues several months, during which they take no nourishment.

We can scarcely conceive two modes of existence more dissimilar than the summer and winter lives of these animals. Does this depend, as some have supposed, on a change of structure, which modifies their mode of being? Or with the same organization at all seasons do they present different phenomena, because their temperament is subservient to changes in the atmosphere?

If we consider that it is only when the temperature sinks in autumn, that these animals retire into the holes in which they are afterwards found, cold and torpid, we may presume that it is the operation of this cause which produces the effect; and after all that we have related above respecting the influence of external temperature on young warm-blooded animals, we shall have little difficulty in believing that it is so. If we suppose that in summer and spring the hibernating mammalia produce less heat than other adult warm-blooded animals, it is a necessary consequence that their temperature should sink with the fall of the year. It might however be supposed that deficiency of nourishment produced these effects; that not being able to procure food in cold weather, their long fast threw them into a state of langour approaching to death, and that their coldness and insensibility, and their interrupted and almost imperceptible respiration were the consequences. Observation and experiment only can decide the question.

These animals have been made the objects of numerous and important researches; but they have been principally examined during hibernation. Spallanzani, Hunter, Mangili, Prunelle, and De Saissy, have ascertained the phenomena which they present during the state of torpor, the means of recalling them to active life, and of again inducing torpidity, as well as several other facts connected with them. Of the numerous remarkable phenomena presented by the hibernating mammalia, those only need arrest our attention which relate to their temperature, since they belong to the subject with which we are now engaged.

They are the more interesting because they seem to influence all the other phenomena. Buffon thought that the temperature proper to the hibernating mammalia was 10° of Reaumur's thermometer, which is equal to 12° 5′ cent. or 54° 5′ Fahr. but the physiologists just mentioned have proved that it is from 35° cent. or 93° Fahr. to 37° cent. or 98° 6′ Fahr. in spring and summer. Here then they do not essentially differ from other adult mammalia; but if we wish to discover whether, notwithstanding this similarity of temperature, they really produce less heat, we must see how they bear the influence of natural or artificial cold.

M. de Saissy has repeatedly examined these animals at different periods during their state of activity. On the sixth of August, the temperature of the air being 22° cent. or 73° Fahr. that of a marmot was 36° 5′ cent. or almost 98° Fahr. in the axilla. On the twenty-third of September, the air was 18° cent. or 64° 5′ Fahr. and the marmot 31° 25′ cent. or 88° Fahr. and on the tenth of November, the air was 7° cent. or 44° 6′ Fahr. and the animal only 27° 25′ cent. or 81° Fahr. which is 9° 25′ cent. or 16° 6′ Fahr. lower than its temperature in the month of August. A garden dormouse examined under the same circumstances had a warmth of 36° 5′ cent. or 88 Fahr. on the third of August; of 31° cent. or 87° Fahr. on the twenty-third of September; and only 21° cent. or 69° 8′ Fahr. on the tenth of November, having lost 15° 5′ cent. or almost 28° Fahr. since the first examination. The examination of a hedgehog gave the following results. It was 37° cent. or 99° Fahr. on the third of August; 35° cent. or 96° Fahr. in September; and only 13° 75′ cent. or 57° Fahr. having lost more than 20° cent. or 40° Fahr.

The author of these observations has not informed us whether these animals were in the constant practice of taking food whilst undergoing this loss of temperature, a circumstance of some importance, to enable us to judge of its cause, and to draw a strict comparison between the hibernating and other mammalia.

M. de Saissy succeeded in torpifying a marmot in the months of May and June. He had inclosed it with a little straw in a copper box, the lid of which was pierced with a hole about half an inch in diameter. After leaving it in an ice-house for twenty-four hours, he exposed it to an artificial cold of 10°c. or 18°F. below the freezing point. It fell into a profound torpor eleven hours after, and although its temperature had fallen from 35°c. or 95°F. to 5°c. or 41°F.; yet its health did not appear more altered than in the ordinary circumstances of hibernation; for, on being afterwards exposed to the warmth of the atmosphere, it recovered from its torpor, and resumed its wonted activity.

This interesting fact shews that hibernating animals may become torpid at any season, and from other causes than the want of nourishment; and also that the phenomena which take place in hibernation do not proceed from any change in the organization of the animal at the end of summer, as some have supposed. We may now compare the hibernating with the other mammalia, with reference to the external temperature during spring and summer, when both are in the enjoyment of the full activity and vigour of life.

In the preceding experiment, the degree and duration of the cold to which the marmot was subjected, were such as might lead us to question whether non-hibernating animals would not have lost as much heat under the same circumstances, enclosed in a box with so small an aperture, that respiration might have been impeded. We may also conclude, from the following observation of the same author, that cold was not the only influential cause:—" The marmot which I reduced to a torpid state at two different times, only became so, I believe, in consequence of its occurring to me, to close the aperture in the lid at a time when its respiration was much enfeebled. It was only in this manner that I succeeded, for all my previous attempts had been vain." We have here, then, a combination of two causes—external cold, and diminished respiration; without being able to distinguish the respective effects of each.

I performed the following experiment, with a view to determine the influence of cold upon an hibernating animal, compared with other warm-blooded animals in similar circumstances. In April, 1819. the air being 16°c. or 61°F. an adult bat, of the long-eared species, recently taken, in good condition, and at the temperature 34°c. or 93°F. was placed in an earthen vessel, which was cooled by a mixture of ice and salt, which surrounded it, till the air within was reduced to 1°c. or 33° 8′ F. It had a cover, which allowed a free communication with the external air. After the animal had been there for an hour, its temperature was reduced to 14°c. or 57°F., being a loss in this short space of time of 20c. or 36°F. Guinea pigs, and adult birds, placed in the same circumstances, lost, at the utmost, no more than two or three degrees, although the influence of the cold was prolonged in this case, to compensate for the difference of size. We see from this, that bats are in the habit of producing less heat than animals which do not hibernate; and it is to this cause that we must chiefly ascribe the reduction of their temperature during the cold season. If we compare this experiment on the bat, with those on young warm-blooded animals, we perceive a remarkable analogy, with this difference, however, that what is a transitory condition in the young of most warm-blooded animals, is permanent in the bat.

We may safely extend the result of this experiment to the whole group of hibernating animals,—and draw this general conclusion respecting them, that whatever causes of a different nature may influence their temperature during hibernation, it is mainly to be attributed to a deficiency in the power of producing heat.

CHAPTER III.

THE INFLUENCE OF THE SEASONS IN THE PRODUCTION OF HEAT.

Age, the influence of which we have just been considering, is certainly not the only cause common to warm-blooded animals which modifies the development of heat. We shall now examine the effect of the seasons in this respect.

From reflecting on the remarkable change produced in the vertebrated cold-blooded animals, by the long-continued action of heat and cold, which has progressively modified their constitution, so that in summer and in winter, though placed, in other respects, in precisely the same circumstances, they have a vitality so different that they would scarcely be known as the same beings, except from observing their form and structure; I was led to presume that the other classes of the vertebrata, though more elevated in the scale of beings, might also experience some constitutional change under the constant action of causes so powerful.

As no inquiries appeared to have been instituted on this subject, I was induced to take it up, and I did so the more willingly, as it is obviously connected with the influence of climates.

I proposed to examine whether, in the opposite seasons of winter and summer, warm-blooded animals, not hibernating, present any difference in respect of their power of producing heat. This was to be ascertained by placing animals of the same species in the same conditions of refrigeration in winter and in summer, and observing if their temperature diminished unequally. In this case it would follow, that their power of producing heat is not the same at these two periods, supposing nothing to be omitted to render the experiments parallel.

It is necessary, in the first place, that the animals selected should be as similar as possible, and that the experiments should be sufficiently numerous to obviate any considerable influence from individual diversities. In order that the mode of refrigeration should be the same, attention must be paid, not only to the temperature, but to the humidity of the atmosphere; for a difference in the hygrometric state of the air would produce a corresponding difference in the evaporation from the lungs and skin, and consequently in the quantity of heat lost.

The apparatus consisted of glass vessels, of the capacity of two pints, placed in a freezing mixture of salt and ice. The air thus cooled, is at its point of saturation with moisture. When it is at zero cent. or 32° Fahr. the animal is introduced, and placed on a false bottom of gauze, to prevent the contact of the cold glass. A lid covered with ice is placed over the vessel, but so as to permit change of air for the free exercise of respiration; and, in order more effectually to secure the purity of the air, a concentrated solution of caustic potass is placed at the bottom, to absorb the carbonic acid, which it readily does, through the gauze. The general results are as follows:—

In the month of February the experiment was made, at the same time, upon five adult sparrows. In the course of an hour they lost, on an average, only 4° cent. or 72° Fahr.—some having lost none, others only 1° cent. or 1° 8′ Fahr. Their temperature then remained stationary, until the end of the experiment, which lasted three hours. In July, I tried the same experiment on four others. Their temperature, in the course of the first hour, sustained an average loss of 3° 62′ cent. or 6° 5′ Fahr.; at the end of the third hour the average reduction from their original temperature was 6° cent. or 10° Fahr. In another series of experiments on six sparrows, in the month of August, the mean loss of temperature at the end of the first hour was 1° 62′ cent. or 2° 9′ Fahr.; and after three hours, 4s 87′ cent. or 8° 76′ Fahr.

These experiments indicate a considerable change effected in the constitution of these warm-blooded animals, by the influence of the seasons; they shew that the continued elevation of temperature diminishes the power of producing heat, and that the opposite state of the atmosphere, provided the cold is not too severe, increases it.

CHAPTER IV.

ON ASPHYXIA.

Having examined the principal points connected with the influence of heat on the animal economy, we shall now pass to the consideration of the air, another physical agent no less important in its relation to life.

The ancients were to a great degree, ignorant of the properties of the atmosphere, and most of them have only been ascertained of late years. Whilst they remained unknown, it was impossible to understand the action of the air upon the animal economy. Already our knowledge of this agent has been advantageously applied: the subject, however, admits of being carried considerably further, but the investigation requires much time.

The earliest knowledge which was acquired respecting the relations of the air with the animal economy, was the most important fact of the necessity of this fluid for the support of life, a necessity so urgent, that man, and the animals which most resemble him in their structure, perish almost as soon as they are deprived of it. Three or four minutes suffice to do away with all appearance of sensation and motion, and though some feeble remains may yet exist in the interior of the body, these are quickly extinguished. The cessation of external sensation and motion is called *apparent death,* and in this case it is scarcely short of *actual death.* So powerfully does the privation of air tend to destroy life, that animals of the largest size, and those possessed of the greatest muscular energy, sink under it almost as quickly as the smallest and the weakest. We might be inclined suppose that those warm-blooded animals, which are constantly obliged to plunge under water in pursuit of prey, or to elude their enemies, would acquire the power of resisting the effects of the privation of air longer than other animals. To ascertain this fact, a moor-hen was plunged in water: at the end of about three minutes, it had neither sense nor motion. Some birds, it is true, perish sooner, and others perhaps, will survive longer; but this fact indicates of what slight avail on this occasion is habit, which, in general has so powerful an influence on the animal economy.

The hope of producing such an alteration in animals as to enable them to support the privation of air, for a much longer term than is natural to them, led Buffon to a very important discovery with respect to young animals. He placed a greyhound bitch of the largest species, when on the point of giving birth to young in a tub of warm water, and secured her in such a manner that she was obliged to bring them forth under water. These were afterwards for the sake of nourishment transferred to a smaller tub of warm milk, but without giving them time to breathe. They remained there for above half an hour, after which they were taken out and all found alive. They began to breathe, which they were allowed to do for half an hour, and were then again plunged in the milk which had been warmed again in the mean time. There they remained for another half hour, and when they were again taken out, two were quite strong, and seemed not to have at all suffered: The third appeared drooping: but was carried to its mother, and soon recovered. The experiment was continued on the other two, they were allowed to breathe a second time for about an hour; and were then plunged once more in the warm milk for half an hour, after which they appeared as strong as before. However, being taken to their mother one of them died the same day, whether by accident or from having suffered from the privation of air could not be ascertained. The other lived as well as the first, and both thrived as well as the other puppies produced after the bitch was removed from the water, and which had not undergone the ordeal.

Le Gallois who does not appear to have been aware of what Buffon had done, made experiments upon rabbits with the same view. He undertook them, for the purpose of ascertaining how long a full grown fœtus could survive without breathing, when separated from its communication with the mother. He found that when he deprived them of respiration by immersing them in water, the mean duration of their life was twenty-eight minutes. But this power of resisting the want of air, rapidly diminishes with the progress of life. Le Gallois observed, that at the end of five days, young rabbits plunged in water, live only sixteen minutes. At the end of another five days, the time is reduced to five minutes and a half, and when they are fifteen days old, they have then reached the limit which adult warm-blooded animals can rarely pass, when they are withdrawn from the influence of the air. The results of these experiments would favour the belief, that the duration of the life of new-born mammalia, under such circumstances, is about half an hour. I was much surprised, however to find that the guinea-pig, at birth, when plunged in water, lives only three or four minutes longer than the adult. I found other species also in which the difference was not greater. I therefore directed my attention to discover the cause of this remarkable difference.

My researches upon cold-blooded animals had enabled me to perceive the great influence of temperature over this mode of existence. Having afterwards found that warm-blooded animals present among themselves marked differences in the production of heat, I concluded that these might give rise to modifications of their system, analogous to those produced by external temperature upon cold-blooded animals.

Let us then compare together the species which have been just mentioned, in this point of view. On the one hand, new-born dogs, cats, and rabbits are similarly affected in asphyxia. They all give signs of life for half an hour, and sometimes longer: now these are the very species in which we have observed so feeble a production of heat. We formerly observed, that in this respect they bore a close resemblance to reptiles and fishes; we see here that they also resemble them in the power of sustaining the privation of the air, which implies an intimate connection between these two phenomena. On the other hand guinea-pigs are in the class of those which produce most heat at birth; and of these I have never seen one which lived above seven minutes under water, and frequently they do not attain even that limit.

We shall be better enabled to perceive the connexion between animal heat and this mode of vitality, by observing their respective modifications in the progress of early life.

We have seen, in the experiments of Le Gallois, that at the end of the fifth day the duration of life during asphyxia is reduced one-half: now this reduction corresponds to a sensible elevation of their temperature. The same is the case after the second interval of five days; the heat is then much increased, and the power of living without respiration is considerably diminished. Lastly, when they have arrived at the fifteenth day, a period when they usually have a temperature nearly equal to that of adults, they scarcely differ from them in the duration of asphyxia. If, instead of passing at once from the first to the fifth day, we examine the young animals in the intervening days, we shall find, that during the first and second, and even, not unfrequently, the third, the duration of asphyxia is only very slightly altered. The production of heat corresponds with this, and both phœnomena likewise concur in the more rapid and striking change that quickly after takes place.

We see that the distinction formerly pointed out between young mammalia, founded in the production of heat, is applicable to them also in respect to the duration of life when deprived of respiration. This duration has its maximum in the group of mammalia which produce the least heat at birth, and its minimum in those which produce the most.

I have made similar researches respecting birds. I have exposed young sparrows separately to the action of the air to compare their rate of cooling. The temperature of the atmosphere was at 16° cent. or 61° Fahr. in the month of May. One of them, which had, in half an hour, cooled from 35° cent. to 19° cent. or 95° to 66° Fahr. was afterwards warmed, and when he had regained his original heat was immersed in water; he lived in it eight minutes; now adults live only a minute and a few seconds. The other, whose temperature was only reduced to 26° cent. or 79° Fahr. was in like manner warmed again and immersed in water; he gave signs of life for only four minutes. Others, which lost little heat by exposure to the air, differed equally little from adults in the duration of life under water.

These facts are sufficient to shew the correspondence between animal heat and the power of living, excluded from the contact of the air; but there is a limit at which this correspondence ceases, and animals soon arrive at it. The increments of heat taking place beyond this limit have no longer any sensible influence upon the duration of life in asphyxia.

Sect. 1.—*Influence of external Temperature.*

In the preceding experiments no notice is taken of the temperature of the water in which the submersion took place. This circumstance has a powerful influence on the cold-blooded *vertebrata*. Is it the same with the warm-blooded?

We shall examine the effects of temperature between 0° and 40° cent., including the range the most nearly related to the animal economy.

I subjected some kittens a day or two old to comparative experiments. In water cooled to 0° cent. or 32° Fahr. they ceased to give signs of sensibility and motion, after four minutes and thirty-three seconds, taking the mean of nine experiments. At a temperature of 10° cent. or 50° Fahr. the duration of life extended to ten minutes and twenty-three seconds. At 20° cent. or 68° Fahr. it increased considerably, being on an average thirty-eight minutes and forty-five seconds; at 30° cent. or 86° Fahr. a retrograde course commenced, as they lived but twenty-nine minutes; lastly at 40° cent. or 104° Fahr. they lived ten minutes and twenty-seven seconds.

It may be remarked, in the first place, that the degree most favourable to life, in the circumstances in which the kittens were placed, may, with reason, be considered as a cold temperature. A bath at this degree, commonly produces upon our bodies a very lively sensation of cold; and is 20° cent. or 36° Fahr. below the mean temperature of warm-blooded animals. The change of the external temperature either above or below this degree, produces deleterious effects, but in different proportions. A rise from 20° cent. or 68° Fahr. to 40° cent. or 104° Fahr. is necessary to produce the same effect as a fall from 20° to 10° cent. or 68° to 50° Fahr.; similar experiments upon puppies, &c. of the same age lead to the same results. This influence of external temperature is not confined to early age, it extends to every period of life; and although the life of adult mammalia (and, yet more, that of birds) is so closely connected with the influence of the atmosphere, that when it is suspended death takes place so soon as to afford but a very limited field for observation, the fact is, notwithstanding, apparent.

There are, then, two principal conditions which influence the life of warm-blooded animals when deprived of air. The one is the quantity of heat developed by the animals themselves, the other is the external temperature to which they are exposed.

CHAPTER V.

ON RESPIRATION IN YOUTH AND ADULT AGE.

After having observed what circumstances modify the duration of life, when animals are deprived of air, let us now examine the influence of this fluid upon the body.

Air, in contact exteriorly with the skin, and interiorly with the lungs, exerts its vivifying influence much more powerfully on the latter than on the former. As soon as air is intercepted from the lungs of warm-blooded animals, although the whole external surface be still exposed to it, they experience the same distress, as if they were entirely plunged in water; and if this interception of the air be continued, asphyxia is produced as quickly as by submersion. If, on the contrary, the contact of air with the skin is prevented, whilst it has free access to the lungs, no inconvenience is found to arise from it.

These facts have necessarily led to the lungs being regarded as the only organ intended to support life by means of contact with the atmosphere. We will, for the present, admit the correctness of this opinion, and shall, in the first place, consider the mutual relation of the lungs and air for the support of life; we shall afterwards examine if the skin does not contribute to the same effect.

The first relation of the air to the lungs, depends on the capacity of these organs, and the extent of surface which they present. The difference in the size of warm-blooded animals, indicates a corresponding difference in the capacity of their lungs. But when we limit the respiration of animals of various sizes to the air contained in the lungs by placing a ligature upon the trachea, the small live almost as long as the large. One might hence infer that the extent of surface of the lungs in animals of the same class is also proportioned to their size. We shall return to this subject.

The air which supports life must be continually renewed. The necessity of this renewal is proved by the accidents which happen when it is suspended. The whole of the air in the lungs is not, however, renewed by an inspiration and expiration; and it is the portion of air which enters and issues from the lungs when these acts are performed, which essentially contributes to the support of life.

Although the will has a slight control over the respiratory motions, yet, on the whole, they may be considered as involuntary; and in a state of health, and in a condition free from the influence of disturbing causes, they have a strong tendency to uniformity.

The capacity of the lungs, as compared to the size of the trunk, is but little affected by age. If it is rather less in youth than in adult age, the succession of respiratory movements is, in general, more rapid, so that, in the same space of time, the quantity of air which comes in contact with the pulmonary surface of a young animal is proportionally equal, if not superior, to that which is inspired by an adult.

From the necessity of the renewal of the air in respiration, it may be inferred that it undergoes some change. In two respects this change is evident. The expired portion has acquired an increase of temperature, and is charged with vapor.

It is, however, an ascertained fact, that the most important alteration of the air during respiration is chemical, and consists principally in the substitution of carbonic acid for a part of its oxygen. When the quantity of air in which a warm-blooded animal respires, is limited, this alteration is progressive, and when it arives at a certain point, the animal dies as it does when altogether deprived of the contact of the air. All warm-blooded animals, when thus confined to a limited quantity of air, alter it to nearly the same degree, and though it still contains a small quantity of oxygen, it is as fatal to them, when placed in it, as submersion in water. The space of time which they live in a given quantity of air, is determined by the rapidity with which they consume the quantity of oxygen, which is susceptible of alteration by respiration. This subject merits particular attention. The difference between individuals in this respect does not wholly depend on the proportionate quantity of air admitted into the lungs, but in part on the constitution of the animals. I placed some sparrows, in every respect as much alike as possible, in vessels of the same form, and containing each a litre, or about a pint and three quarters of air, inverted over mercury. Still further to ensure equality in the condition of the experiments, I performed them simultaneously, thereby avoiding differences in the temperature, pressure and humidity of the air. In a great number of experiments, I ascertained that there was sometimes a considerable difference in the duration of life with the same quantity of air, and that the shortest and the longest duration might differ by one-third. The air, however, was altered nearly to the same degree by all, so that the duration of life was principally affected by the comparative rapidity in the consumption of oxygen.

Since the experiment was complicated by the presence of carbonic acid; to the action of this gas, varying according to the sensibility of the individuals, might perhaps be attributed a difference in the respiratory movements, which would affect the rapidity with which they consumed the air, and thus diminish the respective duration of their lives. The presence of carbonic acid does, it is true, influence the respiratory movements, but the differences above stated, equally existed when I employed means to absorb the carbonic acid as it was formed. We must then admit other causes depending upon individual constitution which affect the rapidity with which the air is consumed.

We shall now proceed to consider the changes in this respect, which the same individual undergoes in the progress of life. In youth, all the functions seem to conspire to promote the development and growth of the body. Digestion is more rapid—the calls of appetite are more frequent and more pressing—respiration is more rapid, and when we consider the necessity of air for the continuance of life, we might be induced to presume, that the relative consumption of air was greatest in youth. Our reasoning may be deceptive; experiment alone can decide the point. In this investigation there are several precautions necessary to arrive at accurate practical results. We must avoid comparing those animals in which age occasions a great difference in size, as the question is to determine the influence derived from the constitution, and not from a difference in bulk. But as the increase of the body and the changes in the constitution are inseparable during a certain period of life, we ought to choose those species whose dimensions vary the least in the progress of life, and in which the other differences are more prominent. The species used in the preceding experiments combines these advantages. To simplify the comparison still more, we must absorb the carbonic acid as it is formed.

The experiment was made with vessels containing a pint and three quarters or sixty-one cubic inches of air, inverted over a solution of caustic potash. The animal was placed on a partition of gauze. The young sparrows which I employed were apparently from eight to ten days old: their bulk was about two cubic inches and a fifth, and that of the adults rather more than twice as much. This difference in size alone would occasion a more rapid consumption of air by the older birds, and it ought not to appear extraordinary that they died sooner. The mean duration of their life was 1 h. 30′ 32″; but the young sparrows prolonged their existence to so disproportionate an extent as to astonish me; their mean term was 14 h. 49′ 40″.

Towards the close of the experiment, when they have altered the air until it is no longer capable of supporting life, we may suppose that they still live as long as they can when deprived of air. We have seen in the former chapter upon asphyxia, that this period is longer in young than in adult animals. But the maximum with sparrows is seven minutes. It is evident, therefore, that in allowing for all these differences, the young sparrows lived much the longest in the same quantity of air, and that consequently their consumption of it is comparatively less.

What is the modification of the animal economy, dependant upon age, which corresponds with this difference in the consumption of air? The most striking difference between the constitution of young animals and that of adults exists, as we have already shewn, in their production of heat. It is in the early steps of life that warm-blooded animals develop least heat. It is also in the early stages of life that the sparrows consumed the air most slowly.

In order to verify this relation under circumstances the most favourable to a satisfactory result, these experiments were continued on the same species at less distant ages.

Their growth is so rapid that they scarcely begin to feed themselves before their size is nearly equal to that of adults. Their temperature then supports itself in the open air. It was at this age that I compared them with adults. Five of these young animals, placed in sixty-one cubic inches of air, and supported by a gauze partition over a strong solution of caustic potass, lived, on an average 2 h. 39′, but adults, in the same circumstances, lived only 1 h. 32′. Here we have no correction to make either for volume, or for difference in the duration of life after the air has become unfit for respiration. These are alike in both these respects, or differ too slightly to deserve attention; but they differ essentially in the production of heat, and we see that the young birds, which produce much less heat, also consume air more slowly than adults.

These researches, following a different method, were extended to mammalia, I placed over mercury, in vessels containing the same quantity of air as in the preceding experiment, puppies a day or two old, and Guinea pigs about a fortnight old. The dogs were taken out at the end of four hours and fifty-nine minutes, and the Guinea pigs after an hour and forty-two minutes. By analysis of the air, the mean of the quantities of oxygen was found to be sensibly the same. Regarding only the difference of bulk, the consumption of air by the puppies ought to have been much more rapid, because they were larger, but when we recollect the fact previously established that young puppies at birth produce much less heat than Guinea pigs, we see here the same relation to exist between the consumption of air, and the production of heat, that we had determined in the case of the sparrows.

These facts regarding the vital heat and respiration of young animals compared with adults, opposed, as they are, to preconceived and general opinion will, on reflection, be seen to be in perfect accordance with the order of nature. Is it not the same relation which the cold-blooded have long been known to bear to the warm-blooded vertebrated animals, and does not the same relation exist between the mammalia and birds?

CHAPTER VI.

ON THE INFLUENCE OF THE SEASONS UPON RESPIRATION.

In the course of the seasons, several changes in the atmosphere occur, which may affect respiration. Such are variations in the temperature, pressure and density of the air.

Let us consider the effects of different degrees of density of the air upon the consumption of this fluid in respiration, independently of other atmospheric changes. We know that we can quickly destroy warm-blooded animals placed under the receiver of an air-pump, by rarefying the air which they breathe. It is true, the rarefaction of the air does not produce one effect only; but whatever other effects it may produce, we can attribute the suddenness of the death only to the fact, that air when dilated is consumed so slowly as not to suffice for the maintenance of life.*

From this condition, which we shall consider as simple, we shall proceed to another which is complicated. When the temperature of the air changes, its density changes also; cold contracts it, heat dilates it. We have noticed the effects of the change of density, but what is the peculiar effect of temperature? We here leave out of view its influence on the motions of respiration and those of the heart, as this exists in extreme cases only. The air being

* See Legallois' Papers on Respiration.

rarer in summer and at the same time warmer, but to a degree which does not affect the motions of the respiratory muscles or of the heart, what is the effect of this elevation of temperature? Does it act conjointly with the rarefaction in diminishing the consumption of the air, or does it counteract such an effect? The respective effects of elevation of temperature and rarefaction of the air do not appear to have been ever examined. Their combined operation has not been altogether neglected. Crawford made guinea-pigs breathe in air of different temperatures, and found by analysis that more carbonic acid was produced in air at the temperature of 8° cent. or 46 Fahr., which may be considered as cold, than in warm air of nearly 35° cent. or 95 Fahr.: but as the colder is likewise the denser air, this latter circumstance may have occasioned the difference noticed by Crawford. Analogous experiments performed by Delaroche, obtained variable and somewhat contradictory results. The subject must therefore be considered as undecided, and I shall not enter upon it here, as it requires the employment of means which I have reserved for another occasion. I have devoted considerable attention to it, and I shall state the facts which I have proved, when I come to speak expressly of the changes effected in the air by respiration.

But another important question remains: Does the change of seasons occasion modifications in the constitution, such, that, supposing the density, temperature, &c. to remain the same, it would be consumed in different proportions at different periods?

The facts which we have already established lead us to form a very probable conjecture as to the result of the inquiry. In following the changes which take place with the progress of age in warm-blooded animals, we have seen, that at different periods from birth to maturity, the consumption of air increases, *cæteris paribus,* with the development of heat. This connexion between respiration and the production of heat, is in accordance with the relation which we have observed that animals of different classes bear to each other, such as the cold-blooded to the warm-blooded *vertebrata,* and the mammalia to birds. We may therefore expect likewise to discover a corresponding relation in cases like that which we are now to examine, in which it may not as yet have been observed.

It has been shewn in one of the preceding chapters, that warm-blooded animals, if in full vigor, and possessed of constitutions adapted to the climate, possess the power of producing heat to a greater degree in winter than in summer. There ought, then, circumstances being in other respects the same, to be a greater consumption of air in winter than in summer, if there really exist an intimate connexion between the two functions.

With a view to determine this question, I made in January, 1819, a series of experiments on six yellow-hammers, (*Emberiza citrinella,*) in separate receivers, each containing 71 cubic inches, or rather more than two pints of air, placed over mercury, with a gauze partition for the bird to rest on. I raised the temperature of the air to 20° and 21° cent. or 68° and 70° Fahr. in order to represent a moderate summer temperature. The average of the duration of their life in this quantity of air was 1h. 2′ 25″. In August and September the same experiments were repeated at the same temperature, upon 13 individuals of the same kind. The average duration of their life was then 1h. 22′.

In January of the same year, the same experiment was performed on four green-finches; (*Loxia chloris.*) They lived on an average 1h. 9′ 15″. In August, for want of opportunity to procure a larger number, the experiment was tried upon only two; one lived 1h. 30′, and the other

1h. 36′. This is mentioned merely, as tending to confirm the preceding; alone, the fact would be altogether insufficient.

The experiments, hitherto, had been made over mercury; consequently, the carbonic acid formed during respiration remained in the receiver, and complicated the experiment. I proposed, in another series of comparative experiments in the two seasons, to suppress this cause of complication, by placing the receivers over a strong solution of caustic potass, capable of absorbing the carbonic acid gas which was produced.

With this modification, I resolved to apply it to a great number of individuals. At the close of December and in January, it was tried upon 16 yellow-hammers. The mean duration of their life was 1h. 7′ 37″. The following summer, at the close of August and beginning of September, the temperature being at 20° cent. or 68 Fahr., and 21° cent. or 70 Fahr.; the same experiment was made upon 12 yellow-hammers;—they lived on an average 1h. 23′ 43″ The concurrent evidence of these varied experiments leaves no doubt as to a change being produced in the constitution of the animals by the influence of the seasons.

I considered that the phenomena presented by animals respiring the same quantity of air, would likewise furnish some data calculated to throw light on the mode in which they consume air in the different seasons.

When respiration is performed in a limited portion of air, that fluid loses its oxygen and receives an accession of carbonic acid. When this last is absorbed as it is formed, the diminution of the oxygen will still occasion the respiration to be oppressed. An unequivocal symptom of this oppressed respiration in birds is the opening of the beak, a symptom which is manifested sooner or later, according to the more or less rapid consumption of the oxygen.

I noted in January, in the case of 10 yellow-hammers, the period at which they began to open their beaks, during an experiment similar to that above described, the carbonic acid being absorbed by a solution of potass. The average period was 52′ 53″ from the commencement of the experiment. Similar observations made at the end of August and beginning of September, upon 12 yellow-hammers, in the same circumstances furnished an average of 1h. 8′ 55″ from the beginning of the experiment. There could not be a more evident confirmation of the conclusion to which the former experiments likewise tended, that the consumption of oxygen is more rapid in winter than in summer.

In order to obviate any objection which would attribute the result to a diminished quantity of air in the winter experiment, it may be mentioned, that the mean pressure was the same in both seasons; but the air with which the vessels were filled was colder previously to raising it to the summer temperature, so that there was really more air used in the winter experiment.

Hence, we may conclude, that the differences in the phenomena of respiration depended on the change in the constitution, effected by the influence of the seasons. Such a conclusion might have been anticipated from the fact proved by former experiments, that the power of producing heat in warm-blooded animals is greater in winter than in summer, and from the evident relation subsisting between the consumption of air and the development of animal heat.

CHAP. VII.

ON PERSPIRATION, OR EXHALATION.

When on the subject of cold-blooded animals, we fully treated the influence of different states of the atmosphere in increasing or diminishing the loss of weight occasioned by perspiration in that class of animals. We shall now resume the same series of researches, with respect to the perspiration of warm-blooded animals.

Sect. 1.—*Loss by Perspiration in equal and successive periods.*

We shall begin by determining what is the proportion between the successive quantities lost in equal times under the same external influence.

Four young guinea-pigs were placed separately in small iron-wire cages. The temperature of the room was 14° cent. 57° Fahr., and the air was kept still, and free from draughts. They had been previously fed, that they might be in the best condition. They were weighed from hour to hour. A plate was placed under the cage to receive their urinary and alvine evacuations. Every excretion was weighed immediately, and the liquid part taken up with silver paper, the weight of which was determined both before and after the operation. The experiment lasted six hours. The quantities lost, compared from hour to hour, were so various, that no tendeney to a regular course could be recognized. But on comparing them at intervals of two hours, the losses decreased successively in some, and a tendency to the same result was manifest in the others; again, in comparing them at intervals of three hours, the diminution of the losses effected in three equal periods were evident in all,

A similar experiment was tried upon another genus of mammalia. Four adult mice were placed in small wire cages, with the same precautions regarding the excretions.

The temperature of the room was 19° cent. or 66° Fahr. The experiment was continued for six hours; it was attended with the same results as that upon the guinea-pigs.

Instead of pursuing these researches upon mammalia, I thought it preferable to take animals in the other class of warm-blooded vertebrata. The more the organization differs in individuals which present the same phenomena, the more certain it is that these phenomena are common to a greater number of species.

These experiments were therefore repeated upon birds. Four sparrows were exposed to the air in a room, at 19° cent. 66° Fahr.; four others to a temperature of 20° cent. 68° Fahr., employing the same methods, and the same precautions, and for the same space of time as in the foregoing experiments, and obtained the most complete confirmation of the former results. It did not seem expedient to prolong the duration of the experiment, lest the powers of the animals should be reduced by too long abstinence.

By stopping short at the irregularities which are observable in successive intervals of a single hour, we might be deceived as to the progress of perspiration, and not discover any certain tendency, and the fact might be united to many other anomalies in vital phenomena in support of the opinion, that they are not susceptible of being reduced to laws. They may, however, be so examined, as that we may perceive in them a greater consistency than was imagined, of which the facts above stated are an instance. Notwithstanding the difference of the species employed in the experiments, they presented analogous results. They were similar, whether presented by mammalia or by birds. The facts being ascertained with animals of both classes, it is needless to multiply examples, and we are warranted in considering them common to warm-blooded animals.

The results, stated generally, are as follows: That the successive losses by perspiration are subject to considerable variations and alterations of increase and diminution, when compared at short intervals, but constantly decrease when considered at longer periods. The periods during which the fluctuations take place in vertebrated animals generally, may be pretty accurately determined. We have always observed, in warm-blooded animals, the alterations to take place with intervals of an hour, and this term may be regarded as a general rule. On examining the whole series of experiments upon vertebrata of different classes, it was observed that the shortest intervals within which the successive diminution took place were those of two hours, and the longest, nine. In taking a mean of six, we may hope to include almost all the cases, for even when a longer space of time was necessary, three hours were sufficient to determine a diminution, if not constant, at least with little variation. In the greater number of cases, it took place in successive intervals of three hours.

The first series of experiments upon the perspiration of warm-blooded animals, having yielded results perfectly conformable to those which were obtained by corresponding researches respecting the cold-blooded, it is probable that we shall discover uniformity in others.

It is almost needless to observe, that with reference to temperature solely, the action of the air similarly influences the perspiration of both warm and cold-blooded animals. The same remark does not hold good with respect to dry and moist air. The testimony of our senses is not adequate to inform us of the effects of these two modifications of the atmosphere. A dry air, by its property of absorbing moisture, may cause perspiration to disappear, and a moist air by the opposite quality allows it to accumulate on the surface of the body. In the first case, it might be imagined, that dry air diminished perspiration, and in the second, that humid air increased it. May not sensibility, which in the higher order of animals is so exquisite, and has so powerful an influence on their secretions, be so affected as to occasion very different results from those which are purely physical? May not dry air produce such a constriction at the surface of the skin, and on the internal surface of the lungs as to diminish perspiration, and may not moist air occasion such a relaxation as to cause the opposite effect?

These considerations are sufficient to shew the uncertainty which must exist on this question, if we have not recourse to direct experiment, and are unwilling to follow the analogy drawn from the facts connected with the cold blooded *vertebrata*. It is surprising that the direct experimental enquiry should never have been made. Delaroche ascertained the comparative effects of dry and moist air upon man at high temperatures, but this condition materially changes the effects of the agents. Other individuals who have examined the subject of perspiration, have made experiments upon it at moderate temperatures, but under such complicated conditions, that the results may be attributable to other causes than those to which they have been assigned.

SECT. 2.—*Influence of the Hygrometric state of the Air.*

In order to compare the effects of dry and humid air, all other conditions such as temperature, pressure, &c. must be equal. To succeed in this most easily, the experiments ought to be performed simultaneously.

As to humid air, the vapour should be transparent, and not that which is termed *vesicular*, the state in which it is exhibited in fogs. The other state of the atmosphere is the most usual, and consequently the most important to be known.

A guinea-pig was placed in a wire cage suspended in a glass vessel, the sides of which had been previously wetted, and which was afterwards placed over water. The vessel contained about 12 litres, or 732·5 cubic inches. It had been previously ascertained that, under such circumstances, the air rapidly arrives at extreme humidity. At the same time, I suspended in a perfectly similar vessel, another guinea-pig of the same litter, and as nearly as possible of the same weight. The vessel was placed over two pounds of quick lime, to absorb the moisture, and tallow was used to intercept the air.* An hygrometer within, indicated the progressive desiccation of the air. The external temperature was 15° cent. or 59° Fahr. Perfect dryness, although easily obtained, at the commencement of the experiment, could not be preserved during its continuance. The animal, if passed through mercury into the vessel might have acquired weight, and when introduced there, its perspiration would furnish moisture, perhaps, faster than it could be

* The vessels or little chambers were not, however, perfectly air-tight, which would have interfered with the respiration of the animals, they were made of panes of glass, so put together as to allow the animals to breathe, yet sufficiently close to secure the requisite hygrometric state.

removed by the means employed to absorb it. By the means which I employed, a high degree of dryness may be obtained, equal to any met with in our climate, except at very great heights.

The necessary conditions of the experiment, prevented us from ascertaining directly the losses occasioned by the alvine and urinary evacuations. We, however, sufficiently fulfilled the condition of equality in this respect, by multiplying the experiments. Let us examine the results, which we shall afterwards submit to another test.

Five guinea-pigs were placed in air of extreme humidity, and five others in air comparatively dry, and whose dryness went on increasing during the whole course of the experiments, which lasted six hours, except on one occasion which occupied eight. In all the cases, the losses in the dry air were much more considerable than in the humid. The influence of individual varieties did not fail to exhibit itself, but it was only in varying the amount of the difference. We see here a confirmation of the powerful action of the hygrometric state of the air, since it prevails over all disturbing causes, originating from varied combinations of organization and sensibility in individuals of the same species.

In order to obviate the objection that the difference in the losses arose from the difference in the weight of the animals, the largest were placed in the moist air, in which, from the experiments on cold-blooded animals, I concluded that the losses would be least. Since, then, the superiority in the weight of the larger animals, though counteracting, did not compensate for the inferiority in the quantity lost, the difference in the weight is only a fuller confirmation of the result of the experiment.

It may likewise be objected, that the diminution of loss in damp air is occasioned, not by a diminution of perspiration, but by that of the alvine and urinary evacuation. But we had occasion to observe, that their excretions were rather increased than diminished by the moisture of the atmosphere.

In order to verify these results in the case of other warm-blooded animals, differing considerably in organization, eight adult sparrows were submitted to similar experiments with the same precautions. This species of animals has the advantage of exhibiting less difference in weight, and other particulars among individuals belonging to it, than many others. The results were similar to those of the preceding experiments.

The numbers representing the amount of loss in analogous cases were very similar, when the experiments were not unduly protracted; but when they lasted above six hours, the want of food and other causes produced a degree of suffering, which, without disturbing the relation between the effects of dry and moist air, may increase perspiration by hurrying the respiration and circulation. The state of respiration has a great influence in this respect.

Hitherto we have considered merely the differences in the quantities lost, but we may approximate to the proportion which they bear to each other. I determined the average weight of the alvine and urinary evacuations of eight individuals of the same species, exposed to the open air of the room for six hours. This I subtracted from the loss sustained by those which were confined in dry and moist air for the same space of time; the remainder furnished the amount of the respective losses by perspiration. Those in the dry air lost 1·04 grammes, or 15·9 grains: those in the moist air only 0·17 grammes, or 2·6 grains. Hence, it follows, that the perspiration was six times greater in the dry than in the humid atmosphere. It is evident, however, that the properties might be rendered much greater, since the air was far from its extreme point of dryness.

These facts appear very simple, they are nevertheless very complicated. They will be explained in the fourth part of this work.

Sect. 3.—*Influence of the Motion and Rest of the Air.*

The effects of rest and motion in the atmosphere, have an intimate connexion with the preceding facts. The evaporation continually taking place from the bodies of animals in an atmosphere not saturated with moisture, creates for them a peculiar atmosphere more humid than the rest of the air. Now, currents of air tend to prevent this effect, by constantly renewing the air by which the bodies are surrounded, and consequently to increase perspiration. Air at rest has the opposite effect. But in the present case, the effect of the motion of the air is not confined to this. The temperature of warm-blooded animals raises that of the fluid in contact with them. In ordinary circumstances, currents furnish supplies of colder air in place of that which is thus warmed, and consequently occasion a fall of temperature, which tends to diminish the perspiration. Now it remains to be ascertained, whether this last effect counterbalances the former.

To ascertain this point, I compared the losses from perspiration in the calmest air which could be procured, and in air moderately agitated. This was effected in the following manner. The apparatus which were employed in the experiments on the hygrometric state of the air, served, at the same time, to prevent, as much as possible the agitation of this fluid. But since during confinement in a close vessel, the air is moistened by perspiration; I compared the losses occasioned by perspiration in a vessel of dried air, with the losses of animals of the same species, in the open air of the room; and I found that not only in mammalia but in birds, the losses were greater in the latter case. Now, in consequence of the greater dryness of the air in the vessels, the perspiration ought to have been the more abundant; but the slight agitation of the outer air was more than sufficient to counterbalance this effect.

In comparing the foregoing results of experiments on the perspiration of warm-blooded animals with the corresponding cases of cold-blooded vertebrata, we observe a striking similarity in the effects produced by the same physical agents. This agreement tends to confirm both; and the confirmation is the greater, as the animals which furnish them, present the greatest differences of structure in the scale of vertebrata.

PART IV.

MAN AND VERTEBRAL ANIMALS.

CHAPTER I.

ON THE MODIFICATIONS OF HEAT IN MAN, FROM BIRTH TO ADULT AGE.

THE results obtained in my experimental inquiries into the influence of physical agents on other warm-blooded animals have been so uniform, that they may, by analogy, be extended to man, although he can scarcely be made the subject of the experiments themselves, and, for this reason, was not mentioned in the preceding part.

Considered in reference to structure, man has been placed in the class Mammalia.

Standing alone in the gift of intellect, he resembles other mammalia, in the effects produced on organization by physical agents.

His life, like theirs, may be endangered by mechanical forces, and his intellect merely enables him to resist those forces by means of others of the same kind. Like them, he suffers from the extremes of heat and cold, and would sink under them, if his intelligence had not discovered the means of setting their destructive influences in opposition to each other. He is not, less than they, subjected to the necessity of the constant contact and renewal of the air, without which, his life would be extinguished as promptly as that of other animals of the same class. He has no privilege from his organization capable of removing him from the power of the physical laws which preside over the formation of vapours, and by virtue of which, a part of the water contained in his body is dissipated in the atmosphere. He has not a sensibility so peculiar, that the function by which the skin excretes upon its surface a part of the fluid, is uninfluenced, as in other mammalia, by the condition of the external temperature.

As a species, man may be affected peculiarly as to degree, and in this respect he presents individual differences, or the same individual may vary at different periods of his life, but as to the manner in which he is affected, he comes within the group studied in the preceding division of this work, and the general truths there established must be equally applicable to him.

We shall first consider how far the power of producing heat, is modified in man by the circumstance of age.

In treating of warm-blooded animals, we formerly observed, that those which are born with their eyes closed, when exposed to the air, in the spring or summer, lose their heat, almost as rapidly as cold-blooded vertebral animals; while those whose eyes are open at birth, preserve, under similar circumstances, a high and constant temperature.

In accordance with analogy, a new-born infant, born at the full time, will have the power of maintaining a pretty uniformly high temperature, during the warm seasons. If, however, birth takes place at the fifth or sixth month, the case is altered; the pupil is in general covered by a membrane denominated *membrana pupillaris*; which character drawn, from the state of the eyes, may be considered equivalent to the closure of the lids. We should, from analogy, conclude that in such a child the power of producing heat would be very feeble.

Let us now proceed to verify these conclusions by observation. An infant born at the full period, and separated from its mother, if exposed to a moderate temperature, scarcely varies in its temperature. It is true that we cannot strip it of its clothes to judge of its power of maintaining warmth under a long exposure to the air, but I have already shown that this trial is unnecessary. Those new-born mammalia which cool in the air almost as cold-blooded animals would do, may in vain be well clothed. They cool, however, the more slowly for it. The new-born infant does not then belong to this group, confirming the conclusion drawn *à priori* from the state of the eyes.

It remains still to be determined whether the human subject at birth produces less heat than afterwards. We shall not, as in the case of inferior animals, expose individuals of different ages to an artificially reduced temperature, in order to ascertain their respective powers of producing heat, but, as substitutes for such experiments, shall employ observations which will furnish satisfactory data. I have not considered slight differences in the temperature of animals as sufficient indication of a corresponding difference in their power of producing heat, and I have, therefore, had recourse, in the researches set forth in the third part, to the plan of artificially reducing the temperature. But, after having ascertained by this method that the young mammalia, which are born with open eyes, produce less heat than adults, we may take advantage of the observations made on their natural temperature. All the animals of this group, which I have examined, have at birth, and for some time after, a temperature inferior to that of their parents. I have observed in this respect a difference of one or two degrees centigrade, or from 1° 8 to 3° 6 of Fahrenheit. This, for our present purpose, is an important index of the difference in the power of producing heat. If a similar difference exists in the human temperature at the two periods of life, we shall not hesitate to regard it as a proof of a difference in calorific power, like that which we have demonstrated with respect to other mammalia.

The temperature of the adult man has frequently been taken, and as it varies in different individuals, it is important to ascertain its limits, and the average. It varies also in different parts of the body. In the mouth it is generally rather higher than in the external parts of the trunk, sometimes to the extent of one degree (of the centigrade thermometer.) To establish, therefore, a comparison between the adult and the new-born infant, the thermometer should, in each, be applied to the same part of the body.

In thus taking the temperature of twenty adults, it was found to vary from 35° 5 to 37° cent. or 96° to 98° Fahr., the mean being 36° 12 cent. or 97° Fahr., which agrees with the best observations. In ten healthy infants, from a few hours to two days old, in the wards of an hospital, under the care of my friend M. Breschet, the limits of variation were from 34° to 35° cent. or 93° to 95° Fahr.; the mean of the whole number was 34° 75 cent. or 94° 55 Fahr. Their temperature is, therefore, inferior to that of adults; a relative difference rendered probable by analogy, and confirmed by observation. I should have laid no stress on so slight a disagreement, if numerous experiments on warm-blooded animals had not proved that this difference in natural temperature coincides with a difference in the power of producing heat, at the different periods of life.

Another analogical conclusion which I wished to verify related to the temperature of infants prematurely born. The facilities for doing so do not often occur, but through the kindness of Dr. Dagneau, who attended a lady who was my patient also, I had an opportunity of ascertaining the temperature of a healthy seven-months' child, within two or three hours after birth. It was well swathed, and near a good fire, but the temperature at the axilla did not exceed 32° cent. or 89° 6 Fahr. Before the period when this child was born, the *membrana pupillaris* usually disappears. If it had been born pretty long before the disappearance of the membrane, there can be no doubt from what has been above stated, that its power of producing heat would be so feeble, that it would scarcely differ from that of mammalia born with their eyes closed.

CHAPTER II.

ON THE INFLUENCE OF COLD ON MORTALITY AT DIFFERENT PERIODS OF LIFE.

The preceding facts being established, let us now proceed to the consequences which flow from them. When the faculty of evolving heat is not the same, the vitality will be different. First, the relation to the external temperature will be changed. The need of warmth and the power of supporting cold cannot be the same where the internal source of heat has not the same activity. There is scarcely any agent which exerts a more powerful influence on life than the temperature of the atmosphere; hence, its relations are amongst those which it is the most important to know. There is, moreover, no agent which we have more in our power to modify and adapt to our necessities. When circumstances prevent our doing so, as when we are exposed to the open air, we have other resources to supply the deficiency. Hitherto our care in this respect has merely been guided by instinct, or by that kind of observation which every body can make. But it requires a more intimate knowledge of our relation to the external temperature, properly to regulate the employment of means, expedient to protect us from the injurious influence of heat and cold.

Let us first examine how far these relations vary according to the modifications dependent on age, as set forth in the preceding chapter. Instinct leads mothers to keep their infants warm. Philosophers by more or less specious reasoning, have, at different times and in different countries, induced them to abandon this guide, by persuading them that external cold would fortify the constitutions of their children, as it does those of adults. We will examine this question by the test of experience, in order to be governed by the observation of nature, rather than by the varying opinions of men.

We shall begin with those young warm-blooded animals, which produce the least heat; viz. mammalia, born with closed eyes, and birds hatched without feathers.

They are, for the greater part of their time, secluded from the effects of external temperature, being warmed in their nests by contact with each other, and more especially with their mother. Under such circumstances, therefore, their heat cannot differ much from that of adults. But if exposed to the air in spring or summer, in the early stages of life, their temperature would not exceed that of the atmosphere by more than a very few degrees. This fact, though not amounting to proof, renders it probable, *à priori,* that the higher temperature occasioned by seclusion and contact with the mother is essential to the support of life.

On 12th February 1819, a kitten, newly littered, removed from its mother, and exposed to the air, at the temperature of 14° cent. or 51° Fahr. being cooled down in nine hours to 18° cent. or 64° 4 Fahr. became stiff, and almost incapable of executing the slightest movements.

The following month the air of the room being 10° cent. or 50° Fahr., I exposed two kittens, of one day old, and having a temperature of 37° cent. 98° 6 Fahr. In 2 h. 25, the temperature of one was reduced to 17° cent. or 62° 6 Fahr. and that of the other to 18° cent. or 64° 4 Fahr. They had become stiff and almost insensible.

In the month of January in the same year, four puppies, littered the day before, were of the temperature of 35° to 36°

cent. or 95° to 97° Fahr. The air of the room was 11° cent. or 52° Fahr. The cooling which they underwent from nine in the morning till ten at night, lowered their temperature to 13° and 14° cent. or 55° 4 and 57° Fahr. They were then so enfeebled that they were almost motionless.

The symptoms of weakness and suffering soon after the young animals are exposed to the air, increases as their temperature sinks. The same circumstances occur with those young birds which produce the least warmth when hatched.

Although the diminution of temperature thus occasioned by exposure to the air, would ultimately prove fatal to these young animals, it is remarkable how long they are capable of enduring a considerable reduction of temperature. New-born puppies or kittens may live two or three days at a temperature of 20° cent. or 68° Fahr. and even 17° or 18° cent. or 62° 6 or 64° 4 Fahr. But the air must not be too cold, or they would soon be deprived of sense and motion, and in a short time this apparent death would become real. When they appeared on the point of expiring, I easily restored animation by placing them before the fire, or by immersion in a bath. These means, if promptly applied, may even prove effectual when they are quite motionless, and, to all appearance, dead.

By the above facts we find that this group of young animals, both birds and mammalia, support a considerable reduction of their temperature, and may even be repeatedly exposed to this trial, provided that they be not left too long in the state of reduction, and that proper care be taken in the restoration of warmth. The exposure is, however, injurious to the animals, and if very often repeated, or too long continued, is fatal.

This facility of recovery, after great reduction of temperature, does not continue in the same degree with the progress of life. I cooled artificially birds of various species, such as jays, magpies, orioles, &c. when they were almost entirely fledged. The temperature of some was reduced to 20° cent. or 68° Fahr., that of others to 18° cents. or 64° 4 Fahr. They had been exposed to the cold but for a short time. They were then very weak and seemed ready to expire. However, they did not fail to recover as rapidly, and apparently as completely as younger birds, but this recovery was temporary; they mostly died in one or two days.

The reduction of the bodily temperature is, then, less injurious, in its permanent effects, in proportion to the youth of the animal. Now, there is here another relation which is deserving of notice; we see that it is according as the power of developing heat increases, that of supporting a reduction of temperature diminishes; and, to be convinced of the intimate connection between these two circumstances, let us compare the adults of different groups of vertebral animals; the cold-blooded animals, the hybernating mammalia, and other warm-blooded animals. In this arrangement they form a scale in which animal heat goes on increasing. Reptiles and fishes, at the bottom of the scale, are, as is well known, those which best support a reduction of temperature; and the hibernating mammalia, inferior, with regard to temperature, to other warm-blooded animals, have, on the other hand, the advantage of being able to survive a reduction of temperature which would destroy the latter.

In the same manner I have shewn that, in young warm-blooded animals, the capability of supporting reductions of temperature is inversely in proportion to their power of producing heat. I shall now point out the necessity for this.

Whatever degree of care parents may take of their young, they cannot always remain with them in order to maintain their temperature at a high degree, if they are of that class of animals which are born with eyes closed, or without feathers. As soon as they leave them to provide subsistence, the temperature of their young begins to be reduced, and if this reduction were as injurious as it is to those animals which produce more heat, the greater part would perish.

Other young warm-blooded animals are not exposed to similar reductions of temperature, because they are born with a more abundant source of heat. But if the external temperature were such that it lowered that of their bodies to the same degree, and as frequently, as with the groups of young animals above-mentioned, a much greater mortality would prevail among them. Hence the danger to which they would be exposed, if born in winter; hence, also, may be perceived the end which nature had in view by generally avoiding their production in that inclement season. This is usually the case with wild animals which are born with the greatest development of heat. However considerable this may be, it would not enable them to support the cold of our climate in the early periods of life; and as they are at the same time stronger, more active, and more independent, their mother could not secure them from the inclemency of the air. They are usually born, therefore, in spring, or at the beginning of summer, during the fine weather. Their power of producing heat gradually increasing, they are more capable of resisting the severity of the succeeding winter.

The following is a general review of the facts relative to the influence of cold, at the different periods of life.

We must distinguish two things — the cooling of the body, and the temperature capable of producing it. The cooling of the body, without regard to its cause, is less injurious in proportion to the youth of the animal.

Lower the temperature of two animals of the same species an equal number of degrees, the younger will suffer less, and will recover more perfectly.

But in order to lower the temperature of animals of different ages, different degrees of external cold will be necessary, being lower, the nearer the animal is to adult age. If, on the one hand, young animals suffer less from the same reduction of warmth, on the other hand they cool more readily. It is on this last circumstance that the mortality in warm-blooded animals, at different periods of life, from birth to adult age, principally depends, so far as it is the result of the influence of external cold.

CHAPTER III.

MOMENTARY APPLICATION OF COLD.

Although animals previously exposed to cold may have regained their temperature, it does not follow that they still retain to the same degree the power of producing heat. If this be the case, on repeating the exposure to cold, about the same time will be sufficient to enable them to recover their temperature. But I have observed, in cooling and warming successively the same individuals, that the time required for the recovery of the original temperature became longer by repetition, which proves that their power of producing heat was thereby diminished. Without that knowledge of the facts which we have unfolded regarding animal heat, we might be tempted to attribute the continuance of the sensation of cold long after the cessation of its cause, merely to the natural duration of all strong sensations. But there is more than the prolongation of a strong impression, more than a simple affection of the nervous system; there is an alteration of function, a diminution in the production of heat.

In a severe winter, in which the Seine was frozen, a young man attempting to cross it, broke the ice and fell into the water, but being strong and active he succeeded in getting out. His health did not suffer, but for three days he had a continual sensation of cold. This case is analogous to those mentioned above. A potent chill acted on the faculty of generating heat, producing a sensible diminution in it, greatly exceeding in duration the length of time in which the cold was applied. And even if the sensible heat were fully restored after such an exposure, its influence would not have been entirely at an end. The calorific function had not recovered all its lost power. We do not know at what interval one may again be exposed to a degree of cold, which might before have been tolerated without inconvenience.

CHAPTER IV.

MOMENTARY APPLICATION OF HEAT.

After an exposure to cold, sufficient to diminish the power of producing heat, continuance in a high temperature tends to the recovery of this power; for, in exposing animals to successive applications of cold, their temperature will fall the more slowly, the longer they shall have been subjected to the influence of warmth. It follows, therefore, that the effect of the application of a certain degree of heat is continued after the cessation of the cause, furnishing the counterpart of what we have stated with respect to the application of cold.

Hence, we see that those who are liable to frequent exposure to severe cold, are rendered more capable of supporting it, by subjecting themselves, in the intervals, to a high temperature; — a practice adopted by northern nations, and justified by the foregoing facts. Attention should be paid to this principle, that the transient application of heat occasions effects which are continued beyond the time of the application, and that it operates whenever the system stands in need of heat. Beyond this point other effects ensue which form the subject of the next chapter.

CHAPTER V.

INFLUENCE OF THE SEASONS IN THE PRODUCTION OF HEAT.

No phenomenon of heat discovered by the thermometer, has excited more astonishment than the uniformity of temperature maintained by man and the higher classes of auimals. — As soon as the formation of vapour was ascertained to be a cooling process, this principle was made use of, to explain the uniformity of animal heat. But although perspiration, by moderating the increase of heat has undoubtedly some influence in maintaining a uniformity of temperature, yet there is another very important element which enters into the solution of the question.

Warm-blooded animals may be divided into two classes, in regard to the influence of the seasons: viz. those whose constitution is perfectly in harmony with the climate, and those whose constitutions are not adapted to it. The first undergo changes corresponding to the season, which allow them the free use of their powers, and that enjoyment of life which constitutes health. According as the temperature falls, their internal source of heat increases. until it attains its maximum in winter, and afterwards declines with the elevation and duration of the external temperature. Here, then, is a new element which should enter into the explanation of the uniformity of animal temperature. A balance is thus maintained between the heat coming from without and that which is developed within, the excess of the one supplies the deficiency of the other.

But the system only acquires this power of accommodating itself to the external temperature with the slow progress of the seasons; at least it is only thus that it is acquired in the highest degree. In summer, a degree of cold which we bear in winter, would take the body as it were by surprise and unprepared. The power of producing heat being then reduced to its minimum, the loss would be insufficiently repaired. In this respect, our states in summer and winter differ in the same manner, though not in the same degree as young animals differ from adults. In the former, the increase of the power of producing heat takes place through the progress of organization whilst under the influence of a mild temperature; in the latter by the influence of cold in degree and duration suited to their constitution.

In the same way, the winter state being acquired, a transient elevation of the external temperature, if it be not excessive, has but little effect on the power of producing heat, which continues to be developed in abundance. To reduce this power, without injury to the health, the heat must be raised slowly, and maintained during a long period.

These changes do not however take place in all animals. There are some whose constitution is not adapted to so great a range of the external temperature. The cold which they can sustain without inconvenience is much less, because they have not the same resources for repairing the loss of heat. When reduced below this limit a fall of temperature produces an effect the reverse of what has been above described; instead of increasing the production of heat, it diminishes it. The type of such constitutions is exhibited in young warm-blooded animals and in hibernating mammalia. They present its operations in the most marked degree, but traces of them, whether found in man or in other warm-blooded animals, though more feeble, are not less truly of the same nature. When we look at the hibernating mammalia, with reference to their peculiarity of becoming torpid, it appears to separate them by an immense interval from other animals of their class, but when we consider them with reference to the function with which we are now occupied, and which seems to be ultimately connected with that peculiarity, we pass by insensible degrees to those mammalia which are in appearance the furthest removed from them. We have shewn that the hibernating mammalia occupy the lowest ranks in the scale of the production of heat amongst adult warm-blooded animals. We have made one group of them without distinction, because they present phœnomena in common — a like diminution in temperature, and a long-continued and profound torpor. — But they do not all exhibit these conditions to the same degree under similar circumstances. The unequal effects which the same degree of external temperature produces on the warmth of their bodies shew that the different species differ considerably in their power of producing heat. Amongst the species which I have mentioned, bats on the one hand, and marmots on the other, may be placed at the two extremes of the scale. Bats cool the most readily, they differ much from the species which immediately follow them, and a considerable interval exists between them and the marmots.

There are some adult species of the class mammalia, which, though not passing the winter in a torpid state, closely resemble hibernating animals, in their feeble power of producing heat. Mice are of this number. On exposing these animals to a moderate cold in winter, I have been surprised at the reduction of their temperature, and this circumstance served to explain to me the use of a very singular habit among them. They make nests at all times, not for their young only, but for themselves. It is known that confined in small cages they do not propagate. I kept in this way some of both sexes and various ages, and observed them, in seasons when I should not have suspected that they required much heat, carefully forming nests like those of birds. I placed near them bits of straw and flocks of cotton, which they pulled through the bars of the cage for this purpose. Thus we see that they seek to preserve the little heat which they develope, and which is necessary to their existence, for, when exposed to the air they often perish from a degree of cold which, to us, would appear moderate. In taking up their residence with man, they have not only the advantage of more abundant food, but also additional facility for guarding against the effects of cold.

This fact leads us to recognise the group of warm-blooded adult non-hibernating animals, comprehending the species the least adapted to increase their calorific power, under the influence of the gradual reduction of the external temperature. Most of those animals which burrow or inhabit caverns, and crevices in rocks or holes, in walls or trees, are of this class. Other causes, doubtless, concur to induce the choice of these retreats, as the necessity of avoiding surprise and of finding a place of refuge, or for a store of provisions against a scarcity. And if they are sometimes chosen only as a magazine, they likewise serve as a protection from the cold, which many of these animals cannot bear with impunity. This is particularly evident with those animals which carefully line their dwellings with materials suited for the retention of warmth.

A similar difference of constitution prevails among men inhabiting the same climate. Some, and these constitute the majority, experience, a salutary effect from the gradual reduction of temperature, not from blunted sensibility, but from increased power of producing warmth. Others, not having the same resources in themselves to counteract the loss of cold which they undergo in winter, are obliged to have recourse to auxiliary means of protection from the effects of winter. There are some who regain their heat with difficulty, even when the cold is moderate, and they require a greater elevation of temperature in their rooms. This class is more numerous than is suspected. It is not confined to chilly persons; for the injurious effect of cold does not always manifest itself by the painful sensation to which we give the same name: it may be indicated by very different sensations; by various states of indisposition, pain and uneasiness, differing from the peculiar sensation generally produced by exposure to cold. The absence of this sensation makes us mistake the cause, and consequently fail in applying the remedy.

We have seen, from the experiments on warm-blooded animals, that the temporary application of cold acts upon constitutions of this kind, by diminishing the power of producing heat, and that this influence extends beyond the period of the cooling process. When, therefore, the exposure to cold is lengthened, the effects of each portion of time are added to those of the succeeding portions. Thus, individuals of this class, by the very duration of the same degree of cold, undergo a progressive diminution in their power of producing heat.

This observation applies to a very remarkable phenomenon presented by hibernating mammalia. Pallas informs us, in his excellent work on some new species of the family of dormice, that, the external temperature remaining the same, the torpor of these animals went on increasing with the duration of the cold. M. de Saissy has made the same observation, which I have also had an opportunity of verifying; but this effect is not unlimited. In like manner, the continuance of the same slight cold increases the power of producing heat in constitutions adapted to the climate, but this increase is necessarily limited.

CHAPTER VI.

ASPHYXIA.

We have already shown, by repeated observations, the intimate connexion which subsists between the power of living for a time with a suspension of respiration and the power of producing heat. Accordingly we divided the young mammalia into two classes. First, Those which produce so little heat that they have, as it were, no temperature of their own. And second, those which produce enough to maintain a high temperature when the air is not too cold. The first live the longest without air; the others for a short time. The external character serving to indicate the class to which any given species ought to be referred, is derived, as has been observed, from the state of the eyes. Now the infant is born with its eyes open, and belongs to that class which produces most heat, from which we may conclude that it will, when deprived of air, live a much shorter time than animals of the first class.

When we speak of the duration of life in asphyxia, it is important to recollect that we only judge by the outward signs which are manifested during the experiment. These signs consist in movements voluntary or involuntary; and when the animal no longer moves spontaneously, we try to excite these motions by pinching it. As soon as this means is ineffectual, we put an end to the experiment. There still, however, exist internal movements; the heart continues to beat, but as we cannot excite visible movements and the animal does not spontaneously perform them, it is then in the state of apparent death.

I shall not here treat of the duration of this state, nor of the conditions which limit it. It is a question connected with an other order of phenomena requiring distinct research, and must be reserved for another occasion.

We shall confine our attention to apparent death induced by submersion in water.

Whether the water be aerated or not, the effects upon mammalia are the same. The phenomena which they present are very different according to the period of the experiment. In the first moments, the motions are varied, repeated, continuous, and evidently voluntary; the animals endeavour to rescue themselves from their painful situation; but soon voluntary motion ceases, and then there is evidently loss of consciousness. Up to this time the mouth remains shut, or is only accidentally opened. But after the animal has lost consciousness, the motions become involuntary; at first suspended for a short interval, they are afterwards performed in an automatic manner, with a certain regularity in their motion and their action. Every part of the body participates; the mouth opens wide, the chest expands, the trunk bends forward, the limbs approach each other, the muscles relax, and the body becomes motionless. These motions are repeated in nearly the same manner till towards the close of the experiment. Then the trunk gradually ceases to bend, the limbs to be drawn together, the chest to be expanded, at least to a perceptible degree, but the mouth continues at intervals to open, though less widely than before, and this motion is the last to cease.

It is remarkable that the voluntary motions are always of short duration, even in individuals which live the longest in asphyxia. Thus puppies, kittens, and rabbits, recently brought forth, although they live in that state for half an hour, commonly lose voluntary motion and consciousness in three or four minutes. The fact has repeatedly arrested my attention.

Buffon was then deceived in his experiments on the submersion of puppies, when he thought that they did not suffer from the suspension of respiration for half an hour. As they were in milk he was unable to see the phenomena which they presented. This celebrated naturalist was deceived by the facility with which the puppies were restored. The fact was, that involuntary movements not having ceased, respiration immediately went on in air; yet one of the puppies which had been three times subjected to the trial died, not indeed immediately after the experiment, but the same day. We must here observe that puppies will exhibit signs of life during more than half an hour's submersion. I have seen them live under water fifty-four minutes, but this case is rare. If left in water until they no longer move, either of themselves or when excited, they would not be restored by exposure to the air; at least I have never seen this take place either with them or any other species of mammalia. I offer this observation only incidentally, for it relates to the possibility of recalling life after apparent death; a subject which I do not propose to treat of here. I shall merely add that man possesses one of the most favourable conditions for restoration by exposure to the air.

From the description which I have given of the phenomena of life during submersion, we may easily judge whether the means proposed by Buffon in the following passage could obtain the end which he had in view in the repetition of the experiment.

"I have not," he says, "pursued these experiments further, but I have seen enough in them to persuade me that respiration is not so absolutely necessary to the new-born animal as to the adult, and that it might perhaps be possible, by proceeding carefully, to prevent, by these means, the closure of the *foramen ovale*, and produce excellent divers; and species of amphibious animals, equally capable of living in air and in water."

Let us suppose that the frequent repetition of submersion, commenced at birth, could preserve to the adult, the same mode of vitality which it possessed in infancy, and by which it was enabled to live a long time without breathing, it must have, to be a good diver, the use of its senses and voluntary motion: now, we have seen that the newly born mammalia generally lose consciousness in three or four minutes, and that they have, in this respect, but little advantage over adults. I have often ascertained, at the swimming schools of Paris, the length of time that the best divers can remain under water, and have found three minutes to be the utmost. There are, indeed, few men who are able to dive for so long a time.

We have seen from the considerable power of producing heat which the infant possesses, that it belongs to that class which at birth are unable long to bear submersion in water.

Warmth, whether produced in the system or derived from without, produces the same effects on the mode of vitality.

There is no physiological character which more eminently distinguishes the cold-blooded from the warm-blooded vertebrata, than the great difference in the duration of their life when deprived of air, but this character depends less on their own nature, than on the circumstances in which they are placed. We have seen that the batrachian reptiles can live two or three days in water deprived of air; but under what circumstances does this happen? This long duration of life depends on two external conditions: 1st, That the water in which they are placed is at 0° cent. or 32° Fahr. or a very little above it; and, 2dly, That the air shall have been, for a long time before the experiment, at nearly the same temperature, in order that the constitutions of the animals may have undergone a modification, dependent on this long duration of cold. (*See the First Part, Chap. II.*) If the same experiment be made in summer, and with water at 20° cent. or 68° Fahr. they live about an hour only, varying more or less, according to the degree of preceding warmth. We may see from this that they scarcely differ from some of the new-born warm-blooded animals, such as puppies, which, as I have shewn above, may live fifty-four minutes in water of the same temperature. If instead of this degree, we raise the water to 40° cent. or 104° Fahr., the mean temperature of warm-blooded animals, batrachian reptiles plunged in it will not live longer than the adult mammalia. The same is the case with fishes, especially with those of small species. With others the difference amounts merely to a few minutes. Grey lizards, in the same circumstances, lived about six minutes. Hence we see that heat, whether internally or externally produced, has the same influence on the duration of life in asphyxia.

With reference to the influence of cold, it is evident, that in experiments upon warm-blooded animals we cannot obtain results equally decisive with those derived from the cold-blooded vertebrata. In the first place, the mammalia and birds cannot, whatever may be their age, endure so great a reduction of temperature as reptiles and fishes; and secondly, at an equal reduction of temperature, warm-blooded animals cannot, in general, remain the same length of time. From these considerations we should be led to conclude, that the hibernating mammalia which are susceptible of a considerable reduction of temperature, and are also capable of living a long time in this state under the influence of a cold air, would bear a strong resemblance to reptiles and fishes in the power of living a long time without the contact of air. It is easy to foresee the probable duration of their life when they are deprived of air by submersion in summer. Let us recollect that in this season they have a high temperature like other mammalia, and that they have been subjected to the influence of this temperature for the whole course of the preceding fine weather. They will, therefore, be in the condition most conformable to the duration of life in asphyxia.

I asphyxiated bats in water at 20° cent. or 68° Fahr., at a period when they were not torpid ; they lived only four or five minutes. Let us now change the conditions of the experiment; let some hybernating animal have undergone the greatest reduction of temperature of which it is capable ; let it have lived a long time in this state, under the influence of cold air; it is easy to foresee that, participating then in the winter mode of vitality of the cold-blooded vertebrata, it will present analogous phenomena in the duration of life, when deprived of air. Spallanzani furnishes us with some interesting facts, shewing the correctness of this conclusion. He placed in a receiver containing carbonic acid, at the temperature of 12° cent. or 53° 6′ Fahr., a marmot completely torpid. It shewed no sign of uneasiness during the whole course of the experiment. Spallanzani took it out at the end of four hours, without its having appeared to suffer from the ordeal, and certainly it would have lived longer if left in the gas. We may remark, in addition, that this gas is very deleterious, that it acts not merely by depriving the animal of the contact of atmospheric air, but also by a property tending to extinguish life.

The facts just mentioned prove that temperature has a similar influence upon all the vertebrata, in prolonging or shortening the duration of life in asphyxia. The range of temperature at which these observations were made lies between 0° and 40° cent. or 32° and 104° Fahr. At the higher limit there is a remarkably slight difference in the times that the animals can live without breathing. It is at the lower degrees only that the differences become more decided, according to the species, and increase in proportion as we approach the inferior limit.

Although the general influence of temperature as above stated, may be considered as demonstrated, it is by no means pretended that, amongst the complicated phenomena of life, other causes do not concur to modify the duration of life under privation of air.

When an animal is cut off from communication with the air, as in submersion, the circulation continues, but the blood loses its arterial quality, and becomes venous. Does the circulation of this venous blood contribute to the support of life? In the first chapter of this work, this question has been decided in the affirmative, with regard to reptiles. Is it also the case in warm-blooded animals? I made, with kittens, experiments similar to those related in the first chapter upon reptiles, and found that the kittens, whose circulation was prevented by cutting out the heart, when plunged in water, lived in general but a quarter of an hour; whilst others, whose circulating system was left entire, gave in water, at the same temperature, signs of life for about half an hour.* In experiments upon adult warm-blooded animals, the influence of the circulation of venous blood cannot easily be seen, on account of the rapidity with which the privation of air causes apparent death, and renders it

* Some experiments made on young rabbits by Le Gallois tend to the same conclusion.

useless to endeavour to determine minute differences, which perhaps cannot be made perceptible; but there can be no doubt that the circulation of venous blood contributes to maintain the life of those animals after the cessation of external motion, and during that state which we call apparent death.

These observations lead us to examine the function upon which temperature acts, according to its degree, so as to prolong or shorten life during asphyxia.

We may suppose that temperature between 0° and 40° cent. or 32° and 94° Fahr. acts, either directly or indirectly, on the motion of the heart of asphyxiated animals. As we have proved that circulation contributes materially to prolong the life of those animals which live long without air, it follows that the different degrees of activity of the heart may exercise different degrees of influence upon the duration of life. It is a fact that the rapidity of the heart is very different in animals plunged under water, according to the temperature of that liquid. In reptiles, as in young mammalia, it is slowest at 0° cent. or 32° Fahr. and very rapid at 40° cent. or 104° Fahr.

We shall suppose that the degree of rapidity of the heart which is the best adapted to prolong the life of asphyxiated animals, is that which is determined by the temperature at which they live the longest, and we shall inquire if this same temperature has not a special action upon the nervous system favourable to its functions? I convinced myself of this in the following manner. Towards the end of December, the previous temperature having been very low, the hearts were taken from eight frogs. Four were placed in water at 20° cent. or 68° Fahr., and four others in water at the freezing point. The first set lived on an average an hour and three minutes, the second eight hours and fifty-five minutes. Temperature, therefore, acts upon the frogs when circulation is destroyed, and which are, as it were, reduced to the exclusive action of the nervous system, in the same manner as in those whose circulation is in full activity. The hearts were cut out of three new-born kittens; one was put in water at 20° cent. or 68° Fahr., another in water at 40° cent. 104° Fahr., and the third in water at 0° cent. or 32° Fahr. The first lived thirteen minutes and thirty seconds; the second seven minutes; and the third five minutes. Now these animals lived only by the nervous and muscular systems, and if we compare the result of these experiments with those related in Part III. Chap. IV. performed on animals of the same species, whose circulation was not destroyed, and which were placed in the same circumstances, it will be seen that the temperature exercised upon both an analogous influence. For it was in water at 20° cent. or 68° Fahr. that they lived the longest, much shorter at 40° cent. or 104° Fahr., and the shortest at 0° cent. or 32° Fahr. Temperature, therefore, within these limits, exerts a direct influence on the vitality of the nervous system.

CHAPTER VII.

ON THE MODIFICATIONS OF RESPIRATION DEPENDING UPON SPECIES, AGE, &c.

If animals differ much in the duration of life, when the contact of air is cut off, they differ no less in their communications with air by respiration. This *pabulum vitæ* is far from being consumed by all in the same proportion. We have given several examples of this in the Third Part of this work, when treating of warm-blooded animals; but the comparison of these vertebrata with others which breathe in air, presents differences much more considerable. Let us select them of about the same size, and at the period when the cold-blooded vertebrata enjoy all their activity. Place a frog upon an open-work partition, in a receiver containing a litre, or 61 cubic inches, of air, over a strong solution of pure potass, to absorb the carbonic acid produced by respiration; do the same with a yellow-hammer of the same size; the latter will live about an hour, the frog from three to four days. This great difference does not arise from the frog's being able, after having consumed all the air which can support respiration, to live long without that function. It breathes continually, as long as the air is respirable, and quickly dies, after it ceases to be so. It was shewn in the first part of this work, that these animals when deprived of the contact of air in summer, could not live above one or two hours. Neither does the great difference in the duration of life depend on the frog's being able to derive greater advantage from the air, by depriving it of its last particles of oxygen. When the experiment is made, as mentioned above, by absorbing the carbonic acid as it is formed, the bird has the power of consuming a greater quantity of oxygen. So little of it remains at the end of the experiment, when the air is no longer capable of supporting life, that the proportions scarcely differ in the two cases. The enormous disproportion in the duration of life of the reptile and the bird, in equal quantities of air, essentially depends on the comparative rapidity with which they consume the air, that is admitted: but it may be well to fix our attention on some of the conditions of this difference.

To consider only the lungs: it is manifest that the surface in contact with the air is more extended in the bird, not because the lungs are larger, but because the air-cells are more numerous. The extent and frequency of the respiratory motions are also an indication of the great quantity of air which enters the lungs of birds. Besides, it is known that their lungs contain much more blood, and it is principally to the blood that we attribute the power of altering the air. All these conditions in favour of birds are to be referred essentially to multiplied communication with the air. They may be considered as physical data, since they consist in relations of quantity, but there are undoubtedly others of a different kind, which are of no less importance. If the blood have a great influence by its quantity, will it not likewise have by its quality? Simple inspection may satisfy us that the blood of the frog is more watery than that of the bird. From this single difference must arise a different relation between the blood and the air, for no one will attribute the alteration of the air to the watery part of the blood, but rather to the animal matter which it contains. Of this the proportion is necessarily less in the reptile. There is still another difference which is quite fundamental. The blood to the naked eye appears void of organization, but by the help of the microscope it has been long known to contain particles of a regular figure. According to the last researches of Sir Everard Home in England, and of Prévost and Dumas at Geneva, these particles always consist of a colourless spheroid, with a red covering.* Although these particles are elliptical both in the reptile and in the bird, they are of very different dimensions, being much larger in the former. Prévost and Dumas have given the measurements in their excellent treatise on the blood. Thus, the quality of the blood in the two species mentioned, differs essentially both in the number and the dimensions of the particles.

The same conditions of organization which retard the consumption of air in the frog, as compared with the bird, exist in all reptiles and fishes.

Mammalia clearly resemble birds in the quantity of air which they consume. This difference in the extent of respiration constitutes a remarkable distinction between the vertebrata, and distributes them into four groups; one including reptiles and fishes, the other mammalia and birds. This division is also founded on another characteristic, which has given to the former the epithet of cold-blooded, and to the latter that of warm-blooded; a confirmation of the

* Prévost and Dumas have certainly given the best statement of the comparative dimensions of the particles of the blood in different animals, but their views as regards their form and structure are not confirmed by later observations made under far more favourable circumstances as respects the instrument employed. As far as the above remarks of Dr. Edwards are concerned, the correction is of no importance. Some account of the physical constitution of the blood will be found in the Appendix.

opinion that there exists an intimate connection between the phenomena of the production of heat and the consumption of air. The following facts tend to the same conclusion.

Having ascertained that the mammalia which are born with eyes closed, and the birds which are hatched without feathers, bear a strong resemblance, in the early periods of life, to cold-blooded vertebrata, in the phenomena of animal heat, I was led by analogy to extend this resemblance to the consumption of air. Experiment has confirmed this opinion, and has shewn that the developement of heat in mammalia and birds goes on increasing with the consumption of air from birth to adult age. Afterwards both these functions undergo variations according to the influence of the seasons. Those individuals which, by the successive reduction of external temperature, acquire the faculty of producing more heat, undergo, at the same time, a change of constitution which occasions them to consume more air. This is not the result of a difference in the rapidity of the movements of respiration, nor of variations in the density of the air, but of a change inherent in the animal economy. The contrary effect is produced by the slow but progressive elevation of the external temperature. There is a diminution in the production of heat, and also in the consumption of air. These observations are only applicable to those individuals which, among warm-blooded animals including man, support well the vicissitudes of heat and cold in the opposite seasons of summer and winter. The other individuals which we have pointed out as not seasoned to the climate, because their production of heat diminishes by the influence of the cold, would present the contrary phenomena. This is actually the case with the hibernating mammalia. M. De Saissy compared the respiration of the marmot, the hedgehog, the dormouse, and the bat in the waking state, in August and November. They consumed less air in the latter.

CHAPTER VIII.

OF THE COMBINED ACTION OF AIR AND TEMPERATURE.

It is easy, with some animals, greatly to vary their relations with the air, without destroying their life, provided they are placed under favourable circumstances. We may avail ourselves of this in examining the influence which temperature exerts on life in those cases in which we vary the extent of respiration. In the first part of this work, many facts are stated upon which may be founded a knowledge of this influence. I shall here briefly recapitulate them, and add several others, that we may judge of its generality. We have demonstrated that several species of batrachian reptiles, such as the frog, the toad, and the salamander, can live under water by means of the air contained in it, and which acts solely on the skin. There is then no pulmonary respiration. The animal is reduced to cutaneous respiration, and even this is at its minimum, because of the very small proportion of air contained in the water. The air in this case must have a very feeble vivifying power, yet it suffices to maintain the life of the animal, so long as the temperature is between 0° cent. or 32° Fahr. and 10° cent. or 50° Fahr.; but if the temperature of the water be raised higher whilst the animals are restricted to the same limited respiration, the majority perish. To counteract the deleterious effects of this low degree of warmth, their relation with the air must be increased, its vivifying power will be augmented, and life will be preserved. The animals cannot increase their relation with the air except by coming to the surface to exercise pulmonary respiration. It is by this means that they preserve the equilibrium between the effects of warmth and the influence of the air. When they are at liberty in the marshes, pools, and small streams, they can keep below the surface so long as the temperature of the water does not exceed 10° cent. or 50° Fahr. as is generally the case in autumn, winter, and the commencement of spring, but however little it exceeds this point, they are obliged to rise in order to draw air from the atmosphere. Having received its vivifying influence by an increase of respiration, they are again in a state to live under water, and this for so much the longer time as the water is less above 10° cent. or 50° Fahr., but in proportion as the temperature is raised their tarriance under water is shortened, and they come more and more frequently to the surface until a point is arrived at, at which they can scarcely support the suspension of pulmonary respiration.

There is another mode of respiration to which these animals are obliged to have recourse during the greatest heat of summer. Pulmonary respiration aided by cutaneous respiration in water, is then unable to counteract the effect of the high temperature. They are obliged to quit the water to bring the skin into contact with the atmosphere. Without this resource they would die in great numbers. This is a necessary consequence of the relation between the effect of heat and the influence of the atmosphere. The following fact communicated to me by M. Bosc confirms what I have advanced. In one of our recent summers, remarkable for its long continued and intense heat, many frogs died in a pool in his nursery. The sides of the bason were too steep to allow of the frogs coming out of the water, and during the great heat they were unable to counteract its influence either by cutaneous respiration in air, or by the cooling power of evaporation.

The combined effects of temperature and air are the same with fishes. Fish in winter can live under water without coming to the surface to breathe. Different species, according to their sensibility to heat, are obliged, as the temperature rises in spring and summer, to increase their relation with the air, by coming frequently to the surface, to breathe the atmosphere. As this is generally the limit of their respiration, they die in great numbers if the heat of the season be intense. But those species which suffer the least by evaporation in air, find the means of supporting this heat, by for a time exposing the skin and bronchiæ to the vivifying action of the air. Thus we may sometimes see certain species keep in the shade, with the greater part of their bodies out of water, resting on the leaves and stalks of the water-lilly, or quitting their own element to throw themselves on the banks. They are then entirely exposed to the action of the air, and breathe like land animals.

The preceding cases are complicated with the substitution of another medium, the influence of which upon the animal economy, may at equal temperatures be very different; and in fact it is so, independently of the effect of evaporation in air, but it will be seen, by the following experiments in which this complication does not exist, that this effect is only an accessory.

The batrachians can live in air with the action of the lungs suppressed. It has been shown in a preceding part of this work, that frogs deprived of their lungs have survived a long time in air, by cutaneous respiration alone, provided the necessary precautions are taken to preserve their humidity. They live in this way in winter, and when the temperature is low; but if the action of the lungs is suppressed in this way in summer, they die almost as quickly as when entirely deprived of the action of the air by submersion in water. The vivifying action of the atmosphere, on the skin is too feeble to counteract the deleterious influence of the summer heat. Observe too that they have the assistance of a more active evaporation to cool them; but this advantage is insufficient. It is essential that they should increase their relation with the air, by means of the lungs, in order to bear this high temperature.

The same relation between the combined effects of heat and air is observed when different means are employed to limit respiration. A solid but porous envelop lessens the contact with the air, yet frogs have lived for a considerable time buried in plaster. (see Part I. Chap. 1.) Those experiments were made in winter, and the animals bore this limited respiration, because the temperature was low. The case is different in summer; I have then known them die when similarly enclosed, almost as quickly as if they had been plunged under water. If at this season sand be used instead of plaster, they can live a much longer time, because the sand admits more air.

There can be no doubt that the relation between heat and respiration extends to warm-blooded animals. An observation of Legallois furnishes a proof that it holds in the case of young mammalia. The cutting of the eighth pair of nerves produces, along with other phenomena, a considerable diminution in the opening of the glottis, so that in puppies recently born, or one or two days old, so little air enters the lungs, that when the experiment is made in ordinary circumstances, the animal perishes as quickly as if it was entirely deprived of air; it lives about half an hour. But if the same operation be performed upon puppies of the same age benumbed with cold, they will live a whole day, In the first case, the small quantity of air is insufficient to counteract the effect of the heat, but in the other it is sufficient to prolong life considerably.

We shall now apply this principle to adult age, and particularly to man. A person is asphyxiated by an excessive quantity of carbonic acid, in the air which he breathes; the beating of the pulse is no longer sensible, the respiratory movements are not seen, his temperature however is still elevated. How should we act, to recal life? Although the action of the respiratory organs is no longer visible, all communication with the air is not cut off. The air is in contact with the skin, upon which it exerts a vivifying influence; it is also in contact with the lungs, in which it is renewed by the agitation which is constantly taking place in the atmosphere, and by the heat of the body which rarifies it. The heart continues to beat, and maintains a certain degree of circulation, although not perceptible by the pulse. The temperature of the body is too high to allow the feeble respiration to produce upon the system all the effect of which it is susceptible. The temperature must then be reduced, the patient must be withdrawn from the deleterious atmosphere, stripped of his clothes, that the air may have a more extended action upon his skin, exposed to the cold, although it be winter, and cold water thrown upon his face until the respiratory movements reappear. This is precisely the treatment adopted in practice to revive an individual in a state of asphyxia. If instead of cold, continued warmth were to be applied, it would be one of the most effectual means of extinguishing life. This consequence like the former, is confirmed by experience.

In sudden faintings, when the pulse is weak or imperceptible, the action of the respiratory organs diminished, and sensation and voluntary motion suspended, persons the most ignorant of medicine are aware that means of refrigeration must be employed, such as exposure to air, ventilation, sprinkling with cold water. The efficacy of this plan of treatment is explained on the principle before laid down.

Likewise in violent attacks of asthma, when the extent of respiration is so reduced that the patient experiences suffocation, he courts the cold even in the most severe weather, he opens the windows, breathes a frosty air, and finds himself relieved.

CHAPTER IX.

EFFECTS OF TEMPERATURE UPON THE FUNCTIONS OF RESPIRATION AND CIRCULATION.

The organization of the vertebrated animals which breathe in the atmosphere furnishes them with several means of quickly modifying their communications with the air. These consist principally, in the first place, in the movements of the thorax and abdomen, and in the second place, in those of the heart and blood-vessels. The former are the movements concerned in respiration, the latter in circulation. It is rare that one set is accelerated or retarded without being accompanied by a corresponding change in the other.

Every body knows that the will can retard, accelerate, or stop the respiratory movements, but it rarely takes any part in them. They are determined by another force which keeps up and controls them. Throughout life they proceed, except under particular circumstances, without our being conscious of them, and when the will occasionally interferes, it only does so for a few moments. They habitually proceed at a regular rate, the same number of movements being produced in certain intervals.

This rhythm is maintained with very little variation so long as the system and external circumstances remain the same. This observation is applicable to all the vertebrata which breathe air.

Let us study the relations according to which the respiratory movements are affected by external temperature.

We know that elevation of temperature accelerates their movements. It is a general phenomenon, but the degree of temperature necessary to produce this effect is not the same for all. From what has been already said, we may comprehend the advantage of this acceleration, which rarely takes place to a very sensible degree, except when the heat is oppressive or very distressing. We then extend our relations with the air, and increase its vivifying influence. The respiratory movements become more rapid or more extensive, and thus more air comes in contact with the lungs in a given time, and reanimates what the heat depresses. From this increase of the respiratory movements necessary to counteract, at least for a time, the effects of external temperature, arises an order of phenomena different from those of health, and which characterize a peculiar type of fever.

There is a certain range of moderate temperature within which the respiratory movements vary but little. This range is of greater or less extent according to the constitution of the individual. We have shewn the effects of temperature exceeding the superior limit; we shall now pass to the effects of temperature reduced below the inferior limit. These effects are not, as in the preceding case, uniform in all the vertebrata. Cold, when it influences the respiratory movements of reptiles, retards them progressively, according to its intensity, until it arrests them. Life then is ready to be extinguished. If, whilst respiration is diminished by the cold, the heat of those animals could maintain itself, life, with the greater part, would soon be extinguished. But reptiles conform very closely to the external temperature; and the diminution of their heat co-operates with that of their respiration for the preservation of their life.

When the cold descends below the point at which respiration ceases, it becomes destructive. To prevent death without changing the external temperature, it is necessary to increase the action of the air, which cannot be done but by increasing the respiratory movements. Reptiles, however, do not appear to have such a resource in themselves, though we shall see that there are animals which have.

Among mammalia, the hibernating animals present such a series of phenomena. In spring and summer their temperature is high and their respiratory movements lively, as in other animals of their class; in the decline of the year, their temperature and motions are observed sensibly to diminish, provided the observations are made at sufficiently long intervals. This simultaneous decrease may go on until the cessation of the respiratory movements, without putting a stop to life. But if the cold becomes more intense, the animal must perish, or extend its relations with the air. The intensity of the cold, under which it is ready to sink, excites the respiratory movements; the air inspired maintains them, at least for a time, and counteracts the destructive influence of the temperature.

Thus, cold may either retard or accelerate the respiratory movements, according to its intensity and the constitution of the animals. We have just seen that it is the most intense cold which produces this last effect upon hibernating mammalia.

However slightly the young of warm-blooded animals may be exposed to cold, it accelerates the respiratory movements or increases their extent. This phenomenon is very remarkable in those which are born without the power of maintaining their temperature in the open air. They, but more especially young birds of this description, are no sooner exposed to cold than their respiration increases in rapidity or extent, and their temperature begins to fall. No doubt they experience a lively sensation of cold, notwithstanding the warmth of the season — their whole being indicates it. They present the phenomena of an attack of *febris algida*, and this state is quickly fatal if not remedied by renewing the heat of the body. Although the acceleration of respiration is a powerful means of counteracting the effects of cold, by extending the contact of the air with the organs best adapted to feel its vivifying influence, this acceleration has its limits; it may diminish, but cannot compensate for the effects of excessive cold. In this case it retards, but does not prevent, death. In other circumstances, when the cold is more moderate, this *vis conservatrix* may prove effectual. The word cold is here used in its strictest sense, but refers to temperatures to which do not ordinarily suggest that idea. The words heat and cold, as is well seen in this instance, are completely relative when applied to the animal economy.

Let us follow these young animals in the progress of life. The same temperature less and less affects the respiratory movements, until at length it has no influence over them: consequently, in adult age, the rapidity of their movements is much less subject to the influence of external temperature. But whatever be the extent of the range in which the movements of the thorax preserve the characteristic type of health, there is a degree of cold which alters them. In all the experiments which I have made upon the refrigeration of adult warm-blooded animals which are not subject to hibernation, I have remarked an acceleration of the respiratory movements until, the powers being exhausted, these movements, like all the others, languish and fail. I do not doubt that there are cases in which an abatement of respiration takes place with these as with the hibernating mammalia, but we cannot now enter into this enquiry.

We have said that there is a range of temperature within which variations scarcely influence the rapidity of the respiratory movements, and that this latitude is greater or less according to the constitution of the animal. Now this is a relation which it is of importance to understand with the greatest possible precision, because, if we know the kind of constitution which in the variations of external temperature more or less preserves that rhythm of the respiratory movements which characterizes health, we should be better able to maintain it, or when it is deranged by this cause, to re-establish it. We have seen that mammalia and birds are more affected in this respect by external temperature, in proportion to their youth. Now the most important modifications of functions which characterize the differences of age in the animals of these two classes, are those of the production of heat and the extent of respiration. It is with the development of these two functions that we see a diminution of the influence of external temperature upon the respiratory movements. This correspondence exists even in those cases where there is no difference of age. We may be convinced of this by comparing, at their birth, the mammalia which are born with closed eyes to those which are born with their eyes open.

It is the same in adult age: thus the hibernating mammalia, which produce less heat and consume less air than the other mammalia, experience a sensible alteration in their respiratory movements, from a degree of cold which would have no effect upon the rhythm of respiration in others.

It follows from these facts, that when an individual experiences a change of constitution which diminishes his production of heat or consumption of air, he cannot endure that degree of cold which previously would have been salutary to him, without experiencing sooner or later an alteration in the rate of his respiratory movements. Hence the necessity, when these two functions have experienced this alteration, as in cases of organic affection of the heart and lungs, of placing the patient in communication with a milder temperature, either artificially or by change of climate.

CHAPTER X.

INFLUENCE OF THE RESPIRATORY MOVEMENTS ON THE PRODUCTION OF HEAT.

In studying the influence of the respiratory movements on the production of heat, we must confine ourselves to the mammalia and birds; because reptiles produce too little heat to enable us easily to appreciate the causes which modify it.

When we see the diminution of the temperature and of the respiratory movements of hibernating animals take place at the same time under the influence of cold, we can draw no conclusion from it in reference to the subject before us, since the cold is the cause of both phenomena. In like manner, when we remove one of those animals from the place where he has been benumbed into a warmer situation, his respiration is accelerated, and his temperature rises, under the same influence of external heat. But there are other facts relative to those animals, in which we recognize the influence of the respiratory movements in the elevation of heat. I shall quote the experiments of M. de Saissy.

The air of the room was 1° 5 cent. or 3° 7 Fahr. below the freezing point. The temperature of a bat profoundly torpid was at 4° cent. or 39° Fahr. M. de Saissy irritated it by mechanical means, and left it exposed to the same temperature at which it had become torpid. It was an hour in awaking; thirty minutes after, its temperature was 15° cent. or 59° Fahr., and in thirty minutes more 27° cent. or 80° 6 Fahr., but it could not pass this limit.

A hedge-hog, equally benumbed, in the same place was only 3 cent. or 37° 4 Fahr. He was excited in the same manner. He did not awake for two hours. His temperature was then 12° 5 cent. 54° 5 Fahr.; an hour after, 30° cent. 86° Fahr.: it rose afterwards only 2° cent. 3° 6 Fahr. in the same interval, and then remained stationary.

In the same circumstances, a dormouse cooled to the same degree was stimulated in the same manner. In an hour, its temperature was 25° cent. 77° Fahr., and in the same space of time, the animal had recovered its natural heat, 36° cent. 97° Fahr.

In these experiments, the external temperature had nothing to do with the restoration of the animal heat, and we shall see by the following experiments of the same author that the means of mechanical excitation had no perceptible share in its production, except by exciting respiration and circulation, thereby showing that the increased respiratory movements and the restoration of heat stand related as cause and effect.

On the same day and hour when M. de Saissy performed the experiments above mentioned, he placed in a window exposed to the north, with the precautions necessary not to arouse them, another hedge-hog and dormouse, whose temperature was 4° cent. 39° Fahr., while that of the atmosphere was 4° below zero cent. 25° Fahr. The respiratory movements were very feeble. The dormouse awoke a little more slowly than in the preceding experiment, and ran into his cage with agility. In the first hour from his exposure to the cold, his temperature, like that of the other dormouse, rose to 25° cent. 77° Fahr., and at the end of the second, to 36° cent. 97° Fahr. The hedge-hog awoke two hours and a half after the commencement of the experiment, when his heat had only risen to 12° cent. 54° Fahr. At the end of five hours, it was 28° cent. 82° Fahr.

In this new set of experiments, it is evident that the cause which produced the awakening was not of a nature to contribute directly to the production of heat. If a moderate cold may favour it, as we have shown elsewhere, a more severe cold has a contrary tendency. In this instance the intense cold produced an impression sufficiently powerful to be perceived in spite of the torpor, and excited to more extended respiration.

We recognize in this chain of phenomena a striking example of that *vis conservatrix* of which so much has been said, and which in general has been perceived rather than distinguished. We shall have on more than one occasion to specify the means which nature employs to contend against the agents which threaten life.

But the cause which has excited the movements of these animals is not adequate to maintain them. They produce too little heat, even in expending all their resources, to resist for any length of time the temperature which has momentarily stimulated them. The cold which has roused them withdraws too rapidly the heat springing up under the influence of respiration and circulation, to allow the play of these functions to continue; their temperature quickly sinks, and they relapse into a lethargy which becomes fatal from the intensity of the cold.

This is not the case when, in a moderate cold, they are excited by mechanical means: after having recovered more or less heat, according to their power of producing it, they return to their original state, from which they may again be excited.

In these observations on hibernating animals, the respiratory movements, at first very feeble and scarcely perceptible, progressively increase to the degree of rapidity and extent which they have in the natural state.* The question now is, what is the influence of these movements on the temperature of the body, when they are raised beyond the rate of health?

We cannot answer this enquiry by observations made on the sick. The circumstances are then too complicated to admit of even drawing conclusions from them: we must seek our examples amongst healthy animals, whose constitutions and the modifications which they undergo from the circumstances in which they are placed, are not unknown to us.

We have said that young birds collected in their nest have a high temperature, although they have then few, if any, feathers, but that their temperature falls as soon as they are exposed to the air. In the first days after they are hatched, their cooling in such a case is constantly progressive until the limit at which the cold benumbs them. Whatever be the modifications of their respiratory movements, this effect always takes place, and it is not at this period that we can discern the influence of respiration upon temperature: they then produce so little heat, that no effort of their organization can rescue them from the successive reduction of their temperature; but some days later, when they develope more heat, we frequently recognize by unequivocal indications that the acceleration of respiration beyond the rate of health is a salutary re-action to increase the heat of the body, and counteract the influence of the cooling process. The following experiments will show the result of observations on several individuals very near the age at which they can maintain their own temperature in

* The acceleration of the respiratory movements does not always stop at this limit; but in these irregular movements, we cannot distinguish the phenomenon which we have next to examine.

the air. One of them had a temperature of 40° cent. 104° Fahr. and 97 inspirations per minute. Taken from the nest, and exposed to the air of the room, which was at 18° cent. 64° Fahr., he lost 3° cent. 5° 4 Fahr. in a quarter of an hour: his respiration, however, had been accelerated. They rose to 120 inspirations per minute, which rate was maintained for twenty minutes. He was then warmed half a degree; some time after he cooled again, but his respiration, which had become a little less frequent, acquired extension; his heat was restored to the same degree, and continued so for some time. Another had a temperature of 38° cent. 66° Fahr. and 84 inspirations per minute; a quarter of an hour after his exposure to the air, he lost three quarters of a degree cent. 1° 3 Fahr., his respiration had risen to 108 inspirations, and continued at this rate; examined at the end of an hour, he had recovered his original temperature. Lastly, in another, the respiration was accelerated, and his temperature, instead of falling, rose one degree.

Here, then, are several cases in which the acceleration of respiration above the type of health may have a sensible effect upon animal heat. In the first, the temperature of the body falls under the influence of the cooling cause; but by the re-action in question, it rises a little, without, however, being restored, and may afterwards fall lower, presenting fluctuations. In the second it diminishes, and afterwards returns to the point of departure. Lastly, in the third, it does not fall, and can not only support itself, but even rise above what it was at first.

From the foregoing facts, it follows, that in the cases in which the temperature of the body progressively falls, notwithstanding the acceleration of respiration, the effect of this acceleration is limited to a retardation of the cooling process.

CHAPTER XI.

ON PERSPIRATION.

Perspiration in the human subject has long been an object of multiplied researches. Sanctorius was occupied with it when experimental philosophy was yet in its infancy. From the small quantity which sensibly escapes in the form of sweat, no conception could be formed of the considerable amount actually lost by perspiration in the condition of imperceptible vapour. It was this quantity which Sanctorius determined, and when he announced that five-eighths of our *ingesta* escape in this form, he must doubtless have excited either astonishment or incredulity. He occupied himself during many years in determining, by the help of scales, the variations in the quantity of perspired matter and the relations which they bear to the food, the alvine and urinary evacuations and other perceptible secretions; to the states of sleeping and waking, of exercise and rest, of ease and of suffering, of sickness and health; to the passions, and to the periods of day and night. These are relations which, with proper precautions, he might determine with precision; but from the state of science at the time when he lived, he had not equal facilities with respect to the action of external agents; he has therefore said little on the subject, and that little is either vague or erroneous. It is more surprising that, as the inventor of statical researches, he has furnished so few numerical reports, and still more wonderful that many of his aphorisms are founded on reasoning rather than on the use of the scales, even in those cases in which the information could only be obtained by weighing. Sanctorius, however, opened the path, and for this he deserves the tribute of our gratitude. His successors have furnished more positive data. Keill, Lining, Rye, Robinson, &c., have published their results in the form of tables, the only sure mode of enabling us justly to appreciate general propositions by shewing what is founded on fact and what merely the produce of the imagination. All these labours principally relate to some of the subjects mentioned above, in speaking of the researches of Sanctorius. Our object, on the other hand, has been to examine the influence of most of the external agents on the perspiration of vertebrated animals. We shall apply to man the general facts which result from these experiments; we shall compare them with the statements of those who have been engaged in statical researches on perspiration; and we shall enter into the development of several points which we have reserved for this (the fourth) part. We found it necessary to begin by determining the rate of perspiration in equal and successive periods, first examining them from hour to hour, whilst external circumstances continued sensibly the same. It was shown by experiments on both cold and warm blooded vertebrata, that the losses varied considerably from hour to hour. The statical experimenters have paid little attention to this subject, but the fact is supported by the following remark of Sanctorius, "*Non qualibet horâ corpus eodem modo perspirat.*"

We have seen, that with the inferior animals this fluctuation disappears when longer intervals of time are employed, and that a successive diminution of the loss by perspiration takes place at intervals of two, six, or nine hours. If a diminution is effected in the shorter interval, *à fortiori* it must take place in the longer. The average of six hours will include almost all cases. The uniformity of this phenomenon in the vertebrata is a sufficient ground for admitting it in man, even though no observations had been made on this point.

Thus, taking man, on getting up, in the state of health, and in circumstances exercising no sensible influence on his perspiration, whatever may be the fluctuations from time to time, we may regard them as uniformly diminishing in each succeeding period of six hours. In some it may be presumed that longer periods will be necessary; nine hours ought to admit few exceptions. In some individuals again, successive diminution of perspiration may be observed in periods of three hours; this I should consider as the minimum. We shall infer, from what has been said, that the period of the greatest perspiration, when no obstructing cause exists, is, in general, from the hour of rising in the morning, say six o'clock, till noon, and that the losses are successively less in similar intervals for the remainder of the twenty-four hours.

To secure this regular course, it will readily be imagined that it is necessary not only to remain quiet, but also to abstain from food and sleep; a condition which was probably not fulfilled by those who have hitherto made statical observations upon man. Nevertheless, it may be inferred from these experiments that periods of six hours are as applicable to men as to other animals.

SECTION I. — *Influence of Meals.*

THE influence of meals requires particular examination, the more so because it has rendered complicated the investigations of Sanctorius, Gorter, Keill, and others, as to the period of most abundant perspiration. But for this complication their results would, there is no doubt, have been more accordant.

In taking food, new materials for perspiration are furnished: but when does the food begin to have this effect so as to augment the perspiration? I shall propose another question, which may appear strange to those who have not had their attention turned to the subject. For some hours after the meal, would not perspiration be diminished? Sanctorius, in one of his axioms, has asserted that perspiration is very slight for three hours after a meal, but it is probable that the theoretical reason which he assigns for it, viz., that nature is too much occupied with digestion to be engaged with perspiration, misled his judgment, and induced him to generalize too hastily; for we may infer, from other parts of his work, that he had frequently ascertained perspiration in such cases to be very abundant. I shall content myself with a single aphorism, *Horâ dormitionis meridionæ a cibo corpora aliquando libram aliquando selibram excrementorum occulte perspirabilium excernere solent.*

Keill, who has made comparative observations upon perspiration before and after dinner, has given us numerical results, from which it appears that it was not less abundant during the process of digestion. The experiments of Dodart and others confirm these conclusions, and even go beyond them.

I do not, however, maintain that perspiration is necessarily more abundant, but only that it is not necessarily diminished, and that if, as I have no doubt, it is sometimes less at this period, it must be attributed to the fluctuation which takes place when the periods of comparison are too short.

There must, however, be limits within which we may observe an increase of perspiration from the influence of food. The following are the conditions necessary to render the experiments comparative and the results conclusive:

1st. They ought to be made at the same hour, that the system may be as much as possible in the same disposition to perspire.

2dly. The space of time during which the perspiration goes on ought to be sufficiently long to obviate the influence of the fluctuations so often referred to: it ought to be six hours. These conditions appear sufficiently united in the researches of Sanctorius to allow us to receive his data.

He expresses them in a general manner, in the 326th aphorism: *Qui vacuo ventriculo it cubitum, eâ nocte tertiam partem minus more solito circiter perspirat.*

We may be certain that this is not a hasty conclusion from two small a number of experiments. He recurs to the same subject in different places in which he gives the quantities lost in both cases. The numbers and the proportions differ, which evinces that he made repeated experiments, and that the desire of uniformity did not in this instance hurry him into premature generalization: instead of a third, he frequently found more than twice that difference.

In some instances the quantity lost by perspiration after supper was not greater than when the individual retired supperless. We must, however, conclude from the average, and remember that there are other causes which affect the perspiration.

The night, in the researches of Sanctorius, is a period of seven hours; and as the increase of perspiration from the influence of food was considerable, we may reduce to the limit of six hours, the interval in which we shall recognize this effect. We cannot appreciate this influence by observing the amount in similar periods before and after a meal.

Section II. — *Influence of Sleep.*

In order to judge of the influence of sleep we must compare it with the state of watchfulness during the same period.

It is only in Sanctorius and Keill that we find observations which can furnish data on this point. Both inform us in their aphorisms that perspiration is diminished during restless nights spent in frequent tossing in bed. It is evident that they compare a night of wakefulness with a night of sleep. Sanctorius returns so frequently to this subject, that he must frequently have observed the relative influence of sleep and sleeplessness. We may draw the same conclusion from the aphorisms in which he speaks of perspiration during the day-sleep or siesta.

I have had sufficient occasion to observe this tendency of sleep to increase perspiration, though I have not ascertained it by statical experiment; I have frequently observed upon children of different ages, in good health, and fast asleep, a degree of perspiration which astonished me when compared with the temperature of the air or the thickness of the bed-clothes. I am satisfied that it was not an accidental effect, but an habitual tendency of sleep. We may at any rate consider it as certain that perspiration during sleep and in a state of health may be increased independently of the action of external causes. It is necessary to be aware of the natural variations in perspiration during the twenty-four hours, and of the manner in which they are influenced by food and sleep, in order to estimate the power of other causes which operate on the perspiration.

SECTION III.—*Influence of the Hygrometric State of the Air.*

In applying to man the results of the experiments made on the vertebrata, we should say that the relative conditions of dryness compared to extreme humidity, considerably increases the perspiration within certain limits of temperature. This qualification is indispensable to render the proposition correct. We shall afterwards see the reason, and the facts upon which it rests. I may add that the individual ought to be in health and in that ordinary state of perspiration in which it is insensible; in this case, moderate degrees of dryness may render the losses of weight by perspiration six or seven times greater than in the cases of extreme humidity, and even go much farther. The temperature at which these experiments were made did not exceed 20° cent. or 68° Fahr. The greater amount of perspiration in dry than in damp air does not take place at all temperatures. The phenomena are reversed at a high external temperature. We may see from this that perspiration is a complex function, partly physical and partly vital.

There is one circumstance accompanying the increase of perspiration in dry air which deserves examination. It is well known that evaporation cannot be increased without producing cold; all water which is converted into vapour requires a certain quantity of heat proportionate to the quantity evaporated. We learn that cold tends to diminish the loss occasioned by perspiration. Now, notwithstanding the refrigeration caused by evaporation, the losses by perspiration do not fail, with the qualification respecting external temperature above referred to, to be greater in dry than in damp air. The authors who have made researches respecting the perspiration of man have no difficulty in admitting that it is increased by the dryness of the air; notwithstanding the complication of circumstances in which their observations have been made, they have recognised this effect.

Section IV. — *Influence of the Motion and Rest of the Air.*

Gorter, although he has paid more attention than other observers to the hygrometric state of the air, and has attributed the increase of perspiration in dry air to increased evaporation, has by no means fully appreciated the power of this cause. Where the states of motion and rest are concerned, he only considers the cooling effect produced by the successive changes of the air heated by the body, and concurs with the aphorisms of Sanctorius respecting the diminution of perspiration by the movement of the air. This is evidently not the result of experiment but of erroneous reasoning. The atmosphere which surrounds the body is not only warm, but humid. That which replaces it is colder, but at the same time drier. It is well known that a current of air, independently of any other difference, may in proportion to its rapidity produce an almost indefinite increase in evaporation.

In other parts of this work, it has been established, by direct experiments, that the motion of the air uniformly tends to increase insensible perspiration. This cause is so powerful, that differences in the motion of the air, which appear very slight, and which are sometimes imperceptible, occasion very great differences in the losses from perspiration; so much do the physical conditions under which evaporation takes place, influence the results of that function.

We have examined two of them, the hygrometric state, and the motion of the air. It is necessary here to recollect, that this effect of the motion of the air is applicable only to those circumstances in which there is not a marked tendency to sensible perspiration or sweat.

SECTION V.—*Influence of Atmospheric Pressure.*

On this subject previous observers have left us nothing but conjecture. Sanctorius, although the contemporary of Galileo, the discoverer of the weight of the atmosphere, had not the instruments necessary for this kind of observation. Keill, although he carefully noted the state of the barometer during his observations could discover no relation between the losses by perspiration and the changes in the pressure of the atmosphere.

It is only within a short period that natural philosophers have succeeded in determining the influence of the weight of the atmosphere upon evaporation. They have taught us that the diminution of pressure upon liquids accelerates their conversion into vapour. After all the proof that we have adduced of the influence exercised by physical causes of this kind upon perspiration, we could scarcely doubt that those which we have just mentioned act in like manner on the animal economy. I have, however, attempted to ascertain this by direct proof. I have compared the perspiration of animals placed under the receiver of an air-pump, containing rarefied air with that of individuals of the same species exposed at the same time to the open air. Cold-blooded animals are the best adapted for this kind of experiments. They suffer little from the degree of rarefaction to which the air must be reduced in order to obtain quick and sensible effects; the causes of complication which would throw a doubt over the results of such experiments upon warm-blooded animals, are thus excluded, and it is found that in air which has been rarefied, the losses by perspiration are increased. These experiments are detailed in the first chapter of this work, and I have no hesitation in applying the result to warm-blooded animals, including man.

Section VI.— *Perspiration by Evaporation and by Transudation.*

There are three conditions which have a notable influence upon perspiration, viz. the hygrometric state, the motion, and the pressure of the atmosphere. They act only upon the insensible perspiration; it is that which is increased by the dryness, the agitation, and the rarefaction of the atmosphere. These causes do not produce sweat, at least directly, and the reason is evident, because they act in a physical manner, they diminish the mass of liquids by causing a part to be converted into vapour. Sweat, on the contrary, is a loss ordinarily produced by a vital action, in the form of a liquid which transudes. This leads us to distinguish two modes of perspiration, one by evaporation, and the other by transudation. They would appear at first synonymous with insensible perspiration and sweat; but these terms, although they can sometimes be substituted for each other, are not synonymous. The distinction is easy; all that is lost by insensible perspiration ought not to be considered as the result of perspiration by evaporation. Is not the skin an excretory organ capable of eliminating from the body a certain quantity of liquid, independently of the co-operation of external agents, in like manner as the urinary organs separate and reject a part of the materials of the blood? All that the skin loses in virtue of this power is by transudation. The quantity of liquid which issues in this way may be so small, or if abundant may be so rapidly dissipated in vapour as to be insensible, and we commonly give the name of sweat only to visible transudation. We may on the other hand apply this term to the product of perspiration by evaporation, when from any cause it happens to be condensed and precipitated upon the skin in the form of a liquid.

All losses by perspiration are referable to these two modes of action. They belong either to evaporation which is a physical process, or to transudation, which is most frequently a vital action.

Perspiration by evaporation takes place in the dead as well as the living body. It is independent of every species of transudation. It is a consequence of that porosity of organized bodies, by which the liquids near surfaces in contact with the air would diminish in quantity by being converted into vapour, even though the pores should be such as not to give passage to a single drop of liquid; but living bodies have the power of eliminating by their external surface a certain quantity of liquid; a function which appears to be always in operation, although varying in activity, which may be modified by external agents, but which essentially depends upon causes inherent in the living economy: it is in this view only that perspiration is a secretion resembling the other secretions of the body. We have already said that if this secretion did not exist, perspiration by evaporation would notwithstanding go forward; on the other hand transudation takes place independently of the other mode of perspiration.

As they are ordinarily combined, it would be interesting to determine their relative shares; we should then know what we owe to physical processes and what we owe to vital functions. Nothing appears easier in theory than to establish this distinction. We have only to suppress the physical conditions which permit evaporation, and if losses are still produced by perspiration they will proceed from transudation. We should then obtain the proportion under given conditions, between perspiration by evaporation and that by transudation. But in order to render this method applicable we must pay attention to the following considerations.

Observe that, wholly to suppress perspiration by evaporation, the air must not only be of extreme humidity, but also at a temperature not inferior to that of the animal. If the air were colder it would be warmed by the contact of the body, it would then cease to be at its extreme of humidity, and would permit an evaporation proportionate to the degree to which it had been warmed. By making use of the cold-blooded vertebrata, we may almost entirely suppress the loss by evaporation. Their temperature is not, as is generally imagined, always superior to that of the atmosphere; it is sometimes even lower; and when it does rise above it, it is usually only to a fraction of a degree, and never more than one or two degrees (centigrade.) The average of the differences is a little above the temperature of the atmosphere, but this is so trifling that it may be disregarded altogether.

In order to find the relative proportions of the losses by transudation and evaporation in dry air, we must subtract from the total that portion which has been incurred in humid air. It appeared from numerous experiments with several species of cold-blooded vertebrata, that the proportion in these modifications of the air is, in the generality of cases, as seven to one.

Since the second term represents the quantity lost by transudation, if it be subtracted from the first, the remainder is equal to the loss by evaporation. Perspiration by evaporation is then, in these cases, to that by transudation, as six to one.

By a series of experiments and inductions we have now been enabled to determine that in ordinary circumstances, in which perspiration is insensible, the losses by transudation form but a small portion of the whole. This serves to explain a great number of phenomena. We can thus conceive how the physical conditions which are favourable to evaporation, notwithstanding the diminution of transudation occasioned by the cold which they produce, do not fail, in ordinary circumstances, to increase the total loss. We may also imagine how much the continual variation in the motion of the air must contribute to produce these variations of perspiration which we have observed to take place with cold-blooded animals in successive intervals of an hour.

All that we have hitherto shewn on the subject of perspiration will considerably facilitate our examination of a question which naturally presents itself. Is perspiration susceptible of being suppressed? It is easier to resolve this question with regard to man and other warm-blooded animals, than with respect to the cold-blooded vertebrata. Let us see what is the result of a very low temperature upon warm-blooded animals. We know, by the effect of cold upon the sweat, that it diminishes transudation. Now let us suppose that it may, by its intensity, suppress it altogether, there will remain perspiration by evaporation, which will always take place however humid the air may be. The high temperature of man and other warm-blooded animals, warms the air in contact with the body, and changes its hygrometric state by removing it from its extreme of humidity, and consequently occasions evaporation. If, on the other hand, the temperature of the air be raised to an equality with that of the body, at the time that it is saturated with humidity in order to suppress evaporation, then perspiration by transudation is excited, and takes place to such an extent in man and other warm-blooded animals, that the sweat will stream from all parts of the body. We can then in no case suppress their perspiration; it will be performed either by evaporation or by transudation. We ought therefore to be careful how we take literally what we find in medical books respecting suppressed perspiration. There can be no such thing. That there may be suppression of sweat, is evident to every one; but it does not follow that even in these cases there is no transudation.

Since it is difficult to assure ourselves directly whether transudation is ever entirely suppressed in man and other warm-blooded animals, let us see what the cold-blooded vertebrata will offer on this point.

The batrachians are the best adapted to this kind of researches, on account of the nakedness of their skin, of the fineness of its texture, of the copious loss which may be incurred through its medium, and consequently of the relation which their perspiration bears to that of man.

On exposing frogs to the temperature of 0° cent. 32° Fahr. in humid air, in order to suppress perspiration by evaporation, they have lost by transudation, in different experiments, the 30th part of their weight. Transudation is more abundant in these animals than in man, though the latter be placed in circumstanees much more favourable. When we consider how sensible these creatures are to cold, how much the activity of all their functions is diminished at a low temperature, and how much they may even then lose by transudation, it is not to be supposed that cold suppresses this mode of perspiration in man, and the less so from his having a temperature of his own which varies very little with the changes of the atmosphere, a condition which has a powerful tendency to maintain transudation. It may be very much diminished by the action of cold, but it appears that it cannot be altogether suppressed.

It is a remarkable, but well known fact, that when life is sinking, and to appearance nearly extinct, the body is covered with sweat—so strong is the tendency to continue this function.

Since we can scarcely determine by direct experiment on man and other warm-blooded animals the variations in the amount of transudation at temperatures below that of the body, because the losses by this mode of perspiration are confounded with those by evaporation, we must have recourse to the indirect means which have already served us under similar circumstances.

Section VII.—*On the Influence of Temperature.*

In studying the influence of temperature upon the transudation of batrachians at different degrees from 0° to 40° cent. 32° to 104° Fahr. in air saturated with humidity, in order to suppress perspiration by evaporation, we have observed that the increase of loss by transudation between 0° and 10° 32° and 50° Fahr. was very slight; that it was similar between 10 and 20° cent. 50° and 68° Fahr. but that at 40° cent. 104° Fahr. the increase was considerable; that on comparing the total of the losses in the space of six hours at the temperature of 0° cent. 32° Fahr. and that which took place at 40° cent. 104° Fahr. they were nearly as 1 to 55.

By raising the temperature of the humid air to 40° cent. 104° Fahr. we may occasion as great loss from transudation, as that which results from perspiration by evaporation solely in a dry atmosphere at a temperature not exceeding 20° cent. 38° Fahr.

What inference can we draw from these facts relating to cold-blooded animals which will be applicable to man and other warm-blooded animals? It is probable from what has just been shewn, that transudation in them, undergoes but a slight increase from elevations of temperature to different degrees between 0° and 20° cent. 32° and 68° Fahr. If we endeavour to verify this application by the indications which simple observation furnishes, in the impossibility of exact appretiation we shall find this presumption confirmed. Every one has had occasion to observe that between the limits of temperature which we have mentioned, sweat is scarcely observable in man, when he is at rest, is in perfect health, and is free from all agitation of mind; but when the temperature rises only 5° or 6° cent. 9° or 10° Fahr. above this limit, transudation becomes evident on a great number of persons in the most tranquil state of body and mind, provided that the air be neither too dry nor too agitated. However little the temperature may be raised, the sweat increases in a proportion which appears much greater than that of the increase of temperature. There will then be a degree at which the loss by transudation may equal that resulting from perspiration by evaporation in a very dry air, at or below 20° cent. 68° Fahr.

Let us follow the modification of perspiration in an atmosphere of a progressively rising temperature. Two effects would result, which we shall now compare. The increase of heat above 20° cent. 68° Fahr. would increase transudation rapidly; on the other hand the air becoming warmer, would increase evaporation in an increasing progression; but the perspiration by evaporation would not necessarily follow the same rate; and for this reason: according as the sweat becomes abundant, it spreads over the body, and forms there an external layer more or less extended. In this space in which the sweat intercepts the contact of air with the skin, there is no perspiration by evaporation; there is evaporation at the expence of the layer of sweat always supplied by transudation, but from these parts no fluid evaporates from within through the pores, and so far no perspiration by evaporation. This suppression will be general when the sweat universally covers the skin. Evaporation will always take place, but it will not be by perspiration. In order that this progressive diminution of perspiration by evaporation should take place in a dry air of a rising temperature, it is evident that the atmosphere ought to be calm or but little agitated; for the motion of the air, in proportion to its rapidity, increases evaporation almost indefinitely; whence it follows that sweat may be so quickly taken off in an atmosphere which is dry, warm, and sufficiently agitated, that the two modes of perspiration, by evaporation and by transudation, may take place at the same time as they do at lower temperatures.

Section VIII.—*Cutaneous and Pulmonary Perspiration.*

Sanctorius and Gorter were not ignorant of the pulmonary perspiration, but the means which they employed to estimate it were so imperfect that it would be useless to give their results. Hales employed more exact processes, but we shall pass them over and proceed to a period at which chemistry and experimental philosophy were much further advanced.

Lavoisier and Seguin estimated the average loss by perspiration from the skin and lungs in twenty-four hours, at 2 lbs. 13 ounces, of which 1 lb. 14 ounces is dissipated by the skin, and 15 ounces by the lungs, which gives the proportion of two to one.*

Of this loss a portion is owing to the evaporation of water from the lungs, and another to the chemical changes of the air in respiration; but it is certain that the water is the predominant portion. The difference in the manner in which this fluid is dissipated by the lungs and by the skin deserves particular attention.

Whatever transudation there may be within the lungs, no liquid can issue from them but in the form of vapour. A new portion of air enters at each inspiration; it becomes warm, and remains there until the whole mass rises nearly to the temperature of the body: in virtue of this acquired elevation, whatever may have been its previous hygrometric state, it converts into vapour the liquid with which it is in contact, and in respiration carries it into the atmosphere. It brings with it no water in a liquid state, nor any other substance in this form. There is then no loss by pulmonary transudation. All the perspiration, as far as water is concerned, takes place by evaporation; making a considerable difference between the lungs and the skin, where the two modes of perspiration are united. This depends on their structure, one of their organs being a cavity which does not permit the flowing out of a liquid; the other a surface so disposed, as to allow its escape at all parts. Here then is one reason for which, in man, the losses by cutaneous perspiration are more abundant than those occasioned by pulmonary perspiration. From this double source of perspiration at the skin, it is subject, as we have shewn, to great variations. From its greater simplicity, the pulmonary perspiration is much more regular, and consequently the losses are much more nearly equal in different periods. However, the loss of water by the lungs is

* See Lavoisier's *Traité elementaire de Chimie,* 3d edit. p. 228.

capable of being suppressed, because, being performed by a physical process, it may be stopped by the physical conditions which prevent evaporation. In an atmosphere saturated with moisture, if the temperature were equal to or above that of the body, there would be no watery perspiration from the lungs, because there would be no evaporations; whilst the cutaneous perspiration would take place, not by evaporation, but by transudation, and that to a very large amount.

Section IX.—*Perspiration in Water.*

Supposing that water, in contact with the skin, had no physiological action upon that organ, it would merely prevent the contact of the air, and consequently suppress perspiration by evaporation from the skin. There would then remain the loss by cutaneous transudation, which must be added to that which takes place by the lungs.

CHAPTER XII.

ABSORPTION IN WATER.

WE have hitherto proceeded on the supposition that water exerted no special influence on the skin, and that it only acted by intercepting the contact of the air. We shall now enquire whether the presence of water produces any other effects which complicate the results. Does any absorption take place when water is in contact with the human skin? Seguin,* after examining the changes in the weight of the body, both immersed in water and out of it, was induced to reject the idea of absorption. The result of these experiments may, however, admit of being differently viewed.

We have proved in that part of this work which treats of the cold-blooded vertebrata, that the batrachians, whether smooth like the frog, or thick and rough like the toad, are capable of absorbing much water by their external surface, and that the quantity absorbed not merely soaks into the tissue of the skin, but spreads through the system, and is distributed to the different parts. These animals, like man, have the skin naked; a condition the most favourable to absorption. It is true that the skin of man, from the nature of its epidermis, is less disposed to absorption; it nevertheless possesses this property to a very great degree. We cannot doubt it, when we observe what takes place in animals,

* See *Memoires sur les Vaisseaux Absorbans, &c.* Annales de Chimie, tom. xc.

whose integuments appear less fitted for giving passage to water. I shall not speak of scaly fishes in which I have proved absorption by the external surface, because in them it might be attributed to the fins, the membranes of which are extremely delicate, but I shall relate some facts respecting lizards, which I have not yet brought forward. Their skin being entirely scaly would appear to offer an insurmountable obstacle to absorption, yet I suspected that this might not be the case. A lizard was exposed to the open air in order to remove it from the point of saturation and cause a sensible loss of weight. Perspiration in these animals being very slight, several days were allowed. It was then introduced into a tube and fastened by a fore and a hind foot. I then placed it in water so as to immerse only the tail, the hind legs and the hinder part of the trunk. It was afterwards weighed at distant intervals and found to have successively increased in weight, until it had supplied the loss incurred by perspiration in the air. The experiment was then stopped. This absorption was not mere imbibition limited to the surface; the water penetrated deeper, and was distributed through the system. The body and the limbs had resumed their roundness and plumpness, and life, which would, ere long, have been extinguished in the air, as had happened to several other individuals, which were exposed at the same time, was prolonged by the liquid which absorption at the external surface had furnished to repair the loss which had been sustained.

If the scaly skin of the lizard permits such an absorption, it is impossible not to attribute this property to the skin of man.

The skin of man, when in contact with water, exercises two opposed functions: transudation and absorption, and according to the predominance of either of these above the other, the weight of the body is diminished or increased, or their proportion may be such as completely to balance their effects.

If the diminution of the weight of a man plunged in a bath be exactly equal to the loss incurred by pulmonary perspiration, the absorption by the skin is equal to the transudation by the same organ.

The proportion, as quoted in the former chapter, from Lavoisier and Seguin, which pulmonary perspiration bears to general perspiration in the air, is 6° to 18°; in their second Memoir on Perspiration (Annales de Chim. vol. xc. p. 22.) they give it 7° to 18°. In the first series of experiments, in which Seguin compares the loss in water to that in air, he finds the proportion 6° 5′ to 17°. Hence it follows, that the loss in the bath did not exceed the pulmonary perspiration. No more is necessary to render this case an example of absorption, since we do not find in it the excess of loss which would result from cutaneous transudation. Seguin, however, infers from it the absence of both functions; and this explanation will equally account for the result; but his own researches oblige him to admit transudation in water at higher temperatures, although he maintains that between the degrees of 12° 5′ and 22° 5′ cent. and 54° 5′ and 72° 5′ Fahr. neither transudation nor absorption takes place.

There are cases in which direct proof cannot be obtained, and in no science does this happen so often as in physiology. When man is concerned experiment is frequently impossible. In other cases in which a trial may be made the results are equivocal, because, as in the present case, the facts may be explained by two hypotheses. We must then have recourse to comparative physiology, and select those animals which admit of decisive experiments, and whose constitution will admit of our arguing *à fortiori* with respect to man.

Let us apply this to the present case.

If we observe animals much more sensible to cold than man, and in which it enchains all the functions in a much greater degree, and find that transpiration takes place in water of a much lower temperature than that which has been supposed to suppress it in man, can we be induced to believe that it really is suppressed at the more elevated temperature in man, in whom the function in question possesses considerable energy?

It will be recollected that in the first part of this work, we treated of the influence of temperature upon the transudation and absorption of batrachians in water. It was there shewn that even at the degree of cold at which water is ready to freeze, these two functions do not cease, which is rendered evident by the alternate diminution and increase of their weight according as either function predominates. It is known that cold of that degree has a most powerful action on these animals, whose activity it diminishes considerably, so that at a temperature a little lower they become torpid. Since transudation is not suppressed in them, notwithstanding the intensity of the cold, will it cease in man in a bath of from 12° 5′ to 22° cent. or from 32° 5′ to 40° Fahr. higher temperature?

Two causes exert the principal influence upon these functions; the quantity of liquid contained in the body, and the temperature of the water in which it is immersed. The greater the fullness of the body the less is the absorption, and the lower the temperature of the water the less the exudation. Now it so happens that the experiments of Seguin were made when the water was at a temperature intermediate between the extremes of zero, cent. or 32° Fahr. at which the increase of weight in the batrachians, if they are a little below saturation, predominates over the loss by transudation, and of 30° cent. or 66° Fahr. at which the latter predominates over the former, and when it was consequently to be expected that the two influences would approximate to an equality. But in order to secure an increase of weight in man by immersion, not only must the circumstances be such, that absorption may exceed transudation, but also that the excess may be greater than all the loss occasioned by pulmonary exhalation during the immersion, which according to the average of Seguin's experiments is seven grains per minute or six drachms an hour. Such a result cannot be expected under ordinary circumstances; but we must not hence conclude that such an increase is impossible and reject the assertion of Haller. We have seen that the preponderance of absorption over transudation depends not only on the temperature, but also on the greater or less fullness of the body. If, therefore, the body had previously undergone a considerable loss from perspiration by evaporation, without repairing that loss, it would perspire the less, and be in the condition the most favourable to increase by absorption.

CHAPTER XIII.

ABSORPTION IN HUMID AIR.

We should not be warranted in concluding, that bodies so impregnated with humidity as those of animals absorb watery vapour from the atmosphere, from the mere fact of their absorbing water when immersed in it. When immersed in a liquid the pores will be penetrated by it, or there will be an interchange with the fluids already contained in them, in consequence of the movements which take place in every medium. Animals, on acconnt of the quantity of fluid which they contain, appear more likely to impart humidity to the atmosphere than to receive it; this is peculiarly applicable to warm-blooded animals, because, having a temperature usually much higher than that of the atmosphere, the air in contact with their bodies is warmed, and thereby becomes more susceptible of imbibing moisture. This at least applies to aqueous vapour; the case may be different with other vapours, such as those which have an affinity to water. However, it is certain that the hair, at least, is hygrometric, even when on our bodies during life, and hence a part of the vapour which it condenses must necessarily be transmitted to the bulb, when absorption will take place.

The difficulty of ascertaining by the weight of the body the absorption of aqueous vapour is still greater than in the question as to the absorption of liquid water. Scattered facts favour the idea of absorption from the atmosphere, but they are, in general, vague and inconclusive.

I cannot doubt, from the vast number of facts attesting it, that perspiration by transudation is a constant phenomenon within the limits of temperature to which my experiments applied. Now I have had occasion to observe that frogs in an extremely humid atmosphere, for the space of an hour, had neither gained nor lost weight; an interval in which I had always found that they sensibly lost by transudation. Instead of concluding that transudation was suppressed, I conceived that there had been an absorption of watery vapour equivalent to the loss by transudation; and the analogy of this process with what I had shown respecting their functions in water, necessarily decided me.

But as these experiments were somewhat equivocal, it was desirable that others less ambiguous in their results should be attempted.

A ring adder (couluivre à collier) was placed in a vessel containing air of extreme humidity. The animal was weighed at different intervals, and found at first to have lost weight; but instead of continuing to do so, it afterwards gained more than fifteen grains, not above what it had weighed originally, but above the point of diminution at which it had arrived.

It has been stated, that in this respect warm-blooded animals are less favourably circumstanced than the cold-blooded, to absorb vapour from the atmosphere, but their organization may compensate for this, or even go beyond it. I cannot but regard the following result, as a proof of the absorption of watery vapour by the mammalia. I compared the loss of weight of several guinea-pigs in dry and in humid air. From the nature of the apparatus I was unable to estimate the loss by perspiration, otherwise than by subtracting from the total loss that of the alvine and urinary evacuations of similar animals in the open air; but in comparison I found that the average of the evacuations exceeded the diminution of weight in the humid air. It appeared, therefore, that the absorption of watery vapour had supplied this difference, as well as the loss by transudation; for it is not to the absorption of a portion of the respired air that this effect can be ascribed.

Now applying these results to the case of man, it cannot, in the first place, be believed that man has not, like them, the power of absorbing watery vapour, and that in sufficient quantity to be rendered evident, either by the weight of the body not losing in humid air, or even by its gaining during some time.

Secondly, these cases must be rare, if compared with those in which we observe a loss of weight by perspiration, notwithstanding the humidity of the atmosphere.

If now we examine recorded facts, we shall find a confirmation of these conclusions.

Gorter has been quoted as furnishing facts in proof of the absorption of aqueous vapour, but I can only find in his work, facts which relate to the cloths. Keill, in one of his aphorisms, says, "*Quæ in ære sub vaporis specie circumvolitant aquæ particulæ à cute nostrâ attractæ cum sanguine commiscentur et corpus pondere augent.*" Without the facts on which it is founded, we might call the correctness of the aphorism in question, or be uncertain as to the precise sense in which it is to be understood; for Sanctorius and Gorter sometimes speak of increase of weight to denote the sensation of heaviness, and sometimes a loss of weight below that which is customary, which is a relative increase.

There is, however, a statement of Keill, which contains a fact. "27 Decemb. hâc nocte octodecim humoris uncias ex aere ad se somnians (corpus) attraxit."

Whatever doubt may be felt regarding either the source or the authenticity of this observation of Keill, it acquires a high value when we combine it with another made by Lining, so circumstantial as to carry conviction along with it:

"The same day again, betwixt $2\frac{3}{4}$ and $5\frac{1}{8}$ P. M. my clothing being the same and having no exercise, I drank betwixt ℥ xxiii. and ℥ xxv. more of punch, and the air being cooled by the clouds overspreading the heavens, the quantity of urine was greatly increased, amounting in these $2\frac{1}{2}$ hours to ℥ xxviii. $\frac{6}{8}$; but the perspiration was so much diminished, that the quantity of humid particles attracted by my skin exceeded the quantity perspired in these $2\frac{1}{2}$ hours, by ℥ viii. $\frac{1}{3}$. Two more instances of this attraction you have in the same table." — Philosophical Transactions, vol. xlii. 1743, p. 496.

CHAPTER XIV.

ON TEMPERATURE.

SECT. 1.—*On the Degree of Heat which Man and Animals can endure.*

SHORTLY after the invention of the thermometer, when meteorological observations were few and incomplete, it was not known that man and other warm-blooded animals could support a temperature superior to that of their bodies. Boerhaave, in reflecting on the use of air in respiration, adopted the opinion, that the access of this fluid seemed to cool the lungs, in which the blood underwent a fermentation, by which a considerable degree of heat was produced, and he thought that life would be extinguished if the temperature of the air were superior to that of the bodies of animals. Some experiments, undertaken at his suggestion, by Fahrenheit and Prevoost, seemed to confirm this opinion.

It was generally received, until the observations of Lining, at Charlestown, in 1748, of Adanson, during his voyage to Senegal, and of Henry Ellis, when governor of Georgia, in 1758, proved that the temperature in those various climates was elevated some degrees above that of man, and yet proved injurious to only a very small number of individuals. But the most remarkable fact that has been published on this subject, is that for which we are indebted to the observation of Tillet and Duhamel, provided that no objection can be made to their measure of the temperature.

During their stay at Rochefaucault in Angoumois, in 1760, a baker's daughter, in their presence, entered into an oven, the temperature of which they estimated to be at least 112° Reaumur, which is equal to 128° 75′ cent. or 264° Fahr. She remained about twelve minutes in this excessive heat without being much incommoded by it. This experiment was repeated several times, after their departure, on another girl, with the same success.*

New researches and observations were afterwards made by Dr. Fordyce, with Banks, Blagden, Solander, and others, in 1775. An association of men so distinguished by their learning and sagacity, who observed on their own persons the effects of excessive heat, must necessarily have given rise to very interesting results.† These experiments were for the most part repeated by Dobson at Liverpool.‡ This subject was not of a nature to be exhausted; MM. Delaroche and Berger took it up in 1806, and supplied several deficiencies.§

The experiments of Delaroche and Berger apply not only to man but to other animals, and as they are the most numerous and varied, we shall begin with some of their results.

1st. *In dry air.* Wishing to ascertain the effects of a dry atmosphere at a temperature little above that of warm-blooded animals, they raised it by means of an iron stove

* Mem. de l'Acad. des Sciences, 1764. p. 185.

† Philosophical Transactions for 1775. pp. 111 and 484.

‡ Idem, 1775. p. 463.

§ Exp. sur les effets qu'une forte chaleur produit sur l'economie, etc. Paris, 1806.

in a small apartment, to a temperature varying between 42° 5′ and 45° cent. or 108° 5′ and 112° Fahr. They exposed to this heat different species of vertebrata, viz. a cat, a rabbit, a pigeon, a yellow-hammer, and a large frog; the greater number at first remained undisturbed, but in about half-an-hour they became agitated and their respiration was progressively accelerated for about three quarters of an hour, until it became panting. There was then a remission of the symptoms in almost all the animals. They remained an hour and a half; when none of them came out in a natural state, but in half an hour or an hour they appeared perfectly recovered.

It would appear, from these experiments, that vertebrated animals exposed to a dry and hot air of 45° cent. or 112° Fahr. are near the limit at which they cannot long survive. Indeed, the same individuals, after having sensibly recovered from the effects of the previous heat, were introduced into a stove, the air of which, at first 56° 25′ cent. or 133° 25′ Fahr., arose towards the conclusion of the experiment to 65° cent. or 149° Fahr. All except the frog perished at various periods, from 24 minutes to 1 hour and 55 minutes. Three other series of experiments on reptiles, mammifera, and birds, made within nearly the same limits of temperature, had the same effects.

I know no example of man having supported for a longer time so intense a heat. However, there have been persons exposed for a short time to a higher temperature of the air. A young man, in Dobson's experiments, remained for twenty minutes, without great inconvenience, in a stove, the air of which was at 98° 88′ cent. or 210° Fahr. but his pulse, which commonly was at 75 per minute, beat 164 in this hot air.

But even this is not the extreme limit. M. Berger supported for 7 minutes an atmosphere of the temperature of 109°.48 cent. or 229°.06 Fahr.; and Blagden, that of 115°.55 to 127°.67 cent. 240° to 260° Fahr. for 8 minutes.

2. *In watery vapour.* It is known that bodies of a different nature, but whose temperature is the same, do not communicate by contact, the same quantity of heat in a given time. Hitherto, however, the investigation of the heating power has been confined to the gases which have been examined, with this object in view, by Petit and Dulong. We are ignorant not only of the amount of the difference between watery vapour and dry air, but even on which side is the superiority. We should naturally infer that the heating power of vesicular vapour is greater than that of dry air, and that animals are consequently unable to endure it at so high a temperature. This inference is confirmed by experiment.

M. Delaroche could not support, above ten minutes and a half, a vapour bath, which, at first, at 37°.5 cent. or 99°.5 Fahr. rose, in eight minutes, to 51°.25 cent. or 124°.25 Fahr. and afterwards fell one degree.

M. Berger was obliged in twelve minutes and a half to come out of a vapour bath of which the temperature had risen from 41°.25 cent. or 106°.25 Fahr. to 53°.75 cent. or 128°.75 Fahr. He was weak and tottered on his legs, and was affected with vertigo. The weakness and thirst lasted the remainder of the day.

These gentlemen, however, supported for a considerably longer time, without much inconvenience, higher temperatures in dry air.

The peculiar sensation which resulted from the impression of heat was much more lively in the vapour bath; it was a sense of scalding. These experiments have been cited, because they are comparative, but not as indicating the extreme of heat, which man is capable of supporting in the vapour bath for that space of time. Joseph Acerbi relates, in his voyage to the North Cape, that the peasants of Finland can remain for above half-an-hour in a vapour bath, the temperature of which is raised to 70° or 75° cent. or 158° or 167° Fahr.

3. *In liquid water*. It is not necessary to make very precise experiments in order to be convinced that a bath of hot water, at the same elevated temperature as a vapour-bath, would act much more powerfully on the animal economy.

I have had occasion to observe in reptiles, the great difference in the action of liquid water and of steam, at the same temperature. I have never seen batrachians which could live above two minutes in water, at 40° cent. or 104° Fahr. although I have taken the precaution of holding a part of the head out of the water, to allow the pulmonary respiration to continue; whilst individuals of the same species (frogs), have supported the heat of air charged with vapour at the same temperature above five hours.

Lemonnier, being at Barèges, plunged into the hottest spring, which was at 45° cent. or 113° Fahr. He could not remain in it above eight minutes. Violent agitation and giddiness forced him to come out.

In comparing the intensity of the action of the different media,—dry air, vapour and liquid water, upon the animal economy when raised by a high temperature, I have only referred to the difference in their heating power. I do not, however, exclude other causes, but propose to examine them as facts arise which relate to them.

In giving the results of these experiments, I have mentioned some of the effects produced by excessive heat, such as the acceleration of the pulse and respiration, and sensation of greater or less heat, and some other symptions connected with the nervous system. There is one which I have not mentioned which deserves separate consideration, I mean the state of the temperature of the body and transpiration.

SECT. 2. — *On the Influence of Excessive Heat upon the Temperature of the Body.*

1. *Man.* The first observations on the permanence of the temperature of the body, notwithstanding the changes of the seasons and the difference of climates, brought to light a very remarkable phenomenon.

At first these observations only related to ordinary variations of temperature within limits below the heat of the human body. Dr. Franklin was, I believe, the first to notice a fact which appeared more wonderful.

He observed, one day in summer, the temperature of the air being 37°.77 cent. or 100° Fahr. that the temperature of his own body was only 35°.55 cent. or 96° Fahr. Now this fact is particularly worthy of attention, first, as proving that warm-blooded animals have the power of maintaining in themselves a temperature inferior to that of the atmosphere, when the latter is above its ordinary limits; and secondly, the temperature of the Doctor's body being below the average temperature of man, excludes the idea that it had received any accession of temperature from the atmosphere.

But the question remains, is any degree of external heat capable of raising the bodily temperature of man and other warm-blooded animals, and if so, to what limit can it raise?

Dr. Fordyce, and his coadjutors, observed that their temperature could be raised two or three degrees of Fahr., equivalent to about a degree or a degree and a half of the centigrade thermometer.

The greatest elevations of the bodily temperature of man, under the influence of external heat, have been observed by Delaroche and Berger in their own persons. The temperature of the former being 36°.56 cent. or 97°.8 Fahr. rose 5° cent. or 9° Fahr. by his staying eight minutes in a stove containing air at 80° cent. or 176° Fahr. Berger, whose temperature was the same, received an accession of 4°.25 cent. or 7°.65 Fahr., after staying sixteen minutes inthe same stove, at 87°.5 cent. or 189°.5 Fahr.; but it might be objected to the estimation of their bodily temperature, that they were taken at the mouth in an atmosphere much warmer, which might have contributed to raise the thermometer, or it might have been the result of a local heat.

To obtain a decision free from objection, Berger and Delaroche placed themselves successively in a case, through which they could pass their heads, and by means of linen cloths surrounding this aperture and their necks, they intercepted the passage of the steam. The temperature of the mouth must then have been the result of the temperature of the other parts of the body. After staying seventeen minutes in this steam-bath, at a temperature from 37°.5 cent. or 99°.5 Fahr. to 48°.75 cent. or 119°.75 Fahr. the temperature of Delaroche increased 3°.12 cent. or 5°.6 Fahr. In the same apparatus, the steam being from 40° cent. or 104° Fahr. to 41°.25 cent. or 106° Fahr. Berger's temperature rose 1°.87 cent. or 3°.36 Fahr. in fifteen minutes.

Of course, experiments upon man cannot be extended far enough to ascertain what is the highest degree which his temperature can attain under the influence of excessive atmospheric heat.

2. *Warm-blooded animals.* In the before-mentioned stove, Delaroche and Berger exposed various species of mammalia and birds to different degrees of dry hot air, the lowest 50° cent. or 122° Fahr. and the highest 93°.75 cent. or 200°.75 Fahr. They left them there till they died. Notwithstanding the diversity of species, and of classes, and of the degrees of heat to which they were exposed, they all acquired nearly the same increase of temperature, the limits of the variation being from 6°.25 cent. or 11°.25 Fahr. to 7°.18 cent. or 12°.92 Fahr. The bodily temperature having been ascertained by a thermometer introduced far into the rectum, is free from the objections above stated.

When we consider the uniformity of the above results, we may infer, generally, that man and warm-blooded animals, under the influence of excessive heat in a dry air, could not, during life, experience a higher elevation of bodily temperature than 7° cent. or 12·6 Fahr., or 8° cent. or 14° Fahr.

3. *Cold-blooded vertebrata.* The temperature of these animals differs no more than one or two degrees cent. from the external air throughout the various seasons of the year, and it is, therefore, to be expected that it would continue to exhibit a similar conformity in higher degrees of heat. In the experiments of Delaroche and Berger, the highest temperature attained, at the period of death, by individuals of this species placed in the stove, was 40°.93 cent. or 105°.67 Fahr. which is within the limits of the bodily temperature of warm-blooded animals.

SECT. 3.—*Comparison of the Losses by Perspiration in Dry Air, Humid Air, and Water, at Temperatures inferior to that of the Body.*

Were we to conclude solely from experiments made at temperatures below that of warm-blooded animals, we should regard it as certain, that the loss by perspiration must be greater in dry than in humid air when both are raised to the same degree of excessive temperature. We do not, at first, see why it should not be so. This was the opinion of Blagden, who, in the experiments undertaken by Dr. Fordyce, having felt the effects of excessively hot air, both dry and humid, could judge from observation. But he did not have recourse to weighing. Delaroche and Berger did, and obtained decisive results.

According to their experiments, made in the stove, and vapour-bath, already mentioned, air excessively hot, and charged with extreme humidity, excited a more abundant perspiration than dry air, at a higher temperature.

The cause of this difference between the results of temperatures higher than that of the body, and those which occur in temperatures inferior to it, may be conceived from what has already been stated on the subject of perspiration by evaporation, and that by transudation.

In the variations of temperature under 20° cent. or 68° Fahr., and in the ordinary circumstances of health, &c. transudation forms only a small part of the general perspiration, but it increases rapidly from the effect of heat in higher temperatures. At an excessive degree of heat, transudation increases so much as to cover the whole surface of the skin; there is then no more perspiration by evaporation at this surface, it being only an evaporation of water already eliminated from the economy. In this state of things perspiration by the skin is performed only by transudation, whether in dry air, or in that which is charged with humidity. Other circumstances then being equal as regards the skin, that state of air which has the greater heating power, will occasion the greater transudation. Now, as we have formerly shown, vesicular vapour, which is that of vapour-baths, has a greater heating power than dry air; whence we conclude, that the loss through the medium of the skin will be greater in the vapour-bath than in dry air. There yet remains the comparison of the loss by the means of the lungs; here the predominance is not on the side of the vapour-bath. The loss, as far as water is concerned, is none, because it cannot be evaporated in air of extreme humidity and of a heat superior to that of the body; but in dry air of the same temperature, the evaporation from the lungs may be considerable. We know, however, by experiment, that the excess of transudation in air loaded with vesicular vapour more than counterbalances the evaporation from the lungs in dry air.

Another circumstance to be considered is the cooling influence of evaporation, which takes places in dry air only, and hence a considerable diminution of transudation.

It is easy to foresee the effects of a water-bath of excessive heat, compared to those of dry and humid air. For the same reason that the loss by perspiration is greater in the vapour-bath than in the dry air at an excessive heat, it will be still greater in water of the same temperature; for the heating power of water is greater than either that of vesicular vapour, or dry air of the same temperature. This conclusion is confirmed by fact. Lemonnier, after staying eight minutes in a water bath at 45° cent. or 113° Fahr. lost twenty ounces, which is at least double that which Delaroche and Berger lost at the same temperature in a vapour-bath, and at a temperature of above 90° cent. or 194° Fahr. in dry air.

Secr. 4. — *On the Influence of Evaporation upon the Temperature of the Body when exposed to an excessive Heat.*

When Franklin had made experiments on the power of evaporation in the cooling of liquids, he referred to the same cause, the faculty which he attributed to animals of maintaining the temperature of their bodies below that of the air when its heat is excessive. This opinion received some support from the experiments of Fordyce, who did not, however, regard the cause in question as the only one. The researches of Delaroche and Berger give a more accurate idea of the influence of evaporation on the temperature of the body when exposed to great heat.

One of those porous vessels, called by the Spaniards alcarazaz, which are susceptible of evaporation from the whole of their surface, was introduced by Delaroche and Berger into a stove, with two moistened sponges and a frog. The temperature of the vessel and of the sponges had been previously raised to that of warm-blooded animals, from 38°.12 cent. or 100°.6 Fahr. to 40°.93 cent. or 105°.67 Fahr. The temperature of the stove varied between 52°.5 cent. or 126°.5 Fahr. and 61°.25 cent. or 142°.25 Fahr. In a quarter of an hour, the vessel, the two sponges, and the animal, were nearly of the same temperature, not exceeding the limit of that of warm-blooded animals. They retained it pretty nearly two hours. This term is remarkable. In order to arrive at it, the vessel and the sponges, instead of being warmed, cooled about a degree; on the contrary, the temperature of the frog, which was at first

21°.25 cent. or 70°.25 Fahr. rose to 37°.18 cent. or 98°.9 Fahr. in the space of fifteen minutes, and remained stationary for the rest of the time, maintaining itself, like the alcarazaz and the sponges, at from 15° cent. or 27° Fahr. to 21°.5 cent. or 38°.7 Fahr. below that of the surrounding medium.

A greater difference is observed, according as the external temperature is higher, but at the same time, the term at which that of evaporating bodies becomes nearly stationary is a little higher, in conformity with this increase of heat in the air.

If we suppose that as long as respiration is performed by an animal, especially a warm-blooded animal, it preserves the power of producing heat, a difference will thence be inferred between its temperature and that of an inanimate body with an evaporating surface, exposed to an excessive heat, although their point of departure should be the same. This conclusion is justified by the results of experiments made by Delaroche and Berger. The evaporation in a rabbit exposed to air from 62°.5 cent. or 144°.5 Fahr. to 87°.5 cent. or 189°.5 Fahr., judging by the diminution of its weight, was quite as great as that of the alcarazaz; the final temperature, however, of the rabbit was superior by at least 2°.5 cent. or 4°.5 Fahr.

Thus the same general cause, viz., evaporation, would alone be sufficient to retain the temperature of animals and inorganized bodies below that of the external air, when the latter is excessive, that is, when it is above the bodily temperature of warm-blooded animals; but below this limit it would be incorrect to attribute to this cause, as is generally done, the real or supposed power of man and other warm-blooded animals, to maintain uniformity of temperature under the vicissitudes of seasons and climates.

SECT. 5.—*On Cooling in Different Media, at Temperatures, inferior to that of the Body.*

Let us now compare the power of cooling in the respective media of dry air, vesicular vapour, and water at temperatures inferior to that of the body. In the dry air, less heat is taken off by contact, that is, its cooling power is less, hence the heat will tend to accumulate in the body; but on the other hand, the evaporation being greater, there will be in this respect a greater cooling effect than in the vapour. The reverse will hold, in both respects, in vesicular vapour. Thus, we see, in both instances, that at inferior temperatures the two agents which influence the heat of the body are opposed in their action, but we cannot compare the result of their opposing powers, being ignorant of the measure of each.

We cannot determine, *a priori*, in which the cooling effect will preponderate.

The same applies to the comparision with water, and our uncertainty is still greater in relation to air saturated with transparent vapour; for experimental philosophers are quite ignorant of the relative intensity of its cooling power, and even whether it is greater or less than that of air. It is to be hoped that they will, ere long, decide a point not unimportant in physics, and certainly important in physiology.

It is, nevertheless, a generally established opinion, that we experience a greater cooling effect in humid than in dry air. This is evidently founded on the sensation, and other effects which it produces on the animal system. It might from this be inferred, that the cooling power of evaporation in dry air was more than equalled by that of the contact of transparent vapour. But this method of judging is very equivocal. Hence I was induced to attempt a mode of investigation that would admit of greater certainty.

We have formerly shewn that many warm-blooded animals at the period of their birth, and shortly after, have not the power of maintaining their temperature when withdrawn from their nest, and exposed separately to the air. I availed myself of this circumstance in order to compare the refrigeration undergone in dry and humid air. I employed large vessels similar in form and dimensions to those which I had used in investigating the phenomena of perspiration. I preferred animals of very small size, that the proportion of air might be greater, with a view to the influence which the alteration of this fluid by respiration might exercise on the temperature of the animals.

Besides, the vessels, formed of large squares of glass connected by their edges, allowed of a change of air through the joints, but with sufficient slowness to permit the production of the necessary changes in its hygrometric state. The same animal was placed successively in the two conditions of extreme dryness and extreme humidity, but at sufficient intervals to prevent the previous refrigeration from influencing that which was to succeed it. Several animals of different ages were made use of, each of which was alternately placed in the two different conditions. It would be tedious to enumerate all the precautions which were taken to render the comparison fair; one only may be mentioned, which was, that the interval between the comparative experiments on the same individual was not allowed to be more than a few hours, since even a day would be sufficient sensibly to increase the production of heat, the progress of which is very rapid at so early a period of life.

Ten experiments were made in dry air, and as many in humid air, upon young sparrows. The average of refrigeration was $6^{o}.5$ cent. or $11^{o}.7$ Fahr., for the dry air, and

7°.7 cent. or 12° Fahr. for the humid. Disregarding this slight difference we may admit, as the general result of these experiments, that the refrigeration was the same in the dry air and in the humid ; whence it follows that the cold produced by the greater evaporation in the dry air, was balanced by the cold resulting from the contact of the humid air.

Such is the average result of these experiments, but in examining each singly, we shall see that in some the refrigeration was exactly the same in both conditions, in others it was greater in the humid air, in others, in the dry air. It is to be observed, that the humid air was at its maximum in almost all the cases, and differed very little from it in the others ; that the degree of dryness of the dry air varied in the different experiments, and that it is precisely in that of which the limits were from 55 to 44 of the hygrometer of Saussure, that the superiority of refrigeration was decidedly on the side of the dry air.

Sect. 6. — *On Refrigeration in Air at Rest, and in Air in Motion.*

In air at rest, at a temperature inferior to that of our bodies, we lose heat in three ways, by evaporation, by the contact of the air, and by radiation. Loss by radiation would equally have taken place in vacuo, and appears not to be influenced by the motion of the gas. Now let the air be agitated, its radiation will not be affected ; but the change of the air considerably increases the quantity of heat taken away by contact, and in a degree proportioned to the rapidity of the current. To the greater loss of heat by this action of the wind, we must add the refrigeration produced by an increased evaporation, which also augments with the rapidity of the wind. It is to these two causes combined, that we must attribute the strong feeling of cold which we experience when no other change of the state of the atmosphere occurs than in the rapidity of its motion.

In the celebrated voyage of Captain Parry to the Artic Regions, there was frequent occasion to remark the difference between the indications of the thermometer and those drawn from the feelings of the voyagers. They bore very easily a temperature of 17°.77 cent. below the freezing point (0° Fahr.) when they were walking in the open air in calm weather. This was not the case if the air was agitated; however, the temperature always rose with the wind, whatever might be its direction. They suffered more cold in a breeze when the temperature was only 6°.66 cent. below 0° (+20° Fahr.) than at 17°.77 cent. below 0° (0° Fahr.) when the air was at rest. The second surgeon in the expedition, Alexander Fisher, who relates the above facts, furnishes a more remarkable example of the cold occasioned by wind. He informs us that the temperature being at 46°.11 cent. below 0° (—51° Fahr.) during calm weather, they were no more inconvenienced by the cold, than when the air was at 17°.77 cent. (0° Fahr.) during a breeze.

CHAPTER XV.

ON THE INFLUENCE OF LIGHT UPON THE DEVELOPMENT OF THE BODY.

Has light, by which we see and are warmed, any other effect on the animal economy? Its influence on inorganic bodies and on vegetables, is unequivocal. The solar rays produce in the mineral kingdom combinations, which cannot be brought about at the same low temperature without it. Unaided by the influence of light, plants would scarcely produce any of the green matter (of Priestly), a substance so generally diffused through the vegetable kingdom, that it seems to be one of the essential productions of this class. When we consider that without light, independently of its heat, there would scarcely exist a trace of vegetation, can we suppose that it is inert with respect to animal life?

But when we cast our eyes on man, and the various classes of inferior animals, we scarcely observe any other sensible relations with light than those of vision, which give the perception of colours, forms and distances. The brown tinge observable upon persons who are much exposed to the sun, as it scarcely occurs except on exposed parts, and does not require a great intensity of heat, is justly attributed to the peculiar action of light. To what are called *coups de soleil,* the heat appears to contribute in a considerable degree. They have been attributed to a concentration of the solar rays, in consequence of a particular disposition of the clouds giving them the faculty of acting as a lens: there is, however, no occasion to have recourse to so bold a supposition. But if it be true that inflammation of the skin comes on most frequently from the effect of a burning sun, it is likewise true that it may be the effect of a somewhat intense light. I have known persons who were subject to that affection when they were in strong day-light, in circumstances in which the sun had little force; which indicates a great sensibility to the peculiar action of light. It is to the combined action of light and heat, that the sudden deaths of those who are long exposed to the sun in hot weather, ought in general to be ascribed.

With respect to the impaired health of those who live in the dark, as for example, in mines and prisons, it is obvious that we cannot distinguish the effect of the privation of light from that of many other deleterious causes which we find united in those unhealthly situations.

I thought that I might perhaps find an example of the effect of light, in the development of animals, that is to say, in those changes of form which they undergo in the interval between conception or fecudation and adult age. This process, previously to birth, is generally carried on in the dark. There are, however, animals, whose impregnated eggs are hatched, notwithstanding their exposure to the rays of the sun. Of this number are the batrachians. I wished to determine what influence light, independently of heat, might exert upon this kind of development. With this view I placed some frogs' spawn in water, in a vessel, which was rendered impermeable to light by dark paper. The other vessel was transparent. They were exposed to the same degree of temperature, but the transparent vessel received the rays of the sun. The eggs exposed to light were developed in succession. Of those in the dark none did well; in some, however, I remarked unequivocal indications of the transformation of the embryo.

But it is especially after birth that it is interesting to determine the peculiar effect of light upon the development of the body, because then almost all animals are more or less exposed to it. Although all, in growing, change their form and proportions, it is difficult to perceive and appreciate correctly slight shades of modification. The choice must, therefore, fall upon species among the vertebrata, whose development presents precise and palpable differences.

These conditions are combined in the highest degree, in the species made use of in the preceding experiments, and, in fact, in the whole batrachian family. All, during the first period, have the form, and even the mode of life, of fishes. They have no limbs, but a tail and gills. In the second period they are reptiles without a trace of resemblance to the exterior form of fishes; four limbs, and neither tails nor gills; the metamorphosis is complete. It is evident from the experiments on these animals detailed in the second part of this work, that the absence of light does not necessarily prevent the development in question, since two tadpoles, out of twelve, in a tin box pierced with small holes for the change of water, and placed at the depth of several feet in the Seine, underwent the change of form, which renders them reptiles. Let us remark, in the first place, that these two individuals were transformed much later than those which were exposed to light, and were at liberty to rise to the surface of the water. To what was this lateness, and also the continuance of the others in the form of fishes to be attributed? Was it to the want of light or of pulmonary respiration, or to the combination of both?

An attempt was made to determine the respective influence of these two causes, first, by putting tadpoles in two large vessels containing ten litres of water, both capable of admitting light; one of glass, but with a partition close to the water, to prevent atmospheric respiration, the other open to allow the animals to rise to the surface and breathe the atmospheric air. Those who were deprived of respiration were certainly later in transforming themselves than the others, but this delay was so short that the influence of the want of respiration appeared to be very slight. It results from the comparison of this fact with the preceding, that the absence of light had the principal share in retarding the transformation of the two tadpoles plunged in water, and in the continuance of the form of all the others. This conclusion was afterwards put to the test of a counter-proof. The experiment was performed upon two tadpoles of the *rana obstetricans*. Both were allowed to breathe at the surface; they were inclosed in vessels into which the light did not penetrate; a large number of others were placed in transparent vessels. One of those which were deprived of light arrived at complete development; but the other retained its original form, characteristic of the first period, whilst all those which enjoyed the presence of the light underwent the change of form appertaining to the adult. It is here very important to observe, that this influence of darkness on the form does not proceed from a decay of the individual. It appeared in perfect health, and what is very remarkable, it attained to a large size, which phenomenon I had also observed in the untransformed tadpoles in the tin-box before mentioned. At the commencement of the experiment they had almost attained the size at which the transformation takes place. Each was weighed before it was placed in the compartment appropriated to it. Several in the course of the experiment doubled and even tripled their weight. These two series of experiments unite, therefore, in proving that the presence of solar light favours the development of form. They also shew the distinction between this kind of growth and that which consists in the increase of size.

We see then that the action of light tends to develope the different parts of the body, in that just proportion which characterizes the type of the species. This type is well characterized, only in the adult. The deviations from it are the more strongly marked the nearer the animal is to the period of its birth. If, therefore, there were any species existing in circumstances unfavourable to their further development, they might possibly long subsist under a type very different from that which nature had designed for them. The *proteus anguiformis* appears to be of this number. The facts above mentioned tend to confirm this opinion. The *proteus anguiformis* lives in the subterraneous waters of Carniola, where the absence of light unites with the low temperature of those lakes, in preventing the development of the peculiar form of the adult.

The principles which we have deduced from experiments upon animals, lead us to the following considerations respecting man. In the climates in which nudity is not incompatible with health, the exposure of the whole surface of the body to light will be very favourable to the regular conformation of the body. This application is confirmed by an observation of Alexander de Humboldt in his voyage to the equinoctial regions. Speaking of the Chaymas, he says: "Both men and women are very muscular, their forms are fleshy and rounded. It is needless to add that I have not seen a single individual with a natural deformity. I can say the same of many thousands of Caribs, Muyscas, and Mexican and Peruvian Indians, which we have observed during five years. Deformities and deviations are exceedingly rare in certain races of men, especially those which have the skin strongly coloured."

On the other hand we must also conclude that the want of sufficient light must constitute one of the external causes which produce these deviations of form in children affected with scrofula, which conclusion is supported by the observation that this disease is most prevalent in poor children living in confined and dark streets. We may from the same principle infer that in cases where these deformities do not appear incurable, exposure to the sun, in the open air, is one of the means tending to restore a good conformation. It is true that the light which falls upon our clothes, acts only by the heat which it occasions, but the exposed parts receive the peculiar influence of the light. Among these parts, we must certainly regard the eyes as not merely designed to enable us to perceive colour, form and size. Their exquisite sensibility to light must render them peculiarly adapted to transmit the influence of this agent throughout the system, and we know that the impression, of even a moderate light, upon these organs produces, in several acute diseases, a general exacerbation of symptoms.

CHAPTER XVI.

ON THE ALTERATIONS IN THE AIR FROM RESPIRATION.

Since the first experiments of Priestley and Lavoisier, on the alterations produced in the air by respiration, philosophers have agreed upon these two points only; 1, The disappearance of a portion of oxygen; and 2, The production of carbonic acid. On the questions,—what becomes of the oxygen? how is the carbonic acid formed? what proportion does the oxygen bear to the carbonic acid? what takes place with respect to azote? and several others connected with this subject, great difference of opinion continues to exist. These differences induced me to resume the subject. When the constituents of the atmosphere were first discovered, their proportions were not exactly determined. This deficiency was necessarily a source of error in the results of experimenters, yet since the analysis of the air has been more complete, these discrepancies have not been removed. Another cause must therefore be sought, either in the mode of conducting the experiment, or in the animals operated upon. As to the mode of experiment, it never strictly resembles natural respiration in the open air. Either the rhythm of the respiratory movements, or the purity of the air is changed.

Hence the results are more or less doubtful in proportion as the natural respiration has been little or much disturbed. The differences, referrible to the animals, depend rather on nature than on the experimenter. These have been the most overlooked. With the exception of Spallanzani and Humboldt, almost all experimenters have confined their attention to the respiration of man and of two species of mammalia, the guinea-pig and the mouse. Man presents other difficulties besides those just stated. His lungs contain a large quantity of air both before and after the experiment. The appretiation of this is indispensable in very many cases, but it is so uncertain, that the results dependent upon it are perpetually the subject of doubt and dispute. The experiments of Spallanzani embrace almost the whole scale of animated beings. The extent and generality of the results, and the sagacity and talent of their author, were calculated to inspire great confidence, yet they had little influence on the views of physiologists respecting the alteration of the air in respiration, either because the analysis of the atmosphere was in his time imperfect, or because his experiments were not made on man. It must, however, be remarked that Humboldt, who with Gay-Lussac, has contributed much to establish the exact proportion of oxygen and azote, has proved, in his extensive labours with Provencal respecting the respiration of fishes, that that class of vertebrata change the air in the same manner as Spallanzani had pointed out with respect to the other vertebrata. About the same time Davy, whose name marks an epoch in the history of chemistry and experimental philosophy, obtained similar results with man, and on other species of mammalia. These successive researches seemed to be decisive. Others were yet undertaken by Allen and Pepys, who by the precautions which they adopted gave great weight to the results. According to them, free natural respiration is reducible to two facts already discovered, viz. the disappearance of a portion of oxygen, and the production of an equivalent portion of carbonic acid. Respiration was thus reduced to great simplicity. The oxygen lost being exactly represented by the carbonic acid, its office appeared to be merely that of entering the lungs to form a portion of carbon and be expelled. Hence it might be argued that no oxygen is absorbed and carried into the circulation, either to enter into new combinations or to excite and vivify the œconomy. The azote appeared to pass for nothing in respiration. However accurate Allen and Pepys may have been in their experiments, it does not necessarily follow that their results must be constant. They only operated on man, and on one other species, the guinea-pig. We are therefore thrown back on several of the causes of doubt mentioned above.

I saw no means of avoiding them but by paying a regard to all, and varying the experiments so as to render the conclusions independent of the sources of error by which previous attempts had been affected. I therefore proposed to myself, to multiply experiments, not only upon individuals of the same species, but also to submit to them individuals of different species taken from the three classes of vertebrata which perform atmospheric respiration: to vary the conditions dependent upon age and external circumstances: to diversify the mode of respiration from the most laborious degree, to that which approaches nearest to the natural state: to establish such a proportion between the quantity of air respired and the bulk of the individuals, that the quantity and bulk of the air remaining in the lungs could not sensibly affect the results: to pay a scrupulous regard to measurement, and to estimate how far the errors inseparable from it can affect the conclusions.*

Since no process leaves respiration strictly in its natural state, it was essential to compare the effects of a respiration more or less constrained, in order to judge of the influence of such extraordinary respiration upon the mode of alteration of the air.

By beginning with the longest confinement possible, and diminishing it successively, in different series of experiments, all the degrees of respiration are gone through. If the effects are similar, without however being precisely the same, we may judge what respiration must be in relation to the alteration of the air, when it is in its natural and healthy state.

I proposed to gain by this method, another advantage, viz. that in the degrees of laborious respiration excited by the experimental process, such alterations of this function must present themselves as are met in diseases of the chest, whence would result applications to pathology.

In the choice of my apparatus, I bestowed particular attention to the means of measuring the quantities of air before and after the experiment. It consists of a glass globe, to which is fitted a tube whose diameter is large enough to allow the introduction of the animal. As the animals subjected to experiment were either adults of very small species, or young animals of larger species, the diameter of this tube was small enough to allow very slight differences in the volume of the air to be determined

* There are two methods by which we may equally obtain a nearly natural respiration. The first by renewing the air and collecting that which has been breathed, as was done by Allen and Pepys. The second by keeping the animal for a short time in a quantity of air which shall be large in proportion to the bulk of the animal. I chose the latter, as the more simple method, which is an important advantage when experiments are to be multiplied.

by the graduation. Each degree was equivalent to a quarter of a centilitre, or ·152 cubic inches. The graduation was double, in order that the level of the mercury might be well observed on both sides. The air, previous to the experiment, was brought to the state of extreme humidity, in order that the perspiration might not change its bulk. The animal was introduced through the mercury, upon a partition of iron wire, to the top of the tube, and supported by a stem of the same material, fastened to the lower orifice.

Section I.—*Proportions of the oxygen which disappears, and of the carbonic acid produced.*

Three puppies, a day or two old, were introduced into separate vessels of the description above mentioned, containing 150 centilitres, or 91·5 cubic inches of air. They remained there five hours. During the experiment it was evident that there was an absorption of air; for the mercury rose in the tube, and it was necessary to pour a fresh supply into the vessel in which it was immersed. It was evident that the absorption was considerable, but the exact measure of it could not be ascertained. From the analysis of the respired air, it appeared that the three puppies had produced nearly the same quantity of carbonic acid, of which the mean was 17·86 centilitres, or about 10·90 cubic inches, and that the absorption, likewise nearly the same in all, was on an average 9·30 centil. or 5·67 cubic inches, and that it was at the expense of the oxygen. We shall now see what light this first experiment throws on the nature of the gas absorbed. Is it pure oxygen, or carbonic acid which mixes with the air during respiration, or lastly, a mixture of both? We shall, for the present, suppose that the two first hypotheses only are possible; we shall afterwards examine the last.

In the early stages of the experiment, when the respiration begins, there is scarcely any carbonic acid in the vessel; but the quantity increases as the experiment proceeds. If, therefore, it is this gas which is absorbed, the absorption will be scarcely sensible at first, will go on increasing, and be at its maximum at the end of the experiment. On the other hand, if it be oxygen, the reverse will take place, since its proportion is largest at the commencement, and diminishes progressively. Now the following are the phenomena presented in the experiment. As soon as the animals are introduced, scarcely any of the expansion of the air, which must necessarily take place from the rise of temperature, is indicated by the mercury in the tube; whence it results that absorption takes place from the beginning, and consequently that it is the oxygen which is absorbed. In confirmation of this we may observe that when the absorption is evident by the ascension of the mercury in the tube, it is more rapid at the beginning than towards the conclusion of the experiment.

Three puppies, like the preceding, were placed in the same conditions as in the first series of experiments; but instead of remaining five hours, they remained only two. The average quantity of carbonic acid produced was 14·86 centil. or 9 cubic inches, and 7 centil. or 4·27 cubic inch of gas were absorbed. In this instance the gas absorbed was rather less than half the carbonic acid produced, and in the former rather more than half. We see from this how little influence the lengthened confinement of these animals in air containing a pretty large proportion of carbonic acid, had on the proportion between the two quantities, whence it is evident that the gas absorbed is principally oxygen.

Three very young cabias, or guinea-pigs, were subjected to the same kind of experiment for 1 h. 42′. They produced each on an average 21·69 centil. or 13·23 cubic inches of carbonic acid, and absorbed 5·44 centil. or 3·32 cubic inches of oxygen. The change of species occasioned a striking change in the proportion of the oxygen lost to the carbonic acid produced. In the case of the cabias, it is as 1 to 4; in the puppies of the second series a little less than 1 to 2. Besides the difference depending on their species, the one being carnivorous, and the other herbivorous, there is another very remarkable one, to which we have frequently alluded, depending on their development. The new-born cabias comes into the world in a more advanced state, which gives them the power of producing more heat.

Hitherto we have seen that the proportion of gas absorbed to the carbonic acid produced, varies principally according to the species and the age; for in casting a glance at the tables, it is evident that the mean quantities differ but little from the particular results. But we shall now observe the results of experiments upon other species, in which the individuals differed much in this respect. These examples are taken from birds, both in laborious inspiration or in that which may be regarded as nearly free and natural. The conditions of the experiment may be so modified as to approach indefinitely to natural respiration, by increasing the quality of air in proportion to the bulk of the animal, and by shortening the period of confinement.

I chose ten adult yellow-hammers, whose bulk was equal to a little more than 3 centilitres or 1·8 cubic inches. Each of these was placed in a vessel of the kind already mentioned, containing 155 centil. or 94·5 cubic inches of air. They remained there only 15 minutes. They produced on an average 5·98 centil. or 3·65 cubic inches of carbonic acid, and absorbed 1·29 centil. or ·787 cubic inches of oxygen. Here the small size of the animals, the large proportion of air, the short duration of the experiment, the quantity of carbonic acid, scarcely any at first, and in small proportions at last, allow no room to doubt that the absorption was almost entirely at the expense of the oxygen, and that the same would be the case in the open air.

In comparing the results of individual experiments of this kind, considerable difference was observable in the proportion of the oxygen absorbed to the carbonic acid produced, ranging between rather less than half, and one-sixth. The quantity of carbonic acid produced was very uniform when the circumstances were similar.

Let us now descend to the other extremity of the scale of vertebrata having atmospheric respiration, in order to ascertain whether the results are similar, as to the absorption of oxygen, by which I mean the disappearance of a portion beyond that which corresponds to the carbonic acid produced.

Since reptiles alter the air much more slowly, their confinement must be considerably prolonged in order to obtain effectual results. Its duration in this case, does not prevent their respiration in a close vessel from sufficiently resembling respiration in the open air, provided that the other conditions before mentioned have been fulfilled, and that they are taken out when they have produced a small quantity of carbonic acid in proportion to the air in the vessel.

During the heat of summer, the thermometer being 27° cent. 80°.6 Fahr. I tried nine experiments with the green frog, *rana esculenta,* in 74 centil. or 45·16 cubic inches of air, in which they were left 24 hours. They produced, on an average, 5·26 centil. 3·23 cubic inches of carbonic acid, and absorbed 2·18 centil. or 1·23 of oxygen. Other experiments, made at lower, but moderate temperatures, did not fail to exhibit a considerable proportion of oxygen absorbed, compared to the carbonic acid produced.

Similar researches, made with grey lizards, had the same success. It results from them that the proportion between the oxygen which disappears, and the carbonic acid produced is very variable. The excess of the former varies in such proportion, that sometimes it exceeds the third part of carbonic acid formed, sometimes it is so small that it may be disregarded. This difference is not solely dependent on the constitution of the species, but also on the comparative degree of development, from difference in age, and on individual differences among adults.

It is easy now to determine to what we must attribute the diversity in the results obtained by the philosophers who have been engaged with observations upon the chemical changes effected in the air by respiration. Those who have inferred from their experiments, that the oxygen which had disappeared exceeded the quantity of carbonic acid produced, were not mistaken, even in the origin of the researches upon this subject, when there were several sources of error, in the measurement of volumes of air, in the endiometric processes, and in the conditions of the experiment, because there are numerous cases in which this excess is so great, that it exceeds the limit of any error which the observers of that period could possibly have committed.

These results do not exclude those of Allen and Pepys, who found the quantities of oxygen lost and carbonic acid produced apparently the same, since we have seen above that proportion varies greatly, and that in some instances it approaches to equality. They employed the greatest care to obtain accurate results, but their experiments were with few individuals and very few species.

This result of my experiments upon respiration, in regard to the great extent of variation in the proportions of oxygen which disappears, and of carbonic acid produced, has appeared to be important, not because it reconciles the results of previous labours, (though even that is not uninteresting), but because it establishes a fundamental fact of importance to the theory of that function.

Since the period at which I presented this result to the Academy of Sciences in 1821, additional proofs have been furnished of the extent in which the proportions of oxygen which disappear, and of carbonic acid produced, may vary according to the species. For these we are indebted to M. Dulong, who has compared, with his characteristic talent, the quantities of heat developed by animals with that which would proceed from respiration, in accordance with the theory of Lavoisier. A notice of them is to be found in the *Journal de Physiologie de M. Magendie*, January 1823.

SECTION II.—*On the Proportions of Azote in the Air inspired and expired.*

We now come to a fundamental question relative to the changes produced in air by respiration, upon which the results of former observations differ still more than in the preceding question. For in that there were only two opposite results; one that the oxygen exceeds the carbonic acid, the other that it equals it; whereas in the present question there are all the contradictions possible. 1st, That the inspired is equal to the expired azote. 2nd, That it is greater; and 3d, That it is less.

The examination of the alteration in the air, by the respiration of new-born puppies and guinea-pigs, in a number of experiments, shewed that, with a single exception, there was in all the cases an augmentation of the quantity of azote. Now, if in some of them the quantities were small, in others they appear to me beyond the limits of any errors that I could have committed, and I therefore conclude that they are the effect of the exhalation of azote; and since this effect took place in both the first and second series of experiments, wherein animals of the same species and the same age had remained in the air during very different times, I considered that the same would hold with natural respiration.

In the tabular view of my experiments upon the respiration of birds, the columns referring to azote exhibit the same result in all the cases in which the birds remained a long time in the respired air, although for different periods, and at different temperatures. The respiration of frogs in my experiments, must be regarded as natural, or nearly so, on account of the slight alteration in the proportion of oxygen and the small quantity of carbonic acid at the end of the experiments. There was, in almost all, an excess of azote, and in the greater part, a quantity sufficiently remarkable to admit of being attributed to exhalation. The same may be said of lizards.

Of the numerous experiments which are recorded in the tables, and of many others which are not mentioned, the result was, that in almost all the cases there was an excess of azote; but the consideration of the differences leads necessarily to the two following conclusions:—

1. In a large number of cases the azote inspired and expired so nearly approaches to equality, that the slight difference may be disregarded, and exhalation rejected.

2. In a great number of other cases the excess of azote is so considerable that the exhalation of this gas cannot be denied, inasmuch as the quantity greatly exceeds the vo-

lume of the lungs, and bears a large proportion to that of the animal.

These results were met with in the experiments of both series and with species the most widely different.

I should have paid no attention to the very small number of cases in the tables, in which there was a loss of azote in the expired air, since in nearly all of them the difference was so small, that I could not have attributed it to absorption, if other experiments had not furnished me with further data.

Let it be remarked in the first place, that the foregoing facts refer to experiments made in spring and summer months.

We shall now call the reader's attention to the tables of experiments made in autumn and winter.

The exhalation of azote was still taking place on the 22d of October, as shewn by experiments on adult sparrows. From the 26th the phænomena are reversed, and the loss of azote becomes as striking as the excess had been before, the attending circumstances still remaining the same. As this series of experiments was made upon individuals, which remained as long as possible in respired air, it might be believed, that this absorption of azote would not have taken place during natural respiration, but it must be observed, that the same phænomenon was produced by so short a confinement, that the respiration approached very nearly to that which takes place in the open air.

Yellowhammers which were kept only 15 minutes in 155 centil. or 94·6 cubic inch. of air, which they very little deteriorated, produced in the month of November, in almost every instance, a diminution in the quantity of azote.

We see that these two series of experiments, made upon adult sparrows and yellowhammers, in a different season, present us with a phænomenon directly the reverse of that which we formerly shewed. I have proved it on the same species of birds, in autumn, in winter, and at the beginning of spring.

During the cold season the exceptions were extremely rare, but in pursuing these researches with the same species of birds, I observed that the phænomena became reversed. I first observed this in the spring; it continued through the summer, and with a very few exceptions during the commencement of Autumn. Although nothing could have made me anticipate the relation between the inspired and expired azote, noticed in the preceding cases, and which continued throughout the period of fine weather, I was led to expect that inverse phænomena would prevail from some period of the autumn. This was a powerful inducement for me to continue this species of experiment, and I obtained results which I have exhibited in the tables. In the series of experiments relating to birds, the quantities of inspired and expired azote are variable, but may be classed under three heads, namely; 1st, of equality between the two gases, and 2ndly, of the excess of one, and 3rdly, of the excess of the other. If the difference between the inspired and expired azote had been quite irregular and confusedly scattered throughout the course of the experiments, no reliance could have been placed in them, but on the other hand, when we see these differences, however slight, constantly bearing for a considerable period in one direction, and then having an opposite tendency during an equal space of time, notwithstanding the identity of the mode of experiment, we cannot but conclude that a real change has taken place in the subject under investigation. But what is more subject to change than a living animal? Have we not already seen in the course of this work, that striking changes take place in the constitution of these very animals? It was this change in their constitutions in successive seasons, which made me presume that a change might also take place in the effect produced upon inspired air, and made me undertake a series of researches continued throughout the year. Considering then the inspired and expired azote abstractedly and without connexion with the causes which may make them vary, we may assert, not only that the one may at times exceed the other, but that their extreme differences are the same on both sides.

This difference in the inspired and expired azote has never equalled the greatest difference observable between the oxygen which disappears and the carbonic acid produced; so that the two former quantities tend much more to an equality. This tendency is so great, that in many cases, the differences have been too slight to be regarded as real.

With respect to the causes of the variations, they are probably very numerous and difficult to be ascertained. As to the influence of the seasons, the effect of which we have observed on adult yellow-hammers and sparrows, it is evident by these very experiments, that that case does not always prevail over the others, since there are cases in which absorption was sensible in summer and exhalation in winter. It is true, that I have observed the absorption of azote in winter, in adult bats and mice; but I have not repeated my experiments upon these species as I have done with the former. As to very young animals, such as young guinea-pigs, I have scarcely ever observed anything but the exhalation of azote both in winter and summer.

Section III.—*On the Exhalation and Absorption of Azote.*

The principal experimenters who have proved the absorption of azote in the respiration of vertebrated animals, are

Spallanzani, Humboldt, Davy, Pfaff, and Henderson. They have frequently done so to an extent which precludes doubt. Allen and Pepys and Dalton found no sensible change in the quantity of this gas when the animals breathed as nearly as possible in the natural way. An increase of the azote was observed by Bertholet and Nysten. Although the latter found an excess of azote to an extent which could not be ascribed to a fault in the analysis, the fact of the exhalation of azote in respiration was the point most requiring confirmation. Independently of the proofs which I have given, it has recently been confirmed by the researches of Dulong. Humboldt and Provençal, in their long series of experiments upon the respiration of fishes, have proved that these animals absorb a large quantity of azote. Spallanzani recognized the absorption of azote by reptiles and various species of warm-blooded animals. Davy observed it in his own person in so many instances as to leave no doubt of the fact. The same was the case with Pfaff and Henderson.

From our being accustomed to find a constancy in the phænomena of inorganic nature, and habituated to judge of the truth of the results of experiment by the possibility of our reproducing them at pleasure, we are led to seek with anxiety for the same character, in an order of facts which are necessarily variable. Hence the difficulty of obtaining general assent to the results of physiological experiments, which by their nature are precluded from offering that uniformity on which the mind reposes with confidence.

Convinced that different and even opposite results do not necessarily exclude each other, when vitality is concerned, I have always endeavoured so to vary my experiments, that I might reproduce some of the phænomena which appear contradictory in the works of other physiologists. This has been particularly the case with respect to the subject of respiration, and especially with reference to azote. It remained to seek for the links by which these differing phænomena might be connected.

In the different experiments which exhibit, on the one hand, a diminution in the quantity of azote, and on the other an increase, there are two ways of considering these results. We may either regard the diminution as attributable to absorption unaccompanied by exhalation, and the increase as arising from exhalation unaccompanied by absorption, or we may regard both functions as acting together, and attribute the diminution or increase to the predominance of the one or the other. The question which of these views is correct, cannot be decided by direct experiment, for we can never observe in the same experiment, that both absorption and exhalation of azote take place at the same time. We must then have recourse to indirect methods, by which, however, the same certainty can be obtained. Let us suppose the case of an animal, giving as the result of an experiment, equality in the inspired and expired azote, or differences so slight that they might be disregarded. On the hypothesis that the result is due to the equal performance of the two functions, there would be a certain means of destroying the equilibrium. We cannot, indeed, prevent the animal from exhaling azote; but he may be placed in such conditions that he could not absorb it, except in quanties so small as not to affect the result.

This was the object of the enquiry in which I engaged, after I had presented to the Academy of Sciences the paper on respiration already mentioned; but my researches were scarcely wanted. Experiments on this subject had already been made, though with very different views. Allen and Pepys performed them with the greatest exactness. The results are unequivocal, and might easily be foreseen after the views which I have detailed.

The apparatus need not be described; suffice it to say, that the animal was placed in such conditions that its respiration was nearly the same as that which it performs in the atmosphere, by keeping up a change of air, by a constant and uniform current.

These authors placed a guinea-pig in their apparatus, in which the animal was as well as in the open air. They found after the experiment, that the quantity of azote was sensibly the same as before. This result, in conformity with others, persuaded them that the azote undergoes no alteration in free and natural respiration.

In the same apparatus, instead of azote, they employed oxygen, which was not absolutely pure, but which contained 5 per cent. of azote. They placed in it an animal of the same species, and maintained a current of the gas as in the preceding experiments. The animal appeared in good condition. From the result it appeared that the exhalation of azote was considerable, and it was impossible to attribute the excess of azote to the quantity in the lungs at the commencement of the experiment; for the volume of this gas expired considerably exceeded that of the animal. The authors were led to believe, that the exhalation was due to the extraordinary circumstance of the respiration of oxygen; but it takes place equally in atmospheric air, as is proved by the results which I have obtained, and by the facts stated by M. Dulong. It is true, the quantity is variable; but there can be no doubt, that this exhalation is a natural phænomenon.

Let us suppose, that an animal is made to inspire an artificial air, composed of oxygen and hydrogen, in the same proportions as those of atmospheric air. According to the views above stated, it may be foreseen, that in consequence of the absence of the azote, the exhalation of this gas will be very evident, and that the hydrogen will be absorbed as all other gases are. The experiment has been made by Allen and Pepys, with the same precautions as in the preceding cases; and the results have been precisely similar to those deduced from the views above-mentioned. The exhalation of azote was so great as to exceed the bulk of the animal, and there was a considerable absorption of hydrogen. Here is a proof that the two functions are performed at the same time, and the key to the observations above recorded, that in different cases, we may find equality, excess, and loss in the azote expired, when compared with that inspired.

Section IV.—*On the Production of Carbonic Acid in Respiration.*

There are only two essentially different ways of considering the production of carbonic acid in respiration. According to one, the oxygen of the inspired air enters through the air-cells of the lungs into contact with the blood in the lungs, unites with a portion of the carbon of this fluid, and thus forms carbonic acid.

According to the other view, the oxygen which disappears is entirely absorbed, and replaced by carbonic acid.

Lavoisier was fully aware of this, and observes in his first memoir on respiration: "On this subject I find myself led to two equally probable suppositions, between which experiment has not yet enabled me to decide. In fact, from what has just been stated, we may conclude, that in respiration one of two things happens; either that the highly respirable portion of the atmospheric air is converted into the aeriform acid of chalk (carbonic acid gas) in passing through the lungs, or else that an exchange is made in those organs. On the one hand, the respirable part of the air is absorbed; and on the other, the lungs substitute for it a nearly equal volume of the aeriform acid of chalk." Notwithstanding the perfect impartiality with which he expresses himself with respect to the probability of these two modes of viewing the formation of carbonic acid, he avows his belief, that both modes operate in the act of respiration. But as he proceeded in his chemical researches on the formation of carbonic acid by the combustion of carbon, and on the heat produced by it, and continued his physiological enquiry on respiration, it was difficult not to perceive the analogy between these two orders of phænomena, and he therefore preferred the former hypothesis, viz. that the carbonic acid is formed in the lungs by the combination of the oxygen with the carbon of the blood. He had not finished these labours on respiration, when he was cut off by a premature and melancholy death. Had he lived, he might have sought to determine by experiment, as he intended to have done, what was the true hypothesis. That which he adopted explained all the known facts connected with respiration, and had the rare advantage of accounting in a satisfactory manner for a most important function, the production of animal heat.

It is remarkable that Spallanzani, whilst he thought that he was adopting the opinion of Lavoisier, held the opposite one, and sought to establish it by experiments, which, had they been exact, would have upset the generally received doctrine. These experiments were not published till after the death of Spallanzani, when M. Senebier gave a translation of the unpublished manuscript. The facts in question, although published in 1803, in a work which was generally known, remained as it were buried, since they had no influence on the views which were taken of the phænomena of respiration, and led no one to undertake a series of experiments to settle the question.

There is only one mode of deciding by experiments between two hypotheses, namely, that employed by Spallanzani. It is evident that if the carbonic acid results from the combination of the oxygen of the air with the carbon of the blood in the act of respiration, it will not be produced in the case of an animal respiring a gas which contains no oxygen. It is unnecessary to add, that regard must be paid to the oxygen and carbonic acid which the lungs may contain when the animal is subjected to the experiment.

There is a difficulty, however, in making choice of a suitable species of animal for this experiment. The quantity of pure azote or hydrogen which man can inspire without danger, is too small to admit of any conclusion being drawn from experiment on human beings. The same remark is applicable to other warm-blooded animals.

On the other hand, if animals are selected from distant classes, like the invertebrata, from the mollusca for example, as was done by Spallanzani, we feel little disposed to admit as a general phænomenon the results of experiments made upon beings of so inferior an order, whose organization appears so different from ours.

Among the cold-blooded vertebrata which perform atmospheric respiration, the batrachians are almost the only animals which unite the necessary conditions, viz. the capability of living for a considerable time without oxygen, the power of producing respiratory movements when so circumstanced, and the possession of lungs of small capacity. But the batrachians do not possess these advantages at all seasons. It has been shewn in an early part of this work, that both a high present temperature and the influence of a continued previous exposure to a high temperature greatly shorten the time which these animals can endure the privation of oxygen ; nor can they at all seasons and in all media perform the respiratory movements in the absence of oxygen. With regard to the lungs, their organization renders them extremely well adapted to experiments of this nature; for by pressing the flanks the air which they contain can be expelled previously to introducing the animals into hydrogen.

I made my first experiments in the beginning of March, when I knew that frogs could live sufficiently long in hydrogen to afford me a satisfactory result. Vessels were employed, similar to those described in page 215. That the hydrogen might be obtained as pure as possible, it was disengaged by means of zinc, sulphuric acid, and water, and, previously to collecting it, I passed it through a strong solution of caustic potass, and it was collected over boiled water, that there might be no admixture of atmospheric air. In addition to all these precautions, it was proved, by means of analysis, to contain neither carbonic acid nor atmospheric air, and its passage through the potass had deprived it as much as possible of any adventitious odour.

The ball was filled with 153 centilitres, or 93 cubic inches of this hydrogen, and was placed over mercury. A frog was introduced upon a partition of wire gauze, supported by a stem fastened to the lower aperture of the tube. The animal performed, for a long time, very ample and very regular respiratory movements. They however declined, and ceased before the end of the experiment, which lasted 8h. 30m. The animal, although it had ceased to move, was not yet dead. Exposed to the air, it recovered some time afterwards. The comparison of the volume of gas, before and after the experiments, evinced that gas had been exhaled. A portion of the air in the ball was afterwards analysed in a graduated tube, with a solution of caustic potass, and found to contain a notable quantity of carbonic acid. The quantity of carbonic acid contained in the hydrogen in which the animal had breathed, amounted to 2.97 centil. or 1·8 cubic inches, which is nearly equal to the bulk of these animals. Now this could not have proceeded from the air contained in the lungs; for, in plunging the animal under mercury in order to introduce it into the apparatus, care had been taken to compress the flanks so as to expel the air. This compression may be carried so far as to push the lungs into the throat where they may be seen absolutely free from air. Besides, these organs are so small that the quantity of carbonic acid which they can contain, would not be sensible to ordinary eudiometers, when so large a volume of hydrogen is employed as was used in the experiment. The carbonic acid was therefore entirely the product of exhalation.

I could scarcely doubt the accuracy of this result; but for greater satisfaction, it was repeated several times, with the most scrupulous attention to the exactness of the measurement; and these repeated trials clearly proved, that these animals placed in pure hydrogen, exhaled carbonic acid in variable quantities, according to the individuals and the duration of the experiment. Sometimes the quantity bore a considerable proportion to their bulk, at other times it equalled if not surpassed it.

As I advanced in this verification, it happened, as I had foreseen in commencing these researches, that the duration of the experiment necessarily diminished from the alteration in the constitutions of the animals. They were begun on the 1st of March. The temperature was then 10° cent. or 50° Fahr., and although it varied in the course of the experiments, it scarcely ran higher during that time. The animals at first lived at least eight hours in hydrogen; but the period ere long sensibly diminished, and at length was reduced to half, shewing the influence of the continued operation of that temperature in changing the winter constitution. But another phænomenon, no less remarkable, is that which is presented by the organs which perform the respiratory movements. When these animals are under the influence of the cold season, they execute the respiratory movements in an atmosphere of hydrogen with the same force and the same continuity as in atmospheric air. They preserve enough of the constitution acquired during winter to breathe in hydrogen in the same manner in the beginning of spring, but as the season advances, they lose that power, and are then affected in this gas nearly as they are in water at all seasons of the year, that is, as soon as they are introduced into it, the respiratory movements cease or become very rare, although they may live for some hours. They therefore then become ill adapted to experiments of this nature from the shortness of the duration of their life, and the cessation or rarity of the respiratory movements. They, however, still continue to produce carbonic acid through the medium of the skin; a fact which I took care to ascertain.

I wished to extend these researches to other vertebrata, and hoped to obtain satisfactory results in the class of fishes. My previous researches on the duration of the life of fishes in water deprived of air had taught me, that the species known by the name of golden-fish, *cyprinus aureus*, was the best adapted for experiments of this kind, since they have the power of living longer than other fresh-water fish in this liquid deprived of oxygen. Although the season was not very favorable, it being in the spring, after I had completed my experiments upon frogs, I nevertheless undertook the enquiry with this species of fish. I put two of them, of small size, into a similar vessel to that which I have before described, and containing pure hydrogen. They were supported, like the frog, by a partition of wire-gauze. Another fish of the same size was placed in a vessel of the same form, but of smaller capacity. The animals in both vessels performed respiratory movements, which consist, as is known, in the beating of the gills, but these movements were weaker, less frequent, and less regular than in aerated water or common air. The experiment lasted 5h. 5m. One of the fishes still exhibited respiratory movements: but they had ceased in the others. Fishes have, like the batrachians, the power of living pretty long after the cessation of the respiratory movements; so that I did not take them out as soon as I ceased to perceive motion. On analyzing a portion of the air respired by the fishes in each vessel, I found that they had produced carbonic acid in the hydrogen.

After having obtained this result with animals selected from two classes of the vertebrata, I descended in the scale and proceeded to the examination of animals without vertebræ. I took the same species of snail that Spallanzani had employed, the *helix pomatia;* I introduced two of them into the tubulated globe, containing 147·5 centil. or 90 cubic inches of hydrogen. Care must be taken that they are drawn into the shell at the commencement of the experiment, in order that no air may be left between that covering and the body.

Placed in the ball and supported on the wire-gauze, they were not very eager to come out of their shells, but ere long they did so, and even crawled about in the vessel. What was most remarkable in this experiment was its duration; although it was made in April, they were seen, 24 hours after their introduction, to exhibit themselves and move in the globe as if they had been in atmospheric air. Their motions were not constant, nor are they so in the atmosphere. I left them in the hydrogen for 48 hours, and although they then had little or no motion, they were still alive.

The measure of the volume of air, before and after the experiment, shewed by its increase that there had been an exhalation of gas. The nature of a portion of this gas could scarcely be doubted after what we formerly stated, and analysis enabled us to discern the presence of carbonic acid as Spallanzani had previously found. The long period of the experiment, during which these animals shewed sufficient activity to allow me to believe, that the function of exhalation had not been much controuled, led me to hope for a considerable quantity of carbonic acid in proportion to their bulk. In this I was not disappointed. I found in the hydrogen 2·79 centil. or 1·7 cubic inches of carbonic acid, which is about equal to their bulk. I obtained a similar result on repeating the experiment.

We may here stop to enquire, whether the phænomenon presented in the above experiments must be restricted to those species exclusively on which the experiments have been tried; or whether it may not, from analogy, be extended over the animal kingdom generally.

There will, probably, be no hesitation in applying to other reptiles the results of the experiments upon the frogs, and in making similar concessions in regard to fishes in general, as well as to the animals without vertebræ. But some may restrict the inference to these limits, and not admit of its application to mammalia and birds, on the plea that these animals are of a superior order, and distinguished from those above-mentioned by the great quantity of heat which they produce. In reply it may be observed, that the phænomenon in question is so fundamental in the animal economy, that it cannot be supposed to be essentially different in mammalia and birds from what it is in other animals; that the power of producing heat is common to all; that the high temperature which seems to characterize the mammalia and birds does not belong to them exclusively, since examples of it are found among insects; that, on the other hand, among the mammalia themselves there are species, which at certain periods, present the principal phænomena of cold-blooded vertebrata: such are the hibernating mammalia in autumn and winter; and lastly, that a great number of non-hibernating mammalia and birds, in the early periods of their life, shew, as far as the phænomenon of heat is concerned, a strong resemblance to cold-blooded animals. I had no doubt of the validity of the above arguments, yet, as I was desirous of giving complete satisfaction, if possible, by direct proofs, I hoped to be enabled to do so by availing myself of the power which certain new-born species of mammalia have of living for about half an hour without the contact of the air. These species appear to combine the qualities requisite for the success of the experiment. When they are deprived of air, the respiratory movements are not suppressed, but they are rare. After two or three minutes, the voluntary motions cease, and are succeeded by others which are involuntary, consisting in strong respirations accompanied by yawnings, and flexions of the trunk. These movements are repeated about every minute, so that if they live an hour they may furnish about thirty inspirations. In the case of reptiles, such a period would be much too short, from their languid power of producing carbonic acid; but the mammalia, although they produce less carbonic acid in the early periods of their life than they do afterwards, furnish sufficiently more than reptiles to compensate for the short duration of the experiment, and the limited number of inspirations. The smallness of their lungs is another advantage without which, indeed, the experiment ought not to be attempted. I made trial of it upon a kitten, three or four days old. I employed the same apparatus with 146 centilitres of hydrogen. It performed movements, at various intervals, for 19′ only; which gave, after the cessation of voluntary motion, about as many inspirations. I drew it out four minutes after, and when the apparatus was completely cooled, I found a very sensible quantity of carbonic acid, being 1·96 centil. or very nearly 1·2 cubic inches. Now it became a question to compare this with the capacity of the lungs. I removed them from the animal with the trachea; and then inflated them as much as possible, which distended them beyond their natural bulk; I then introduced the air which they contained into a graduated tube. It amounted to only four-fifths of a centilitre, or ·48 cubic inch. Now, supposing that all the oxygen of this bulk of air had been converted into carbonic acid, which is an exaggerated supposition, there would then have been only the fifth of this bulk of carbonic acid, or 0·16 centil. which is about 0·97 cubic inches, whereas we found 1·96 centil. or about 1·2 cubic inch.

In applying to the process of respiration in atmospheric air, all that can be fairly inferred from the preceding experiments, we conclude, that at least a portion of the carbonic acid produced, is not the result of the immediate combination of the oxygen of the air with the carbon of the blood in inspiration, but is the product of exhalation.

We have now to ascertain whether what is true of a part is true of the whole; whether one portion of the carbonic acid is exhaled and another formed entirely in the lungs from the oxygen of the air and the carbon of the blood; or whether it is entirely the product of exhalation, since both suppositions are possible.

If, when an animal breathes in atmospheric air, a part of the carbonic acid is exhaled, and the other formed at once in the lungs by the presence of oxygen, it follows that in making it breathe from hydrogen, the quantity of carbonic acid produced will be reduced to that which he exhales, and that it will be less than the quantity produced in atmospheric respiration by the whole portion which is supposed to be formed in the lungs. Spallanzani compared the production of carbonic acid by snails placed in hydrogen with that of others in atmospheric air, and obtained this remarkable result, that they produced at least as much carbonic acid in the former case as in the latter. I shall give the result of my own experiments made with vertebrated animals. Here the choice of the species is more confined than in the preceding experiments. In them it was sufficient that the animal breathed, no matter how, but without regard to the manner, provided it was sufficiently long to give assurance of the exhalation of carbonic acid. Here it is necessary in addition, that the animal should breathe with the same rapidity and to the same extent in hydrogen as in atmospheric air, which never can take place in warm-blooded animals, whatever be their age or their species. It is only the cold-blooded vertebrata which are at all adapted to it, and even among them the number of species, and the season at which they have this power, are very limited.

At the temperature of 18° cent. or 64° Fahr. in July 1821, three frogs, each placed in a tubulated glass globe, as above described, containing 74 centilitres, or 45 cubic inches of atmospheric air, after a confinement of twenty-four hours, gave on an average, 2·57 centil. or 1·56 cubic inch of carbonic acid.

The same experiment repeated upon three individuals of the same species in October, the air being at 14° cent. or 51° Fahr. gave in the same time, about the same quantity, viz. 2·77 centil. or 1·69 cubic inches. These results differ little from the average.

When formerly treating of the subject of the respiration of frogs in hydrogen, we mentioned that we had obtained 2·79 centil. or 1·7 cubic inches of carbonic acid. The temperature was nearly the same; but it is remarkable, that this quantity of carbonic acid was exhaled in 8h. 30m.; whilst in atmospheric air we find a similar quantity at the end of twenty-four hours. I shall not, however, insist upon this difference in favour of exhalation in hydrogen ; I shall be satisfied with concluding from this comparison, that the quantity of carbonic acid which these animals exhale in hydrogen, under favourable circumstances, is not inferior to that which they produce in atmospheric air, and that consequently when they breathe in the atmosphere, carbonic acid is not formed at once in the act of respiration by the combination of the oxygen of the air with the carbon of the blood, but is entirely the product of exhalation.

This result with frogs is so completely in accordance with that obtained in Spallanzani's experiments with snails, that I thought it needless to repeat his experiments.

Now, as the fact in question has been verified on the one hand upon the cold-blooded vertebrata, and on the other hand upon the mollusca, and as the comparison cannot be established by experiments upon warm-blooded animals, nothing appears to me to prevent our admitting the principle as general.

Some of these experiments afforded fresh opportunities of confirming the exhalation of azote as stated in the preceding section. In some instances it amounted to five or six per cent., considerably exceeding the volume of the animals. The absorption of a no less considerable portion of hydrogen was also noticed.

The above experiments only prove the fact of the exhalation of carbonic acid. From what source does this gas proceed ?

It may be said that it proceeds from the blood, the common source of all the secretions, or it may be supposed that it proceeds only from the air with which the tissues may be impregnated.

The latter supposition is certainly possible, but is it equally probable ? An animal is placed in hydrogen, and when the conditions are favourable, it is seen to perform respiratory movements to the same extent and frequency as if it were in atmospheric air. If we suppose a vertebrated animal having atmospheric respiration, the hydrogen which he respires becomes loaded with carbonic acid, which, we are to suppose, it does not produce in the same manner as in atmospheric air; also, that its skin, which furnishes carbonic acid in both of the gases, does not furnish it in the same manner, but that its tissues, which are impregnated with this gas, disengage it as soon as it is in contact with the hydrogen; whilst, when placed in atmospheric air, it wholly retains the gas in its tissues, and, notwithstanding, produces an equal quantity of carbonic acid, by the contact of oxygen with the blood at the surface of the body.

This supposition thus traced to its consequences seems to refute itself; we shall therefore conclude, that both in hydrogen and in atmospheric air, the carbonic acid is due to exhalation, and that it proceeds wholly or in part from the blood.

Here the question presents itself, whether the carbonic acid is wholly formed in the general circulation, or whether it is produced only in the lungs and the skin, where it is exhaled as it is produced.

There are well attested facts, which prove that carbonic acid exists in the mass of the blood: such are the results obtained by Vauquelin, Vogel, Brande, and Sir Everard Home. The experiments of Vauquelin are, I believe, unpublished; but for a number of years he has shewn in his lectures, that blood placed in hydrogen disengages carbonic acid.

Section V.—*General View of the Alterations of the Air in Respiration.*

The oxygen which disappears in the respiration of atmospheric air is wholly absorbed. It is afterwards conveyed, wholly or in part, into the current of circulation.

It is replaced by exhaled carbonic acid, which proceeds wholly, or in part, from that which is contained in the mass of the blood.

An animal breathing atmospheric air also absorbs azote; this is likewise conveyed wholly, or in part, into the mass of the blood.

The absorbed azote is replaced by exhaled azote, which proceeds wholly, or in part, from the blood.

Here are four fundamental points:

1st. The absorption of oxygen which disappears.

2d. The exhalation of carbonic acid which is expired.

3d. The absorption of azote.

4th. The exhalation of azote.

The two first relate to the oxygen, the two others to the azote.

According to this view, respiration is not a purely chemical process, a simple combustion in the lungs, in which the oxygen of the inspired air unites with the carbon of the blood, to form carbonic acid, to be expelled; but a function composed of several acts. On the one hand there are absorption and exhalation, attributes of all living beings; on the other the intervention of the two constituents of atmospheric air, oxygen and azote.

This view is not a preconceived idea, but a result to which we have been necessarily led by a multitude of facts.

It exhibits to us animated beings drawing from the composition of the atmosphere two of their constituent principles.

It furnishes us with numerous inferences, several of which are supported by facts already received in science.

Thus the oxygen which disappears being absorbed, and the carbonic acid exhaled, the relative proportions are necessarily variable, from the nature of the two functions which must vary in the extent of their action. The fact is beyond doubt. They may vary in three ways. 1. The carbonic acid may be expired in smaller quantity than the oxygen which disappears; 2. in equal quantity; 3. in excess. The first is the ordinary case; the second is supported by the experiments of Allen and Pepys; the third, if it is not yet established, will probably be so hereafter. I might even say that it is so already, when we revert to the experiment of Allen and Pepys, relative to respiration in factitious air, composed of oxygen and hydrogen. The same observation applies to azote absorbed and exhaled.

Let us return to the oxygen, and consider what becomes of it in the system. When it is absorbed and carried into the blood, there is every reason to believe, that it contributes to the formation of carbonic acid. But the experiments which I have already detailed prove, that it cannot be the only source of the gas contained in the blood.

Since we have shewn, that certain species of animals can exhale in a given time, as much carbonic acid in hydrogen, as in atmospheric air, there must be one or more subsidiary sources for the carbonic acid contained in the blood. It is easy to point out one. We know, from the researches of Jurine, Chevreul, Magendie, and others, that this gas exists in almost the whole extent of the alimentary canal. We cannot but admit, that it is formed in the process of digestion. It is in contact with almost the whole mucous surface of the alimentary canal, and a part must be absorbed. If any doubt of this were entertained, cases might be cited in which water impregnated with carbonic acid, and drunk in sufficient quantity, has produced symptoms of asphyxia. Doctor Desportes has communicated observations on this subject to the Royal Academy of Medicine.

With respect to the oxygen which is to contribute to the formation of the carbonic acid contained in the mass of the blood, one of two things must happen. It enters into combination either suddenly or slowly. In the latter case there will be oxygen in excess, circulating in the mass of the blood. This pure oxygen will therefore be subject to exhalation, which will take place in the organs adapted for giving passage to it, as happens in fishes, in the air bladders of which animals oxygen is found. I propose following up this subject, and examining different kinds of blood, in conjunction with M. Dumas.

CHAPTER XV.

APPLICATIONS.

As examples of the applications resulting from the general conclusions which we have established, let us first take some facts regarding the power of producing heat.

It has been shewn, that this power may vary considerably in the same individual, in health; much more must we expect, that it will do so in a state of disease. Let us deduce from the facts formerly laid down, what may happen in those cases in which the power of producing heat is reduced below the type of health.

I. There are two principal forms, as we have formerly shewn, in which this state presents itself: a successive diminution in the activity of the principal functions, producing torpor; or, on the contrary, an increase in the rapidity of the movements of respiration and circulation. The first case is that of hybernating animals; the second that of warm-blooded animals not hybernating. Without entering into an examination of the conditions which determine these forms, there are many reasons for believing, that they may take place in adult individuals of any class among warm-blooded animals. There is too great a diversity of structure in warm-blooded hybernating animals to believe, that there is only one peculiar organization which is susceptible of presenting these phænomena. Structure, no doubt, exerts some influence on the facility with which this state of hybernation is produced, and on the period of its duration; but no organization would appear to be absolutely incapable of it. Constitutions may so change by a concurrence of circumstances, and the continuation of their influence, that a similar change in animals, which we have not hitherto observed in that state, is by no means impossible. The species of warm-blooded animals, known to be hybernating, are not so necessarily. They may cease to be so. There are individuals among them which do not become torpid in the season of hybernation; a fact frequently observed in the domestic state. It is sufficient in this case, that their power of producing heat should be increased; a power, in all animals, susceptible of varying between very distant limits. This change can even be produced at pleasure, in some, by suitable food, and a graduated temperature.

Let it not be supposed, however, that the principal phænomena of hybernation, in animals habitually susceptible of it, are only determined by a certain reduction of the external temperature. I have observed in many of those animals, that sleep, in summer, reduced their temperature considerably, that their respiration was likewise diminished, and that they became torpid; only their torpor was not so profound as in other circumstances. This effect of natural sleep is so clearly connected with the power of producing heat, that it is in general most strongly marked when this power is the least. Bats and dormice afford a striking illustration of this. Although this modification of function is essential to the production of the phænomenon, I am far from contending that it alone is always sufficient. The state which characterizes hybernation may, and does take place, without being preceded by a reduction of external temperature. The influence of external cold is inversely proportional to the power of producing heat. The temperature of summer, although very high, is commonly very inferior to the usual heat of the mammalia and birds. The individuals in which the power of developing heat is feeble, will, in summer, undergo refrigeration in proportion to the feebleness of this power; a circumstance which takes place when they sleep. Observe, that their sleep, at this period, is perfectly natural, since it is not determined by external conditions, but is the necessary sequel to wakefulness. Now, as the foregoing observation respects different genera of mammalia, and the connection between the two phænomena is in them exceedingly well marked, we may conclude, that the state of *natural* sleep is in general accompanied by a diminution in the power of producing heat. I say, *natural* sleep, to indicate the most ordinary form of this state, since modifications occur which change the relation. It follows, as a consequence of the relation between the two phænomena, that the external causes of refrigeration must take a stronger hold of the system in the state of sleep. This explains why a damp and cold air, or a dry and piercing air, which is borne without inconvenience when the individual is awake, even without the aid of exercise, may be hurtful during sleep. It may, in addition be remarked, that the effect of exposure to cold during sleep, must necessarily vary according to the power of producing heat.

Natural sleep, in many species of hybernating animals, merits the denomination of *lethargic sleep*, from the remarkable diminution of temperature, respiration, and circulation, as well as of the external motions and excitability of the senses. It differs in intensity according to individuals and species. We have shewn the modification of the constitution which has the greatest influence in this respect; so that it will be easily imagined, that changes of this kind may take place in man, which will render his sleep lethargic, which state, however, is not to be confounded with the effect of disease or accident, such as privation of air or exposure to noxious gases. Instances of the lethargic sleep here alluded to, are to be found in medical works and are apt to be regarded as fabulous, but my own experience has convinced me, that such cases do occur.

II. We have pointed out another order of phænomena, observable in warm-blooded animals, in which the production of heat is feeble. We have seen, that in the early periods of life this constitution is common to all, that they differ in the degree of energy of this power, so as to form two groups in each class of the warm-blooded vertebrata. Those which produce the least heat, are commonly found in external circumstances which supply it, and which maintain their health; but as soon as they are withdrawn from them, they present the following series of symptoms; a lively sensation of cold, or an appreciable reduction of the temperature of the body, with an acceleration of the circulation and respiration. We have proved, that these symptoms arise from their not producing sufficient heat, and that the external temperature does not supply this defect, although they may be exposed to the warm air of spring or summer.

The cold and shivering with which they are seized, notwithstanding the warmth of the weather, and the accelerated motion of their respiration and circulation, present so lively an image of the cold stage of an intermittent fever, that we are led to admit a connexion between the two orders of phænomena. We know that young animals present these symptoms, because their power of producing heat is feeble; now, if this power in man undergoes diminution to a sufficient degree, it is natural that the same phænomena should follow. Let us now inquire, whether this function in man is really thus altered in the cold stage. In the first place the lively sensation of cold which he experiences is a strong presumption in favour of that opinion. But there are facts which clearly prove it. If, in this stage, the patient be subjected to cold affusion, such a degree of cold is produced as may risk the loss of life. (See Dr. Currie on Cold Affusion.) Now, the exposure to a relatively low temperature, or the application of cold, is the very means employed in the course of this work to estimate the power of producing heat in animals, we must, therefore, draw the same conclusion from the employment of this means in the present case.

Let us proceed now to the consideration of the temperature of the body. Respiration and circulation have no longer their usual rhythm; their movements are accelerated. From the facts detailed in Part IV. Chap. X., it may be seen how accelerated respiration tends, more or less, to re-establish the heat of the body, according as the means employed by the animal, in the state of health, for the production of heat, are more or less feeble; and that from these extraordinary effects, result different states of the temperature of the body; that it may fall, or remain stationary, or rise above its original limit.

Let us take the favourable cases, or those in which the acceleration of respiration and circulation re-produces sufficient heat. If the movements which have developed it have not been too much deranged, a time will arrive when they will cease, inasmuch as the cause from which they originated no longer exists, for the external warmth supplies, in a great number of cases, as we have formerly shewn, the deficiency in the production of heat.

It might be thought that the suspension of the stage of which we have spoken must necessarily be momentary, and that it cannot be prolonged beyond the time that is necessary for the heat to be dissipated; but we have shewn in Part IV. Chap. IV., that the application of heat in those cases, when the system does not develope it sufficiently, produces effects which continue for a longer or shorter period beyond the time of their application, by increasing the power of producing heat by the usual means pertaining to health; a distinction exceedingly important, for the energy of those means is not measured in this case, by the greater or less activity of the movements of inspiration and circulation, as we have formerly shewn.

There will then be an intermission of greater or less length, according to the degree to which the power of producing heat shall have been injured.

The extreme cases of this intermission are found, on one hand, in the restoration to health after a single attack; and on the other hand, in the *febres intermittentes algidæ*, described by Torti, in which this power is so impaired, that the patient dies in the cold stage, at the end of two or three accessions, if suitable remedies are not employed.

III. Since the application of external heat tends to re-animate the power of producing it, this means may be substituted for the extraordinary efforts of the system, which tend to the same object. It may be done either to prevent them, or to shorten their duration.

This means will have more or less success, according to the measure and mode of application, and the degree of danger of the case. A striking example of it is to be seen in the employment of the vapour-bath by M. Chomel, in a case of intermittent fever. (*Nouv. Journ. de Medicine,* t. x. p. 270.)

This mode of applying heat has a remarkable advantage over many others, which will be readily understood on reverting to the facts mentioned in Chap. VIII. of Part. IV. Compare, for example, the effects of a liquid-bath and those of a vapour-bath, both raised to a high temperature. The heat in the latter case will be so much better borne as recourse may be had to the vivifying action of the air. In the liquid-bath, the action of the air is suppressed upon almost the whole extent of the skin ; in the vapour-bath, on the contrary, it is in communication with its whole surface ; so that, *cæteris paribus,* the heat of steam will be borne much longer than that of water.

In general, whatever be the temperature of liquid water, one of the effects of the bath results from the limitation of the communication of air with the system : hence, there are many persons who experience a difficulty of breathing, which essentially depends upon that cause. The same may be said of the faintness which results from long continuance in the water, and it will readily be conceived, that these effects will vary in different individuals, according as the pulmonary respiration has a greater or smaller extent. (See Chap. IV. Sect. II. Part. I.)

IV. In our inquiries into refrigeration in dry and humid air, (Part IV. Chap. XIV. Sect. V.), we saw that these two modes of refrigeration were different ; but that they tended, in a great number of cases, to produce the same physical effect, that is, the same reduction of the temperature of the body. It is evident, that refrigeration in dry air is produced by a greater evaporation ; but the mode of refrigeration in humid air is not so clear. We observed, that philosophers had not determined the relative quantities of heat abstracted by dry air and by watery vapour; but, whatever be the result of such researches, it will not furnish all the elements necessary for explaining the mode of refrigeration of animals in humid air; for supposing it established, that watery vapour abstracts more heat than dry air, it will be conceived, indeed, that this mode of physical cooling may be equivalent, in a great number of cases, to that which results from a greater evaporation in dry air; but observation of the refrigeration in both cases shows that there is another element. In the experiments cited above, in which the reduction of the temperature of the body was the same in both cases, or even greater in dry air, the animals appeared to me to suffer more in humid air: I judged by the shivering and the acceleration of respiration. I am aware that such signs do not furnish rigorous tests; but they have led me for some time back to pay peculiar attention to the respective sensations, which dry and humid air produced on myself and others in cold weather.

Humid air, at an equal or even superior temperature, produces a peculiar sensation of cold which differs, not in its intensity, but in its nature. It is more profoundly felt, and seems to penetrate the whole system, and particularly disposes to paleness and shivering. By these characters, I could not mistake a species of refrigeration, which consists in the diminution of the power of producing heat.

In dry air, on the contrary, a sensation is experienced, which is called *a sharp cold,* and which designates rather the nature than the degree of the sensation; moreover, it is superficial, and when the reduction of temperature is not too great, an increase of activity is experienced, the skin reddens; and in extreme cases, the limbs have a tendency to stiffen, instead of yielding to their irregular and involuntary motions, which constitute shivering.

It may be seen, by this comparison, and by what we have stated above, that damp cold must tend to produce, in individuals whose power of developing heat is rather feeble, the series of actions which constitute the accession of an intermittent fever, especially if they are exposed to that influence during sleep. The confirmation of this will be found in the study of medical topography. In a great number of cases, these fevers are ascribed to marsh-miasmata in fine weather, but others occur in places and at seasons at which the atmospheric constitution which we have mentioned predominates.

V. We shall now speak of the effects of climate, some of the elements of which subject will be found in Chap. V. Part IV. relating to the influence of the seasons on the production of heat.

Since there are, in this respect, a summer and a winter constitution, we shall compare the former with that of the inhabitants of warm climates, and the second with that of the inhabitants of cold climates; but, there will be this difference, that the modification which characterizes the summer constitution in our climate, will be much more strongly marked in warm climates.

We have shewn, that here, in individuals whose constitution is suited to the climate, this modification consists in a diminution of the power of producing heat in summer, and an increase in this respect in winter; whence we conclude, that this power will be feebler in the inhabitants of warm, than in those of cold climates; and that consequently, when they change their climate, they must be, in general, less capable of supporting the cold, than the natives of the country.

If we were to judge from their sensations only, we should frequently be led into error. Many individuals coming from warm climates, are, at first, less sensible to the cold than the natives of the country; which may be easily conceived from the following experiment. If in winter, in a room of moderate temperature, the hand be held for some time in iced water, and it be wiped afterwards, the sensation of cold ceases gradually, and a feeling of heat succeeds, and this sensation is so lively, that this hand would be thought warmer than the other. But the illusion is destroyed when they are applied to each other; it then appears colder to the touch. It is really so, as can be proved by the thermometer. This is because the heat of the hand which had been cooled returns quickly; and the rapid increase in the development of heat is accompanied by a sensation comparable to that which is experienced when the heat is greater, but the development stationary.

The natives of warm countries, who most readily adapt themselves to the climate of cold regions, experience a rapid increase of the power of developing heat; and the corresponding sensation, just spoken of, will render them less sensible to the impression of cold. This state, however, does not last long; it diminishes progressively, and scarcely extends beyond two winters. But those whose constitution does not adapt itself to a change of this kind, or who do not experience it to a sufficient degree, will be exposed to all the inconvenience and all the danger, which result from the action of too cold a temperature, if they do not take the necessary means to guard against them.

On the other hand, the natives of cold countries, if they continue to produce the quantity of heat adapted to their own climate, when they remove to the equinoctial regions, would have an excess of heat which would prove injurious to them. The high temperature of this new climate, as well as its duration, tends, perhaps, to diminish the activity with which heat is produced; but the measure of that action is not always in proportion to the wants of the system; it is often too strong or too feeble, according to the constitution of the individuals.

VI. They find, indeed, in the increase of evaporation from great heat, if the air be not too humid, a cause which tempers its effects; but whose influence has been exaggerated, when it has been supposed that it could effect an exact compensation.

Conditions of evaporation may be conceived, which counterbalance certain elevations of temperature; but do these usually take place in man? Evaporation, all other circumstances being the same, is in proportion to the surface considered in reference to its extent, and its proportion to the mass. Water contained in vessels, and that of rivers and seas, is commonly below the temperature of the air, from the evaporation which deprives it of heat; and although the difference of heat, dependent on evaporation between the two media, is greater in summer than in winter, it is always slight within the limits of the temperature of the seasons and of the hygrometric variations of the air. But as animals present a surface proportionally greater, their bodies are better adapted to evaporation. They are, in this respect, more like inanimate bodies, furnishing vapour from the whole surface. If, for example, a sponge be soaked in water so as to saturate it, and its temperature be examined within the limits of the usual temperature of the seasons, it is found to vary considerably with the heat of the air, and to differ from it but a few degrees.

If now we wish to know what takes place in animals, we must choose the species best adapted to this kind of observation.

There are none which lose more by evaporation than frogs, and as their production of heat is extremely feeble, we shall disregard it. Notwithstanding the difference of their losses by evaporation in summer and in winter, their temperature follows pretty closely the variations of that of the air. Much more then will evaporation, which is less active in the other animals, be insufficient to compensate for the elevation of external temperature. It follows, that the equality of temperature of warm-blooded animals in summer and in winter, supposing the fact ascertained, is not wholly maintained by evaporation. The cause which co-operates with it, to render their heat constant or nearly free from variation in the vicissitudes of the seasons, has been elsewhere examined. (Part IV. Chap. V.)

VII. We are now led to a question not hitherto considered, viz. Does the temperature of man and of warm-blooded animals, vary according to the seasons? It had generally been believed, previously to the period of these investigations, that it was constant in the state of health and in ordinary circumstances, notwithstanding the heat of summer and the cold of winter. In order to be informed on this point, I tried a series of experiments upon the temperature of yellow-hammers and sparrows, at different periods during the course of a year. They were tried upon a great number of individuals recently taken, rather than on such as might have had their constitutions modified by confinement. I found that the averages of their temperature ran progressively from the depth of winter to the height of summer, within the limits of two or three degrees cent. The observations upon sparrows gave me the greatest difference. The average was in February 40°.8 cent. or 105°·4 Fahr.; in April 42° cent. or 107°·6 Fahr.; in July 43°.77 cent. or

$110^{\circ}\cdot78$ Fahr. I afterwards noticed the contrary course in the decline of the year.

Hence I judged, that man also would experience variations of temperature under the influence of the seasons and of the change of climate, if not to the same extent, at least within appreciable limits. Dr. John Davy, on his return from Ceylon, informed me, that the temperature of the inhabitants of that island, was higher than ours by one or two degrees of Fahrenheit, and that he had observed a similar change in the same individuals before their departure and after their arrival.

VIII. The increase of temperature of which man is susceptible in disease, without deriving it from surrounding heat, is much more considerable. Dr. Prevost of Geneva communicated to me a most remarkable instance of this. One of his patients, a boy of twelve years, was affected with tetanus, accompanied by an extraordinary development of heat. To determine the extent, he placed a thermometer in the axilla; and found it 35° R. equal to $43^{\circ}\cdot75$ cent. or 110·75 Fahr. Supposing that the original temperature of the child was $36^{\circ}\cdot756$ cent. or 98·2 Fahr., which is above the average for that age, here will be an elevation of 7° cent. or $12\frac{1}{2}$ Fahr.

IX. It will be admitted, that it is important to moderate the excess of heat, not only in such extreme cases, but in others in which it is not so high, whether it proceeds from without or from within. There are circumstances in which heat increases by a salutary effort of nature, and we have given examples of it above. Even then these efforts are frequently irregular, and art must interfere to moderate them. Often the excessive production of heat has no salutary tendency when it is necessarily still more important to moderate it.

The most powerful means furnished by external agents, consists in the application of water of a suitable temperature. It is evident in the first place, that it tends, by a physical action, to reduce the temperature of the body. It is true, that its employment cannot be of long continuance; but even when only a temporary reduction is obtained, this respite will itself be very advantageous; and the repetition of the means, applied according to the exigence of the case, will multiply the intervals. But it produces another effect, which we have mentioned elsewhere. Cold, whatever be its nature, if it be sufficiently marked, tends to diminish the activity with which heat is developed, and damp cold is, of all external means of refrigeration, the best adapted to bring about that change. This serves to explain the advantage which has frequently been derived from the use of cold water, under the varied forms of baths, *douches*, and affusions, in cases of the extraordinary development of heat.

X. When these means are not considered expedient, the other external methods of refrigeration, if they have a less speedy and powerful effect, afford some compensation in the continuance which they admit of. Thus, the sponging of various parts of the body, although they be wiped immediately, and whatever be the temperature of the water, provided it be not excessively hot, produces from the moistened surface a more abundant evaporation, whence results a salutary refreshment which may be continued indefinitely.

XI. When care is taken to maintain the adequate ventilation of an apartment, this method of refrigeration depends not only on the quantity of heat abstracted by contact, but also on the increase of evaporation. Few results of experiments have struck me more forcibly than the difference of perspiration by evaporation in a calm air, and in one that is slightly agitated.

XII. If perspiration by evaporation produces a salutary refrigeration, it produces also other effects, which, when excessive, may in many instances, be very injurious.

We have seen, in the experiments upon fishes, that by the perspiration from the gills and skin, either of these organs may become dry, although the body should lose nothing of its weight, on account of the absorption of the water in contact with the rest of the surface. (Part II. Chap. II. Sect. V.)

The intensity of this effect, capable of causing the death of these animals, directed my attention to circumstances, in which considerable evaporation from the surface of the lungs or skin, would be injurious to man.

One of the situations in which perspiration by evaporation is considerably increased, is found in the higher regions of the atmosphere, upon high mountains. Many persons experience in such situations distress and anxiety in the chest, which they refer only to the rarefaction of the air limiting the extent of respiration. This cause may have its share in the effects; but that which I have just pointed out, acts in a more extensive and more general manner.

We may thus distinguish their respective influence. The rarefaction of the air in high situations is accompanied, in fine weather, by a considerable dryness; and hence such an increase of perspiration by evaporation, not only from the skin, but also from the lungs, that the loss of water suffered by those organs will produce a feeling of distress in the chest, proportionate to the desiccation.

If, as frequently happens upon mountains, the weather change quickly, loading the air with humidity, the evaporation becomes moderate, and the distress diminishes, or ceases entirely. If it still continue, it is owing to the rarefaction of the air. The effect of evaporation is felt the first, and that which is owing to a want of air comes long after : it requires even a much greater height to produce it than one would be inclined to believe when the two sensations are confounded.

Thirst is a symptom which attends the ascent of mountains. It is sometimes intense, when it cannot be ascribed to the fatigue of exercise. It is only momentarily satisfied, even by abundant and often repeated draughts. But if the air becomes charged with moisture, the thirst at the same time disappears. Here is an example perfectly analogous to that which we have elsewhere mentioned as the effect of a partial desiccation, although the body may be furnished with a sufficient quantity of water to prevent its losing its total weight, the distribution of the liquid to the different parts not being in sufficient proportion to repair local loss. It is obvious that this influence will be very differently felt by different individuals, according to the state of the lungs.

XIII. There are great differences in the effects arising from the rarefaction of the air, depending on variety of constitution. The symptoms proceeding from this cause are to be distinguished from those of evaporation, by placing animals in conditions in which the influence of this last cause may be disregarded, as in the large receiver of an air-pump. If the vacuum be quickly made, the rarefaction acts on the respiration before evaporation or the alteration of the proportions of the air can produce sensible effects. We see weakness of the body and acceleration of respiration inducing hurry of the circulation. These symptoms are not in this case complicated, as in the ascent of mountains, with those which proceed from excessive fatigue and other causes.

We have now to determine, if those symptoms are really owing to the rarefaction of the air which limits respiration. This fluid cannot be rarefied in the air-pump without at the same time diminishing its elasticity, which is equivalent to a diminution of pressure, and the phænomena which the animals present may arise from either cause, or from both combined. Let us suppose in the first place, that each has its peculiar phænomena, and let us see what results from limited respiration in the cases in which there is no diminution of pressure. If a warm-blooded animal is placed in a limited quantity of air, whether we leave the carbonic acid which it produces, or absorb it, taking care to maintain the same pressure, the general effects observed are the same as we have referred to the rarefaction of the air. Now, as these various means act by limiting respiration, the common effects ought to be ascribed to that cause. Even though the diminution of pressure were to act in the same way, it is still true to say, that the class of phænomena which we have described, are owing to the rarefaction of the air : only their intensity would be increased by the concurrence of another cause.

There is a symptom connected with respiration limited by rarefaction of the air, which appears to me not to have attracted attention, or not to have been looked at in this point of view : I mean the disposition to vomit. In order to judge of it, it is advisable to choose those warm-blooded animals, in which vomiting is most easily excited : such as birds of small species, as yellow-hammers, sparrows, fringillas, &c. The rarefaction of the air, when carried far enough, produces this effect on a great number of individuals, and what proves that it is referable to limited respiration is, that it takes place in whatever manner the extent of this function may be limited by other modifications of the air.

It is easy now to refer to their respective causes several phænomena which have been observed in man when he is elevated to great heights, whether on mountains or in balloons. If the disposition to vomiting has been but seldom observed in these circumstances, there are, nevertheless, persons who have experienced it, and I have been convinced by their statements, that it depended on the same condition as that which produced it in the animals submitted to experiment. I have alluded to this circumstance, not to add to the number of symptoms which may manifest themselves, but to connect it with a great number of others, in which respiration is limited in different ways, as in acute or chronic congestions in the lungs, when the disposition to vomiting and vomiting itself, are frequently symptoms arising from the diminution of the communication of the system with the air.

XIV. Species and individuals vary very much in the power of supporting limited respiration. The extent to which the rarefaction of the air can be carried, without sensibly distressing the respiration of a great number of the superior animals and of men, is truly surprising; but the limits at which extreme rarefaction produces effects almost as rapid as those of the absolute privation of this fluid, are sufficiently near to allow of few differences in this respect in warm-blooded animals. The pressure at which the yellow-hammers, which I subjected to the experiment, were on the point of dying, corresponds, taking the average, to 5·31 inches of the barometer, and the average for the guinea-pigs to 3·58 inches. I quote the species which presented the extreme results.

XV. Facts connected with an excessive evaporation from the lungs, may be observed in other places besides elevated regions. In winter, when during a very sharp cold, an apartment is warmed by means of a stove, many persons experience a painful sensation in the chest. The air, in a frost, contains scarcely any watery vapour, and the heat of the stove by raising the temperature of the air increases its capacity for vapour, so that, at an equal temperature, the quantity of liquid dissipated by evaporation is much greater than in summer. It is an old custom to place upon the stove a vessel containing water, in order to remedy the uneasiness to which we have alluded; but this is commonly insufficient, from an incomplete application of the principle upon which its utility is founded. It would be necessary to produce a more abundant evaporation, in order to bring the air to the degree of humidity which renders it suitable to our constitution.

In the situation just pointed out, I suppose the heat moderate, and even then it may appear excessive, because the uneasiness alluded to is attributed to that cause.

In arid districts, effects are likewise ascribed to the heat of the air and of the wind, which arise in a great degree from the evaporation occasioned by the dryness of the atmosphere. Dr. Knox, who travelled in the interior of Africa, to the north of the Cape of Good Hope, has related to me facts which justify this opinion.

XVI. It is known that in an agitated atmosphere, not extremely humid, evaporation, generally considered, may be as great as in a calm and dry air; but, supposing two conditions of the atmosphere, in which the effects of motion in the one would equal those of dryness in the other, their respective influence upon perspiration by evaporation would not be the same. Air in motion only acts upon exposed surfaces, as the integuments of the body; those of the lungs are sheltered, and notwithstanding their communication with the atmosphere, the agitation of the air has but a slight share in the quantity of vapour which they furnish. This consideration will serve to determine the choice of suitable places for the residence of delicate persons. Those to whom the increase of evaporation from the lungs is injurious, ought to prefer an atmosphere less dry, but slightly agitated, when it is important to obtain an agreeable freshness.

XVII. In a great number of acute diseases, the skin, and a part of the air passages manifestly become dry. Now, after seeing the fatal effects which may be produced upon animals by the desiccation of either organ, the necessity of remedying it as much as possible, must be strongly felt. We have seen how insufficient drinking is: the exhalation of watery vapour to which recourse is sometimes had, produces only temporary relief, and in many cases its cure is impossible. If the atmosphere of the patient be rendered humid, by maintaining near him a sufficient evaporation of water, he will continually and without effort, breathe a vapour, which will not only arrest the desiccation of the respiratory organs, but will also tend to put a stop to that condition, by means of the absorption of which that vapour is susceptible. (Part IV. Chap. XIII.)

More active measures are necessary for the skin, in order to soak it with water to a greater or less extent, and afterwards dry the surface; a precaution in general necessary in order to procure for the patient, by immediate contact, the advantage of the vivifying influence peculiar to atmospheric air, or in other words, of cutaneous respiration. Such measures adopted with assiduity and discernment, would contribute to diminish the mortality in this kind of diseases.

XVIII. Very important considerations result from the difference of constitution at different periods of life. If the attentions which children require in climates and seasons little favourable to the preservation of their existence, were generally understood and put in practice, it would considerably reduce one of the most powerful sources of mortality affecting that age in our climate. It is not confined to children whom the misery of their parents cannot guard from the rigor of the weather, but it operates to a great extent, without being either perceived or suspected, in families enjoying affluence, and in which it is believed, that the necessary precautions are taken, because cold being relative, it is difficult from our own feelings to judge of its effects on others, and because it does not always manifest itself by determinate and uniform sensations. They do not feel the cold, but they have an uneasiness or an indisposition which arises from it; their constitution becomes deteriorated by passing through the alternations of health and disease, and they sink under the action of an unknown cause. It is the more likely to be unknown, because the injurious effects of cold do not always manifest themselves during or immediately after its application. The changes are at first insensible; they increase by the repetition of the impression or by its long duration; and the constitution is altered without the effect being suspected.

There is a general precaution which would tend to prevent these effects, and which it is sufficient here merely to point out. It is to watch the changes which may come on during health at the decline of the year, and in the course of the cold season; and, however little it may be liable to derangement, to preserve heat by a warmer clothing. If the clothing is adapted to the wants of the individual, it will contribute powerfully to guard him from the alterations dependant on the influence of the season; he will enjoy at the same time the advantage of being exposed to the open air in conditions of the atmosphere which would not injure his health.

In countries in which the cold is excessive, the feelings so strongly impress upon the inhabitants the necessity of guarding their children against it, that the particular care which they take renders this cause of mortality, perhaps, less in them than it is in temperate countries. It is sufficient, then, to feel this necessity, in order to find suitable means to meet it. These means are referable to several heads: 1, The modifications of the air to adapt it to the system. 2, The preservation of the natural heat by clothing. 3, The changes to be produced in the constitution of the individual, in order to increase his power of developing heat, so as to extend the limits of the atmospheric variations to which he may be exposed without danger.

People are frequently dissuaded from the use of warm clothing, and the external application of heat under the form of baths, by the idea that they may induce delicacy and greater sensibility to cold. This opinion is undoubtedly founded upon very general experience, and I think that the observations which I have made on this subject do not weaken it; but other facts equally well attested tend to circumscribe it within just limits, and shew us that when the system does not develop sufficient heat, the means which we have just pointed out contribute to increase the power of producing it.

Although the want of it is actually felt, the use of warm clothing is often declined from the wish to reserve it for an advanced age. But it frequently happens, that this very precaution is the cause of preventing that age from being attained.

The employment of the warm bath is dreaded because water enervates, but this effect is obviated by reducing the duration of the bath, and thus making the application of heat predominate.

XIX. It is well known to be difficult to rear children that are born long before the full time, such as those of about the sixth month of pregnancy. In general, the care which is employed for preserving the heat by means of clothing would be insufficient, as I have ascertained in the case of young animals, which are born in a similarly imperfect state. There ought to be a continued external application of heat until the body has acquired sufficient development. What I have said of childhood in general is applicable to every period of life, when the constitution from any cause approximates to the modification in question.

Although the condition of hospitals has been considerably improved, and although it might be easily shewn that a sensible diminution of mortality has been the result, yet a great number of these institutions are still susceptible of amelioration in respect to their temperature in winter. St. Bartholomew's Hospital in London, may be mentioned as an example of the judicious means employed to combine this indication with others which have reference to the salubrity of the air.

XX. Persons are often led to attribute to suppression of perspiration, effects which principally result from the action of cold upon the system; and it sometimes happens, that the perspiration is supposed to be suppressed in cases when it is really increased. We have already shewn that it is only the sweat which can really be suppressed ; which, however, does not necessarily imply the suppression of transudation. It is then diminished so far as to be insensible, but it may continue to take place.

It is not however indifferent to the system, whether the same loss in weight is occasioned principally by evaporation, or by transudation. In the first case, the liquid dissipated is nearly pure water; in the second, transudation being a secretory process, the water carries with it a notable proportion of animal matter.

Thus, considering the effects only as connected with the proportions of liquids and solids, perspiration by evaporation merely bears on the diminution of water in general, and tends to the partial desiccation of some organs important to life. Transudation, at the same time that it diminishes the total mass of water, diminishes also that of animal matter, and instead of drying the organ which is the seat of it, tends on the contrary to moisten it.

Hence, in the comparison of the effects of losses equal in weight by each process, it may be imagined, when they are considerable, how much more that, by sweat, ought to weaken.

The absorption of water equivalent in weight, may, nearly or quite, repair the loss occasioned from perspiration by evaporation; but it would be far from repairing a loss of equal weight occasioned by sweat.

These two modes of perspiration differ also in their progress. Perspiration by evaporation has a tendency to diminution, in equal and successive periods; transudation or sweat, being determined by heat, in favourable circumstances, tends, on the contrary, to uniformity within certain limits.

There is this other difference, that when the physical conditions which increase perspiration by evaporation cease, that process diminishes in proportion. This is not the case with transudation occasioned by a very high temperature. This continues to a very great degree after the application, if the heat has ceased.

XXI. When, independently of the particular modifications of the matter perspired, we wish to know the mean quantity which an individual loses by this means in the course of a day, it is not immediately obvious what assistance can be derived from the statical observations which have been made on this subject. Indeed the results vary, according to the persons who have stated them, within very distant limits, from 27 to 60 ounces per day. These differences are often attributed to differences in the constitution of the individuals, from age, climate, and unknown causes. But it happens here, as on many other occasions, that the facts, although they arise from a number of causes, which, it would appear, must occasion infinite variations, are, however, capable of presenting a result so uniform as to admit of its being foreseen.

In order to find a result in the case of perspiration which will approximate to this regularity, it is necessary to pay attention to a relation which exhibits itself in all the statical researches on the perspiration of man, continued for a long course of time.

On comparing the daily average of meats and drinks during the course of a year, with the sum of all the losses by perspiration, and the alvine and urinary evacuations, it will be seen, that they are nearly the same. It is then of importance to examine the proportion of these evacuations to each other. The proportion of urine to perspiration varies in the tables of Robinson and others, but on taking the average of these proportions, it approximates remarkably to equality, and is found to be : : 1 : 1·08.

The alvine evacuation forms but a small portion of the total loss. The mean of all the quantities eliminated by this way, in the tables to which I have alluded, is four ounces. By substracting this quantity from the sum of the meats and drinks, and taking the half of the remainder, we shall have an approximate result of the mean product of the perspiration of a day in the course of the year.

In order to judge of the degree of approximation which may be attained, by making use of these data with the mere knowledge of the sum of meats and drinks, we give the comparison of the results furnished by experience with those deduced by calculation from the preceding proportions.

Mean losses by perspiration in a day.

	Robinson. 42 yrs.	Robinson. 64-5 yrs.	Keill. 39 yrs.	Rye. 42 yrs.	Lining. 40 yrs.
By observation.	45 oz.	27 oz.	30 oz.	56 oz.	60 oz.
By calculation.	41	27	35	46	62

It may be seen from the tables of these authors, that the vicissitudes of heat and cold, when well marked, as in the countries in which they resided, tend to occasion a predominance of perspiration over urine in warm weather, and the contrary in cold. The observations of Rye, are the only exceptions; for his mean perspiration, even in winter, exceeds the urine, although it approaches to an equality.

The rule then which we have given for estimating the mean perspiration, is applicable only to the climates of which we have just spoken.

In warm climates it is probable, that the average of perspiration for the year would sensibly exceed the mean of urine. The observations of Sanctorius, though incomplete, indicate that this is the case in Italy; and *a fortiori*, will it be so in hotter countries.

XXII. We have formerly considered the effects which various modifications of air produce upon perspiration; we shall now examine certain effects of the same conditions of the atmosphere upon respiration.

The slight agitation of the atmosphere, when its hygrometric state and temperature are adapted to the system, produces such a feeling of well-being, that the chest dilates in consequence, and admits a large proportion of air. This is a phænomenon which has particularly attracted my attention, and which I have observed, wherever, from the space being extended, the air admitted a greater variety of movements. I have frequently had occasion to ascertain, that persons who have what is called delicate lungs, owe in a great degree, the difficulty and oppression which they feel, to the smallness of their apartments, a difficulty which decreases on going into a large room, or into the open air.

Whatever difference of purity may be attributed to the air of small and of large rooms, of narrow and of wide streets, of town and of country, the degree of agitation of the air has the most marked influence on the extent to which the chest dilates itself: the agreeable sensation which is experienced on breathing in the country is principally due to that cause.

We cannot be too careful in distinguishing the cases in which difficulty of breathing arises from a want of extent in the movements of the chest, from those in which it is owing to a mechanical obstruction. The means of remedying the first, are more numerous and more powerful than may be imagined; and often even when organic change exists, attention to external circumstances may afford much relief.

XXIII. There are modifications of structure, connected with respiration and circulation, which scarcely manifest symptoms of disease, except under certain external circumstances. It would be as vain to attempt, at least, in the present state of our knowledge, to reduce some of these modifications of structure to ordinary conditions, as to desire to change those which characterize a species. The art consists then in suiting the external circumstances to this state of organization. The limits within which persons so constituted can enjoy life, are more confined than in other persons; but the knowledge of these limits serves to procure for them health and even longevity. The principles deduced from the observations and experiments detailed in this work are such as to furnish applications of this kind.

In connexion with these modifications of structure, to which we have just alluded, I would refer to the 9th Chapter, Part IV., which treats of the effects of temperature upon the functions of respiration and circulation; and also notice a series of clinical observations which serve as illustrations of these principles. We owe them to M. Rostan, who has recorded them in his interesting memoir, *Sur l'Asthme des Vieillards.*

He has found, that the affection which he calls by this name, corresponds to certain organic affections of the heart, the great vessels, or the lungs. These cases are extremely numerous in the *Hospice de la Salpêtrière,* where he every year sees persons labouring under it, who enjoy, in general, pretty good health in fine weather; but who, when the heat declines in autumn and winter, come in numbers to the wards of the hospital, with the palpitations and the laborious respiration which characterize the disease.

XXIV. When a mechanical obstacle, such as an engorged state of the lungs, prevents the entrance of a sufficient quantity of air, there is another order of considerations relative to those diseases, which has been suggested to me by my researches upon animals. The great number of cases in which the engorgement of the lungs diminishes the communication with the air, directed my attention to the circumstances which determine the power of supporting limited respiration. The reader will find facts relating to this subject in Part IV. Chap. VIII.; but we shall look upon it here in another point of view. It has been long known, that the young mammalia sink less rapidly than adults, when they are entirely deprived of air. But it is also known, that this difference ceases soon after birth, and even that it is very slight between a very great number of new-born mammalia and adults. (See Part III. Chap. IV.) It is not the same with limited respiration. Long after the period at which the state of asphyxia is scarcely of longer duration in young animals than in adults, I ascertained that the former much better support the effects of limited respiration. In the repeated experiments which I have made upon the respiration of adults, in limited quantities of air, in no instance, after leaving them there until they sunk, have they been restored to life by exposure to the open air. But several young birds which had altered an equal quantity of air, so as no longer to give signs of life, recovered after they were taken out, though I have never observed this when they were entirely deprived of the contact of air as in the case of submersion.

By limiting the action of the air in a different way, it will be seen in a very sensible manner how much better the constitution of young animals is adapted for supporting respiration, than that of adults. If the chest of an adult is widely opened, the lungs collapse, and the external motions cease almost as rapidly as if the animal were immersed in water. Meantime the air which is in contact with the surface of the body, and with the lungs, visibly maintains a respiratory action, since the heart continues to beat, and the blood becomes scarlet at the surface of the lungs.

The same operation was performed upon kittens, one or two days old, some of which were deprived of the contact of the air by putting them under water: the others were exposed to the open air. The experiments were performed at the temperature of 20° cent. or 68° Fahr., that most favourable to the duration of life under water. The mean term of the life of the kittens in water was 38 minutes, that of those exposed to the air was 1h. 2m. It is to be observed, that in this circumstance, as in others, we have judged of life by external acts only, and not by the feeble motions which go on within, when these have ceased.

Hence it follows, that children in whom respiration may be limited by engorgement of the lungs, will, all other circumstances being the same, be less in danger than adults, when communication with the atmosphere may be limited in like manner and to the same degree; and as the disturbance of the system, marked by the acceleration of respiration, circulation, &c., is so much the greater as the want of air is more pressing, the symptoms of pneumonia will be more intense in adults, in cases in which the relative extent of disease is equally limited.

The facts formerly detailed prove, that the principal characteristic which distinguishes warm-blooded animals, at different periods, from their birth to adult age, is derived from their power of producing heat. We have also shewn the connexion between this power, and that of supporting the total privation of air. It is the same with limited respiration. As we have shewn, that adults may differ much in their power of developing heat, we may conclude, that they differ also in their power of supporting diminished respiration.

XXV. These considerations lead us farther. If an individual is affected with pneumonia, so far as to endanger his life by diminished communication with the air, the most urgent indication is to employ the best means to bring back his constitution to that state which would enable him to support this limited respiration. Now, although this has not been kept in view, in the treatment at all times adopted in this disease, the indication has, however, been fulfilled. In whatever manner the blood contributes to the production of heat, we cannot doubt that it does exercise a considerable influence over it. A small abstraction of blood cannot in this case produce a sensible effect, but a sufficient evacuation could not fail to diminish the power of producing heat; and keep it within the limits compatible with life. The more serious the case, the greater ought to be the abstraction of blood.

XXVI. The present state of our knowledge respecting the blood, presents new views which are intimately connected with physiology and pathology. MM. Prevost and Dumas, who have analyzed the blood of a great number of species of the cold-blooded vertebrata, and of warm-blooded animals, have found, that the proportion of water was the greatest in the cold-blooded vertebrata, less in mammalia, and at the minimum in birds; or, reciprocally, that the relative number of globules (particles) increased in the order of the preceding classes. It is evident, that if we could change the proportion between the water and the globules, (particles) we should have another means tending to approximate the constitution of the mammalia to that of the cold-blooded vertebrata.

Suppose that we have recourse to the injection of water to effect this change, it will then be found, by what we have formerly established respecting absorption, within what narrow limits this change will be confined.

In the experiments upon animals, the best adapted for manifesting the effects of the absorption of water, we saw that there was a point of saturation which they do not pass so long as their constitution does not experience certain changes, however multiplied and prolonged may be the contact of the water with the absorbing surface. The point of saturation at which absorption ceases, is determined by the maximum of liquid which the body can contain in the natural state.

Let us now suppose, that the body is at its point of saturation, absorption will cease only for the moment; for the body will rapidly recede from the point of saturation, by the losses which perspiration continually occasions, without mentioning other excretions. Absorption will take place in consequence, as we have shewn elsewhere.

The body will then tend to maintain itself at the point of saturation; but it will not maintain itself exactly at it:—it will undergo fluctuations dependent on excretion and absorption, and so long as food repairs the losses of animal matter, the injection of water, however abundant, will have little effect upon the proportion of this fluid to the globules (particles) of blood.

If strict abstinence is observed, as in acute diseases, the losses of animal matter not being repaired, the proportion of globules necessarily diminishes, but this change is too slow for the most severe cases.

The most prompt and most efficacious means of effecting this change, consists in the abstraction of blood. Bleeding, at first affects only the quantity of blood, and not the proportion of its constituent parts; but the depletion, according to its extent, has removed the body from its point of saturation; absorption is increased in consequence, and is then principally operating upon the water in contact with the absorbing surfaces. The body may thus be restored to its original weight, or very nearly so. It follows, that the number of globules being diminished by the abstraction of blood, and absorption supplying this loss by water, which brings scarcely anything with it but the materials which it holds in solution, the proportions of the blood in relation to the water and the globules may change very rapidly, and to a great extent compatibly with life.

If these deductions should leave any doubt respecting the justness of the conclusion, it may be removed by direct observation. Prevost and Dumas have proved, that the blood drawn at a suitable interval, after previous bleeding, presents a diminution in the proportion of globules.

I refer those who wish further to examine the subject, to the memoir read by Magendie in 1820, on the mechanism of absorption in animals with red and warm blood, (*Journal de Physiologie*, tom. 1.) and also to Fodera's Experimental Researches on Absorption and Exhalation.

When it is considered, that the cold-blooded vertebrata differ from warm-blooded animals, not only in their power of supporting limited respiration, but also in their resistance to a multitude of other deleterious influences, it will be acknowledged, that the plan of treatment which tends to produce an approximation to their constitution, in individuals of superior classes, within the limits which their organization admits, would put, them also, in the most favourable conditions for escaping the same causes of destruction.

XXVII. We have seen, by the comparison of the blood of different species, and the action of some means adapted to modify this fluid in a determinate manner, how this change can be effected. But this change has limits which depend not only on the proportions of water and of globules, but also on the nature of those globules themselves. They differ, as we have already pointed out, from the researches of Prevost and Dumas, according to classes and species, by their form and their dimensions. No means with which we are acquainted can effect alterations of this kind; and even, if we had them at our controul, their employment might, perhaps, be not salutary, but fatal. These physiologists have, indeed, been able to restore life and health to animals, which appeared deprived of them through loss of blood, by infusing into them blood having globules of the same kind. But when they endeavoured to produce the same effect with blood, the globules of which were of a different kind, they succeeded so much the worse, as the form and dimensions of these globules were further removed from those of the globules of the blood of the individual subjected to the experiment, and in cases of extreme difference, although at first they re-animated the animal, they caused horrible convulsions, quickly followed by death.

XXVIII. There are other characters, besides the dimensions and the form of the globules, which have intimate relations with the mode of vitality. They are derived in the first place, from the apparent change which the globules undergo in their colour; a change common to all the vertebrata. The globules are composed of a central white nucleus, and of an envelope of a red colour*; which alone undergoes that modification which makes the particles pass from a dull red to a bright vermillion; and according to the shade, they exert upon the phænomena of life an action no less powerful than that which is derived from their form and

* See the correction of this view in the additional matter subjoined to the Appendix.

dimensions. Their communication with the air determines the extent of this change. A great number of facts recorded in this work are referable to it, some immediately, others in a more remote manner, and they lead to the determination of other facts which the state of science places within reach; such especially as those which I have given relating to the alterations of air from respiration in Part IV. Chap. XVI.

As these facts lead us to consider the oxygen which disappears in respiration as really absorbed, we should now follow its traces in the system, and establish the nature of its combinations and of its actions. Here commences a new order of researches. The same is the case with the azote absorbed in the act of respiration, and the sources from whence the exhalation of this gas and of carbonic acid is derived: it is also the limit prescribed to this work; but I cannot terminate without pointing out how this order of researches necessarily connects itself with the recent discoveries on the composition of the blood, and the action of the nervous system.

XXIX. We have considered the composition of the blood only in relation to the water and globules; but these are not its only constituents. It is known that the limpid portion is not pure water; it contains in solution, among other substances, albumen, salts, &c., and forms what is called *serum*.

It is sufficient to draw blood from a living animal and analyse it by the known means, to find there several of these substances and determine their proportions. But if we confined ourselves to this method, we should not discover other constituent parts, the knowledge of which throws a great light, not only on the composition of this fluid, but also on the secretory functions. The differences which had been observed between the immediate constituents of the blood, and those of several other fluids in the system, had caused them to be attributed to a different origin. Thus, urea, the characteristic principle of urine, not being found in the blood by known methods, it had been concluded, with much apparent reason, that it did not exist in it, and that it owed its formation to the kidneys. This opinion has been universally adopted since the discovery of urea. Prevost and Dumas thought, that this secretion might be regarded in two different lights, either the one which I have just mentioned, or the following. They discovered the means of deciding the question. They supposed that the kidneys, instead of forming urea with the materials derived from the blood, might give passage to it according as the blood furnished them with this principle already formed. In this case, it would be found in so small a quantity in the blood drawn from the animal in the natural state, that it could not be recognized by the ordinary means of chemical analysis; but these means would be sufficient, if, on the supposition that the kidneys give passage to this principle, this passage were arrested. The extirpation of the kidneys, with the necessary precautions, must fulfil this indication, and then the urea, accumulating in the blood, would become manifest by the ordinary methods of analysis. They thus discovered in the urea a new principle in the blood which they found in great quantity, and made us acquainted with one of the principal secretions of the body in a new point of view.

The fact just mentioned relates to the state of health; but there are others which constitute new relations between the composition of the blood in the state of disease, and the secretions which depend upon it. Children are subject to a disease characterized by *induration of the cellular tissue.* Chevreul, on analyzing the fluid secreted by this tissue, found that it contained a substance which coagulates whilst cold. He has also recognized its existence in the blood of the same patients, and this in great proportion. It is the same with the colouring matter of jaundice which frequently accompanies this disease. Thus, morbid secretions are connected with the constitution of the blood, by the co-existence of the same principles in this and in the other fluids.

XXX. These relations will doubtless be multiplied. Since a great number of the immediate principles of the organs and of the secretions, must now be referred to the blood, it is natural to inquire how they come to make part of it. It is known that the digestive system furnishes a great number of them; but the origin of all of them cannot be referred to that source.

Although the course which is taken by the oxygen which disappears in respiration has not been discovered, it is a necessary consequence of the absorption of any substance, that it passes in a greater or less proportion into the blood. Here then is evidently one source of the changes in this fluid, which may give rise to some of the immediate principles which constitute it. It remains for further researches to determine them. Enquiries of this nature appear intimately connected with the study of the nervous influence, especially since the labours of Dr. Wilson Philip have made us acquainted with the share taken by the nervous system, in converting the food carried into the stomach into chyle. The accuracy of the Doctor's researches have been verified by the experiments of Breschet, Vavasseur, and of my brother Henry Edwards. (*Arch. gen. de Med. Août*, 1823, p. 485.)

XXXI. We find, in the changes which the blood can undergo as to its composition, a fertile source of the changes in the mode of vitality. It would appear at first, that it is only through this medium that we can act on the nervous system, in order to modify its action so as to change the constitution of individuals; on account of the extent in which this fluid can vary, and of the apparent immutability of the nervous system in its form and structure.

It is evident, that the dimensions and proportions of that system have limits assigned by nature to the modifications which their vitality can undergo; it is, however, susceptible of considerable changes, not discernible by inspection, but which manifest themselves by the actions which result from them, and which do not arise from the influence of the blood. Such effects may, as we have formerly proved, be produced by temperature, by light, electricity, and a number of other influences by contact, to say nothing of moral causes. It is this which I have had in view in speaking of the special action of the air on the system, and which I have designated vivifying influence.

It is thus that the impression of the air serves to reanimate a life almost extinguished in the case of apparent death, and here man has an advantage over all warm-blooded animals, even the hybernating. Their skin, covered with hair or feathers, is less accessible to the air; and I have never seen an adult individual which, after the cessation of all external motion by submersion in water, has been recalled to life by exposure to the air. Man, on the contrary, whose skin is bare, delicate, and sensible, may be re-animated by the action of the air, when he appears to have lost, under water, sense and motion.

We have shown elsewhere, that new-born children, when deprived of air, would not give signs of life during so long a space of time as young mammalia of the same age, which are born with closed eyes; they will, however more easily recover from apparent death, because their skin is adapted to receive a stronger impression from the air.

We have have seen how fatal heat is in cases of asphyxia, and of very confined respiration. Now, when the action of the air is reduced to the effects which it produces upon contact with the skin, its influence is the weakest possible, and at first it cannot easily be conceived what advantage can be derived from the application of heat. If that application be of long duration it will be fatal; in some cases it may be useful, if it is of short duration. When an animal is plunged in water, at the temperature of 40° cent. or 104° Fahr., its motions, are much more forcible, but less numerous than at inferior temperatures. There are circumstances, then, in which heat may be momentarily applied in order to excite the movements of the chest. The immersion of a great part of the body in warm water, is frequently an efficacious means of re-animating a child just born without signs of life. As soon as motion is produced, or if it be slow in manifesting itself, it will be right to abandon a method, the prolonged use of which, would be fatal.

We must, therefore, look upon the vivifying influence of the air in two points of view, its direct action on the nervous system by contact; and its action on the blood by the changes which it produces in it. In like manner, the vitality of individuals may be modified by a number of other causes which act immediately, either on the nervous system, or on the blood. Many facts mentioned in this work, are examples of both modes of action.

APPENDIX.

ON ELECTRICITY.

In relation to the animal economy, the phænomena of electricity may be divided into two classes: one comprehending the actions of the external fluid upon the body of the animal, and the other the electrical influences which he exercises upon himself.

We shall examine, in the first place, the effects produced by tension, or the state of a body when charged with electricity. If a man, or other animal, be placed upon an insulated stool, and put in communication with a body charged with free electricity: from the moment of contact he will give the signs which answer the presence of that species of electricity.

We shall now proceed to the effects which result from the passage of a single species of electricity through a conductor interposed between the source which furnishes the fluid, and the common reservoir in which it is going to lose itself. The molecules of which it is composed will tend to separate, on account of the repulsive action which they acquire while charging themselves with a similar electricity. Were this influence to become sufficiently powerful to over-come the force of aggregation which holds the molecules together, the body would be reduced to powder.

This property may be applied without difficulty to physiological phænomena, and explains them in a manner which leaves little to be desired. If an electric spark be passed through a small drop of blood, the particles which it contains, will be seen instantly to assume the appearance of raspberries, which indicates the partial separation of the elementary globules of which they are formed. If the same experiment be tried upon a liquid containing spermatic or infusory animals, a similar effect will be observed, and these various beings will instantly lose the spontaneous motion with which they were endowed. In all these cases, the disorganization seems to consist merely in the forced separation of the organic globules of which the tissue is composed. But if the same trial be made upon bodies composed of various heterogeneous tissues, it is manifest, that the strongest action will be received by the portions best adapted for transmitting the electric fluid. In a vertebrated animal, it will therefore be the nervous tissue which will suffer the most from the effects of an electric shock, and if its intensity be such, that if the globules which compose the nervous fibres shall be disjoined, all the functions of that system will be instantly destroyed, and life will be irrecoverably lost. Such is the effect of a stroke of lightning, and such are the general symptoms which manifest themselves in man, and other animals, which have been struck in this manner. No experiments have, indeed, as yet been made calculated to show the nature of the disorganization undergone by the brain and its dependencies on such occasions, but it is very well known, that muscular irritability diappears at the very moment at which life is destroyed by an electric shock, while it is preserved long after death from other causes. It is also observed in animals struck by lightning, that their blood does not coagulate, as in most other cases after death, but remains fluid, or at least presents only a few inconsiderable clots.

There is another kind of influence which deserves still more attention, since it appears, that it is to it that the reaction, which the body of an animal is capable of exerting on itself, are to be referred.

In 1789, Galvani observed by chance, that a metallic circle, composed of two heterogeneous metals, placed in contact on one hand with the muscles, and on the other with the nerves, instantaneously produces contractions of the muscular structure comprehended in this circuit. The physical explanation of this fact was furnished by Volta, who demonstrated, that two conductors in contact, become charged with opposite electricities, and that when they are united by a third body, capable of transmitting the electric fluid, a current is established within, owing to the neutralization of the fluid collected in the metals. It is this current which determines muscular convulsion, when the nerve of a muscle serves as a conductor, and sensation, when one of the cerebral nerves is employed, as in the experiments of Galvani and in others, equally remarkable, which are related in the more ancient work of Sultzer, entitled *Théorie du Plaisir.*

Let us more clearly examine each of these properties, and we shall see to what order of phænomena, we are now enabled to refer them.

It is well known to physiologists, that the integrity of the division of a nerve which supplies a muscle, and the free circulation of the blood through the vessels which are distributed to it, must be considered as the necessary conditions of the contractile power.

Anatomists are aware that the muscles present considerable analogy in all the animals in which they can be observed with sufficient plainness. They are bundles of fibres, soft, flexible, yielding, and of very various lengths. A cellular tissue of great delicacy unites them together, and their extremities lose themselves in the common mass, or attach themselves to tendons, which form the medium of connection between the muscle and the parts which it is designed to move. The manner in which their fibres are grouped is very various, but the muscular tissue appears to be strictly the same in all cases. Its colour is white, and if in warm-blooded animals it appears red, this must be attributed to the fluid which bathes it. We shall subdivide the muscular fibre into three orders. We shall call *tertiary fibres,* those muscular filaments which are found on cutting the muscle longitudinally : we shall call *secondary,* those obtained by the subdivision of the former : they are very well marked, inasmuch, as it is impossible to subject them to any mechanical alteration without arriving at the *primary fibre,* which the labors of Home, those of Henry Edwards, and our own, have made known in a very satisfactory manner. Henry Edwards found the elementary fibre the same in all animals and at all ages, and formed, in all cases, of a series of globules of the same diameter. From the combination of a bundle of primary fibres, result the secondary fibres, upon which our attention must be fixed, inasmuch, as the contractile movements are effected by their means. When they are examined with a magnifying power of 300 diameters, they exhibit themselves frequently in a very peculiar manner, which might lead into error respecting their real composition. They are seen like cylinders, crossed by a considerable number of little sinuous lines placed at the regular distance of the 300th part of a millimetre. This appearance seems owing to the membranous sheath in which they are invested, and is not found in secondary fibres which have been cut or torn. It disappears likewise in certain states of illumination, when the true muscular structure becomes manifest, and appears composed of a considerable number of small elementary threads placed parallel or nearly so.

If a muscle be taken sufficiently thin to be examined as a transparent object, without its being necessary to divide it, it will be seen that it results from the combination of a certain number of secondary fibres placed sometimes with little order, one beside another, parallel, or nearly so, and often grouped so as to produce the muscular bundles which are conspicuous in thick muscles. These are held together by an adipose cellular tissue, and are traversed in various directions by vessels and nerves which seem to pervade the muscle, without having any easily observable connexions with it. We cannot now enter into the history of the circulation peculiar to these organs, and shall therefore only observe, that if there exists a material communication between the muscular fibres and the bloodvessels, it can only be conceived on the supposition of transudation taking place through the coats of the vessels. The passage from the arteries to the veins is easily traced, and does not present the extreme division which would be indispensable to the nutrition of the organ, if it took place as it is generally imagined.

Let us now examine these muscular bundles, independently of the accessory organs, and with a very weak magnifying power, to obviate all objections to which the use of the microscope is liable. If the muscle be at rest, we merely see a number of straight parallel fibres, which are very flexible and so disposed as easily to change their position with the least motion of the muscle.

When this appearance has become familiar to the eye, we can appreciate the changes which are effected at the moment of contraction. For this purpose, we take a muscle recent and thin, the sterno-pubic muscle of the frog, for example. We place it under the microscope, and submit it to galvanic influence by means of a very simple arrangement, described in our Essay upon Spermatic Animalcula. As soon as the current is established, the muscle contracts and presents a most remarkable appearance. The parallel fibres which compose it are suddenly bent zigzag, and present a great number of regular undulations. If the current be interrupted, the organ resumes its original appearance, and bends again when it is re-established. It is even easy, when the muscle is strong and irritable, to repeat the experiment a number of times. In general, however, the muscle must be renewed after two or three trials.

The precision and instantaneousness of these changes render this phænomenon one of the most curious in physiology. On examining it with attention it will be perceived, that the flexions take place at determinate points, and do not change their position, which seems to indicate, that it is occasioned by the momentary attraction of these points to each other.

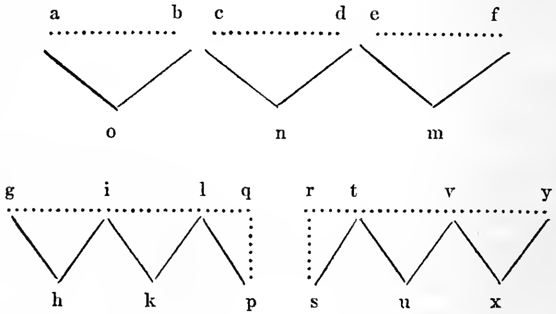
In all the muscles the same peculiarity is discovered. Warm as well as cold-blooded animals exhibit it; and birds as well as mammifera. It is also perceived without difficulty in the muscles of the stomach, the intestines, the heart, the bladder, the uterus, &c.

On the surface of the secondary fibres, and in the inner part of the angle which they form, when contracted, may be remarked wrinkles or folds, owing evidently to the forced bending to which they have been subjected. This appearance is frequently very well marked; in other cases it is, less so. This arises only from the energy of the contraction. When it is weak, the angle is obtuse, and the fibre does not undergo sufficient flexion to occasion these wrinkles; but if it becomes more acute, the inner part of the bundle must necessarily be compressed, and thus form wrinkles. It is even probable, that this cause limits the energy of the contractions, and prevents them from passing a certain angle. At least, it is certain, that in the muscles of locomotion, we have never been able to produce contractions occasioning angles so acute as 50°. The fibres of the intestinal muscles, however, frequently exhibit themselves at angles even more acute. But, in the first place, the summits of the angles are sensibly more distant than in the other muscles, and in the next, their secondary fibres are thinner, and are displayed over a larger surface. It will readily be conceived, therefore, that they are in a situation altogether peculiar, and that each fibre contracts, as it were, independently of the others, and without being constrained by the surrounding bundles.

Having observed the phænomena just described, it was essential to determine all their conditions. It was possible, that the muscular fibre might have undergone other changes besides those which we had perceived, and that, on this supposition, it might undergo a variation in bulk. Ancient anatomists, and among them Borelli, had believed, that the bulk of the muscle was sensibly augmented at the moment of contraction. This opinion, which was not founded upon any measurement, was overturned by Glisson. He caused the arm of a man in the state of rest to be immersed in a bucket filled with water, and he thought he perceived a lowering of the surface as soon as the muscles came into play. This experiment was repeated with more care by Carlisle, who uniformly arrived at the contrary result. More judicious observers have, however, perceived, that these experiments were deceptive, since no allowance was made for the alterations in the skin, and sub-cutaneous cellular tissue, when compressed by the muscular effort. Blanc pursued a method similar to that of Carlisle; but he took the precaution to make use of a compact muscular mass, and placed in the vessel a piece of an eel, which he stimulated by means of a pointed metallic wire. This method having shewn him no alteration in the level of the liquid, he inferred from it, the equality of volume in the two states of the muscles. But before him, and without his knowledge, Barzoletti, by a much more elegant experiment, had arrived at precisely the same conclusion. He suspended in a bottle, the posterior part of a frog, filled the bottle with water, and closed it with a cork, through which was passed a narrow graduated tube. He then found the muscle to contract by the stimulus of galvanism, and in no case could he observe the least variation in the column which the tube contained.

The apparatus which we employed did not differ, in its principal conditions, from that of Barzoletti; though we made use of larger masses of muscle, and not having perceived any disturbance of the level, we also came to the same conclusion as Blanc and Barzoletti.

The experiments just related were sufficient to show, that the muscle underwent no alteration, except in the direction of its fibres. Great importance being thus given to the examination of the sinuosities which the muscular fibres describe, we were induced to devote some attention to this subject.



On one of the muscles of the leg of a frog being placed under the microscope, and made to contract by means of the pile, we held over it, in several places, the broken lines as above, and carefully compared them with the natural sinuosities, making use of both eyes. We afterwards completed the triangles by means of the dotted lines and took the following measures.

Length of the lines.		Distance of the points.	
a o	— 10mm.	a b	— 17mm.
o b	— 10	c d	— 16
c n	— 10	e f	— 16
n d	— 10	g q	— 42
e m	— 10	r y	— 39
m f	— 11	Total	130
g h	— 10		
h i	— 10		
i k	— 11		
k l	— 11		
l p	— 12		
s t	— 11		
t u	— 12		
u v	— 12		
v x	— 10·5		
x y	— 12		
Total	172·5		

If we suppose the 16 lines in the above table to form a series, we shall have 172·5 for the distance of the points, a and y, when the fibre is straight, and only 130, when it is contracted. This indicates, a shortening of 0·23 in the fibre.

But it was possible to assure ourselves directly of the truth of this fact, by taking the same muscle and measuring it with care, in the two states of relaxation and contraction. For this purpose, as soon as we had taken the muscle from the body of the animal, we placed it under the microscope, to ascertain that its fibres were quite straight, and then measured its length by means of a pair of compasses. We afterwards stimulated it to contraction by the current of a weak pile, and took a new measure whilst it was in that state.

Relaxed muscle	25mm.	Contracted	17mm.
	20		15
	25		18
	20		15
	90		65

By this method the diminution was found to be 0·23. We may then conclude, that the flexion of the fibre represents in reality the quantity by which it is shortened, which proves, that the change which it has undergone, is referrible to direction only.

This consideration is so much the more important, as many facts commonly known prove to us the elasticity of muscular fibre, and there might be some reason for supposing, that that property was concerned in the phænomenon of contraction. We shall now detail what we know with precision on the subject. Living muscle, abandoned to itself, always assumes the regular state in which we subjected it to examination; but, when its two extremities are fixed, and the distance of the points of attachment is increased, the fibre lengthens in virtue of its elasticity, as shewn by old experimenters, who have endeavoured to estimate the weight necessary to produce its rupture. It is evident, that this action is of an opposite nature to that which produces contraction, and which it ought to resist in its effects; at least, we are authorized to think so after the following experiments. We took female frogs a short time before spawning. Their abdomen was very much distended by the eggs, and the sterno-pubic muscles must have yielded to the increase of bulk and become lengthened. We separated these muscles from the cellular tissue, and from the other parts of the abdominal parietes. We determined their lengths, and then we cut one of their extremities. At that very instant they underwent a remarkable shortening; but on examining them with the microscope, we ascertained that that phænomenon was not accompanied by any flexion of the fibre, and that it consequently differed from its ordinary contraction. Being afterwards subjected to galvanic influence, the same muscles further diminished in length, presenting the usual sinuosities. We shall here give the numerical proportions, which express the conditions of these two phænomena.

Muscle in its place	45mm.	Cut 34mm.	Contracted 22mm.
	49	36	25
	51	37	27
	145	107	74

These numbers are nearly in the proportion of 30—20—15; in other words, a muscle, whose strong contractions are equal to a quarter of its length only, may be brought, by means of continued traction to the distension expressed by the proportion 2 : 3, without undergoing any alteration in its contractile power.

When we reason upon this fact, a view is presented to the mind, which altogether destroys the objection which might be drawn from certain cases of extraordinary contractions, apparently difficult to be reconciled with our theory. In fact, the stomach, the intestines, and the bladder, exhibit to us variations of volume, which are almost incredible; and, although, their muscular structure is such, that it is easy to explain why their contractile power produces results much more powerful than those, the intensity of which we have measured in the mucles of locomotion, it is no less true, that they would be much less than they are, if the elasticity of their fibres did not act an important part in that phænomenon. The explanation of the facts becomes very easy if we make use of the two following principles: 1. The muscles are elastic, and consequently capable of being lengthened under the influence of traction exerted at their point of attachment; 2. Their contractile power may act in all cases; but it probably increases in force as it approaches to the natural state of the muscle. It results from these two properties, that the stomach and intestine, for example, may be greatly distended by the pressure of alimentary matters. If in such circumstances, any stimulus is made to act upon them, they will undergo successive contractions, which will gradually follow the foreign bodies included in their cavity, until at length they attain their point of rest. Their muscular fibres were straight while they were distended, they are so still in this latter state. This power of extension is much facilitated by the secondary fibres of these muscles being very thin and very long. They are disposed nearly on the same plan, and are united by means of a very loose cellular tissue. These various circumstances permit them to be easily separated; they do so in fact when the organ is stretched, and if this trial is carried too far, rupture takes place, and that in the intervals between the muscular fibres.

The contraction of these organs differs then entirely from that of the muscles of locomotion. The latter are fixed in an invariable manner at their extremities, and can scarcely undergo more than a single contraction, or, if they undergo a certain number, they are alternate, and always bring back the organ to the same point. In the abdominal viscera, on the contrary, it is by means of a series of contractions that the muscles gain the point of rest.

Let us now examine the connexions which exist between the phænomena just adverted to and the nervous system. It is well known that a muscle contracts, 1. When its nerve communicates freely with the brain, and there exists in that organ the will to produce a contraction; 2. When the nerve is pinched, after its connexions with the brain have been destroyed; 3. When the current from a galvanic pile is passed through it; 4. When it is touched with active chemical stimuli, such as concentrated mineral acids, chlorurets of antimony, bismuth, &c.; 5. When placed in contact with a hot body. Since it is evident that the presence of the brain is not necessary to the exercise of the contractile power, we shall at once set it aside, and pass on to the examination of the contractions determined by means of the pile.

If one of the poles of a galvanic pile be placed in contact with the nerve, and the other in communication with the muscle, the latter undergoes contractions. In order that we may form a distinct idea of the course of the galvanic fluid in this experiment, it is necessary to study more closely the relations which exist between these two organs.

The nerves present to the naked eye, a satin-like appearance, which was first particularly described by Fontana.

It is particularly sensible in the nerves of the cat, the rabbit, the guinea-pig, and the frog. When they are examined with a power magnifying from 10 to 15 diameters only, alternate lines of light and dark are seen, which forcibly suggest the idea of a spiral coil situated beneath the neurelema. After a series of varied experiments, we were convinced that this appearance, like that of the tendinous tissues, was owing to a little pleating of the fibres of the neurelema, which loses its transparency in some parts, and preserves it in others. This would merit little attention, did it not present a very certain criterion for recognizing the little nervous threads, and render them easily distinguishable from the blood-vessels or lymphatics. If the neurelema of a nerve be divided, and the nerve be then spread out under water, it is seen to be composed of a great number of very small parallel fibres, which appear to be continuous throughout the length of the nerve; at least, they are no where seen to divide or unite. Their filaments are flat, and composed of pure elementary fibres, placed nearly on the same plane, which gives them the appearance of ribbons. These fibres are formed of globules, and present a remarkable circumstance, namely, that the two outer are the most distinct. The middle series can be seen only occasionally, doubtless because the pressure which they undergo effaces the line of demarcation of the globules composing them. The number of these secondary nervous fibres is very considerable as the following calculation will shew, although the data of the observation may not be admitted as rigorously correct. Let us suppose, that each elementary nervous fibre occupies in the section of the nerve, $\frac{1}{300}$ of a square millimeter, we shall have 90,000 for every square millimeter. But we know that the secondary fibres include four elementary fibres, there must then be 22,500 in the same space, or about 16,000 for a cylindrical nerve of a millimeter in diameter, such as the crural nerve of a frog, for example.

If a nerve be examined at its entrance into a muscle, and followed attentively, it will be seen to ramify in a manner which at first appears not to be very regular, except that there is a tendency in the branches to direct themselves perpendicularly to the muscular fibres. This observation may be easily made upon all muscles, such as those of the ox, cat, &c.; but it requires in this case precautions regarding light, which render it painful and fatiguing. It is on the contrary, very easy to discern it in the thin muscles of the frog, on account of their transparency. After having thus followed one of the nervous branches as far as observation with the naked eye and the lens will permit, it becomes easy to fix the point at which we mus stop, and to continue the examination assisted by higher magnifying powers. As the nerve arrives at its ultimate ramifications, it widens, and its secondary fibres separate and display themselves, precisely as when it has been stripped of its neurelema. This little nervous trunk then presents the appearance of a fibrous cloth, from which some threads are occasionally seen to shoot into the muscle, perpendicularly to its own fibres. Sometimes there are two nervous trunks, parallel to the fibres of the muscle, which proceed at some distance from one another, and mutually send across little threads, which are seen to pass through the muscular space which separates them, cutting it at right angles. Sometimes the nervous trunk is itself pressed close to the fibres of the muscle, and the threads which it furnishes spread out, preserving this direction, run through the organ, and return on themselves, forming a loop. But in all cases, it is observed, 1st, that the extreme nervous ramifications are parallel to each other, and perpendicular to the fibres of the muscles; and 2ndly, that they return into the trunk which has furnished them, or anastomose with a neighbouring trunk. But, in all cases, it appears very certain that they have no termination, and that their relations are the same as those of the blood-vessels. Now, let a galvanic stream be passed through a muscle examined in this manner, and it will be seen, that the summits of the angles precisely correspond to the passage of these nervous filaments. Before admitting this fact, we subjected it to all the verifications that we could think of, and it was only after having repeated and varied our experiments in every possible way, that we considered ourselves warranted to adopt it. All preparations do not succeed, but in the delicate muscles of the lower jaw of the frog were found the best specimens that could be discovered.

It becomes then very probable, that the nerves approach, and thus determine the phænomenon of contraction. Now, what is the cause which forces them to advance towards each other? It is impossible here not to recognize the application of the beautiful law discovered by M. Ampère. It remains to investigate how far it is applicable. If two streams attract each other, when they go in the same direction, it will be enough to suppose, that the nerve transmits the galvanic fluid more easily, and in more considerable quantity than the muscular stratum itself, (which is quite in accordance with experiment,) in order to form a clear idea of the phænomenon in question. Indeed, if we interpose a muscle between the poles of a pile, it will be found to be traversed by the fluid, but in an unequal manner, on account of the better conducting power of the nerve. The branches of this being parallel, will reciprocally attract each other, and will thus determine the flexion of the fibre, and the shortening of the muscle.

Admitting the correctness of this opinion, it will easily be conceived, that the living muscle is really a galvanometer, and the short distance between the conducting branches on the one hand, and their tenuity on the other, unite in giving it an extraordinary sensibility. We shall now consider it in this point of view, and compare the phænomena of muscular contraction with the experiments on the moving power of electricity, with which natural philosophy has of late years been enriched.

The beautiful experiments of the Italian philosophers upon muscular contraction, produced by the contact of heterogeneous matters, are generally known, and we have now increasingly strong grounds for believing, that these motions are owing to the passage of a small galvanic stream. But admist all these results, it will be remarked, as Humboldt has clearly shewn, that the contractions manifest themselves at the moment when the communication between the nerve and the muscle is etablished, by means of a single metal. This is generally explained, by supposing that the metal and the muscle become placed in opposite states of electricity, and that the neutralization of the two fluids is effected through the nerve.

If to each end of Schweigger's galvanometer be fitted similar plates of platina, and if around one of them be fixed some ounces of muscle, recently taken from a living animal, and if they are then plunged into blood or water, slightly impregnated with salt, the magnetic needle will deviate, and the stream will go from the metal to the muscle.

It appears then, that the view which has been adopted is accordant with experiment, and we might regard this method as an excellent means of comparison between Shweigger's galvanometer and the frog. In fact, if we arm the muscle and nerves of the animal with portions of the wire, which forms the galvanometer, and then bring the two ends of the apparatus in contact with the *armatures*, the contractions will be strong and frequent. The needle, however, in the majority of cases, will not change its position; and if, sometimes, slight oscillations are thought to be perceptible, they only serve still further to prove the want of sensibility of the instrument.

The animal, with exquisite sensibility, indicates all the electric currents which influence the galvanometer, as for example, the action of an incandescent metal upon a cold one, that of an alkali upon an acid, and that of two oxidable wires, unequally immersed in an acid. It is very certain, however, that if we did not possess the galvanometer, it would be impossible to present an exact analysis of these various phænomena, since the frog does not indicate the the direction of the current.

We easily perceive, in all that has been adduced, the power of the electric fluid in producing muscular contractions, and we know, from other experiments, that it is indispensable, that that fluid should be in motion. If a frog, prepared and insulated, be brought near the charged plate of an electrophorus, the nerves, like all other light bodies, will be strongly attracted. The frog will give very marked signs of free electricity; but the attractions will manifest themselves, only at the moment when the spark is taken. Thus, every time that the galvanic current passes through a living muscle, the contractions of that organ betray its passage. We have now to show, that in all cases, where contractions are produced, there also exists a development of electricity.

For this purpose, let two similar platina wires be fitted to the ends of the branches of the galvanometer; let one of them be plunged in the muscles of the frog, and the nerves of the animal be touched with the other, heated to redness.

The contractions will be strong, and the deviation of the needle very sensible. Both these phænomena will be produced, but with less intensity, if the heated metal be applied to the muscles.

Let a platina cup, filled with nitric acid, be now substituted for one of the wires, and fix to the other a fragment of nerve, muscle, or brain; at each contact the needle will deviate, and the stream will proceed from the acid to the animal matter. Similar effects may be obtained with chloruret of antimony.

With regard to pressure or pricking, which are only modifications of the same thing, we have not been able, in this kind of experiments, to detect the electricity which they must excite; but the beautiful discoveries of Becquerel leave no room for doubt on this point; and the difficulties which we have experienced depend on circumstances which render modifications in the apparatus necessary.

Besides, we ascertained, by experiment, that by the slightest pressure, two living animal substances acquire opposite states of electricity. It is sufficient for two insulated persons to touch hands, and then withdraw from the contact, to develop an excess of electricity sufficient to affect the electroscope of Caulomb.

It is proper to remark, that the greater part of these effects are not connected with the existence of life, but it is very evident, that when death has affected the organs which are submitted to this kind of action, the conducting power of the nerves may have been essentially modified. It is even possible, that that may be the only circumstance which determines the irritability of the muscles, without which, it would not always produce the approximation of their nervous branches. The arrangement of the tissues is so delicate, that when matter abandoned to itself is withdrawn from the power which had organized it, it must, in a short time, lose the properties with which it had been endowed.

It may be supposed, that this hypothesis is not applicable to all the circumstances of contraction; but the results recorded in our memoir, and which may be obtained with great facility, readily shew that it is.

The insulation of the nervous fibres, is produced by the abundant fatty matter, for the discovery of which, we are indebted to Vauquelin. It surrounds each of the fibres, and does not permit the electric fluid to pass from one to the other.

Besides this arrangement, which exists in the interior of the nerve, under the neurelema, there is always, round the nervous trunk itself, and external to its covering, another bed of fat, which exhibits itself in its most minute ramifications. It will be obvious, that by means of these provisions, the electric fluid, which has arrived in the nerve, cannot deviate to take a different route.

Hitherto we have rather regarded the effects resulting from an extraneous action upon the animal economy. It now remains to shew, that there are internal phænomena, the result of the reaction of one organ upon the neighbouring organs, which may also receive some light from known physical facts.

The fluid with which the sanguineous system is filled, contains pure caustic soda, in sufficient quantity to impart to it manifest alkaline properties. Now, the greater part of the materials separated from the blood by the secreting organs, differ from it entirely in this respect. Some, as the bile and the saliva, are alkaline also; but they contain, in proportion to the quantity of animal matter, a much larger quantity of soda than is found in the blood. Others, as milk, and chyme, are, on the contrary, acid, and owe this property to the presence of the lactic, phosphoric, and other acids, which are also met with in the blood, but are naturalized by alkaline bases. Lastly, the urine and sweat, in the state of health, present themselves under two different conditions. They are generally acid, but sometimes neutral. What is called sweat, in the ordinary acceptation of the word, is always acid; but the liquid, which is continually evaporating from the skin, as well as the water which accompanies the air as it issues from the lungs, is found, when collected, to be neither acid nor alkaline, and their analysis shews only a small proportion of animal matter, accompanied by some traces of alkaline hydrochlorates. The urine is always acid in health; but this character is scarcely apparent, if the individual has drunk a large quantity of water some hours before.

If we seek among the facts known in chemistry, for an explanation of this difference between the constitution of the blood, and that of the fluids secreted from it, we may soon be convinced, that the action of the voltaic pile, is the only one which approaches to it. Moreover, it appears possible, artificially to imitate the principal conditions of the secretions, and to separate from the blood, by means of the pile, a liquid resembling milk, and from the food itself, a material resembling chyme.

We recommend this subject to the attention of medical men, inasmuch as they may find in it some valuable hints respecting the use of various medicines. Many of these act too evidently on the secretory functions, and too plainly disturb their equilibrium, for us not to ascribe their action in a great degree to this peculiar effect. We need merely mention, mercury for the bile and saliva, and diuretics for the urinary functions.

We cannot conclude this chapter without remarking, that if muscular motion and the secretions may be regarded as owing to electrical movements, the production of animal heat can only be suitably explained in the same manner; for it is known to electricians, that the conducting wire acquires considerable heat during the action of the pile. M. de la Rive, the learned Professor of Chemistry at Geneva, was the first to seize the happy idea of referring the phænomena of animal heat to electric agency.

ON MUSCULAR CONTRACTIONS PRODUCED BY BRINGING A SOLID BODY INTO CONTACT WITH A NERVE WITHOUT A GALVANIC CIRCUIT. BY DR. EDWARDS. *Read before the Royal Academy of Sciences,* MAY, 1825.

THE experiments of galvanic or muscular contraction excited an interest in the scientific world, which gave rise to many important researches. It seemed that a new epoch in physiology had commenced. This was really the case, not merely as regards the singular character of the newly discovered phænomena, but with respect to the fundamental results to which they led. The creation of a new branch of natural philosophy, was another and not less remarkable consequence.

Physiologists, who at first had hoped for too much, were too soon discouraged. From the close of the last century, when Humboldt's celebrated work on Galvanism made its appearance, up to a very recent period, but little attention was directed to researches of this kind. It was natural that attention should again be excited by the new impulse given to this branch of science by Professor Œrsted, nor could the researches of Becquerel, which have so greatly increased our knowledge of this subject, fail to revive the hope, that electricity might be satisfactorily called in to explain some of the phænomena exhibited by animal life. Accordingly, Prevost and Dumas shortly after, laid before the Academy of Sciences, a memoir, which called forth a very lively interest. They described the terminations of nerves, and exhibited their relation to the muscular fibre, in a manner which introduced perfectly new ideas on the subject of muscular contraction. Their proofs are grounded on the evidence of the senses, and the testimony of several of the members of the Academy confirms the accuracy of their observations. (Their views are given in the preceding Appendix, to which it is sufficient to refer, more particularly to page 302, *et seq.*, where they endeavour to shew, that in all cases in which muscular contractions are induced by external excitation, there also exists a development of electricity.) This fact, is very much in favour of the opinions advanced by these physiologists, that the muscular contractions thus excited, depend upon the electricity developed by the action of these stimuli. There is, indeed, the simultaneous production of electricity, and of muscular contraction, but the question may be asked—is it by virtue of this production of electricity, that the contractions take place?

Though it is well known that electricity, provided it be in sufficient quantity and applied in a particular manner, gives rise to muscular contractions, we do not know whether the fluid, disengaged by the three modes of excitation alluded to, is in the conditions necessary to produce such contractions.

Being occupied with some researches on the nervous system, I had occasion to examine a mode of mechanical excitation, which appeared to have been previously neglected, and which led me to some observations which bear on the question above stated.

The procedure consists in passing a solid body along a nerve, in the same manner in which we pass a magnet along a bar of steel which we wish to magnetize. In doing so, the object is not to act by pressure, although, more or less must be exercised in every form of contact, but rather to touch various contiguous portions successively, and we have it always in our power to bear as lightly as we please.

In order to pass the exciter along a certain portion of nerve, it is necessary that the nerve should be supported and kept more or less tense. These conditions are fulfilled when a portion of nerve is simply laid bare, while its connexions, superiorly with the rest of the nervous system, and inferiorly with the muscles to which it is directed, are left unimpaired.

Expose the entire sacral portion of the sciatic nerves of a frog, by removing the skin and muscle which cover them. Take off the skin from the lower limbs, in order to see the contractions of the muscles, and pass under the nerve a slip of oiled silk to bring them better into view, and also to make them even with the sacrum.

We have then by this preliminary step, an animal, whose sciatic nerve can be both better seen and touched, and in whose lower extremities, the slightest muscular contraction cannot take place without being visible. In order to cut off voluntary movements, which would interfere with and derange the experiment, the spinal marrow should be divided immediately below the head.

Having the animal thus prepared, touch the sciatic nerve in the before-mentioned manner, with a slender rod of silver. The muscles of the corresponding limb will be thrown into contractions, and such will continue to be the result whenever this treatment is repeated, however delicate the contact.

The exciter is to be drawn along the whole extent of the denuded nerve, which will be from a quarter to a third of an inch in length. Contractions are also produced by rods of various other metals, such as copper, zinc, lead, iron, gold, tin, and platina. I took care to employ metals of the utmost purity, in which state I was supplied with them by the essayers of the mint. It is not necessary that the rod should be metallic; I succeeded with glass or horn.

To produce muscular contractions, it is sufficient that the nerve be touched with any solid body in the manner above related.

This method of producing contractions, by successively touching contiguous points of a small extent of nerve, employing only a single body which has no connexion with the muscles of the leg, appeared to afford a favourable opportunity for examining the principle of this excitation; that is say, of ascertaining whether it causes contractions through the intervention of an agent altogether unknown to us, or whether it does so by means of electricity, mechanically excited.

My first researches were directed to discover, whether any difference of action resulted from the use of different exciting substances, whilst all the other conditions remained sensibly the same: I plainly saw that iron and zinc produced far less vigorous contractions than other metals, but I was unable to establish in a satisfactory manner, the scale of gradation. I could not even hope to do so, for variations in the state of the animal, occasioned differences in the contractions, under the influence of the same exciter, as great, or perhaps greater, than those which depended on the nature of the metals employed.

I was satisfied with having ascertained, that these exciters sensibly differed among themselves, and gave up the idea of a scale, which the subject would hardly admit of, and which, moreover, would not directly lead to the object which I had in view.

The question in fact, as before stated, was to determine whether in the preceding experiments, the muscular contractions were occasioned by an agent altogether unknown to us, or whether they were effected by electricity, which is developed every time one body exerts a mechanical action on another.

If electricity produced by the contact of the exciter with the nerve, were really the cause of the contractions, we might by greatly diminishing the quantity of electricity in the nerve, either sensibly diminish, or altogether suspend, muscular contractions. Now, these effects may be produced by varying the conducting power of the substance placed under the nerve. Thus, when the nerve maintains its natural relations, it rests on muscle, which is an excellent conductor of electricity. If, whilst the nerve is so situated, it be acted upon by a given quantity of electric fluid, this will be divided between the nerve and the muscle, and thus there will be a diminution of the excitation of the nerve, and of the intensity of the phænomena resulting from it. If, on the contrary, we place under the nerve which we wish to excite, a non-conducting body, the whole of the electricity will be concentrated upon the nerve, and we shall obtain from the fluid the full effect which we are desirous of producing. This precaution is had recourse to in galvanic experiments, when it is wished to excite muscular contractions by very small quantities of electricity; such for instance, as are produced by the contact of two metals.

To ascertain the respective influence of the insulation, and non-insulation of the nerve, the comparison must not be made without giving attention to the state of the animal.

If the animal be very fresh and excitable, the contractions will, in both cases, be so strong that the difference will not be perceptible: for no conclusions can be drawn from the comparison, if motion takes place in the limb, under circumstances the most unfavourable, since we should then be commencing almost where gradation ceases.

On this account, it is proper to wait till the animal is so far exhausted, that no muscular contractions, sufficient to move the limb, can be excited by the action of two metals on the nerve whilst it is resting on muscle. We may thus obtain the simple contraction of the muscle without locomotion, or even suffer muscular contraction to cease.

If, in this state of things we place a non-conducting body, as a piece of glass or oiled silk, under the nerve, and then establish the circuit by means of two different metals, we immediately cause the agitation of the limb.

This fact, and the principle on which it depends, being well established, the next step was to ascertain, whether in the preceding experiments, in which the nerve was touched with only one body, and no circuit was formed, the muscular contractions were to be referred to the action of the same cause. It will be remembered, that a slip of oiled silk was placed under the portion of denuded nerve. A comparison was now to be made between an animal so prepared, and another in which the nerves, instead of being insulated, reposed on the subjacent flesh. I made use of small rods, with which I easily excited contractions, when I drew them from above to below, along the portion of denuded nerve, which was supported by the oiled silk; but I was unable to excite them when I passed them along the nerve of the other animal, in which they were not insulated. Frequent repetitions assured me, that the want of effect did not depend on difference in the degree of contact; I tried the experiment on many animals of the same species, lest there might be anything in individual peculiarity. As in the one case the nerves were brought farther into view, and kept somewhat tense and even with the sacrum, by means of the slip of oiled silk, whilst in the other they had no such support, I restored the parity of position, by placing under the unsupported nerves, a portion of muscle, corresponding to the slip of oiled silk, as well in size as mode of insertion, and still was unable to produce contractions by treating the uninsulated nerve, whatever was the material of the rod employed as the exciter. The difference was rendered still more striking, when instead of making the comparison between two individuals, it was made upon the same animal. After having in vain attempted to produce contractions by contact of a nerve resting upon muscle, I found that they might still be induced, if the oiled silk were had recourse to, and I was able to command their alternate appearance and disappearance, by using sometimes a non-conductor, and at others, a conductor for the support of the nerve.

The manipulations which cannot be avoided, in making these trials, exhaust the nerve if they are too often repeated.

The difference is here as marked as possible. So decided a contrast as this was not necessary; a less, would have sufficed, provided it were really manifest. The reason is not obvious, why contractions should not sometimes be produced when the nerve is not insulated, since in galvanic experiments, the quantity of electricity, elicited by the contact of two metals, will or will not produce contractions, according to the state of vitality of the animal, which not merely differs in different individuals, but varies in the same individual at different moments. This extreme of contrast in the effects, at first very satisfactory, as more strongly exhibiting the influence of the respective states, and throwing light on the nature of the cause, seemed, on a closer view, to prove too much, by uniformly exhibiting the same difference.

I wished to be able sometimes to produce contractions by touching the uninsulated nerve, as happens in ordinary galvanic experiments, in which the contact of two metals is employed, though they might be expected to be less marked than in the latter case, since my method of excitation was one of inferior energy. I at length succeeded in this point. In observing the difference of effect in touching an insulated nerve, more or less rapidly, I discovered that contractions were the most constantly produced by a quick and light touch.

Having found that I produced contractions more easily by increasing the rapidity of the taction, I made trial on an animal whose nerve was not insulated, and frequently obtained slight contractions.

In the preceding experiments, choice has been made of the extremes from amongst the good and the bad conductors, suitable to be placed under the nerves, for it is necessary that they should lie on a soft material, in order not to be irritated, and compressed between two hard bodies. Thus, the slip of muscle, and the piece of oiled silk, are both soft and flexible, but the one is the best conductor, and the other the best calculated to insulate; they, therefore, offer the most favourable conditions for obtaining distinctly, marked, but opposite results. Notwithstanding the difficulty of obtaining appreciable differences, when employing substances of intermediate properties, I did not restrict myself to the two before-mentioned. Having prepared a frog, in the manner already described, I placed under the sciatic nerves, a piece of the skin of the animal, and under those of another, I introduced a slip of moistened paper, and perceived a marked difference; when, in the same manner, and with moderate quickness, I alternately touched the nerves, first of the one, and then of the other. The frog, whose sciatic nerves were supported by the piece of skin, remained motionless, whilst the same degree of taction applied to the nerve, resting on moistened paper, produced contractions of the muscles. To find out whether the difference of effect was referable to the different conducting power of the slips placed under the nerves, I instituted by means of galvanic experiments, in which I employed two metals, a comparison between the conducting power of the skin of the frog, and that of the moistened paper, and ascertained that they differed essentially.

The frog's skin conducted much better than the moistened paper, which is but an imperfect conductor. It is needless to enter into the detail of these experiments, M. de Humboldt having already established the fact, that the conducting power of animal substances, is superior to that of vegetable matter in its recent state, and having shewn that this difference does not depend on the water which they contain, but on the nature of the organized structures themselves. These experiments are easily conducted; they are founded on well known principles, and they appear satisfactorily to prove that, cæteris paribus, the muscular contractions, produced by the contact of a solid body with a nerve, are much less considerable, or even wholly absent, when the nerve, instead of being insulated, is in communication with a good conductor, and it would seem to follow as a legitimate conclusion, that these contractions are dependent on electricity.

ON ATMOSPHERIC ELECTRICITY. BY M. POUILLET.

Various theories have been formed by meteorologists to account for the electricity sensibly present in the atmosphere. Of these, Volta's was, perhaps, the only plausible one. That philosopher was induced to believe, that bodies, in passing from one state to another, undergo a change in their electric condition, and supposed that the electricity lost in storms, was constantly being renewed by that produced by evaporation perpetually going on from the surface, as well of the land as of the water.

The recent and interesting researches of Pouillet, were instituted, not merely to ascertain the truth of the Italian Professor's hypothesis; he was also desirous of discovering the efficacy of another cause, which he believed to be of no small importance in the production of electricity, and of bringing to proof a theory of his own, relative to the distribution and accumulation of this principle in the atmosphere.

Numerous and various experiments have brought him to the conclusion, that the mere passage of a body, from the solid form to a state of vapour, is unaccompanied by the development of electricity, that the result is similar, when vapour is condensed into the liquid, or solid form.

He conceived that Volta, though too accurate an observer to be mistaken as to the fact of the presence of electricity in his experiments, was, nevertheless, deceived as to the cause of its production, by the formation of carbonic acid, which mixed with the vapour of water, and complicated his experiments.

In 1782, Volta, Lavoisier, and Laplace, shewed, that electricity was developed during chemical action, but as experiments relating to this point, are liable to afford different and contradictory results, from slight differences of circumstances, the question has been regarded as undecided. It became, on this account, an object of special attention with M. Pouillet. He finds that in the combustion of charcoal, there is an unequivocal production of electricity, that the acid produced is in the positive state, whilst the charcoal always becomes negative. It is necessary, in order uniformly to obtain the same result, that the combustion should take place only at the upper part of the piece of charcoal, and by no means extend over the whole of it; otherwise the contact, both of the charcoal and of the carbonic acid, with the plate of metal destined to receive the electricity, will render the experiment irregular. To discover whether the electricity, rendered evident in the preceding experiment, was to be attributed to chemical action, or to the conversion of the charcoal from the solid to the gaseous, he examined the flame produced by the combustion of hydrogen. The external part of the flame constantly exhibits vitreous, and the interior resinous electricity. Thus, by the act of combustion, the combustible becomes electrified negatively, and the body which is actually burning, becomes positively electrified, whilst a transfer of electricity is taking place between the molecules, which are combining, and those which are about to do so. This fact is supported by a great number of experiments on the combustion of phosphorus, sulphur, the metals, alkohol, ether, fat substances, and vegetable matter.

As plants during vegegation exert a chemical action on the atmosphere, sometimes converting its oxygen into car bonic acid, and at others, decomposing the carbonic acid already existing in it, the idea suggested itself, that if electricity were developed in the process of vegetation, their very extensive operation would warrant one in attributing to them a considerable portion of the electricity of the atmosphere.

To investigate this subject, Pouillet examined the vegetation of seeds in an insulated situation, having a condenser connected with the soil. Till the germs appeared at the surface, no signs of electricity could be detected, but as vegetation advanced, it became very evident. For the success of this experiment, it is necessary that the air should be in a state of considerable dryness; when this does not happen to be the case, the apartment must be artificially dried by quick lime or some absorbent. It is obvious, that the soil could not acquire one electric state, without the opposite state, in a corresponding degree being communicated to the atmosphere.

If, then, a languid vegetation, on a surface of five or six square feet, be capable of producing very decided effects, may we not reasonably conclude, that the influence of the same cause, operating over a large portion of the surface of the earth, is fully adequate to the production of many of the phænomena, which we observe.

A second memoir, by the same author, carries this subject still further, and exhibits other causes besides the process of vegetation, which contribute to supply the atmosphere with electricity. In the first memoir he had shewn, that when two bodies combine, electricity is developed ; in the second he proves, that similar phænomena attend the separation of bodies which were previously combined, and he applies this fact to the numerous instances of decomposition which nature is spontaneously producing on the surface of our terraqueous globe.

Pouillet, in his experiments connected with this inquiry, employed two processes — the first resembles that adopted by Saussure, in his experiments on evaporation, and consists in connecting one of the disks of the condenser with the heated vessel, in which the subject of the experiment is to be placed. By the other process, the heated vessel is dispensed with, and he makes use of one of Fresnel's large lenses, to heat the body whilst it rests on a plate of platina. It should be remarked, that when vessels of copper, iron, or of other materials, on which the substance under examination can act chemically are employed, the result will be a complication of effect, by which the phænomena will sometimes be heightened, and at others neutralized.

The results of these experiments are,

1. That by mere evaporation, whether rapid or short, no signs of electricity are produced.

2. That evaporation from an alkaline solution, however recent, whether it be of soda, potass, baryta, or strontian, leaves the alkali electrified positively.

3. That when other solutions, either saline or acid, are employed, evaporation leaves the body which was combined with the water, electrified negatively. Of the numerous saline solutions which were essayed, that of muriate of soda was naturally the one which excited the greatest interest. It formed no exception to the rule. Hence it can hardly be doubted, that evaporation from the surface of the sea forms one of the most important sources of atmospheric electricity. Even lakes and rivers must have their influence, since their waters are never perfectly pure.

EXTRACT FROM AN ESSAY ON SOME OF THE PHÆNOMENA OF ATMOSPHERIC ELECTRICITY. BY LUKE HOWARD, F.R.S., &c. *Read before the Askesian Society in* 1800.

FROM an attentive examination of Read's observations, I have been able to deduce the following general results.

1. The positive electricity common to fair weather often disappears, and yields to a negative state before rain.

2. In general the rain that first falls after a depression of the barometer is *negative.*

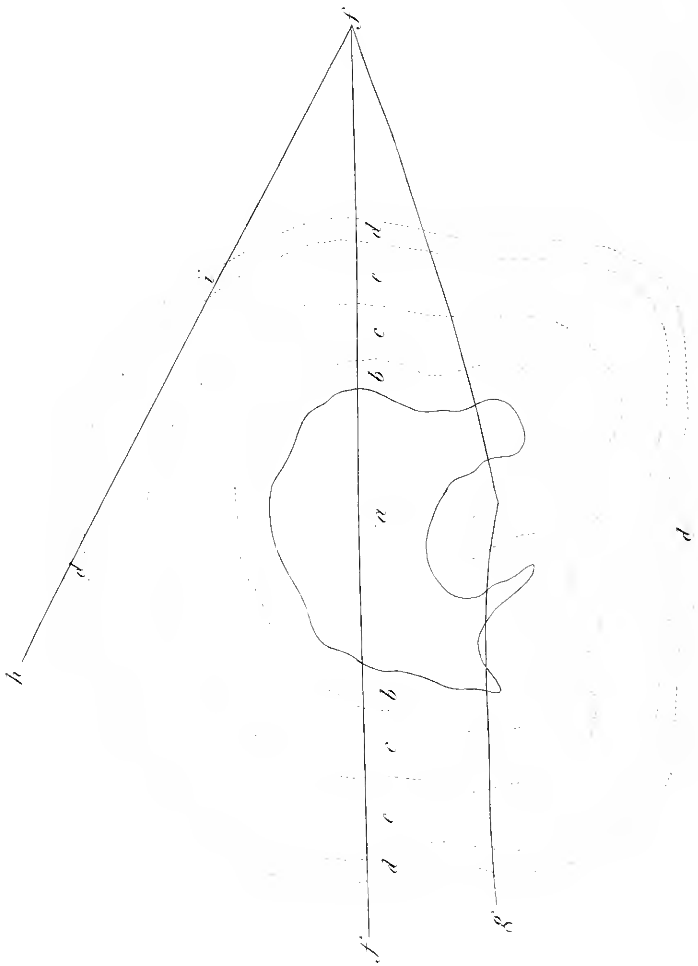
3. Above 40 cases of rain in 100 give *negative* electricity, although the state of the atmosphere is *positive* before and afterwards.

4. *Positive* rain in a *positive* atmosphere occurs more rarely; perhaps 15 times in 100.

5. Snow and hail unmixed with rain are *positive* almost without exception.

6. Nearly 40 cases of rain in 100 affected the apparatus with both kinds of electricity; sometimes with an interval in which no rain fell, so that a positive shower was succeeded by a negative, and *vice versa;* at others the two kinds alternately took place during the same shower, and it should seem with a space of non-electric rain between them.

The regularity with which the latter phænomena sometimes occurred, seem to furnish a clue for explaining some of the preceding cases, and indeed for constructing a hypothesis of local rain. I shall submit to the consideration



of the Society my conjectures, in the confidence of their meeting with a candid examination, and on this account I ought to add, that the latter part of my investigation of Read's Journal has been performed with this supposed clue in my hands; that I have met with some facts to which it is not applicable, and am, therefore, willing to distrust its guidance, except on those points were it applies directly to the phænomena. The members may do well to compare what I shall advance with the Journal at large, since objections may occur to them which escaped me.

Let fig. 1. represent the area on which a local shower falls; a. being a certain portion in the centre in which the rain is charged *positive;* b. b. a surrounding portion in which the positive charge terminates, and which may be considered as occupied by non-electric rain; c. c. the remainder of the area surrounding the two former portions, and occupied by a *negative* charge, which also extends into the surrounding atmosphere, e. e. to a distance proportioned to the intensity of the central positive charge. The non-electric boundary of the negative charge is represented by the line, d. d. d. d. Without this line, the atmosphere is supposed positive as usual when free from clouds. In a shower so constituted, the electric signs obtained by observations made in a single and fixed station, (as Read's were,) would be subject to the following variations.

1. The central area remaining the whole time over the instrument, the observation would *be positive;* and

2. The circumferential area doing the same, it would be *negative.* Many cases in Read will be thus explained, and it is favourable to the hypothesis, that the *positive* observations are to the *negative* nearly as 1 to 3; but, on the other hand, this does not account for the fact of several showers being negative in succession, nor for the relation

which seems to obtain between depressions of the barometer and negative rain.

3. The rain beginning with the central area over the instrument, and ending with the circumferential, the observation would be first *positive*, then *negative* after an intermission of the electric signs.

4. The circumference being first examined, and the rain ceasing by expenditure during the charge from the centre; the order would be the reverse of 3.

5. The cloud passing over in the zenith of the apparatus, and the latter describing under it the line f. f., all the appearances would agree with those cases in which a shower commencing with *negative* electricity, shews itself to be *positive* in the middle and terminates as it began, with *negative.*

6. But the line which the apparatus may be considered as describing under the cloud, in consequence of irregularities either in the motion or form of the latter, may resemble the curve, g. f.; and after having entered the shower or commenced within it, may pass and repass the non-electric boundary several times during its continuance. It may also happen to commence or to terminate in the latter. This will serve to explain some of the most irregular cases in the Journal.

7. It frequently happens, that the apparatus is charged in consequence of rain falling at such a distance, that not even the skirts of the shower come over it. This is particularly the case in thunder storms, and the phænomena are such as ought to take place, according to the hypothesis, when the centre of the mass of clouds and rain (which electrically considered form one aggregate) passes at a certain distance from and parallel to the line, f. k., on which we now suppose the apparatus to be. The latter then loses its *positive* charge at i., and presently acquires a *negative,* which

becomes more intense as the rod enters further into the negative area, and dies away as it quits it, till at k. it becomes extinct.

8. If the station of the observer, during a thunder storm, happened to be in any part of the circle, d. d. d. d., he might be unable, if the time devoted to the observation were short, to obtain any signs whatsoever from his apparatus, although he might both see and hear the successive discharges in the horizon.

I have witnessed such an occurrence myself, and I suspect that what Read has noted under June 22, 1790, is from the same cause. The centre of the storm in this case, appears to have been about Salisbury, distant 80 miles. When we consider the elevation which was necessary to render even the extremity of this storm visible at Knightsbridge, we shall not find this distance too great for the semidiameter of the total area in which its effects might be sensible with a good instrument.

To some of the cases these explanations seem clearly applicable; in others there is room for correction by future observations, which would be far the most instructive if conducted in concert, by several persons at different stations, within the compass of a few square miles. It will be readily seen, that I have made the accumulation of positive electricity in a certain portion of the atmosphere, the basis of the whole system. The remainder follows as a necessary consequence from the known laws of electricity. But the production of positive electricity is not necessarily confined to the centre of an aggregate of clouds, nor its effects to a lateral direction only. Cases may occur in which one extremity of the aggregate may be positive, and the other in consequence, negative; there may be positive electricity in a certain stratum of the atmosphere, and from hence may result a negative counter-charge in a contiguous stratum above or below. In continued rain such a distribution most probably obtains, but we must have more observations to be able to prove it. Our present object is to shew how a local shower is organized, and if possible to trace its immediate origin to electrical causes; for it is in vain that the principles of chemistry alone are appealed to in this case. Let us see therefore how it happens, that the centre of a shower is often strongly *positive*. The clouds originate from vapour, which is first formed in contact with the earth. It is not therefore then electrified, except the surface on which it is formed be at the time super-induced. But the latter is the proper effect of impending clouds, and although a truly electrised vapour may be thus formed, and being condensed, may constitute a part of the system of clouds in a thunder storm, yet our present enquiry goes further: we want to account for the super-inducing charge.

It would be a difficult undertaking to ascertain by experiment the electrical state of vapour, and of the surface on which it originates in the natural process. Experiments have been made on insulated substances at high temperatures, the results of which, even if more conclusive, would be quite inapplicable to this case. I shall therefore offer some conjectures on the origin of atmospheric electricity, which will in the first instance proceed on the supposition that vapour is originally non-electrised. A body in order to be charged must be first insulated, and the charge will continue during perfect insulation, but the latter seems unattainable. There is always a small degree of conducting power in the very atmosphere when at the maximum of dryness, and this is greatly augmented by what is called moisture, by which I understand, diffused and suspended (not elastic and gaseous) water.

We can scarcely imagine a body more perfectly insulated than the first particle of water which, separating from vapour that has ascended into the higher atmosphere, begins to obey the law of gravity. There are two sources from whence such a particle may obtain an electric charge, viz., the surrounding air, and the vapour out of which it was formed, and which may, though in itself non-electrified, afford to the water, now reduced many hundred-fold in volume, a real positive charge. Appearances, likewise, are much in favour of the opinion, that the precipitation of water in the higher atmosphere is sometimes effected by a double affinity, in which electric air and gaseous water are mutually decomposed, the former seizing the caloric, the latter the electric fluid.

At all events we are certain of the fact, that clouds are insulated and charged conductors. Franklin supposed, that clouds arising from the sea were positive, those from the land negative, and that their rencounters in the air were the cause of thunder storms. Kirwan, and others, go a little further, and say, that a positive cloud (become such in the way I have stated) may affect another with a negative state by its approach, and thus attract it to form rain. But all these explanations fall short of the phænomena. Had this been all the process, we should have known nothing of the electricity of rain, for a negative and positive cloud would unite in those proportions only, which should form non-electric rain. * * * *

The reader who may be curious further to pursue these highly interesting meteorological considerations, is referred to a new edition of Luke Howard's work on the Climate of London, now in the press, and in which will be found the whole of the paper of which the preceding is an extract, together with much new and important matter on collateral subjects.

I have been induced to give the preceding extracts, from the idea that they may tend to throw some light on the very interesting, but still imperfectly understood subject of the influence of electricity upon vital phænomena.

The observations of Prevost and Dumas, contained in the Appendix, relate to the supposed operations of electricity, as an agent in some of the functions carried on within the body, and more especially in conjunction with muscular contraction. The views which they contain are extremely ingenious and interesting, but I must confess myself unable fully to adopt them.

The experiments and operations of Pouillet, respecting the development of electricity by the process of vegetation, led me to conclude, that a similar development must take place in the production of carbonic acid by the respiration of animals, and also by the vinous fermentation of fluids. My attempts to demonstrate the correctness of this suspicion have not yet been successful. I hope hereafter to pursue the enquiry, and in the mean time I shall relate a few facts which seem to bear on the subject. It has long been observed that individuals of highly sensitive constitutions are concious of uneasiness, sometimes amounting to absolute pain, or the disturbance of function during the existence of a thunder storm; this is by no means necessarily connected with fear, or other mental emotions, produced by the loud sound and vivid light, or any other phænomenon cognizable to our senses. The influence of which I am speaking is frequently felt before the storm has commenced, and has occasionally been experienced by individuals so far removed from the skirts of the storm, as not to be conscious of its existence at the time, except by the intimations afforded through the symptoms in question. Such cases seem to be analogous to the instance alluded to in the paper of Luke Howard, in which Read's apparatus, set up at Knightsbridge, for the examination of aerial electricity, was influenced by a storm supposed to have passed over Salisbury. Persons who watch the habits of leeches, have frequently remarked their peculiar agitation when the electric state of the air is disturbed by storms, and it is believed by persons accustomed to the rearing of poultry, that storms sometimes have an injurious, and even a fatal influence upon eggs undergoing incubation. It is a generally admitted fact, that liquors undergoing the vinous fermentation, suffer a great disturbance in this process during the existence of a thunder storm. These facts taken together, led me to question, whether the negatively induced electricity may not have a tendency to disturb the production of carbonic acid, which Pouillet has shewn to escape in a negatively electric state.

We have as yet but few well-conducted and satisfactory observations, respecting the influence of an artificially disturbed electric state upon living organized beings. Some observations have been made with reference to vegetable physiology, and as this may often be appealed to for assistance, in our attempts to elucidate the more difficult subject of the physiology of animals, it may not be amiss briefly to relate them.

With respect to dead animal and vegetable matter, the experiments of electricians completely tally with what has been observed to be the effect of the electric disturbance of the atmosphere. The observations of Achard of Berlin support this assertion. They are briefly noticed in the Encyclopædia Metropolitana, from which the following short statement is extracted.

It is a well known observation, that after a storm, flesh, either raw or boiled, acquires a putrid smell, which, in the latter, is particularly acid. It is known, also, that grain suffered to ferment for the purposes of brewing or distilling, undergoes, during stormy weather, very sudden and perceptible changes. On such occasions, it is often extremely difficult to observe where the first degree of fermentation ceases. It passes so speedily that the second degree, or the acetous fermentation, takes place before one is aware of it. To ascertain, therefore, whether the electric matter, which, during stormy weather, is so abundant in the atmosphere, has any share in these phænomena, the following experiments were made.

A piece of raw beef was cut into three parts. One of these parts was electrified positively for ten hours without any shock; a second was electrified negatively for a similar time; and the third was not electrified at all. The three pieces were left in the same apartment, exposed to the same degree of heat. When examined next day, both the pieces which had been electrified appeared to be tender, but were free from the least bad smell. On the fourth day, the electrified flesh had an intolerably fœtid smell, and that which had not been electrified began to smell a little.

M. Achard repeated these experiments with boiled veal. That which was electrified had, the next day, an acid smell, and an unpleasant taste; but that which had not been electrified, continued sweet for three days, and only on the fourth day began to have an acid smell.

Several birds were killed by electric shocks, and others were deprived of life, by sticking a needle through their heads, and then placing all in the same temperature, they were covered with glass receivers in order to preserve them from insects. Observing the gradual progress of corruption in both sets, M. Achard plainly perceived, that it took place much sooner, and advanced more rapidly in those killed by electric shocks, than in those deprived of life by the needle. In those also, to which a stronger shock had been given, the degree of corruption was far greater than in the others.

Van Marum made a similar observation with respect to the rapid decomposition of eels, which had been killed by electricity.

It clearly follows from these experiments, that electricity accelerates corruption, and that the putrefaction of flesh after a storm, must be ascribed solely to the more abundant accumulation of the electric matter at that time. M. Achard saw that this was the case in regard to several persons killed by lightning. The body of a farmer, who lost his life in this manner, between five and six o'clock in the evening, emitted next morning a very perceptible fœtid smell, which in the evening was totally insupportable.

Having stated these effects of electricity on dead vegetable and animal matter, which are sufficient to shew its power in modifying and accelerating chemical changes, we may now inquire after what is known of its influence when life is present. We shall commence with vegetables, in which the direct physical effects are less complicated, from their not being mixed up with what may be regarded as its moral effect on a highly sensitive nervous system.

It is well known that trees may be killed by lightning, but in these instances there is so much violence and destruction of texture, that we can draw no conclusion from them as to the influence of electricity. Cavallo, though he disputes the correctness of the statements of some electricians, with respect to the influence of electricity on plants, has shewn that the *balsamina impatiens* is killed by shocks which are too slight to impair the structure. A branch of this plant died the day after receiving the shock—the branches of other plants survived longer. A laurel branch lived fifteen days and that of a myrtle a whole month.

Van Marum and Nairne confirm the deleterious effects of electric shocks, shewing that they kill some plants and prevent slips from taking root and budding.

If electricity in the form of shocks has the power of destroying vegetable life, we may reasonably presume, that a less violent application of this agent would produce some sensible modifying effect. The first experiments upon the application of electricity to living vegetables, appear to have been made by Mainbry, in Edinburgh, in the year 1746. In the autumn of that year, he subjected two myrtles to gentle electric action during one month, and observed that they subsequently put forth leaves earlier than similar trees which had not been electrified. The Abbé Nollet, Jallabert, Boze, Menon, Dr. Carmoy, the Abbés D'Ormoy and Bartholon, maintain the power of electricity as a stimulant to vegetables; the last named experimenter, in particular, has been extremely zealous in this enquiry. He regarded electricity as a most powerful stimulant to vegetation, and recommended its practical application in horticulture, for which purpose he contrived an apparatus, called electro-vegetometer, with which he employed artificial excitation. He proposed the direction of atmospheric electricity to the same object, and believed that he produced some good effect by watering plants with water charged with electricity. Although there can be little doubt, that the Abbé's enthusiasm in his subject led his imagination to the over-straining of facts, yet it is by no means improbable, that there is more truth in his observations than Cavallo and Ingenhouse are disposed to admit. It has been asserted, that plants grow with encreased vigour in the neighbourhood of thunder rods. It might have been supposed, that if plants in general are influenced by electricity, those plants which offer the most striking proofs of sensibility would be the most signally excited by it. Van

Marum was, therefore, led to try its effects on the *mimosa pudica*, and on the *hedysarum gyrans*, but he could not detect that their movements were positively affected by it. It will be well to bear this fact in mind, when considering the motions of animals and vegetables.

The fatal effects which violent discharges of electricity produce on animal life, are more notorious than in the case of vegetables. Scarcely a summer passes without numerous instances occurring in which man and other animals are killed by lightning. The discharge of an ordinary battery is sufficient to kill insects and worms, and the shock from a tolerably large one will kill mice. It is truly surprising to see how instantaneously some of the lower animals, which are remarkably tenacious of life, are completely deprived of it by the electric shock. I once discharged a battery of considerable size through a common earth-worm, which would in all probability have shewn signs of life long after minute division. Its death was as sudden as the shock, and the semi-transparent substance of the animal was changed like albumen which has been exposed to heat.

The artificial accumulation of electricity in batteries of very large size, has been found sufficient to kill not only rabbits, but even large and vigorous hogs, a fact which was completely proved by my friend Charles Woodward in the presence of Dr. Scudamore.

It may now be interesting to notice some of the phænomena induced by electricity, within those limits which are compatible with life. With the hope that this short exposition may tend to assist, either in the extension or application of our present knowledge of the subject, I shall endeavour to class these phænomena under the following heads.

Electric tension—its effects on the system generally—difference of the positive and negative charge—its effects on particular functions, such as circulation, exhalation, and secretion and respiration—the effects of the transmission of shocks—of a continuous stream—sparks—aura—application.

It is well known, that that state of the atmosphere which is unfavourable to electrical experiments from its being adverse to the insulation, and consequently to the electric tension of bodies, is also ungrateful and oppressive to our feelings; and that precisely the opposite effect is experienced in clear and frosty weather, and in other states of the atmosphere which facilitate the working of electrical machines. We might regard these as coincident, rather than connected facts, if it had not been observed that an artificial repletion with electric fluid produced a similar effect in exhilarating the spirits, a fact for which, with many others here related, I am indebted to my friend Charles Woodward of Islington, a gentleman who has long and successfully devoted his attention to electricity.

This fact conducts us to the enquiry, whether there is any difference, as far as the influence on the animal economy is concerned, between a positive and a negative charge. Here, I regret to say, that I have very little of a decisive character to bring forward, yet I may state on the same valuable authority which I have just given, that a negative charge continued for about half-an-hour, has caused an unequivocal perception of languor and oppression. Do we not find an obvious parallel to this experiment in the powerfully oppressive and sometimes distressing influence of which highly sensitive individuals are conscious on the approach of a thunder storm, or during the prevalence of a north-east wind, which is characterized as peculiarly un-healthy and productive of a sensation of dryness and cold, unaccompanied by a corresponding depression of the thermometer? It was shewn, in the preceding article on Atmospheric Electricity by Luke Howard, that a highly electrified thunder cloud is surrounded to a considerable extent by atmosphere which is in a negative or neutral state. The north-east and east winds are often in a similar condition.

Except in cases of the transmission of a strong electric current through some part of the nervous system, which produces an instantaneous disagreeable, or even fatal effect, it would seem that a considerable portion of time is necessary for the production of anything like a sensible effect from disturbance of the electric equilibrium. Leeches, as I before observed, are said to be highly susceptible to very slight alterations in this respect. I was, therefore, led to enquire what would be the result of a great, but sudden and transient encrease of their electric tension. I was careful to avoid subjecting their bodies to the direct effect of a spark or shock. I placed several active healthy leeches in a glass vessel containing water, which being thus insulated, I kept for a little while strongly electrified positively, but without producing any sensible effect. Precisely the same result attended a negative charge. I next tried what would be the effect of converting the vessel containing water into a Leyden jar, by applying a partial coating of tin-foil outside. Having given a moderate charge, I suddenly restored the equilibrium by discharging the jar, but I could not perceive any unequivocal appearance of uneasiness in the leeches, which remained perfectly healthy for several days, after which they were no longer watched.

Although the statements which are made respecting the effects of increased electric tension upon the circulation at first appear contradictory, a little consideration will satisfy us, that these decrepancies are analogous to such as attend the application of other stimulants. Walker and Carpue have both recorded, that the blood flows more freely from an opened vein when the patient is electrified. The force of the circulation must, therefore, have been increased. Cavallo was assured by an experienced medical electrician, that in a diseased state of body, an evident acceleration of the pulse is often observed to result from the application of electricity. Van Marum took considerable pains to investigate this point. Eleven persons were selected, and the experiment was repeated four times upon each, both with positive and negative electricity. These persons were placed in a room which was at such a distance from the machine, that they could not hear the noise it made in turning; they were insulated, and the pulse of each was felt when the machine was in motion, as well as when it was at rest, (which last circumstance was unknown to them,) and the beats were counted by a good observer, provided with an excellent watch. In some cases a few more beats were observed, but, on the whole, there was no important acceleration. In general, however, there was great irregularity in the pulse, both during the time the persons were electrified, and during the time the machine was at rest.

There can be little doubt, but that the individuals who were the subject of this experiment, were much influenced by mental emotion, and it seems very probable, that the interruption in the application of the electric influence, contributed to vitiate the result.

The following observations on this subject were communicated to Charles Woodward, Esq., by his friend F. Smith of Fordham, and are confirmed by his own experience.

The pulsations were carefully watched before electrification. The electric influence was passed through each person without sparks, and continued fifteen minutes, after which, while the fluid was still passing through them, the pulsations were again noticed.

Patient.	Pulse at first.	Reduced to
1	80	75
2	64	51
3	90	76
4	66	61
5	70	60
6	74	62
7	65	58
8	100	90

These individuals were all males.

Electrification for less than ten minutes did not in general appear to affect the pulse.

It may be imagined, that in the subject of this experiment, the previous apprehension had raised the pulse, which subsided when they were quietly placed on the stool.

This objection appears to be met by two cases of ague, in which the pulse was soon reduced by electricity thirty beats per minute during the paroxysm. As one of these instances occurred in the electrician's own person, it cannot be supposed that the pulse had been raised by trepidation.

When the Abbé Nollet had shewn, that the evaporation of volatile bodies is promoted by an electric charge, it became a matter of curiosity, whether it had any influence on the insensible perspiration of animals. Many experiments were made which seemed to prove, that that function was increased whilst the animals were electrified. Van Marum, who afterwards undertook to examine this question, came to a different conclusion.

For the purpose of ascertaining the increase of insensible perspiration, he employed a very delicate balance; one scale of which was insulated by means of a silk cord. On this scale he placed a boy, eight years of age, connected with the conductor; and the balance was brought to a state of equilibrium. He then ascertained the loss of weight sustained in half an hour, before the boy was electrified, and found it to amount to 280 grains. By a similar experiment on another occasion, the loss of weight, before being electrified, was 330; and after exposure to electricity only 310. A girl of seven years old lost, before being electrified, 180; and when electrified, 165 grains. A boy of eight years and a half lost, before being electrified, 430; and when electrified, 290 grains. Another of nine years unelectrified, 170; electrified, 240. As the last boy was exceedingly quiet during the experiment, it was thought that the increase was the consequence of electricity; on this account he was several times subjected to the experiment, and the results were: in the unelectrified state, 550; in the electrified, 390, 300, 270, 550, and 420.

If we consider these experiments of Van Marum, in conjunction with the light thrown on the subject of perspiration, by the observations of Dr. Edwards, given in this volume, we shall have no difficulty in perceiving, that they by no means warrant the inference, that increased electric tension is unfavourable to perspiration. It will be remembered, that it was proved by Dr. Edwards's experiments, that the loss by perspiration in a given period, progressively diminishes as the body perspiring recedes from the point of saturation. Circumstances may for a time produce apparently contradictory results, which, when investigated, rather confirm than invalidate the principle. It can scarcely be doubted, that electricity promotes evaporation from the surface of inanimate bodies, but it must still be regarded as an undecided question, in what manner it effects the insensible perspiration of animals. From facts recorded by medical electricians, it appears that some secretions are promoted by electricity, but it has been found, both in Germany and in this country, that the excessive secretion of urine in diabetes has been repressed by electricity applied in the form of galvanism to the loins. The function of absorption appears, in some cases, to be salutarily excited by electricity. It has been successfully applied to tumours, with a view to promote their dispersion. In one case, which occurred to Philip Smith of Fordham, sixteen applications of electricity, employed for another object, had the gratifying effect of removing a chronic hydropic affection. Even ovarian dropsy, than which no variety is regarded as less under the controul of medical means, seems, in some instances, to have yielded to its influence. Although there is no function more important to life, or more intimately connected with other functions than respiration, and none which possesses so decidedly chemical a character, and consequently bears so obvious a relation to the changes of inorganic matter, to which electrical phænomena are accessary or concomitant; yet, we are still perfectly in the dark as to the relations which may exist between this function and electricity. I have already mentioned this subject as one which I have in vain wished to investigate, and, therefore, have little to offer respecting it. There is, however, one circumstance which I may mention, not only because it tends to shew that a connection between respiration and electricity is not purely imaginative, but because it may serve as a hint for one of the modes in which the enquiry may be pursued. It has been asserted, that hens hatch their eggs after a shorter period of incubation, when they and their nest have been insulated and kept in a state of state of increased electric tension. Now the only function by which the eggs are in relation with surrounding objects, is that of respiration, carried on by the vascular membrane within the shell, through the pores of which the atmospheric influence is exerted.

The passage of the electric influence is probably productive of no less important effects on the animal economy, than increased tension. In some instances their effects may be combined. The sensation produced by the electric shock, is the most notorious and perceptible effect which electricity produces in the system, yet it is so transient, that except in those cases in which it has been of sufficient force to be injurious to life, it is generally limited to the inappreciably short interval occupied by the discharge. The rapid succession of sparks received at one part of the body, and given off at another, the individual being insulated for the purpose, is an approximation to a continued current of electric influence, and though much milder than the shock, as far as the feelings are concerned, exerts a much more powerful influence on the system and is of far greater service as a medical application. This is still more strikingly the case, when the sparks, whether given or taken, are reduced to a very small size, though encreased in number by bringing the metallic ball nearly into contact with the person, a thin piece of flannel alone intervening between it and the skin. It is in this form that the electric current has been long and very successfully employed in a variety of maladies by C. Woodward.

The electric aura affords the means of applying a yet more equable and continuous current, the effects of which appear to be proportionately superior, notwithstanding they are even less perceptible to the senses. The reality of this influence is confirmed by phænomena connected with dead inorganic matter, as well as by its effects on parts of the living system. All attempts to make the compass deviate from the magnetic meridian by means of common electricity, as Œrsted had done by galvanism, had been unsuccessful, until C. Woodward conceived the idea of applying electricity in the form of aura to the wire destined to produce the deviation. This plan he found completely answer, and repeatedly exhibited it some time before Dr. Wollaston, by whom the fact has been announced, appears to have had his attention arrested by it. The hand of a lad had been long permanently and powerfully clenched in consequence of a blow from a hammer. Every mode of treatment completely failed, until the electric aura was applied, which effected a perfect cure.*

The transmission of the electric influence seems to act directly upon the nervous system of animals. I have already alluded to the sensation which it excites, and which is manifestly to be referred to the nervous system. Defects in the senses of feeling, seeing, and hearing, and distressing neuralgias, have been relieved by its influence on that system. Through the medium of the nerves, the electric current acts powerfully on the muscular system. This is most conspicuously seen in recently dead or expiring animals, but it is also evident in many cases of paralyzed limbs. Some of the phænomena connected with this part of the subject are so striking, that it is by no means surprising that some physiologists should have regarded the natural influence of the nerves in the production of muscular motion, as of an electric character. Porret, Prochaska, Dr. Wollaston, and others, have endeavoured to shew by analogy, that secretion is effected by an electric current. Dr. Young has proposed a similar theory. Drs. Wilson Phillip and Hastings have laboured to prove, that the fatal effects of the

* See a cast of this contracted hand in the Museum of Guy's Hospital.

division of the eighth pair of nerves depend on the consequent interruption to secretion, which the application of electricity to the divided nerves will in a great degree restore and maintain. Dr. Milne Edwards opposes this view, and contends that an equally advantageous effect is produced by the mere mechanical excitation of the divided nerves.

The following fact is of no less practical importance than physiological interest. I am informed by my friend C. Woodward, that it is an essential rule in the application of electricity to medical purposes, that the current should pass in the direction from the trunk to the extremity of the affected limb. In this way it often affords prompt relief; but if the current be reversed, the evil is aggravated.

The experiments which I have next to relate appear so truly wonderful that, but for the good authority by which they are supported, I should feel unwilling to give them a place here. The remarkable results which accompanied them can scarcely be explained as coincidences, and they appear to open the way to new and important inquiries; but numerous observations and experiments must be made before we can be warranted in drawing any certain conclusions on the subject. P. Smith of Fordham, to whose experiments I have already alluded, having relieved numerous patients, labouring under gout, rheumatism, and other painful affections, was induced to try the effects of electricity upon intermittent fevers. He intended to employ it as the cold stage was coming on, but his first patient having mistaken the time of the accession of the fit, he did not apply it until the hot stage had commenced: he insulated the man, and caused him to receive sparks at the epigastric region, while he took them from him along the course of the spine; the pulse was speedily reduced thirty beats per minute. The next patient was electrified on the coming on of the chill, which it immediately checked; nevertheless the hot stage ensued, when electricity was again applied, and, as in the former case, it reduced the pulse thirty beats — there was no return of the paroxysm, but the application of electricity was repeated for some days. Another case of ague, which had lasted four months, and obstinately resisted bark, arsenic, and other medicines, was quickly cured by a few applications to electricity. The most extraordinary circumstance remains to be mentioned. P. Smith himself had held the ball, with which he took sparks from his first patient, during the hot stage. In the same evening, he found himself unwell; but had no suspicion of the nature of his complaint, until the recurrence of the paroxysm convinced him that he had become the subject of ague. He allowed these to recur to the seventh time, before he attempted the cure by electricity, which was speedily effected, being the second case already alluded to. As he had never been the subject of ague, and had not been more than usually exposed to causes calculated to give rise to it, he felt persuaded that it had been communicated to him by electricity from his former patient.

In order to ascertain this, he was desirous of trying experiments on some persons labouring under a disease which was inflammatory, but not considered infectious; he, therefore, had one of his men vaccinated. On the seventh day, the man was placed on the insulating stool, and connected with the positive conductor; a small incision was made with a lancet in the pustule, and an incision was also made in the arm of a lad with a *new* lancet; a wire four inches long was passed through a glass tube, one end of which touched the pustule on the man's arm, and the other the incision on the boy's arm — the electrification was continued for eight minutes, when the boy was removed. His arm was daily examined, and it was found, that he was as completely vaccinated by electricity as any person could be by the usual mode. My friend afterwards endeavoured to communicate the virus to two girls, by passing the electrical fluid from the pustule on the boy's arm, who had been vaccinated by electricity, to incisions made in theirs. For three days the medical gentleman supposed it had taken effect; but, on the fourth day, all appearances of vaccination died away. These girls were, however, afterwards vaccinated in the usual way in four places, two of which died away, and the other two took but very slightly. My friend Charles Woodward afterwards repeated this experiment upon an infant, the child of one of his friends, but with this difference, that he did not allow the conducting wire to come in contact with the child's arm. The electric fluid was consequently transmitted in the form of small sparks. The disturbance which these produced, though trivial, prevented the application from being prolonged for the full time, which my friend would have wished; inflammation however succeeded, and, until the sixth day, was such as to induce the medical attendant to believe, that the vaccination had been complete; from that day, however, the pustule died away.

Circumstances, which it is wholly foreign to this work to relate, interrupted the continuance of the research. The facts already related have come to my knowledge at too late a period to allow of my pursuing the investigation; but I trust, it will not long continue neglected.

DISSERTATIO PHYSIOLOGICA

INAUGURALIS

DE ABSORBENDI FUNCTIONE;

QUAM EX AUCTORITATE

ORNATI VIRI, D. GEORGII BAIRD,

ACADEMIAE EDINBURGENAE PRAEFECTI;

NECNON

AMPLISSIMI SENATUS ACADEMICI CONSENSU;

ET NOBILISSIMAE FACULTATIS MEDICAE DECRETO;

PRO GRADU DOCTORIS,

SUMMISQUE IN MEDICINA HONORIBUS AC PRIVILEGIIS
RITE ET LEGITIME CONSEQUENDIS;

ERUDITORUM EXAMINI SUBJICIT

THOMAS HODGKIN,

ANGLUS,

SOCIETATIS REGIAE MEDICAE EDINENSIS, ET PHYSICAE GUYENSIS SOCIUS.

" Τὸ δὲ ὅπως ἐγένετο τοιοῦτον, ἐὰν ἐπιχειρήσης ζητεῖν ἀναίσθητος, φωραθήσῃ καὶ τῆς σῆς ασθενείας, καὶ τῆς Δημιουργοῦ δυνάμεως."—GALENUS.

Die i. Mensis viii. (Kal. Aug.) horâ locoque solitis.

VIRO OPTIMO

ALEXANDRO HUMBOLDT,

Philosopho, qui eximiis ingenii dotibus et omnigena scientia instructus, magnam AMERICAE partem obiit, ubi quaecunque proponit natura in illo magnifico suo theatro contemplatus est, diversarum gentium mores, urbesque cognovit, ANTIQUORUM INCOLARUM fugaces ac perituras historiae reliquias indagavit, collegit, oblivioni eripuit, et quodcunque dignum memoria sibi visum est, aere perennioribus scriptis mandavit; ita ut iis, qui peregrinari velint, exemplar imitatione dignum, in omne aevum posuerit; ILLI PRAESTANTI VIRO, qui, dum scientiae fines assidue ampliaret, inter omnia, nil humani a se esse alienum, nunquam non putabat; propter meam singularem in eum observantiam, atque in testimonium animi memoris beneficiorum, quae mihi indulsit, cum Lutetiae Parisiorum studiis incumberem, opusculum hoc offero atque dico. O utinam pro ipsius meritis digniora possem referre!

THOMAS HODGKIN.

PRAECEPTORI SPECTATISSIMO

ANDREAE DUNCAN, JUN. M.D. R.S.E.S.

Collegii Medicorum Edinensis Praesidi, materiarum medicinalium, itemque medicinae clinices, hac in academia professori, &c. Qui cum scientiam praelectionibus promovet, tum insigni exemplo industriam, liberalitatem, et candorem commendat et illustrat; ob paternam solicitudinem, qua me, dum scarlatina laborabam, curavit, et propter fructus et commoda nunquam obliviscenda ex ipsius disciplinis percepta, cum in hujus urbis valetudinario sub illo clinicis officiis et munere fungerer, hoc tentamen, pignus exiguum, animi non ingrati defero, et honoris amicitiaeque causa illius nomine inscribo.

T. H.

PATRI SUO

THOMAS HODGKIN, S.P.D.

OFFICIO sane deessem, si in ÆSCULAPII aedem ingrediens, mei amoris et observantiae erga te, cui tot et tanta debeo, notam non ponerem.

Me prohibet, illa tua modestia, ne nunquam obliviscenda tua in me officia ac merita, enumerare suscipiam. Lector, cui forsan, hauds ecus ac mihi, optimo patre uti concessum est, mente finget, ea quae verbis exprimere non possum. Plura igitur non addam.

Licebit tamen tibi precari, ut diu vivas, et ita *pancraticè* valeas, ut si quid in meâ arte feliciter valeam, potius ex aliis scias, quam propriâ experientiâ sentias.

Edinburgi, Die i. *Mensis* viii. (KAL. AUG.) MDCCCXXIII.

DE

ABSORBENDI FUNCTIONE.

PROEMIUM.

Priusquam ad rem veniam, de quâ in hoc tractatu disserere institui, pauca praefari liceat mihi, ne quis hoc mihi vitio vertat, vel ex unâ parte, quòd rem chemicè tractem, vel ex alterâ, quòd curiosissimas et minutissimas elementorum e quibus constent corpora investigationes nimis negligam, et quòd ipse perpauca et quidem rudia experimenta, minimè cum hodiernae chemiae subtilitate congruentia instituerim.

Nequaquàm profectò me fugit multos et quidem praeclaros esse physiologos, qui vasorum absorbentium actionem, et cum illâ arctissimè conjunctas functiones, nimirùm concoctionem, secretionem, et nutritionem, extra chimiae provinciam esse, et omninò sub vitae solius ditione positas esse contendant. Hanc opinionem Cel. Adelon, de absorbendi functione scribens, iterùm atque iterùm protulit, ut ex sequentibus excerptis abundè patebit. "Cette action n'a en elle rien de chemique, comme nous l'avons dejà dit; et en effet il n'y a nuls rapports chemiques entre la composition du chyme et celle du chyle; de la connaissance chemique de l'un on ne peut conclure à la formation de l'autre."* Sic quoque in alio loco.

"L'action d'absorption n'est pas d'avantage une action chemique, car il n'y a nul rapport chemique entre les matériaux des absorptions internes, et les fluides, lymphe, et sang veineux, qui en sont les produits; de la composition chemique des unes, on ne peut, à l'aide des lois chemiques generales, conclure à la formation des autres; ces absorp-

* Dictionnaire de Medecine, vol. i. p. 133.

tions ont enfin pour résultat de créer des matières organisées, et la vie seule, comme on le sait, a cette puissance."*

Hîc vero supervacaneum foret plura excerpere, ut opinionem omnibus cognitam existere demonstrem.

Ego autem huic sententiæ assentire nequeo. Quoniam enim intimam corporum compositionem, rationem scilicet, et proportionem, quibus minutissimae particulae, rerum primordia, vel corpora prima LUCRETII inter se junguntur, mutatam esse manifestum est, et cùm porrò talium mutationum, nisi sint in corporibus animantium peractae, investigationem chimia omnium pace sibi sumat, declarare non dubito, etiam in vivo corpore has investigationes chemici esse.—Et sanè mihi videtur hanc meam sententiam non leviter esse confirmatam, eo quòd ex chemiâ ipsâ praecipuè cognoscamus, istas fieri mutationes, quarum rationem dare, ad chemiam non pertinere praedicatur.† Nonne physiologi in errore versantur, vel ex unâ parte chimiam nimis rejiciendo, vel contra, varias mutationes, quae in vivis corporibus perficiuntur, jam notis chimiae legibus, in corporibus carentibus vitâ agentibus, explicare conando? Has leges ad supra dictas functiones explicandas non valere, lubentissimè concedo. Haec autem impotentia probat solummodo scientiam ad summum perfectionis suae nondum pervenisse. Quòd si etiamnum calorici, et vis galvanicae facultates ignorarentur, et phaenomena ex his pendentia chimico cuivis obvia venirent, non modo rationem dare non valeret, sed cognitas theorias oppugnari fateretur.

Nonne in pari pene difficultate, quoad varias vitae functiones, adhuc versamur? At, me judice, nondum satis tali ratione physiologi vitae vires contemplati sunt. Pariter ac innumeri sunt sinus et fretus

"Sub utroque mundi axe jacentes,"

adeo nive et gelu praeclusi, ut hucusque exploratorem

* Dictionnaire de Medecine, vol. i. p. 150.

† Quippe cùm earum materiarum, quae variis animalium organis subjiciuntur, et quae in iis mutationem subeunt, elementa, antéquam in corpus accipiuntur vi ejus attractionis, quae a chemicis affinitas vocatur, inter se retineantur, necesse est ut ad eam vim opprimendam validior alia, et chemicis facultatibus pollens, in corpore insit, et praetereà, cùm prior saltem, et verisimillime altera quoque vis constans sit, necesse est ut effectus constantes sint, et certis legibus obedientes.

quemque frustrati sint, quos geographus non ideò extra suam esse provinciam unquam protulit, licet incertae liniae in chartis expressae mortalium impotentiam testentur, donec Parrius et comites, illisque similes, quos

. . . . "non Boreae finitimum latus
Duratae que solo nives
. . . . abigunt,"

forsit olim opprobrium tollant: sic neque nos ob confessas difficultates ulli scientiae suum munus et officium surripiamus. Hae enim difficultates, aut improbo labori cedent, aut illam Galeni sententiam huic tractatui praefixam confirmabunt:

"Τὸ δὲ ὅπως ἐγένετο τοιοῦτον, ἐὰν ἐπιχειρήσῃς ζητεῖν, ἀναίσθητος, φωραθήσῃ καὶ τῆς σῆς ἀσθενείας, καὶ τῆς Δημιουργοῦ δυνάμεως."

Restat modo ut paucis chemiae fautores placem. Tantùm abest ut labores illorum, qui variarum corporis texturarum, et humorum, qui in his texturis insunt, vel ab his secernuntur, compositionem, cùm in sanis, tùm in vitiatis corporibus minutissimè examinârunt, parvi faciam, ut si quid illorum gestis addere conarer ego mihimet arrogans viderer. Inter illos enim numerandi sunt viri meritò celeberrimi Lavoisier, Wollaston, Fourcroy, Vauquelin, Proust, Marcet, Berzelius, Chevreul, H. et J. Davy, Brande, Prout, permultique alii ingenio et scientiâ praediti.

Verùm illorum experimenta quanquam maximè laudanda, et ad chimiae scientiam utilissima, physiologiae parùm profuisse negari non potest. Hoc autem, ni fallor, variis causis attribuendum est. Si chimicus non adest, si instrumenta, et alia necessaria paranda sunt, tempus varias mutationes ciet, ita ut novae materiae, a vitâ omninò alienae, reperiri possint; hujus modi quoque materias in complicatis ipsius chimiae operationibus saepe formari credendum est. Ad haec, cùm etiam chemico cuivis peritissimo, si subtilem investigationem inire velit, necesse sit ut materiae examinandae copiam quandam in manibus habeat, humores in tenuissimis vasis, in quibus tamen inexplicabiles illae mutationes fieri creduntur, hoc modo examinari non posse manifestum est. Docent praeterea et ratio et experientia, varias corporis materias proportione elementorum non esse constantes: Sunt denique et aliae causae, quas hîc proferre supervacaneum reor: Satis est dicere, me jam prolatis argumentis inductum, illas minutias praetermittere, et potius proprietates manifestas, et constantes indagare maluisse. Sic enim existimabam absorbendi functionis leges meliùs esse investigandas. Quod si ego forte ea duntaxat vestigia indicavi, quae alii sequentes vitae viam non palantes exquirere possint, sum voti compos, meque permagno laboris praemio donatum arbitror.

DE ABSORPTIONE.

Animalium corpora absorbendi facultate praedita esse, adeo omnibus liquet, ut argumentis ad hoc probandum nequaquam opus sit. Quis enim bestiam benè pastam, vires et auctam molem acquirentem, contemplari potest, quin Lucretii verbis exclamet

"Dissupat in corpus sese cibus omne animantum."

Si vero hanc facultatem accuratiùs inspicimus, eam esse functionem insignem, cum multis aliis arctissimè conjunctam, atque nulli, momento inferiorem constabit. Non tantùm alimentis dat viam, quâ in corpus intrare, idque nutrire possint, sed quoque, et rebus alienis, quae nonunquam in densissimis structuris reperiri possunt; ex hoc, modo morbos, inducit, modo curanti medico, ad illos levandos auxilium praebet. Haurit etiam ex ipso corpore, et submovet eas particulas, quae aliquandiu in corpore manserunt, et vitae diutius prodesse non possunt.

Alioqui necesse esset ut animal, superpositis accumulatisque particulis, tanquam crystallus incresceret; id quod ipsa partium forma refutat. Ut mihi videtur, in variis secretionibus absorptio haud parum agit, certé jam secretos humores non nihil mutat.

A meo proposito alienum est, in hâc dissertatione omnes formas sub quibus absorbendi functio sese praebet separatim tractare. Non enim libello, sed volumine opus esset. Illas tamen breviter enumerare licebit, et ordine quem secutus est Adelon in excellente tractatu jam citato, quanquam cum illo auctore non in omnibus ejus opinionibus consentio, lubentissime utar.

Forma simplicissima hujus functionis in animalibus inferioris ordinis, nec intestina, nec cavum in quod cibus accipiatur habentibus reperitur.* Haec enim animalia toto corpore alimenta ex circumfluentibus aquis undique imbibere videntur. Foetus quoque humanus, qui dum maturitatem assequitur cum variis inferioribus animalibus non ineptè comparatur, hoc modo a nonnullis physiologis, quos inter numerandus est BLAINVILLE, optimus ille professor, in primis diebus nutriri dicitur.† In superioribus animalibus multiplex est; sed variae formae in duas classes sic distribui possunt. Ad primam classem pertinet omnis absorptio, quae constans, et ad sanguinis formationem, vel ad nutritionem necessaria est. Hîc igitur numerandae sunt.

1*mo*. Absorptio quâ ab cibo et potionibus, quodcunque nutrire potest abstrahitur. Haec in superiore parte tenuis intestini, in homine saltem, observanda est. In aliis quibusdam animalibus, ut e. g. in equo, potiones ex crassiore intestino majori quantitate absorbentur. Non desunt qui cutem etiam hâc facultate non omnino carere existiment. Experimenta vero quae SEGUIN et alii instituerunt, hanc opinionem refutant.

Attamen negari non potest, aquâ ad corporis superficiem admotâ, sitim levari posse, sed hujus effectûs rationem dare non difficile est.

2*do*. Absorptio quâ, inter inspirandum, principium ad vitam necessarium ex aëre hauritur. At licet haec absorptio inter suos fautores BLAINVILLE et ADELON habeat, nondum demonstrata esse videtur.

Experimentis a quibusdam chemicis, et presertim ab optimo viro GULIELMO ALLEN et socio PEPYS institutis, aëre adeo nihil in pulmonibus amittere apparet, ut etiam aliquid

* Ob teneram texturam quorundam inferiorum animalium, et ob defectum coloris in eorum humoribus, vasa in talibus animalibus deesse, non facile demonstrari potest. Non diu abhinc, optimus vir G. CLIFT mihi hujusmodi animalium exemplum ostendit, in quo magnum vas, quod non antea suspicatus est, invenit.

† Nonnulli existimaverunt humores, etiam in vivis corporibus perfectorum animalium, per varias texturas permeare. Ita visum est celeberrimo BOYLE, et ALBINUS, MECKEL, et HALLER, talem opinionem complexi sunt. FORDYCE, CRUICKSHANK et plerique nostris temporibus, istam notionem rejiciunt: PROCHASKA tamen, humores varias structuras permeare contendit, et ad hujus sententiae confirmationem, experimenta a seipso, et à PARROT instituta adducit. Haec autem mihi contradictionibus obnoxia videntur. Quaedam MAGENDIE experimenta, et recentiora Doctoris COINDET, PROCHASKAE sententiae non nihil favent.

accipere videantur. Nitrogenium enim immutatum expellitur, et item oxygenii pars, at pars altera, cum carbonio conjuncta, acidum carbonicum evadit. Videbatur etiam clarissimis DAVY, et GAY LUSSAC majorem acidi quantitatem ex pulmonibus expelli, quam ex amisso oxygenio formari potuerat. Haud tamen reticendum est non nihil nitrogenii in quorundam exploratorum experimentis evanuisse.

Illud vero in pulmonibus fuisse absorptum minus certum est.* Alii volunt cutem respirandi functionis esse participem. Haec sententia quod ad homines, et ad alia superiora animalia spectat, plane rejicienda est, sed, ut testatus est EDWARDS, physiologus inter nostri aevi subtilissimos habendus, in ranis, et in his similibus bestialis, res ita sese habere videtur. Quamvis in ambiguo sit, utrùm respirationis essentia in absorptione consistat, nec ne, luculenter patet ex investigationibus Professoris MEYER, et aliorum, membranam mucosam bronchia munientem humores mirum in modum absorbere posse.† Talis autem absorptio ad hanc divisionem non pertinet, sed potius, vel

* Non omnino reticenda sunt, THOMAE EDWARDS experimenta. Cum supra narratis non concurrunt; illi videbatur oxygenium aestivo tempore, ei nitrogenium hieme, in pulmonibus absorberi. Fatendum est respirandi functionem, nondum satis fuisse exploratam.

† GOODWYN, AUTENREITH, SCHLOEPFER, et discipuli scholae veterinariae Lugduni, jam ostenderant, aquam aliosque humores, in asperam arteriam, illaesâ vitâ, injeci posse, cum MEYER hanc rem accuratiùs investigare suscepit. Hic varias materias multò citiùs ex pulmonibus, quam etiam ex intestinis absorberi invenit. Operae pretium foret hujus absorptionis usus investigare. Fateor me de hâc re nulla experimenta fecisse. Attamen suspicor eam constitutam fuisse, ni mucus unquam in pulmonibus tantâ copiâ accumuleter, ut cellulas impleat, commercium sanguinem inter et aëra impediat, et ita respirationem supprimat. Non desunt experimenta, quae aëra, qui inter inspirandum expellitur, majorem vaporis copiam ex superiore parte asperae arteriae, ex faucibus, et ex ore, quam ex toto pulmone accipere indicant, licet in hoc, multò ampliori quam in illis, humidae superficii exponatur. Hoc etiam ob alias causas, mihi explicatu difficile videtur. Haec autem absorptio in causâ fortasse partim habenda est. Multum inter Physiologos disputatum est, de octavi nervorum paris functionibus, itemque de eorum sectionis effectibus. Mihi sanè verisimillima videtur C. B. BRODIE de his nervis sententia. Eos scilicet, pulmonibus, ut alii nervi aliis partibus, sensum praebere, quo sanguinem nigrum in ipsis inesse percipi possit, quâ perceptione ad cerebrum dılatâ, phrenicos, intercostales aliosque nervos, necessarias musculorum respirationi inservientium contractiones excitare docet. Haec autem Theoria non omnino nova est. Egregius ille Physiologus, ROBERTUS WHYTT, hujusmodi opinionem diu antea protulerat. Ipse quoque, nonnullis meis familiaribus huic similem sententiam prius protuli, quam suam BRODIE vulgaverat. Non tamen hanc solam esse nervi pneumogastrici utilitatem arbitror. In parte sequente hujus dissertationis effectum, quem nervi in absorbendi

inter absorptiones quae afficiunt secretos humores dum in corpore manent, vel inter adventitias quae classem secundam constituunt, recenseri debet.

3*tio*. Ea absorptio quae veteres et jam depravatas particulas, ut novis locum cedant, submovet. Haec functio a variis physiologis, varia nomina accepit, sic a JOANNE HUNTER, *Interstitial Absorption* vocabatur, a BICHAT *Absorption décomposante ou nutritive*, a BUISSON, *Absorption Organique*. De hâc formâ minime dubitatur. Non modo partium formâ, ut supra dictum est, indicatur, sed experimento probatur. Sic testantur DUHAMEL et HUNTER animalium ossa, ex rubeâ tinctorum devoratâ, colorem rubrum accipere, intermisso autem medicamento, coloratum os seriùs ociùs demoveri. Ad hoc accedunt plurimae mutationes quae aetatis processu fiunt in corpore. Glandula thymus, et aliae partes foetui peculiares, in adulto vix ac ne vix quidem reperiri possunt. Maxilla in juvene lata et valida, in sene multùm minuitur. In sene quoque ossa calvariae saepe tenuantur, et, ut alii dicunt, cervix femoris non nihil mutatur. In morbis aliquando inter novarum particularum formationem, et veterum abstractionem consueta ratio manifestè perturbatur, inde macies oritur, vel, si ossa praecipue afficiuntur, et pars terrea insolitâ ratione submovetur, mira sequitur mollities.

4*to*. Ad quartam et ultimam primae classis divisionem referenda est omnis absorptio, quâ iterum in sanguinem resorbetur qualiscunque secretus humor, cui nulla patet via ex corpore exeundi. Cujus modi sunt halitus, qui membranarum serosarum glabras superficies, et cellulosae membranae interstitia assiduè humectant, item humores oculi et auris labyrinthi, synovia in articulis, in bursis mucosis, et in thesis tendinum : et si quid in corpore thyriodio, in capsulsis renalibus, aut glandulis lymphaticis formatur, inter haec recensendum est. Ad hunc locum referre debemus resorptionem in caeteros secretos humores agentem, eam scilicet, quâ fel ex vesiculâ crassius evadit, quâ urina in vesica jam dudum retenta, minùs aquosa est recentiore. Agit ita quoque in ore, in oculo, in mammâ, et in aliis partibus, et, ni fallor, ea causa est cur stercora in crassiore intestino concreta, et interdum arida fiant.

functionem habeant, breviter indicare conatus sum, et hic conjecto octavi nervorum paris sectionem efficere, ut humores in bronchiis secreti, non solitâ ratione absorbeantur, unde spiritus difficilis magnâ ex parte oriatur.

In secundâ classe enumerandae sunt omnes absorptiones quae, cùm nec perpetuae sint, neque ad vitam necessarie, adventitiae nominari possunt. Imo etiam ut perficiantur necesse est, vel, ut quidpiam alienum, idemque haud esculentum extet in corpore, vel, ut aliquid insolitum in eo eveniat.

1*mo*. Quamvis ut supra dictum est, corpus per cutem ali posse incredibile sit, non ideo sequitur, cutem nullo pacto sorbere. Sed contra, acres quaedam materiae, praesertim si eodem tempore perfricatur cutis, hâc viâ, ut omnibus liquet, facillimè in corpus intrant, nec dubitari potest, quin quorundam morborum principia non aliter sese in corpus insinuent.

2*do*. Nusquam sanè, aut frequentiùs, aut manifestiùs quam in membranis mucosis haec functio exercitur, ut quotidianis exemplis, unicuique innotescit.

3*tio*. Denique absorptio ex omni corporis parte fieri potest, sive materies aliena in quâvis texturâ inseritur, sive in ipso corpore aliquid morbo generatur, aut naturalis humor errat, et insolitum locum invadit. Sic in morbo regio, bilis a jecinore per totum corpus dissipata, decedente morbo, rursus ab omni parte absorbetur.

HAEC FUNCTIO VETERIBUS NON INCOGNITA.

Definita jam breviter absorbendi functione, considerabimus paulisper quid eâ de re veteribus notum fuerit.

Quamvis Graeci diligentiùs quam caeterae nationes medicinam cognatasque scientias coluerint, ab illis tamen rudis anatomia scientiarum in numero vix (ac ne vix quidem) inserta est; inde necessario manca, et erroribus implicata Physiologia.

Antiquissimis nihilominus illorum medicis animalium corpora variis modis absorbere non latebat, ut sequentibus excerptis manifestum fiet.

Incipiam ab Hippocrate Coo, primo quidem ex omnibus memoriâ dignis: dicit ille medicinae pater.—" *Σάγκες ὁλκοὶ καὶ ἐκ κοιλίας καὶ ἔξωθεν· δῆλον ἡ αἴσθησις ὡς ἐκπνόον καὶ εἰσπνόον ὅλον τὸ σῶμα,*" et in alio loco, " *Ἕλκει μὲν γὰρ τὸ σῶμα ἀπὸ τῶν βρωμάτων καὶ ποτῶν ἐς τὴν κοιλίην—ἡ ὁμοίη ἰκμὰς τὴν ὁμοίην διὰ τῶν φλεβῶν.*"

Hîc, et in sequentibus excerptis non tantum actionem, sed instrumenta quoque indicat. "Καὶ γὰρ αἱ φλέβες, αἱ ἐκ τῆς νηδύος, καὶ τῶν ἐντέρων, ἐις ἃ ξυλλέγεται τὰ σιτία, καὶ τὰ ποτὰ, ἐπειδὰν θερμανθῇ ταῦτα, ἕλκουσι τὸ λεπτότατον, καὶ τὸ ὑγρότατον, τὸ δὲ παχύτατον αὐτέου καταλείπεται, καὶ γίνεται κόπρος, ἐν τοῖσιν ἐντέροισι τοῖσι κάτω." Item "Εἰσὶ δὲ καὶ ἀπὸ τῆς κοίλης, φλέβες, ἀνὰ τὸ σῶμα, πάμπολλαι τε καὶ παντοῖαι, δι' ὧν ἡ τροφὴ ἐν τῷ σώματι ἔρχεται." Dubitare non possumus quin ERASISTRATUS vasa chylifera vidisset quum dixerit—"Ἐν γὰρ τῷ διαιρεῖσθαι τὸ ἐπιγάστριον, ἅμα τῷ περιτοναίῳ, κατὰ τὸ μεσεντέριον ἀρτηρίας ἰδεῖν ἐστι σαφῶς, ἐπὶ μὲν τῶν νεωθήλων ἐρίφων, γάλακτος πλήρεις."

Nec magis haec vasa HEROPHILUM, quanquam illorum finem non intellexit latuisse videntur, ut sequentibus GALENI verbis apparet. "Πρῶτον μὲν γὰρ, παντὶ τῷ μεσεντερίῳ φλέβας ἐποίησεν ἰδίας ἀνακειμένας αὐτῷ, τῇ θρέψει τῶν ἐντέρων, μὴ περαιουμένας εἰς τὸ ἧπαρ· ὡς γὰρ καὶ Ηρόφιλος ἔλεγεν, εἰς ἀδενωδῆ τινα σώματα τελευτῶσιν αὐτὰς αἱ φλέβες, τῶν ἄλλων ἁπασῶν ἐπὶ τὰς πύλας ἀναφερομένων.—GALENUS quoque corpus absorbere docuit, et absorbendi functionem in quâdam attractione consistere existimavit. Cuti etiam hanc facultatem tribuit, dicit enim—"Ὥσπερ, διὰ τῶν εἰς τὸ δέρμα περαινομένων στομάτων, εκκρίνωσι μὲν ἔξω πᾶν ὅσον ἀτμῶδες καὶ καπνῶδες περίττωμα, μεταλαμβάνουσι δὲ εἰς ἑαυτὰς, ἐκ τοῦ πριέχοντος ἡμᾶς ἀερος, οὐκ ὀλίγην μοῖραν· καὶ τοῦτ' ἔστι τὸ πρὸς Ἱπποκράτους λεγόμενον ὡς ἐκπνοῦν καὶ εἰσπνοῦν ἐστιν ὅλον τὸ σῶμα."—Arterias quidem illam aliquantò habere suspicavit "Ἀτμὸν μὲν οὖν ἔχουσαι, καὶ πνεῦμα καὶ λεπτὸν αἷμα, κατὰ ταῖς διαστάσεις ἕλκειν αἱ ἀρτηρίαι, τὸν κατὰ τὴν κοιλίαν καὶ τὰ ἔντερα περιεχόμενον χυμὸν, ἢ οὐδὲ ὅλως ἢ παντάπασι συνεπισπῶνται βραχύ.*"

In saeculis ignorantiae quae postea secuta sunt medicina et physiologia cum aliis scientiis communis ruinae participes fuerunt. Harum reliquiae ab Arabibus conservatae, potius quam excultae fuisse videntur. Solebant medici Arabes ad cutem admotis medicamentis uti, cum vel urinam, aut vomitum excitare, vel ventrem movere, vel sudores elicere vellent. Nec ita fecissent nisi cutem sorbere existimassent. Illos tamen ad hanc medendi rationem

* In nostris diebus, opinio huic simillima, a CL. PROCHASKA esse renovata videtur.—Vid. cap. viii.

iatralepticorum exemplo adductos fuisse verisimillimum reor.

Reviviscentibus tandem scientiis, anatomia cultoribus non caruit.

Anno MDLXIII EUSTACHIUS ductum thoracicum primus invenit, sed nescius illius naturae, venam albam thoracis nominavit. Anno MDCXXII ASSELLIUS, Italus, vasa chylifera diu ante ab HEROPHILO et ERASISTRATO visa, sed parum cognita, animadvertit. Hic vero vivis animalibus dissectis, ista vasa acerrime scrutatus est, et eorum functionem, chyli scilicet absorptionem, sagacissimè assecutus. Ea tamen in homine oculis cernere, ut cupiebat, nunquam ipsi datum est. Hoc autem contigit feliciori VESLINGIO, qui etiam in ductum thoracicum vasa chylifera secutus est primus. Post chylifera vasa ab ASSELLIO deprehensa viginti octo circiter annis, lymphifera, OLAUS RUDBECK Suecus invenit. Cum illo tamen de hoc honore certant et JOLIVIUS Anglus, anatomicus peritissimus, Danusque BARTHOLINUS.

Post hos alii anatomici haec vasa scrutati sunt, et eorum actionem conjectârunt. At tandem GULIELMUS HUNTER, lymphifera et chylifera vasa non nisi unum esse et integrum vasorum ordinem, per universum corpus extensum, et ubique absorbendi functione praeditum comperiit. Hujus egregii physiologi discipulis HEUSON et CRUICKSHANK, horum vasorum in corpore humano dispositionis scientiam praecipue debemus. HEUSON quoque et J. HUNTER haec vasa in avibus, in animalibus amphibiis, et in piscibus primi invenerunt, et exploraverunt, nisi forsitan BARTHOLINUS in pisce Diodonte, ex familiâ Gymnodontium, CUVIERI, eorum vestigia prius aspexerit. Nec tacendum est ALEXANDRUM MONRO secundum, qui tunc temporis in hâc urbe florebat, et anatomiam comparativam magno fructu excolebat GULIELMI HUNTER et discipulorum observationes, non multo post, iterasse. Quinetiam cum illis de primae inventionis laude certavit, quam tamen hâc contentione, non adeptus fuisse videtur.

Dum haec de vasis chyliferis et lymphiferis agerentur, insignes physiologi HARVEY, H. et K. BOERHAAVE, SWAMMERDAM, HALLER et alii veterum opinionem de absorbendi facultate in venis insitâ, et ratione, et experimentis probare susceperunt. Proinde orta est veterum fautores inter et sectatores G. HUNTER notabilis controversia, de quâ etiamnum sub judice lis est.

Priusquàm hanc controversiam ineamus, non omninò erit inutile, organa, functioni a nobis investigandae subservientia paulisper contemplari. Minimè tamen meo proposito convenit; nec res ipsa postulat, ut longam et completam descriptionem anatomicam hîc proferam.

Primò igitur de venis loquar. Haec vasa non alitèr ac arteriae, quas et numero, et capacitate longe superant, ex duabus membranis constant, atque etiam ut aliis vasis, sic et illis circumdatur involucrum ex membranâ cellulosâ. Tunicae venosae, arteriosis sunt longè tenuiores, et distentu faciliores, sed et eaedem validiores, ut testatus est WINTRINGHAM. Membrana interna tenuis, laevis, et glabra omni circuitûs sanguinis apparatui, ut alii dicunt communis est, at alii venis dextroque cordi esse peculiarem volunt. Externa, sive propria, ex fibris non circularibus, ut in arteriis, sed potius in longum dispositis constare reputatur, ita sàltem visum est celeberrimo BICHAT; declarat tamen MAGENDIE se nunquam hanc dispositionem reperire potuisse.

Non desunt qui et arteriis, et venis, tres membranas, et vim muscularem tribuunt, at licet HUNTER, BLUMENBACH, RICHERAND, MONRO, alter, et tertius, et multi alii ita docuerint, horum vasorum functio et compositio prohibent, ne eam opinionem amplectar.

Veruntamen nec irritabilitate, nec sese contrahendi facultate, venae penitus carere videntur. BICHAT enim, qui venis istas facultates vix concedit, et miro quodam errore resiliendi vim illis omnino denegat, venarum contractiones se bis terve animadvertisse confitetur. CHAUSSIER, MAGENDIE, et ADELON, has facultates in venis inesse negant. BECLARD autem, rèctius, ut opinor, vim vitalem sese contrahendi, neque vero magnam, venis concedit, non tamen reticendum est venas cavas, ubi dextrae auriculae committuntur, et ex eâ non nullas musculosas fibras accipere videntur, ibidem irritabilitate manifestâ non carere.

Imo etiam decedente vitâ, in hâc parte quae revera ultimum moriens vocari potest, irritabilitas sese postremum ostendit, et mirabiles contractiones perficiuntur, ut amicus meus vir acutissimus R. KNOX M.D. praecisis squalorum thoracibus, saepius et iterum conspexit.

Haec vasa, venâ portae et quibusdam aliis exceptis, valvulis ex membranâ interiore constantibus muniuntur. Ubi minores venae in majores sese effundunt, valvulae plerumque reperiri possunt, at in cursu vasorum nullâ certâ ratione ponuntur. Hae valvulae interdum arctiores sunt quam ut officio fungantur, id quod BICHAT, venae distentioni solummodo attribuit: longe autem verisimilius est, in talibus exemplis conformationem esse peculiarem. Atque haec vetus sententia à MAGENDIE et aliis hodie promulgatur. Extremae venae eâ sunt tenuitate, ut vix (ac ne vix quidem) illas persequi possimus. Illarum tamen investigatio ad nostram rem maximè pertinet.

Docent, SOEMMERING et PROCHASKA extremas arterias in quâque corporis texturâ peculiari ratione distribui, sed quonam modo sanguis ex illis in venas transeat, videre non datur.

BICHAT hîc posuit peculiarem vasorum ordinem, quam *système capillaire* nominavit. At, me judice, ita innovando adeo non bonam distinctionem fecit, ut potius duos, ad minimum, vasorum ordines confudisse videatur.

Antiquiores Anatomici structuram specialem, arteriis et venis intermediam imaginati sunt. Verùm ex quo tempore MALPIGHI, LEEUWENHOEK, COWPER, et SPALLANZANI venas arteriis continuatas ostenderunt, atque quotidianis fere experimentis venas ex arteriis injectas materias accipere demonstratum est; haec opinio suos fautores amisit. Ita sàne quarundam venarum initia ostenduntur, at nequaquam exinde sequitur, alias venas non aliter originem ducere. Recentiùs autem PROCHASKA, structuram vasis carentem existere strenùe contendit, illius tamen sententia non veterum opinionem omnino renovat, ab illâ enim non nihil discrepat. Quinetiam CHAUSSIER et ADELON, structuram extremis vasis interpositam non rejiciunt, neque tamen jam confirmatam habent. Venae quoque a variis internis superficiebus, et etiam a solidis corporis texturis, ut e. g. a musculis, apertis osculis incipere, a nonnullis reputantur, sic testatus est KAAW BOERHAAVE aquam, vel ceram per haemorrhoidales venas injectam, in intestinorum cava exire. Narrat etiam MECKEL se venas in pelvi sitas, ceram aut aërem in vesiculas seminales, vel in vesicam injiciendo, sine partium laesione implevisse. Et sic quoque HALLER se gluten piscarium, caeruleo colore tinctum, in pericardium et in cerebri ventriculos non semel impulisse testatur. LEIBERKUHN materiam in venas injectam, ex intestinorum villis defluere animadvertit. Vidit etiam aëra per venas immissum, membranam cellulosam pervadere.

MAGENDIE quoque haec confirmat, quibusdam experimentis ad venas cordis spectantibus. Talibus autem in experimentis CRUICKSHANK partium structuram laesam fuisse existimavit. Item FORDYCE, haec venarum ostiola in vivo corpore existere posse non credidit. Quanquam vero haec experimenta mihi non sic rejicienda videantur, nostrâ tamen refert recordari illius τῶν Ἐμπειρικῶν sententiae, nobis a CELSO traditae, scilicet "non quicquam esse stultius, quam quale quidque vivo homine est, tale existimare esse moriente, imo jam mortuo." Multa enim longè aliter in vivâ ac in mortuâ structurâ se habent. Si haec sententia confirmatione egeret, experimenta ab ALEXANDRO HUMBOLDT viro omnigenâ scientiâ ornato, et a clarissimo Professore BECLARD in cutem facta memorare possem.

Ille, microscopio tricenties et duodecies millies et quadringenties formam augente, cutem externam exploravit, sed nequaquam potuit poros detegere; hic eandem cutem altae hydragyri columnae subjecit, metallum verò nullo pacto exudavit. Ast in vivo homine, ut omnibus notum est, sudor facilè, et interdum magnâ copiâ cutem permeat. Solus igitur vitae

———— Calor ille vias, et caeca relaxat
Spiramenta.

Sunt quoque et alia experimenta ad nostram rem quam maximè spectantia, atque hanc sententiam abundè confirmantia. JOANNES HUNTER in pluribus tentaminibus materias per venas in intestina impellere, et vice versa, venas ab intestinis implere frustra conatus est. Mortuo tamen animali, per venas meseraicas intestinum inflavit.

Aliae venarum origines ab Anatomicis enumerantur, ut ex folliculis, ex glandulis, et ex structurâ quae *erectile tissue* vocatur. Verum illas specialitèr tractare minime hîc opus est. Venis igitur relictis, jam ad alterum vasorum ordinem, vasorum scilicet absorbentium vulgò dictorum, pergamus.

Cum haec vasa contemplamur, quaedam illis cum venis communia, quaedam autem propria videmus. Vasorum absorbentium pellucidae tunicae, venosis tunicis tenuitate praestant, quibus tamen firmitate non cedunt. Sicut venae, ex duabus membranis constant, ut NUCK primus demonstravit. Interior membrana venarum interiori continua, huic non dissimilis videtur, ac pariter valvulis ejusdem generis, sed multò frequentioribus, munitur, exterior, secundum CRUICKSHANK, MAGENDIE, SHELDON, GOODLAD, et alios fibrosa est. Hanc autem structuram BICHAT detegere non potuit. HALLER, CRUICKSHANK, MONRO, GOODLAD, multique alii vim muscularem vasis absorbendibus attribuunt.

Alii autem istam facultatem in illis existere omnino negant. BICHAT nullas oculis percipiendas contractiones ab illis effici credidit. Non tamen omnino negavit illas, darti in more, posse sese contrahere. Permulti, quos inter numerandus est, Cel. BLUMENBACH, licet vim muscularem rejiciant, sese contrahendi facultatem in his vasis insitam admittunt. Professores TIEDMANN et GMELIN ductum thoracicum ex aëris contactu, vel ex affixo ligamento se contrahere saepè conspexerunt. Non dubito quin hae contractiones sint ejusdem generis ac contractiones a Doctore PARRY in detectis arteriis observatae, quasque *tonicitati* (parce verbo) adscripsit.

Nihil, magis quam glandulae, per quas in quibusdam corporis partibus transeunt, haec vasa ab omnibus aliis distinguit.

Rariùs, sed aliquando tamen, vasculum hujus ordinis per totum suum cursum cum nullâ glandulâ committitur, id quod HEWSON in lymphifero vase ex pollice pedis proveniente, CRUICKSHANK in lumbis, et MAGENDIE in equis observavit. Usque adhuc, harum glandularum structura, et usus non satis comperta sunt.

Notatu quoque dignum est, vasa absorbentia, etsi magno in numero concurrant, nusquam ampla fieri. Ipse etiam ductus thoracicus, inferiores ramulos capacitate saepè non multò superat.

Pariter atque ostiola, quibus minores venae in majores sese effundunt, valvulis muniuntur, sic etiam ubi haec vasa cum venis se committunt, valvulae inveniuntur.

Dubitari non potest, quin in plerisque exemplis pars maxima chyli et lymphae, per ductus thoracicos dextrum et sinistrum, in venas subclavianas sese effundat. In multis de anatomiâ libris hae solae terminationes describuntur. Quinetiam HALLER et CRUICKSHANK alias non existere contenderunt, et hanc sententiam participant LIEUTAUD, HEWSON, PORTAL et SOEMMERING. Haec autem vasa prae aliis, quod ad distributionem attinet variant, et multa ab aliis anatomicis exempla prolata sunt, quae satis superque demonstrant vasa, quae absorbentia dicuntur multimodis cum venis rubris conjungi.

MASCAGNI quoque, qui tales communicationes impugnat, eas tamen in mesenterio, etsi rarò, invenit. MECKEL saepè, in venas hydrargyrum injecit per vasa absorbentia.

ASTLEY P. COOPER, Baronettus, eodem modo hydrargyrum in venam portae impulit: idem quoque fecerunt ROSEN, WALERICUS, LOBSTEIN, LINDNER, et TIEDMANN, et GMELIN. Docet MAGENDIE lympham ex sinistro capitis latere provenientem, non raro per ductum proprium, in venam subclavianam transmitti.

ABERNETHY vasa lymphifera efferentia a glandulis in venam prosecutus est. Vidi et ipse vas lymphiferum ex pulmone ortum, cum venâ sine pari committi.

In equo meus amicus B. CLARK receptaculum chyli sese in venam quandam lumborum effundentem invenit.

Denique ductus thoracicus a DUVERNEY, ASTLEY COOPER, DUPUYTREN, et FLANDRIN in bestiis saepissimè ligatus est. Hoc experimento, plures necatae sunt, aliae autem vitam conservaverunt, at in his, ut DUPUYTREN demonstravit, aliae viae patebant, quibus lympha ad cor usque pervenire posset.

Quod ad horum vasorum radices extremas attinet, nullo pacto faciliores assecutu sunt, quam venarum origines.

Chylifera vasa, etiam ab ASSELLIO, ex internâ superficie intestinorum oriri existimata sunt. BARTHOLINUS, NUCK, COWPER, SENAC, BERGERUS et FERRIER, lymphifera arteriis esse continuata rati sunt. MALPIGHI vero haec a folliculis tantummodo oriri voluit. HALLER, G. et J. HUNTER, CRUICKSHANK, MECKEL, et plerique alii physiologi vasa lymphifera ab omnibus internis superficiebus oriri docent. Professori autem MECKEL cum his superficiebus non tam facile, quam venae, communicare videbantur, et experimenta, quae nuperiùs instituit RIBES, ad eandem opinionem ducunt. Incipiunt quoque ex glandularum ductibus, ut e. g. ex ductibus fellis, ex tubulis lactiferis, nec non viis urinariis. Ad hanc sententiam accedunt, HAMBERGERUS, FERRIER, HALLER, CRUICKSHANK, DESGENETTES, SOEMMERING, BECLARD et alii. Postremo ab omni corporis parte nascuntur, et BARTHOLINI sententia de communicatione arterias inter et absorbentia vasa, nonnullis experimentis confirmari videtur.

Affirmant enim HALLER et CRUICKSHANK materies in arterias injectas, in lymphifera penetrare, et cum illis consentit MAGENDIE.

At contra hujusmodi origines strenùe contendit MONRO secundus. Beclard etiam se de his communicationibus etiamnum dubitare confessus est. Cum venis quoque tales communicationes, a Vieussenio olim creditas, Meckel, et Ribes existere demonstrârunt.

In foetu, Cruickshank vasa lymphifera per venam umbilicalem implevit.

Hae omnes origines ab effectu monstratae esse videntur, at ipsa oscula eâ sunt tenuitate, ut cerni non possint, nisi forsan in felicioribus quibusdam exemplis chyliferorum vasorum initia, in villis intestinorum microscopio percipi possint, et hoc modo se ea vidisse affirmat Cruickshank. Singuliis villis duodecim circiter horum vasorum radiculas, patula ostia habentes et radiorum more in unum vas convergentes adscribit. Bohlius quoque haec ostia vidisse fertur. Quae scripsit Leiberkuhn de ampullis, ex quibus chylifera vasa incipere credebat, ab omnibus rejiciuntur.

De lymphiferorum ostiolis plures conjecturae prolatae sunt. Hunter ea erucae os, Haller autem et Richard puncta lacrymalia referre imaginantur. Bichat existimabat ea in variis structuris, modis peculiaribus incipere, et nonnunquam a capillaribus vasis oriri. Docet Blainville, egregius professor, haec vasa definitis ostiis carere, et potius gradatim, et inexplicabili transitione, ex membranâ cellulosâ origines habere.

Chaussier et Adelon, ut supra notavimus, structuram extremis arteriis, venis, et lymphiferis vasis interpositam non rejiciunt.

Morbi quibus vasa absorbentia (dicta,) interdum laborant, venarum affectibus non sunt dissimiles. Utraque vasa ad inflammationem magis quam arteriae, ad conversionem in ossìam materiam minùs sunt proclivia. Utraque etiam sponte aliquando clauduntur. Ad haec, lymphifera vasa non nunquam mirum in modum dilatata reperiuntur, quod vitium profectò, cum varicibus aliquatenùs comparari potest. Non igitur veteres omnino ineptè, haec vasa tanquam venarum genus habuisse opinor.

Quoniam omnis absorptio, omnium pene consensu aut uni, aut alteri horum vasorum ordini, aut ambobus conjunctis ordinibus tribuitur, absorbendi functionem meliùs tractare non possum, quam utriusque ordinis officia propria considerando. Nunc itaque ad antea dictam controversiam revertamur.

Argumenta et Auctoritates, quae suadent absorptionem fieri per vasa chylifera at lymphifera.

Assellius horum vasorum illustris inventor, chylum per illa absorberi demonstravit, longâ serie argumentorum et experimentorum, quae in canibus, in felibus, in porcis nec non in vaccis exenteratis vivis, et etiam in equo ita inciso instituit.

Bartholinus venas chylum absorbere omninò negavit. Nunquam sanguinem cum chylo commistum invenit, nullum aditum admisit, quo ex intestinis in venas penetrare posset. Narrat praetereà probatum fuisse, ligatis venis chylum non ideò minùs absorberi, at ligatis Asselli i vasis restitare chylum, nec ex ventriculo, neque ex intestinis ulteriùs progredi.

Ruysch his vasis absorbendi facultatem tribuit; sed venas illam participare credidit, imo etiam in animalibus aetate provectis, hanc functionem a venis solummodo perfici docuit.

Ingeniosus Morgan, cujus opera mechanicas theorias nimis redolent, lymphifera vasa serum ex sanguine, ut in corpore refrigeretur accipere, et postea iterum in venas effundere putavit. Ea tamen, absorbendi quoque facultatem habere docuit, ut ex primâ propositione libri ejus de re medicâ mechanicâ constat. "No sort of substances can pass the lacteals, recipient lymphatics, or concoctive strainers, but in the form of a fluid previously reduced to an exceeding fine and imperceptible vapour," et iterum; "and it cannot be doubted but the absorbent vessels, or recipient lymphatics, which are spread over all the surface of the skin, are as fine, or rather much finer, than the other (the cutaneous emunctories); since otherwise the same unavoidable mischief must necessarily follow, and certain parts or portions of matter would be received into the animal fluids, that could never afterwards be evacuated, and which must therefore occasion the most desperate and mortal obstructions.*

Gulielmus Hunter, ut supra dictum est, chylifera et lymphifera vasa propria absorptionis organa esse, et non

* Vid. *Philosophical Principles of Medicine*, and *The Mechauieal Practice of Physic*, by T. Morgan, M.D.

nisi unum et eundem vasorum ordinem constituere, primus docuisse habetur, nisi forsan THOMAS MORGAN cum illo de hâc laude disputare possit.

Lymphiferorum vasorum actionem conjectavit. 1*mo,* Ob similitudinem inter lymphifera vasa et chylifera, quae tunc temporis chylum absorbere omnium pene consensu credebantur. 2*do,* Ob symptomata, quae morbiferum virus in corpus intrans plerumpue comitantur. 3*tio,* Theoriam suam praecique confirmatam habuit experimentis a fratre suo institutis.

JOANNES HUNTER procul dubio inter strenuissimos hujus absorptionis fautores meritò habendus est.

Praeciso canis ventre, duas portiones intestini, unam in superiore parte, alteram in inferiore, postquam iis contentas materias submoverat, inter affixa ligamenta inclusit, implevitque lacte, quod brevi per pellucida vasa permeare conspexit.

Venae autem nec candidum humorem accipere, nec turgescere videbantur. In superiorem partem ovis intestini, tenue decoctum amyli, caeruleo colore tinctum injecit, et chylifera vasa, quae antea, ob protractum jejunium lympham tantum colore carentem continuerant, caeruleo humore conspicua visa sunt.

Indicum autem in venarum sanguine detegere nequivit. Dein, ut indigo in venis siqua pars ab illis absorberetur faciliùs detegere posset, lac per arterias miseraicas adegit, donec purum per venas rediit, minimè tamen caeruleum colorem accepit.

In aliam partem ejusdem ovis intestini lac injecit, et vasa absorbentia tumuerunt, licet album colorem non acceperint.

Aliam partem purâ aquâ tepidâ implevit, sed nullo modo effectum a KAAW BOERHAAVE productum obtinere potuit. Injecit etiam in asini intestinum, moschum in aquâ solutum, et illius odorem in lymphâ agnovit. Sed cùm medicamenti portio in peritoneum effusa fuisset, hoc experimentum in dubium revocare licebit. Maximâ curâ sanguinem a venâ non contaminatum collegit: odore omnino carebat. Alio tempore in pleuram, et in peritoneum injecit coloratam aquam, quam brevi tempore in lymphiferis vasis invenit.

ALEXANDER MONRO secundus postquam his vasis diù operam dederat, sic suam sententiam expressit. "Tandem mihi persuadebam vasa lymphatica valvulosa, per totum corpus venarum absorbentium systema efficere, neque ab arteriis, ut vulgo receptum est, emanare."

In hujus sententiae confirmationem citat. 1*mo*, Phaenomena, quae sese praebent in quibusdam morbis, qui postquam partem aliquam affecêre vasa absorbentia, et glandulas conglobatas ei continuatas invadunt. 2*do*, Experimenta in cadaveribus injectione facta. 3*tio*, Facta et opiniones ex pluribus auctoribus excerpta. E quibus, sequentia hîc proferre, mihi liceat. " PEYERUS vir fide omninò dignus lympham non nullis experimentis circa hepar flavescere vidit, cui FALLOPIUS et KIRKRINGIUS addunt, lympham non tantùm subflavam, sed etiam amaricantem in vase lymphatico, per summam fellis cystidem reptante, se reperisse.

" Quae itidem testantur SYLVIUS et RIVERHORSTIUS, claro omnino documento per vasa lymphatica, bilem resorberi."

Ille etiam clarissimus HELVETIORUM physiologus indefatigabilis HALLER absorbendi functionem in his vasis insitam admisit, et nobis sequens experimentum tradidit. " In vivo animali, aut nuper mortuo, non solus ductus thoracicus, qui verè de genere vasorum lymphaticorum est, et perinde vasa lymphatica hepatis, ad oleum vitrioli tactum contrahuntur, sed imprimìs in animali, cui plena fuerant aut chylo, aut lymphâ, aut ceruleo liquore, quem animalia absorbere coegi, sub ipsis intentis meis oculis toties vidi haec, sive lymphatica vascula, sive lactea evanescere."

Docent quoque MASCAGNI, LISTER, BLUMENBACH, et RICHERAND, absorptionem his vasis fieri. In hujus opinionis confirmationem, exemplum citari potest, quod nobis memorat CRUVEILHIER qui ipse aderat, cùm DUPUYTREN explorationem fecit; hic chirurgus celeberrimus corpus examinabat faeminae, quae mortua erat tumore in superiore et internâ parte femoris: pus ibi coactum est, et membrana cellularis eo loco inflammatione fuerat correpta.

Professor cutem summâ curâ dissecans membranam cellulosam albidis lineis distinctam animadvertit; hae lineae pendebant ex vasis lymphiferis ex corrupto loco provenientibus, et pure repletis; glandulae inguinales et lymphifera vasa usque ad glandulos lumborum, eodem humore distendebantur.

Dùm praelectiones ab ASTLEY P. COOPER pronunciatas discipulus frequentarem, inter plurima pretiosa morborum exempla, ab optimo preceptore conservata, specimen et ipse vidi, in quo lymphifera vasa a teste Fungo Haematodi correpto progredientia, et etiam ductus thoracicus albidâ, et cerebrum referente materiâ, huic morbo peculiari impleta sunt. ALLARD in opere de sede et naturâ morborum venas absorbere negat, sed multum tribuit vasculis lymphiferis in venas se effundentibus: et MASON GOOD qui in Physiologico proemate ad *Eccriticos* morbos spectante contra absorptionem venarum contendit, hujusmodi usus est argumento, quod praecipue nititur experimentis Professoris MECKEL (vid. p. 30 et 32). GOOD praetereà in auxilium vocat contra MAGENDII experimenta, lymphifera vasa ad ipsas arterias et venas pertinentia.

Argumenta et Auctoritates, quae suadent absorptionem fieri per Venas.

Nequaquam necesse est hoc in loco ad veterum opinionem de venarum absorbendi functione reverti. Jam satis multa de hâc re in superiore parte memoravimus. Incipiam igitur ab eo tempore, quo ASSELLIUS, novis vasis intentis, in duas partes scidit contrarios Physiologos.

Celeberrimus HARVEIUS venas absorbere strenuè contendit; per alia vasa id fieri negavit.

1*mo*, Propterea quòd chylo duobus itineribus nequaquam opus esset. 2*do*, Quod ASSELLII vasa in pluribus animalibus (ut ait) non existebant, eo enim tempore in multis non fuerant inventa.

SWAMMERDAM quanquam humorem album in vasis chyliferis vidisset, humorem istum chylum fuisse negavit, sed potius quendam succum peculiarem esse, ex glandulis mesentericis provenientem, existimavit. Chylum vero venis mesentericis absorberi credidit; vidit enim sanguinem in his aliquando striatum, et albis lineis permistum.

HERMAN BOERHAAVE venas absorbere contendit, 1*mo*, quòd sanguis in venis mesentericis sanguini in aliis venis dissimilis esset, ita enim ille credebat. 2*do*, Quod venae arteriis capacitate praestant.

Narrat Kaaw Boerhaave se vidisse puram aquam tepidam in ventriculum immissam, a venis bibulis absorberi,—per venas gastricas majores, et venam portae defluere, et tandem per jecur in venam cavam pervenire.

Quùm tamen chylum, vasis quae lactea dicuntur, absorberi ferè omnes confiterentur, antiquae opinionis fautores, venas tanquem absorbendi functionis tantummodo participes, vindicare contenti sunt.

Sic celeberrimus Haller, qui chylifera vasa, et humorem per ea progredientem accuratè descripsit, de Joannis Hunter experimentis scribens, sic suam sententiam expressit. "Multum tribuo Cl. viri experimentis, in quibus candor cum industriâ conjungitur, sed contrarii alia numerosa argumenta habemus; ut non possim a praeceptoris (Boerhaavii) sententiâ recedere."

Docuit quoque Meckel materias venis nonnunquam absorberi; atque etiam Bichat, postquam varia argumenta perpenderat, venas ad absorptionem conferre valde suspicatus est, quanquam tamen pro certo non habuit.

Recentiori tempore experimenta hanc opinionem suadentia à multis instituta sunt Physiologis, quorum inter primos notandi sunt Emmert et Flandrin. Investigationes porrò Professoris Halle, experimentis Joannis Hunter, Halleri, et Doctoris Musgrave, quod ad absorptas materias coloratas saltem attinet, adversantur. Chylum enim arguunt rerum devoratarum colore non affici.

At profectò Magendius, naturae scrutator acerrimus, tum venarum absorptionis defensor, tum Hunterorum oppugnator strenuissimus habendus est. Iteravit enim clarissimi viri experimenta, eventa tamen omnino diversa obtinuit. Praetereà bestias infusum Rhei, Prussiatem Potassae solutum, et alkohol sorbere coegit; has autem materias in ductu thoracico detegere non potuit, quanquam in sanguine, aut in urinâ manifestè adessent. Quinetiam decoctum nucis vomicae in ventriculum aut in rectum canis, post ductum thoracicum ligatum, immissum, nequaquam ideo tardiùs veneficos effectus edere, monstravit. In alio tentamine intestini portionem inter ligamenta inclusit, omnemque connexionem cum reliquo corpore, nisi per unam arteriam, unamque venam, quibus etiam membrana cellulosa maximâ curâ detracta erat, submovit. Dein hanc intestini portionem nucis vomicae decocto implevit. At neque in hoc experimento veneni effectus tardati sunt: Experimentum huic mutatis vicibus respondens, anno praeterito, in Academiâ Regiâ Gallicâ narratum audivi. Physiologus, cujus nomen me fugit, intestini portionem supra dicto modo praeparavit, et connexionem cum reliquo corpore per vasculum chyliferum tantum reliquit: venenum deinde injectum, effectus peculiares non edidit.

Denique MAGENDIUS, ne quis ad vasa absorbentia in ipsis vasorum tunicis insita confugeret, crus cani praecidit, ita ut membrum non nisi per arteriam, et venam cruralem cum corpore connecteretur; et haec etiam vasa divisit, postquàm pennarum fistulas ad conservandam connexionem interposuerat, deinde upas, lethale venenum, subter pedis cutem infixit, et bestia protinùs emortua est. Ad haec, cum celeberrimo chirurgo DUPUYTREN centum et quinquaginta experimenta instituit, in quibus varios humores membranis serosis subjecit, nunquam tamen hos humores lymphifera vasa ingredi conspexit. Ex his et aliis hujusmodi experimentis concludit, 1*mo*, Quanquam certum sit, chylum vasis chyliferis absorberi, dubium tamen esse, num quid praetereà ab illis absorbeatur; 2*do*, Lymphifera vasa absorbere posse, non esse demonstratum; venas autem hâc facultate manifeste pollere. In alio loco veterem opinionem renovat, lympham scilicet ex arteriis in vasa lymphifera effundi. Praetereà contra absorptionem per vasa lymphifera hôc argumento usus est nempe haec vasa reperiri non posse, in oculo, in aure interno, aut in cerebro, in quibus absorptio nihilominùs, aut manifestè exercetur, aut optimo jure conjicitur. Experimenta JOANNIS HUNTER tanquam imperfecta et male instituta rejicit. Praetereà quod HUNTER in chyliferis vasis invenit, et lac esse existimavit, MAGENDIE potius chylum fuisse suspicatur. MEYER quoque, olim Bernae Anatomiae Professor, multa experimenta ad absorbendi functionem spectantia instituit. In his investigationibus varias experiendas materias, per asperam arteriam in pulmones plerumque injecit, quippe, ut supra diximus, ex bronchiis, et pulmonum cellulis, citiùs et copiosiùs, quam ex ullâ corporis parte absorptio perficitur. Hanc absorptionem per venas pulmonales fieri docet. Materias enim injectas in sinistro corde primum invenit, et ligamentum ductui thoracico affixum nequaquam absorptionem impedivit. Materias hoc modo injectas in vasis lymphiferis seriùs invenit. In his experimentis prussiate et muriate potassae usus est, quae non tantùm in sanguine, in urinâ, et in aliis secretis humoribus, sed etiam in quibusdam concretis corporis structuris detegere potuit. Experimenta quae instituit Ev. Home, dum vias, quibus potiones ex ventriculo et intestinis evadunt, et lienis functiones exploraret, absorbendi facultatem in venis insitam demonstrare videntur.

Hanc partem priùs finire non possum, quam recentiores labores Professorum Tiedmann et Gmelim breviter memoraverim. Quod si illorum experimenta enumerare susciperem, hoc opus certe nimis fieret prolixum. Sufficiet igitur consequentias, quas ex illis inferunt enunciare. 1*mo*, Materiae colorantes veluti rheum, indicum, rubia tinctorum, coccinella, cambogia et similia, in chyliferis vasis non reperienda sunt. 2*do*, Materiae odorantes tanquam camphora, moschus, alkohol, oleum essentiale terebinthinae, oleum empyreumaticum animale, gummi assafoetida, et allii sativi radix aeque ac res colorantes chylum non afficiunt. 3*tio*, Plerique sales non minus vias chyliferas effugiunt. Hujusmodi sunt acetas plumbi, hydrargyri acetas et prussias, ferri murias, barytae murias, potassae prussias, &c. Sulphas autem potassae in chylo inventum est. Et devorato sulphate ferri, ferrum in chylo non omnino deesse videbatur. Neque reticendum est plumbum et prussiatem potassae, utrumque semel in chylo fuisse repertum. Haec omnia, vel pleraque saltem, in venarum sanguine, in urinâ etiam, et in quibusdam aliis excretis homoribus reperiri possunt. Interdum quidem has materias in urinâ repererunt cùm eas in sanguine detegere non potuerunt. Tale autem phaenomenon diu antea a Doctoribus Wollaston et Marcet observatum est.

Horum experimentorum, quae tantâ perspicuitate venas absorbendi facultate pollere ostendunt, ea explicatio quam Mason Good et alii proposuerunt, venas scilicet absorptas materias ex nonnullis curtis lymphiferis vasis ad ductum thoracicum non tendentibus accipere, mihi prorsus nugatoria videtur; nam licet pars chyli talibus vasis in venas manifestè effundatur, si venosum sanguinem supradictis proprietatibus donare quiret, necesse esset ut istae proprietates in chylo multo magis conspicuae forent, secundum illud scholarum axioma, "Propter quod unumquodque est tale, id ipsum est magis tale." At in chylo, raro aut nunquam percipi possunt. Quòd si responderetur, ea chylifera peculiaria esse, ad res quasdam absorbendas constituta, et nunquam non in venas sese effunderc, nihil aliud esset, ac λογομαχίαν excitare; concessum enim foret venarum radiculas materias absorbere.

Haec sunt praecipua argumenta quae in hâc controversiâ prolata sunt. Quum vero in hujusmodi quaestione factis contraria facta a clarissimis viris confirmata, in utrâque parte reperiamus, veritatem ab utrâque participari verisimillimum est. Itaque dubitare non possum quin multi cum CHAUSSIER et ADELON consentiant, et nec JOANNI HUNTER venas nihil absorbere concedant, neque cum MAGENDIE, *Hunterianas* opiniones de absorbendi functione in lymphiferis vasis insita rejiciant. Hîc tamen quaestio proponenda est.

Si natura duobus vasorum ordinibus absorbendi functionem concessit, num haec vasa in varias materias aequè agunt? Sin aliter, quasnam leges sequuntur, et quid his, quidve illis absorberi debet? Experimenta supra narrata, venas et lymphifera vasa in diversas, potius quam in easdem materias agere, probare videntur.

In sequentibus paginis secunda quaestionis pars consideranda est.

Quae proprietates materias ut Venis, et quae ut Lymphiferis Vasis absorbeantur, aptas reddunt?

HAEC quaestio, ut opinor, nondum satis Physiologorum studium in se convertit. Jamjam supra memoravimus MAGENDIE nil nisi chylum, seu cibi portionem ad nutritionem aptam, chyliferis vasis absorberi, caeterasque res cunctas, quae in corpus intrare possunt, venarum iter sequi existimare. Atque huic opinioni congruere videntur consequentiae, quas ex suis experimentis TIEDMANN et GMELIN inferunt. Verùm enimvero si res ita sese habere conceditur, quod tamen etiamnum in dubium revocare licet, quaestio jam proposita non ideo minus responsionem flagitabit. Parum enim constat, cur aliquid, ob usum cui post nonnullas mutationes inserviturum sit, ex officio istos aditus aliis materiis negatos ingredi sinatur, nisi speciem quandam, et naturam singularem, etiam in limine, prae se ferat. Praetereà, quamobrem natura, id quod nutrire possit a non nutriente tantâ industriâ sejungeret, et paulo postea in venis conjungeret, et cordis contractionibus apprimè inter se commisceret?

Res profectò difficultatibus scatet, et non sine diffidentiâ meam opinionem propono: Scilicet acida, et quae inter acida quodammodo referri possunt absorberi venis, alkalina vero, et his cognata lymphiferis vasis esse commissa. Hanc conjecturam in trimestri libello ad res medicas et chirurgicas spectante, hâc in urbe à Doctore DUNCAN juniore, clarissimo Professore edito, jam olim breviter protuli, et quanquam multa experimenta instituere necesse sit, priusquam pro certo haberi possit, tamen fortasse non omnino indigna aestimabitur, quae hîc paulo fusiùs tractetur. Incipiam ab argumentis, quae, ut opinor, res alkalinas chyliferis et lymphiferis vasis absorberi indicant.

Quanquam chylus pro ratione cibi multùm compositione variet, ut bene demonstravit MARCET, chemici omnes, licet aliter de eo inter se discrepantes ei certè facultates alkalinas, tanquam uno ore, tribuerunt.

In hôc enim consentiunt VAUQUELIN, MARCET, BERZELIUS, BRANDE, et alii. Experimenta Professorum TIEDMANN et GMELIN ad eandem conclusionem spectant. In plurimis exemplis vires alkalinas in chylo observaverunt, nec aliter res sese habuit dum tota intestini canalis acidas materias contineret. At forsan aliquis mihi objiciet, ea experimenta esse vitiata, miris materiarum farraginibus, quas infelices bestiae devorare coactae sunt. Talem quidem conjecturam mihi in mentem venisse confiteor. Sequens igitur experimentum institui. Cuniculum, qui paulo antea rumicis acetosae folia, materiam certè nullis contradictionibus obnoxiam, devoraverat, vivum incidi.

Chylifera vasa humore leviter albido, et lac valde dilutum referente, atque manifestas proprietates alkalinas habente implebantur. Semicoctae tamen materiae, quibus intestina distendebantur, proprietates acidas non minus conspicuas praebebant. Sapor etiam rumicis, et odor quoque aliquatenus manebant.

Lympham vero alkalinas facultates habere non tam bene observatum est. BRANDE declarat illam nec ut alkali, neque ut acidum agere. TIEDMANN autem et GMELIN alkalinas proprietates uno quidèm experimento notaverunt.

Ipse quoque in ovibus nuperrimè occisis exploravi lympham, cujus contactu rocellam acidis rubefactum, caeruleum colorem accipere vidi. Neminem latet vasa absorbentia conspicua fieri glandulasque affici, in illis qui variolae succumbunt. Humorem itaque ex variolarum pustulis examinavi, et alkali facillimè agnovi. Item in quodam homine, cui vesicatorium pectori ulcus induxerat, ex quo glandulae axillares doluerunt et tumefactae sunt, lympha, quâ ulcus madefactum est, fuit manifestè alkalina. Praetereà glandularum tumor et dolor, subacidâ lotione ulceri admotâ, citò levati sunt. Non tamen inde opinor, glandulas ab alkali fuisse exasperatas, sed potiùs virus in eo solutum aptiùs fuisse factum ut vasis lymphiferis absorberetur.

Magendie contra absorptionem per lymphifera vasa objicit, glandulas interdum, punctiunculâ novâ et incontaminatâ acu factâ, tumescere. Hoc vero mihi nugatorium videtur, ipsa enim inflammatio a vulnere orta materiam gignit, quae absorpta glandulas afficit, et parum dubito quin ea, sicut permultae aliae materiae in corpore productae, proprietates alkalinas habeat.

Meâ theoriâ de lymphiferorum vasorum actione admissâ, experimenta quae magno effectu contra horum vasorum absorbendi facultatem Magendie protulit, facilè ita explicantur, ut magnâ ex parte vim amittant. Strichnia, picrotoxia, brucia, et alia principia vegetabilium venenorum, licet quibusdam alkalinis proprietatibus polleant, pene insolubilia sunt, itaque, ut docet Orfila, parum agunt nisi cum acidis conjuncta. Quinetiam in veneficis istis medicamentis cum acido superante existunt, et inde, ni fallor, idonea fiunt quae venis absorbeantur. Barytas fortasse est solum venenum alkalinum in aquâ dissolubile, experimentis vero nequaquam idonea videtur, 1*mo*, Quòd neque haec valde dissolubilis est. 2*do*, Quòd propter vehementes attractiones chemicas, cum aliis rebus protinùs conjungitur, et novas proprietates acquirit. 3*tio*, Quòd cum antea dictis venenis comparari non potest, quia licet genus nervosum manifestè afficiat, tamen inter acria et caustica venena potius quàm torpefacientia recensenda est, et ubi partes erosae sunt, contradicendi occasio deesse non potest. In processu quo chymus formatur, materiae colorantes separantur, ut benè demonstravit Magendie; itaque non miror eas in chylo non fuisse inventas, praesertim si, tanquam indicum, in acidis potius quam alkalinis humoribus solvendae sunt.

Bilis autem, humor naturâ flavus et idem alkalinus, ut supra diximus, vasis lymphiferis absorbetur, quod porrò confirmant Soemmering et Desgenettes.

Nonne ex lymphiferorum vasorum actione, quam nunc propono, pendet ea strumae curatio a Brandish et aliis laudata, in quâ potassa pura, caeteraeque praeparationes alkalinae usurpantur. Quidam sales medii cum lymphiferis vasis, tum venis absorberi posse videntur; hujusmodi sunt murias sodae, sulphas potassae, etc.: in multis aliis res non ita se habet. Operae pretium foret de his salibus investigationem specialem inire. Non oblitus sum HALLERUM acidas proprietates chylo tribuisse; dicit enim: "Utilitas chyli proxima est, putrescibilem naturam sanguinis, acido succo suppeditato, contemperare." At cùm ipse confiteatur chylum nullo rubore succum heliotropii tingere, et ad pinguitudinem acorem obvolventem confugere cogatur, Cl. viri opinio hâc in re nullam vim habet. Parùm dubito quin a nonnullis physiologis multa meae opinioni objiciantur: quaedam tamen dicenda sunt, quae forsitan nonnullas contradictiones avertent. Nondum nervorum actionem cum absorbentium vasorum functionibus conjunctam, physiologi satis exploraverunt. Ex hâc tamen conjunctione, chemicae mutationes vitae peculiares magnâ ex parte pendent. Sequentes notationes ab hàc re non omnino alienae sunt. Anno proximè elapso, lympham in equo ad feras in horto regio Parisiensi alendas occiso examinavi. Contra expectationem hanc lympham acidam esse inveni. Non leviter attonitus, mihimet ipse exclamavi; actum est de theoriâ meâ. Rem tamen in animo perpendens, hanc exceptionem tempori, quod inter equi necem, et lymphae tentamentum elapsum erat, attribui. Crassum intestinum aquâ cum concocto foeno commistâ, et ex eo colore subviridè fusco tinctâ, plenum erat. Vasa lymphifera ex hâc parte intestini progredientia, humorem acidum, ut supra dixi, continebant, qui, licet translucidus, et multo dilutior, ab humore in intestino, colore non valdè differebat. Strenuus HUNTERI fautor hîc sine dubio diceret, lympham ex intestino colorem accepisse. Hoc tamen, licet probabile, ego non assero. Hâc notione de mutatâ post mortem actione perculsus, experimenta JOANNIS HUNTER accuratius examinavi, et fateor me cum opinione MAGENDIE de celeberrimo illo viro consentire non posse. HUNTERUM enim etiam ex istis illius experimentis, quae ad absorbendi functionem pertinent, peritissimum, eundemque vitae parcissimum existimandum duco. Lubentissimè concedo HUNTERUM absorbendi functionis scientiam imperfectam liquisse. Si ille omnia fecisset, MAGENDIE gloria minor fuisset. Quis autem imperitus, inciso animalis abdomine, vità perstante, octo difficilia ex-

experimenta instituere potuisset? et quis nisi vitae parcus fuisset, non potiùs octo animalia ita incidisset? In primo experimento nervi ligati fuisse videntur. Itaque haud miror lac fuisse absorptum, quia, ut in mortuo animali, actio nervorum defuit. MAGENDIE humorem in chyliferis vasis repertum chylum fuisse contendit. At nequaquam dubito quin physiologus ille sapiens ab hâc opinione decedat, cùm consideraverit. 1*mo*, Lac nec humori gastrico, neque pancreatico fuisse subjectum, neque cum bile commistum. 2*do*, Lac injectum fuisse in eam partem inferiorem intestini, in quâ, ut testatus est HUNTER, chylus deerat. In alio animali nervi fuisse intacti videntur, et, ut HUNTERUS ipse agnovit, lacteus humor in lymphifera vasa non penetravit licet lac in intestinum injectum esset. Indicum, aeque ac lac, ligatis nervis absorptum fuisse videtur.

HALLERI et quorundam aliorum physiologorum experimenta non adeo enucleatè narrantur, ut dici possit utrum meae opinioni faveant, an repugnent. Unum nihilominus inter Professorum TIEDMANN et GMELIN experimenta, hìc non reticendum est.

Acetas plumbi et tinctura rhei cani data sunt et, occisa bestia, rheum, quod fieri non solet, in chylo repertum est, plumbum quoque in chylo aderat. Praetereà hic humor aut medius (neutral) aut etiam acidus videbatur, quia necesse fuit alkali addere ut rheum conspicuum fieret. Ex quo conjicio plumbum, quod paralysin inducere solet, in nervos agere, et absorbendi funcionem perturbare posse. Chylus alkali carens hujus plumbi effectûs optimum indicium videtur; nam ubi ob minorem plumbi quantitatem hic effectus non productus est, et materiae colorantes non absorptae sunt, chylus solitâ ratione alkalinas proprietates habuit. Non possum non credere, discrepantiam, quam supra inter quosdam egregios Physiologos existere notavi, ex hac mutatâ vasorum absorbentium actione praecipuè pendere.

Modo paulisper venas inspiciamus, et consideremus quantum in hâc parte conjectura mea factis nitatur. Quod si vasorum, quae absorbentia nuncupantur, actio obscura est, et explicatu difficilis, multò venarum difficilior. Cùm venarum officium evidentissimum sit, sanguinem, jam ad extremas arterias perventum, non nihil mutatum ad cor reportare, necesse est ut minùs evidentis officii, absorbendi scilicet, effectus difficiliùs assequamur, et necessariò simplices nunquam contemplari possimus. Praeterea, licet permulti sanguini operam dederint, discrimen sanguinem arteriosum inter et venosum nondum satis prolatum est. Admissâ theoriae meae parte priore, ad vasa lymphifera spectante, haec secunda ratione quodammodo indicari videtur. Nam si lymphifera vasa partes alkalinas absorbent, etiam si chymus medius antea fuerat, et a fortiori si acidus, necesse est ut reliquum acidum fiat. At magna pars hujus, quodcunque sit, reliqui, venis absorberi videtur*, et intestini finem versus acor plerumque deest.

Haec autem ratiocinatio, factorum confimatione non omnino caret. ANTHONIUS TODD TOMPSON, dùm acidi oxalici in cuniculos effectus exploraret, acidum in sanguine venoso invenit. Hoc quoque ab amico meo LUDOVICO PEREY saepè observatum est. Non modo charta Lichene Rocellâ tincta rubefacta est, sed sanguis mirabilitèr nigruit. Et licet recentiora experimenta a Doctoribus COINDET et CHRISTISON instituta, cum supra dictis non omnino consentiant, meae tamen theoriae non adversantur; his enim exploratoribus acidum per absorptionem agere videbatur, et quanquam acidas proprietates in venoso sanguine detegi posse negent, mutationem sanguinis coloris effectam, ab acidi contactu procul dubio provenientem confirmant. Quae de acidorum mutatione scripsit ingeniosus COINDET, nondum legendi occasio mihi data est.

Res profècto quae maximâ curâ exploretur dignissima est, chymica enim est actio, cujus leges prolatae, etiam si causa in aeternum lateat, physiologiam necessario promovebunt.

Acidum nitromuriaticum, aeque ac oxalicum in corpore mutari videtur. Doctor COMBE, cujus dissertatio de acido nitro muriatico, Regiae Societatis medicae palmâ donata est, acidum in sanguine nequaquam reperire potuit. In bestiis vero, hoc acido non aliter ac oxalico necatis, sanguis venosus niger est, at in sinistro corde majore etiam quam solitâ ratione rubet. Physiologi multum inter se de naturâ sanguinis venae portae discrepaverunt. Alii enim hunc sanguinem non solitâ ratione coire declaraverunt: hoc autem ab aliis negatur. Mihi persuasum est hanc dis-

* Reliquum strictissimâ dictione non bonum esse verbum confiteor, quippe cum venae eodem tempore, quo lymphifera vasa agant, sensus autem satis evidens est.

crepantiam, potiùs ex quâdam varietate a materiis absorptis proveniente, quam ex scriptorum fide proficisci. At bene cognitum est acida parvà quantitate cum sanguine commista illius coitum impedire posse. Nondum novimus quomodo sanguis arteriosus in venosum mutetur, neque statum carbonii quod in hoc, majore quantitate existit. Argumenta vero non desunt, quae illud etiam in venis, tanquam acidum immaturum existere indicent, Hoc autem admodum dubium est. De principio colorante in rhei radice existente, haud multum novimus; notatu tamen dignum est, hoc principium, quod ex experimentis Everardi Home, et Professorum Tiedmann et Gmelin venis absorberi videtur, alkalinum contactum exigere, quò in sero sanguinis manifestum fiat. Indicium, meo quidem judicio, alkalinum sanguini naturale ab aliquo acido superatum fuisse. Indicum praeterea, quod acidis praecipue solvitur, venis manifestè absorbetur, etsi color postea, alkalino in sanguine existente mutatur. Ad haec, cùm oleum, fixum, ut vocatur, et alkalinis consentaneum, chyliferis vasis absorbeatur, quod cremore chylo supernante ostenditur, essentialia contra, et empyreumatica, non minus acidis convenientia, venis absorbentur. Si venae materias acidas accipiunt, nonne hoc in causâ est, cur sitientes acidulos humores appetant? Venae cum ob majorem capacitatem, tum ob faciliorem cum intestinis communicationem citiùs et copiosiùs quam chylifera vasa, humores imbibunt: Sapiens itaque natura venis aptissimae potionis desiderium sitientibus excitasse videtur.

Alkohol magnâ quantitate et omnino sine mutatione venis absorbetur. Fateor me nondum conjectare potuisse, quamobrem his vasis accipiatur, et illis rejiciatur. Ab acido tamen acetico, compositione non longè distat.

Priusquam commoda, quae ex absorptione modo supra conjecto perfectâ, oriri possunt, et quantum haec conjectura cum phaenomenis in variis functionibus apparentibus conveniat, consideremus, abs re non erit, paulisper examinare quid physiologis visum fuerit, de modo, quo variae absorptae materiae, sive in vasa chylifera, sive in venas accipiuntur.

Nonnulli existimavere pressuram intestinorum super ea quae in illis continentur, liquidas chimi partes in patula vasorum ostea adigere. Plures vero, quos inter numerandi sunt Haller et Monro secundus et tertius, materias primo per attractionem vasorum capillarium sorberi, et postea vasorum contractionibus, valvulis adjuvantibus, promoveri.

MAGENDIE quoque, licet in opere suo de Physiologiâ ignorantiam de hâc re confessus sit, tamen in dissertatione quam recentiùs Academiae Regiae Parisiensi subjecit, istam attractionem, tanquam absorptionis causam, admisisse videtur. Quòd si absorptio, in tali actione constaret, tum sane necesse foret, omnes humores, sine discrimine in vascula utriusque ordinis intrare. Ad hanc difficultatem vitandam, BICHAT sensum, et vim contractionis organicae, ut ea nominavit, finxit.

At praeterea quod illas proprietates sensibus nostris nequaquam assequi possumus, theoria variis contradictionibus est obnoxia: Haud quaquam necessarium est hîc referre omnes difficultates de quibus alii physiologi mentionem fecerunt; sequens mihi insuperabilis videtur. Cùm illarum facultatum, quas imaginatus est BICHAT, actio tota foret in rebus quibusdam accipiendis, aliisque rejiciendis, patet eas, si existerent, mechanicam tantummodò separationem esse perfecturas. Ea autem elementa, quae inter se chymicâ attractione retinentur necessariò sine mutatione manerent.

BRUGMANS in auxilium vocat vitam propriam in absorbentibus vasis insitam; et clarus BLUMENBACH, ni fallor, cum illo quodammodo consentit. At ita profectò difficultas nequaquam explicatur. Quidam inter celeberrimos nostri aevi philosophos et physiologos, YOUNG nimirum, et WOLLASTON et WILSON PHILLIP et PROCHASKA in hujusmodi vitalibus actionibus, vim galvanicam multum perficere existimant. Verum enimvero si a tantis viris decedere sit juveni permissum, ego quidem fateor multas difficultates obstare, quo minus talem opinionem amplectar. Mihi quoque praematurum videtur causam mutationum chimicarum, quae in vivis corporibus perficiuntur, imaginari, priusquam phaenomena ipsa, et leges quibus parent, meliùs cognoscantur. His autem inventis, sed non antea, exquirere licebit, num vis, unde proveniunt, galvanicae, aut alius jam notae vis modus tantùm sit, an potius novum prorsus principium.

Multum a physiologis inquisitum est, quomodo partes concretae absorbeantur. HUNTER, ut supra notavimus, extrema resorbentia vasa, tanquam erucae, rodere conjectavit. Notavit BICHAT, absorptionem cum corpusculis agere, et difficultatem de submovendis partibus concretis, omnino esse imaginariam. Haec sententia tam verè de multis aliis mutationibus quam de absorptione proferri potest. Penitus itaque absurdum videtur, easdem visibiles particulas in sanguine, in variis corporis texturis, et in multis secretis humoribus, imo etiam in coctis carnibus exquirere.

Vanae itaque aestimandae sunt plures microscopicae investigationes, ab EVERARDO HOME, et BAUER institutae, "quoniam" ut ait LUCRETIUS

"——— Extremum cujusque cacumen
"Corporis est aliquid nostri, quod cernere sensus
"Jam nequierunt."

Constat experimentis MAYER et MAGENDIE varias materias a matre in foetum per absorptionem transire: de hac autem absorptione nihil dicendum habeo.

Vegetabilia non minus quam animalia absorbendi functione fruuntur, illis autem non aequè ac his, operam dedi; licebit tamen, sequens experimentum proferre. In figlino, lauto sabulo impleto, gramina varia, fabas, pisa, atque alia semina conserui; totum deinde haud tenui infuso croci madefactum, in locum obscurum seposui, ne color ullus in plantis ipsis generaretur, et subinde irrigationem cum supradicto infuso repetivi. Germina brevi pullulare sed nequaquam croco tincta. Infusum tamen colorem magnâ ex parte amisit, et odorem peculiarem acquisivit. Infusum autem, in quo semina non aderant, vix, aut ne vix quidem, mutatum est. Parùm dubito quin supra dictae mutationes seminum cotyledonibus, ut vocantur, et putaminibus partim productae sint, multum tamen tribuo decompositioni ab ipsâ germinatione provenienti. Ex hoc experimento conjicio effectus, quos DESFONTAINES et alii obtinuerunt, ex vasorum sectione pependisse.

Nunc tandem, quantum meo proposito convenit, et forsitan ampliùs quam lectoris patientia condonabit, absorbendi functionem exposui. Restat modo pauca proferre de quibùsdam effectibus, qui, ut mihi videtur, ex absorptione modo quo fieri suspicatus sum perfectâ, provenire possunt.

DE QUIBUSDAM PHAENOMINIS CUM ABSORBENDI FUNCTIONE UT SUPRA PROLATA EST CONJUNCTIM CONSIDERATIS.

Quoniam hanc dissertationem jamjam, insolitâ ratione, produxerim, rerum duntaxat fastigia sequi licebit.

Quamvis haud necessarium sit plura dicere, de utilitate duorum vasorum ordinum, ad varias materias a chymo separandas, quippe cùm lector ex superiore parte, meas de hac re opiniones facillimè concipere possit, non omnino supervacaneum erit eorum utilitatem, in variis corporis partibus examinare. Quaeri enim potest. Si hae partes uno tantum vasorum ordine, arteriis scilicet, deponuntur, quamobrem ad illas submovendas duobus opus est? Respondeo: Corporis partes fere in hoc modo elaborari suspicor. Cùm sanguis arteriosus ad extrema vasa pervenit, cum venarum et lymphiferorum vasorum initiis committitur. Haec enim vasa ubique in corpore adsunt.

Nunc si haec vasa diversas materias attrahunt, modo in variis structuris diversâ proportione concurrant, et tertia substantia relicta varietates totidem exhibebit. Novae autem particulae natura, partìm pendet ex naturâ adjacentium particularum, atque multùm, ni fallor, ex compositione particulae cui successura est, et quae cum reliquo sanguine, vasis absorbentibus submovenda est. Errant autem, qui tales corporis ruinas, in istis vasis exquirunt, nam ut supra diximus, omninò decomponuntur. Praetereà licet veteres particulae, a novis quodammodo discrepent, tamen cùm inter se utraeque commutatae sunt, sanguis ad cor rediens, postquam illas amisit, et his acceptis, venosus evasit, eo usque reparatur, ut in omnibus texturis eandem speciem habeat.

Multum a Physiologis disputatum est de secernendi functionis naturâ. Alii existimavere secretos humores ex sanguine minutis vasculis esse colatos. Alii multum motui tribuunt, atque alii fermentationem in auxilium vocant. Nonnulli dicunt secretionem ex peculiari vasorum actione dependere. Hoc autem, rem nequaquam explicat: factum tantummodo alitèr exprimit. Vis galvanica, cui tantum tribuitur a quibusdum claris viris, secretionis causa habetur. Nuperrime chemicus ille celeberrimus THENARD postquam deutoxydum hydrogenii, fibrinâ sine ullâ mutatione hujus materiae, decomponi posse invenerat, humores in corporibus animalium, fibrinâ secernentium organorum decomponi, et ita secernendi functionem explicari posse conjectavit. Ego vero suspicor, hanc functionem in actione constare simillimâ actioni quam corporis partes elaborare supra imaginatus sum.

De secernendi functione quaestio inter physiologos orta est, quam omninò praetermittere nolo. Alii enim, quos inter numerandus est ADELON omnes corporis partes, atque omnes secretos humores ex arterioso sanguine formari docent: Alii autem sanguinem venosum ad bilem saltem formandam conferre existimant. JACOBSON verò, ex quibusdam observationibus in piscibus institutis, sanguinem venosum, multo majore functione fungi conjectat. Cum illo quoque consentit BLAINVILLE, qui plurimos secretos humores, huic sanguini attribuit, et credit etiam vasa pampiniformia, et lien quoque, organa constituta fuisse, quae sanguinis moram efficiant, quâ ad nonnullos secretos humores formandos aptior fiat. Licet hanc notionem non amplectar, effectum peculiarem sanguinis venosi morae esse attribuendum reor, formationem scilicet nigrae materiae quod naturalitèr in oculo, et in Ethiopis cute existit, et pigmentum constat, et quod morbo, saepe in pulmonibus, non raro in liene, et interdum in aliis partibus, ut in cute hominum etiam a Caucasiâ stirpe generatorum existit.

Ecce argumenta quibus haec opinio nititur.

Primo ea exempla examinemus, quae nobis pathologia profert, horum enim causae plerumque evidentiores sunt.

Materies nigra, quae in pulmonibus et in glandulis bronchialibus reperitur a PEARSON, carbonio cum aere inspirato, perperam tribuitur. In parvâ quantitate nullum incommodum parit, et in adulto adeo vulgaris est, ut multis medicis nequaquam morbida videatur; interdum vero, magnâ quantitate existit, ut in iis qui in arctis urbium vicis habitant, aut in profundis fodinis vitam degunt, at longè prae aliis in iis qui phthisi laborant, aut alio quovis morbo, qui per longum tempus functiones magnae pulmonum partis impedit. At in omnibus his exemplis sanguis per pulmones, non solitâ ratione cursum habet, et imperfectè ab excessu carbonii purgatur. Lien quoque in se multum sanguinis venosi habet, qui praetereà tardo circuitui obnoxius est, praesertìm si mea de hoc organo conjectura admittitur.*

Si eorum exemplorum, in quibus cutis nigruit, historiam investigabimus, hoc effectum a repentino et opprimente terrore ortum fuisse inveniemus. Sequens exemplum proferre sufficiet. Inter horrores Gallici imperii eversionis, femina capite damnata est, quâ sententiâ eo usque affecta est ut cutis more supra dicto brevi nigruerit: remissâ vero paenâ, illa permultos annos vixit, et nigrum calorem, tempore tamen levitèr minutum conservavit.

Post mortem cutis explorata est, et materies colorata in rete mucoso reperta est, vel, ut diceret BLAINVILLE, super rete vasculare verum pigmentum impositum est.

Nunc ad oculum respiciamus, in quo certè naturalitèr, et in sano statu nigra materies existit. Nonne venae contortae sunt (unde etiam vasa vorticosa vocantur,) eo proposito, ut sanguis moretur, et pigmentum deponat, quo lucis errantes radii absorbeantur, et oculi globus opacus reddatur? Vidi ante paucos dies HEUSINGRI recentem de pigmento nigro dissertationem. Hic auctor quoque existimat materiam nigram ob excessum carbonii formari, et eodem principio tribuit oculi pigmentum, materiem nigram in pulmonibus, et materiem colorantem capillorum, unguium et pennarum. Nonne meae opinioni consentaneum est, ma-

* Vide Edinburgh Medical et Surgical Journal. In hoc libro, lien organum esse proposui, quo graves effectus ex auctâ copiâ fluidorum, et ex diminutâ capacitate vasorum prohibeantur. Tunc temporis nesciebam Doctorem RUSH, Philadelphiensem, eandem sententiam emisisse, et Doctorem BROUSSAIS in praelectionibus suis huic similem notionem proferre. TIEDMANN et GMELIN volunt hoc organum, vasis chyliferis et lymphiferis tanquam appendicem esse, in quâ lymphâ coeundi facultatem, et subrubrum colorem adipiscitur. Hae autem proprietates manifestae sunt in lymphâ quae nec lienis actioni subjecta est, neque lympham ex liene provenientem accepit. Credo equidem lympham has proprietates in omnibus corporis partibus comparare posse, etsi in iis praecipue, quae in se multum sanguinis habent. Non tamen reticendum est illos physiologos ad experimentum confugisse. Lienem canis execuerunt: bestia paucis diebus convaluit functiones parum turbatae sunt, modo paulò macrior quam antea videbatur. Post dies octodecem occisa est, et exploratores testantur, partem chyli coactam, solitò minorem fuisse. Hanc autem observationem omnino vitiatam aestimo, eo quod bestia prussiatum potassae devoraverat, qui alio experimento, integro liene, chylum fluidiorem quam solitum fecisse videtur. Macies partim vulneri tribuenda est, partim canis graviditati, quinque enim foetus in tubis reperti sunt, qui miro quodam errore, quartum vel quintum mensem attigisse dicuntur.

teriem colorantem pene deesse in iis, quibus arteriae exuperant.

Nunquam mihi sepiam explorandi occasio contigit, scire tamen exoptarem, num organum, quo atramentum secernitur, cum meâ conjecturâ congruat.

Si sanguis ad fines arteriarum, eo modo disponitur, quem supra conjectavi, mihi videtur hoc ad sanguinis motum nonnihil conferre debere. Sic in canalibus et rivulis objectaculis munitis, his apertis, fluxus inducitur. Similem quoque originem Cl. Franklin quibusdam procellis attribuit.

Imaginari non possum quomodo extremorum vasorum contractiones, circuitum sanguinis promovere possint.

Itaque non modo Cl. Bichat horum vasorum contractiones organicas, sed etiam actionem quam illis tribuit Wilson Phillip, planè rejicio. Dispositionem sanguinis ad fines arteriarum plus efficere, quam ullam istorum vasorum actionem, ex hoc argumentor. Scilicet ubicunque adest seminatum ovum, sive in utero, in tubo, in ovario, vel etiam in peritoneo, illic sanguinis cursus attrahitur.

Parum adhuc causam intelligimus, cur quaedam partes prae aliis interdum insigniter increscant. Factum tamen non incommodè a Blumenbach exprimitur, et nisui formativo attribuitur. Hoc in annua cervorum cornuum renovatione conspicuum est, at hîc quoque auctum sanguinis cursum non primarium esse, sed secundarium, et supra dicto more inductum, existimo. In foetibus corde carentibus, sanguinis motum, ut opinor, multò meliùs huic causae quam aut arteriarum contractionibus, aut alius foetùs cordi, attribuendum est. In vegetabilibus, cum nullum organum ad succum movendum specialitèr constitutum habeant, multùm actioni ei quae supra prolata est simillimae tribuendum est. Actio quam nunc ad sanguinis circuitum auxiliarem protuli, mihi longè verisimilior videtur, quam ea quae ad hanc finem imaginatus est Carson. At ne quis existimet, me arterias non nisi tubos inertes habere. Licet illis non attribuam facultatem ullam, quâ sanguinis circuitus effici possit, lubentissimè concedo, motum sanguinis ab aliis organis excitatum, modificationes ab arteriis accipere. Sic ob vim resiliendi, ad motum inaequalem, ex cordis alternanti actione provenientem, constantem et aequabilem efficiendum tendunt. Ob *tonicitatem* morbo mutatam, sanguinis rivum non semper eâdem ratione amplectitur, et inde partim oriuntur mirae pulsuum diversitates. Vasa quoque in ipsum sanguinem agunt, et ejus conditionem afficiunt.

Sic HEUSON sanguinem in propriis vasis stagnantem, solitâ ratione non coire demonstravit. Hunc effectum sagacissimus HUNTER nequaquam ignoravit, et recentiùs ingeniosus BLUNDELL, hâc de re non nulla experimenta instituit. Non modo GULIELMI HEUSON, de retardato sanguinis in vasis coitu, assertionem bene confirmavit, sed praeterea, vasa, postquam tunicae suae, aut frigore, aut nicotianâ infusâ, aut alio quovis modo valde laesae sunt, hâc facultate sanguinem liquidum servandi carere demonstravit. Ex quo vitam sanguinis ex mutuâ sed inexplicatâ actione hunc inter et vasa sanguifera pendere, conjectavit. Nonne igitur natura, in mirabili illâ functione, quâ creationis opus quodammodo aeternum fecisse videtur, sanguinem, ex quo faecundi humores formandi sunt, per longas, tenues, et tortuosas arterias spermaticas transmisit, eo proposito ut *hypervitalis*, ut ita dicam, evadat.

Haec enim conformatio in illis animalibus, quae hâc functione prae aliis pollent, ut in tauro et in ariete*, praecipuè observanda est.

Multùm profectò de absorbendi functione, ad morbos spectante disseri potest. Non modo multis in morbis maciem, signum quidem notabile inducit, sed etiam haec functio laesa, non nullorum morborum origo, seu causa proxima habenda est. Omnium pene consensu struma, et quaedam hydropis varietates hûc referuntur.

Nec multum abest quin mihi persuasum sit, febris ipsius essentiam constare, in eâ functione laesâ vel pene suppressâ, quâ variae corporis partes assiduè mutantur. Haec autem quaestio multo copiosior est, quam ut hìc tractetur. Inflammationem quoque, ut opinor, et scorbutum, si functiones cum absorptione conjunctae non nimis fuissent neglectae, multo meliùs intelligeremus. At ne lectoris benevoli patientiâ diutiùs abutar, de his, itemque de Hydrargyri et aliorum quorundam medicaminum in absorbendi functione effectibus, et agendi rationibus, conjecturas meas proponere in aliud tempus differam.

* In excellenti opere CUVIERI, ornatissimi quidem Philosophi, et animalium scientiae excultorum omnium facile principis, haec facultas in ariete multò minoris aestimatur, ut abundè demonstrant facta, de quibus in Sussex comitatu Anglico ovium culturâ notatissimo, certior factus sum.

Since writing the preceding Thesis, I have had the advantage of perusing some valuable articles relating to the subject of absorption, which have been written subsequently to, or near the same time, with my Essay. The most important of these, are two papers by Leonardo Frunchini, published at Bologna, in 1823; that of Fiscinus and Seiler of Dresden, republished in the "Journal Complimentaire du Dictionnaire des Sciences Medicales;" that of Michael Fodera on Absorption and Exhalation; several papers by Regulus Lippi, Professor of Anatomy at Florence; and of Fohmann and Louth of Germany, on the Anastomosis of the Lymphatics with the Veins; the researches of Dr. Barry on the Influence of Atmospheric Pressure; and of my friends, Dr. Addison and John Morgan, on the Modus Operandi of Poisons.

I must not swell this volume with an analysis of these publications; but restrict myself to mentioning the principal facts which they severally relate, whether in support or refutation of the statements which I have made.

It has been objected to the view which I have taken of the function of absorption (p. 346.), that it is far too chemical; but I would beg to state in reply, that I apprehend that in this respect I have been somewhat misunderstood.

I am fully prepared to admit, that the science of chemistry, as it at present exists, is unquestionably inadequate to explain many of the phænomena presented by animal life; but I consider, that as in many of these phænomena chemical affinities are counteracted, and numerous new chemical combinations formed, processes essentially chemical are going forward, which we can never fully understand, even to the extent which our limited powers will allow; unless we investigate them with the assistance of chemistry. In saying this, let me not be regarded as considering life as merely a chemical phænomenon. The functions of living organized beings involve the operation, not only of chemical, but of every other physical agency, and in our investigation of these functions, if we wish really to improve our knowledge of the attributes of life, we must not reject our knowledge of those agents, but rather regard whatever is fixed in them as a known quantity, which may greatly aid us in ascertaining other quantities, as yet unknown. Scarcely any important step is taken in any department of physical knowledge, which does not give us a clearer view of some vital function. The labours of Nysten, Dr. Stephens, and Dutrochet, afford good illustrations of this assertion.

Those who feel interested in this subject, will do well to read the Essay of Dr. Pritchard on the Doctrine of a Vital Principle, which contains many important considerations connected with the study of the phænomena of life.

The permeability of the animal tissues, alluded to in the second note, p. 349, is further considered in the sequel.

Page 350. The experiments of Dr. Edwards, referred to in the first note, have been detailed in the previous part of this volume.

The functions of the eighth pair of nerves, which are glanced at in the second note, will be noticed in conjunction with the consideration of the influence of the nerves on absorption.

Page 354. The elaborate article of Fiscinus and Seiler, who conducted their enquiry at the Veterinary School at Dresden, contains a much more detailed account of the discovery and investigation of the lymphatic system, and of the controversy which ensued, than I had met when composing the Thesis, or than I have seen in any other work since.

To the names of the early contributors to our knowledge of the anatomy of the lymphatic system, which I have already enumerated, must be added those of Pecquet, Van Horn, Rolfink, Severinus, Hilden, Wurm, and Highmore; and of a somewhat later period, Steno, Nuck, Stalport, Vander Weil, Brunner, Rudly, Ruysch, Mery, Saltzmann, Winslow, and Blaes.—From the second half of the seventeenth century, and during the first part of the eighteenth, Swalve, Walæus, Diemerbrœck, Lower, Pauli, Bohn, Cole, Wharton, Gottsched, Blanch, Glisson, and Charleton, declared themselves in favour of lymphatic absorption. Two periods are particularly interesting in reference to this subject during the second half of the eighteenth century, viz., from 1780 to 1799, when the Hunters, Sograffi, and Heuson, and subsequently, from 1780 to 1799, when Cruickshank, Mascagni, Sheldon, Sœmmering, Blumenbach, Ludwig, Haase, Werner, Feller, Desgenettes, Oudmann, and Schreger, contributed, both by experiments and arguments, to overturn the old doctrine of venous absorption, which from the year 1798, was scarcely supported by any one.

Those who are disposed to investigate the controversy, which was rather renewed than commenced by the Hunters and their opponents, may consult Haller, Cruickshank, Sheldon, Rezia, Mascagni, Lindner, Lupi, Sœmmering, Ludwig, Oudmann, and Lorinser.

Page 355. Although I have expressed a doubt as to the propriety of admitting the capillary system, as proposed by Bichat, from the idea that mere minuteness is not a sufficient ground of distinction to lead us to separate, either the small arteries, or veins, or lymphatics, from the set of vessels to which they belong, the recent highly interesting observations of my friend, Dr. Marshall Hall, appear to afford demonstrative evidence of a system of minute vessels, intermediate to the extreme arteries and veins, and anatomically distinct from both. "It is quite necessary," says Dr. Hall, "to distinguish the capillary vessels from the minute arteries from which they arise, and the minute veins to which they belong. The minute vessels may be considered as arterial, as long as they continue to divide and subdivide into smaller and smaller branches. The minute veins, are those vessels which gradually enlarge from the succession of smaller roots. The true capillary vessels are obviously distinct from each of these; *they do not become smaller by subdivision, nor larger by conjunction,* but they are characterized by continual and successive union and division, or anastomosis, whilst they retain a nearly uniform diameter."

The anastomoses of minute arteries, though mentioned by many authors, the Doctor states to be of so rare occurrence, that after diligent search he has not been able to meet with a single instance of them. Anastomes between the roots of veins, he regards as not uncommon. These differences between the true capillary vessels, and the arteries and veins, have a manifest influence on the mode in which the blood circulates through them. Its course becomes of only half its former velocity, and consequently the particles, instead of moving too rapidly to be seen, become distinctly visible.

Dr. Hall suspects, that the true capillaries are mere canals, instead of being real tubes, by which, I suppose him to mean, that they are not possessed of distinct coats, independent of the structure through which they pass. It is obvious, that this difference must materially contribute to modify their functions. (See Dr. M. Hall on the Circulation of the Blood, a work abounding in important facts and views.)

The existence of the capillary vessels above described, and the want of any sufficient proof, that either arteries or veins in a state of integrity, are furnished, with open mouths, go far to do away with the idea, that there is a peculiar structure void of vessels intermediate to the termination of the arteries and the origin of the veins. But the admission of the continuity of the arteries and veins, through the medium of the capillaries, seems to imply the existence of a non-vascular structure, through which they circulate. The microscopic examination of the animal tissues favours the same conclusion.

Page 358. The fullest investigation of the communications by which the lacteals and lymphatics, discharge themselves into the venous system, is that which we owe to Fohmann, Louth, and Lippi. The last author has justly received, as the reward of his indefatigable labours, one of the prizes awarded by the Royal Academy of Sciences of Paris, and I cannot do better than refer the reader to his work, intitled *Illustrazioni Fisiologiche e Patolgiche del Systema Linfatico-Chilifero mediante la scoperta di un gran numero di communicazioni de esso col venoso.* It must be regarded as a valuable and important contribution to anatomical knowledge, whatever be the opinion entertained of the author's physiological views.

Some of these communications had also been described by others, viz., those of the lymphatics of the broad ligaments with the hypogastric veins by Wepfer; Steno had pointed out those of other lymphatics, with the cava and the axillary and jugular veins. Nuck traced those of the arm to the lumbar veins; Lobstein those of the spleen to the vena portæ; and Mertrud others to the subclavian. Seiler often saw mercury pass into the veins from the lymphatics. It frequently happened with the lymphatics of the thigh, and was almost always the case when the mesenteric glands were injected. The last mentioned fact completely accords with the observations of Meckel, whose opinion has been adopted by Falconer, Lindner, Ludwig, Caldani, Wrisberg, Lenhossek, F. P. Meckel, and Ribes.

Page 359. *Communications between the internal surfaces of various cavities and canals, and the lymphatic vessels.*—Seiler has often seen the lymphatics injected from the duct of the pancreas, the vas deferens, and the canal of Steno. Walter has observed the same thing in the lactiferous tubes. Lippi filled the neighbouring lymphatics of the liver from the cavity of the gall bladder. It frequently happens, that the lymphatics of the liver and spleen are filled when those organs are finely injected.

Page 359. *Communication between the arteries and absorbents, or lymphatics.*—Lippi says, that the lymphatics arise from the arterial system, as well as from the surface of membranes, and wherever secretion or effusion takes place.

Page 360. *Communication between the minute extremities of lymphatics and veins.*—Lippi describes the lymphatics of the kidney inosculating with those of the emulgent veins by a kind of capillary vessel.

The following authors may be mentioned, in addition to those whose names are cited in the Thesis, as supporters of the doctrine of absorption by the lymphatics:—Glisson, Wharton, Derelincurtius, Needham, Lenoble, Collins, Van der Sande, Sografi, Fœlix, Ludwig, Feller, Haase, Oudman, Schreger, Fohmann, Lippi and Louth.

The work of Oudiman, is regarded by Fiscinus and Seiler as an able performance in support of lymphatic absorption, but they consider him not sufficiently rigid in the examination of the experiments of his party, nor does he attach sufficient importance to the arguments of his opponents.

The experiments of Schreger, which are some of the latest that have been brought forward to establish the exclusive right of the lymphatic system to the character of absorbents, claim particular consideration from the author being an acknowledged good observer, as well as from his having been previously a strenuous advocate of venous absorption. Yet, the opinions of both these experimentors, but more especially of the former, appear to be nearly as much founded on the inability to detect milk, and other substances, in the veins, as on direct proofs of their absorption by the lymphatics. The following experiments of Schreger's seem to require some confirmation or explanation. A bandage was tied round the hind leg of a puppy, as near as possible to the pelvis. The limb was kept twenty-four minutes in tepid milk — the lymphatics were then filled with milk — the veins contained none. The bladder of a dog was filled with milk; the crural arteries were tied — in twenty-four minutes the vesical veins were almost empty, and the little blood which they contained, was unmixed with milk; but there was some in a few of the lymphatics. In bleeding a woman from the foot, it happened, that a lymphatic vessel was divided; there continually flowed from it a quantity of lymph; the lower part of the foot was immersed in tepid water mixed with a solution of musk. The lymph was collected in a cupping-glass, and blood was taken from a vein opened on the upper part of the foot; the former soom smelt of musk, the latter not in the least. In at least one of the experiments performed by Fiscinus and Seiler, it happened, that when madder had been given, the chyle was remarkably red. In five of their experiments, in which an alkaline solution of lead was either given internally, or applied externally, lead was detected in the chyle. Traces of arsenic, and also of nitrate of silver, have also been found in the contents of the thoracic duct. In two other experiments, in which turmeric had been employed, the chyle presented a yellow colour, which was heightened by the addition of an alkali.

Lippi, the friend and disciple of Mascagni, is the last and most strenuous advocate for the doctrine of absorption, performed by the lacteals and lymphatics, exclusively. I have already alluded to the anatomical facts on which his views are founded. These views manifestly present lymphatic absorption, under a modified character, in which it seems to evade some of the objections urged against it by the supporters of venous absorption; but they appear to be liable to others, some of which, I have anticipated at p. 367.

Page 365. Besides the recent experimenters whom I have mentioned, and to whose labours I shall presently revert, the following names may be added to the list of those whom I have cited, as supporters of the ancient doctrine of venous absorption,—Bills, Jacques de Back, Schneider, d'Azout, Horn, Mortimer, Brendel, Darwin, Walter, Lenhossek, Malfatti, Caldani, Lupi, Hartman, Prochaska, Treveranus, Reil and Jæckel.

The experiment referred to at p. 366, was performed by Segalas and read by him in the Royal Academy of Science of Paris. I cannot omit this occasion for protesting against inferences drawn from experiments, in which the functions of the animal, or part of the animal, are so completely interfered with, as they must have been in this instance, in which the lymphatic vessels alone remained to connect the intestine with the animal. This objection applies almost to the same extent to the celebrated experiment of Magendie, in which the poisoned limb was connected with the body of the animal, solely by the means of the circulation maintained through the pieces of quill, which united the divided vessels. Though this experiment may succeed in the hands of the able physiologist who devised it, yet it must be regarded as one of the greatest difficulty, since, besides the objection already urged, there is another of equal practical importance in the coagulation of the blood in the quills. This obstacle proved insurmountable, in various attempts made by my friend J. Morgan, and equally defeated another distinguished physiologist, who attempted to repeat the experiment. It may be objected against some of the experiments performed by Magendie, that the production of the peculiar effects of a poisonous agent, is no sufficient proof of that substance having been absorbed. This is fully admitted by Franchini, especially in those cases in which the quantity of poison employed is very small, and the operation of the poison is violent and rapid. The strongest confirmation of this objection, is furnished by the researches of my friends, Dr. Addison and J. Morgan, respecting the operation of poisons on the living body. They have clearly proved, that the effects of poisons do not depend on the contamination of the fluids circulating through the system. A connection having been established between two dogs, by means of the large vessels of their necks, the blood from the one, expiring under the influence of a poison, with which it had been inoculated, was transmitted with impunity into the second. To render this trial as complete as possible, a mutual interchange of blood was established, the trunk of the carotid of one dog supplying the branches of the other, and the jugular vein of one discharging itself into the heart of the other. The animal, which had not been wounded with the poison, exhibited not the least indication of its influence. Although the widely received doctrine, which ascribes the influence of poison to absorption, and more especially to venous absorption, is thus invalidated, the question of absorption is left untouched. Without reference to the effects produced on the system, it is very evident, that many substances are introduced, with little change into the body, though the channels by which they do so may elude our search. Leonardo Franchini, a disciple of the old doctrine, undertook his experiments with a view to elucidate this subject. He repeated with great care, the various experiments of Lister, Musgrave, Hunter, Haller, and others, who have described the absorption of coloured, odoriferous, and other substances, by the lacteals and lymphatics, but his results drawn from fifty experiments, most essentially differed from theirs. On the other hand, he found many of those substances in the blood of the vena portæ, and other veins. He sums up the results of his experiments, in the following conclusions: — 1. "The white, or lymphatic vessels of the intestines, are the true absorbents of the chyle. 2. It is not proved, that they absorb any other fluids from the interior of the intestines. 3. There are no valid arguments by which it is proved, that the lymphatic vessels of other parts are absorbents. 4. The veins certainly absorb from the interior of the intestines, from the cavity of the abdomen, and from the bronchi. 5. It is not proved by experiment, whether the lymphatics of the veins absorb from the minute cavities of the cellular membrane, and from the interstices of the different tissues. 6. Many facts, both physiological and pathological, render it very probable that the absorption of fluids from these cavities, is, at least, to a great degree, performed by the sanguiferous vessels." Perhaps, the most important and original part of Leonardo Franchini's paper, consists in the satisfactory explanations which he has offered of some of the contradictory statements, which the supporters of the conflicting opinions have advanced. He had frequently repeated the experiments of Lister, Musgrave, Hunter, Haller, and others, in which indigo, and other blue pigments, had been employed, without perceiving any blue colour in the lacteals, or lymphatics; when on one occasion, having kept the animal fasting longer than usual, he observed that these vessels presented a light blue tinge. At first he conceived that the protracted fasting had favoured the absorption of the colouring matter; but having afterwards observed the same appearances, produced after continued abstinence, whether the blue colouring matter had been given or not, and even when a red pigment had been employed, he was led to discover that the blue tinge of the vessels, was nothing else than their natural appearance when filled with a limpid fluid. He placed some of the fluid from these vessels on white paper, and found that it did not present the slightest trace of any blue pigment: he examined it with a microscope, with the same result. Having made this discovery, with regard to the apparent absorption of blue pigments, he looked for similar explanations of those cases, in which other colours appeared to have entered the lymphatic system. Although in his own experiments, he had never seen anything to make him believe, that undigested milk is ever taken up by the lacteals, or lymphatics, he found that a mistake might easily be made, by not disdinguishing milk from the true chyle, and he believes, that in those cases, in which milk is reported to have been found in these vessels, chyle, the product of a previous meal, or of the digestion of the milk itself, had been mistaken for it.

Though, I believe, this last explanation of Franchini, may, in some cases, be correct, there are others to which it does not apply, as I have already observed, in noticing the same objection as urged by Professor Magendie, see p. 327. In respect to the absorption of red coloring matters, Franchini made observations, similar to those above noticed, respecting blue pigments; at first it appeared that some absorption of madder really took place when the animal had been long fasting; but he discovered that the same red, or pink tinge, might be produced when the animal had taken no colouring matter, or even matter of a different hue. The red colour could not, like the light blue, be ascribed to an optical deception; but an explanation of its occurrence was found in the fact, that after long fasting, the fluid in the lymphatics and lacteals, really possesses a reddish colour, which is sometimes proper to itself, and at other times appears to be owing to an accidental admixture of blood.

Fiscinus and Seiler, may be numbered amongst those experimenters, who have most unequivocally proved the absorbing power of the veins, but they do not go so far as Magendie and Franchini, in restricting the office of the lacteals to the absorption of chyle only. They do not consider it as proved, that the lymphatics in other parts of the body are endowed with the power of absorption. Although their numerous and varied experiments appear to have been conducted with great care, they candidly admit, that they do not consider the subject of absorption as by any means settled.

The results of their experiments may be briefly stated as follows:—1. They found indigo in the venous blood, but not in the chyle or lymph. On two occasions, a slight trace of turmeric was detected in the chyle, but they in vain sought for lac, madder, and indigo, in this fluid. They confirm the frequent appearance of a red tinge in the chyle and lymph, when no red colouring matter had been given.

2. The odour of Dippel's empyreumatic animal oil was frequently met with in the venous blood, never in the chyle or lymph. They do not appear to have found camphor in either.

3. They generally found prussiat of potass in the venous blood and urine, but only in one or two occasions, a trace of it in the lymphatic system. In five experiments they found traces of lead in the chyle, when an alkaline solution of that metal had been employed. They appear likewise to have found this metal in the venous blood. Nitrate of silver and arseniate of potass, produced indications of silver

and arsenic, both in the blood and chyle. They support the observations of Wollaston and Marcet, respecting the occasional presence of foreign matters in the urine, when they cannot be detected in any of the other animal fluids. They object to the lymphatics being possessed of a secerning power so delicate, that having absorbed all substances indiscriminately, they transmit some entirely to the veins, whilst they retain others and convey them to the thoracic duct. They think it far more accordant with the laws of affinity, which regulate organized bodies, that vessels containing venous blood should take up certain substances, which are insusceptible of being absorbed by the lymphatics, whose structure is different.

Page 367. The celebrated Rasori, whom I had the pleasure of visiting at Milan in 1824, informed me that he had published, in the Annals of Science and Literature for 1810, some facts relating to absorption and the passage of substances into the urine, but I have not had the advantage of a reference to that paper.

Page 368. *What are the properties which render some substances liable to absorption by the lacteals, and what are those which render others liable to absorption by the veins?* Since the original publication of the Thesis, I have had but little opportunity of inquiring into the accuracy of the principle which I there proposed; but so far as I have been able to do so, either by observation, or by appeal to the results obtained by more recent experimenters, I am led to comfirm rather than recede from the opinion which I there advanced. The following experiment was performed with a view of observing the effect, produced by giving a decidedly alkaline character to the contents of the stomach and intestines, without rendering them of a more irritating quality than could be avoided; and also, of trying a peculiar pigment, which is both an animal substance, and of a colour not liable to be confounded with any of the hues which the lacteals, or lymphatics, or their contents, may present.

The subject of this experiment was a full grown, half-bred, bull bitch, which had pupped about three weeks before. After fasting for about twenty-four hours, she received some calcined magnesia in new milk — this was taken with avidity —The object was to remove all acid from the intestinal canal. This dose was repeated. Sub-carbonate of soda coarsely powdered was next given her at short intervals, inclosed in small pieces of fresh boiled horse-flesh, and some of the same salt was also given to her dissolved in milk. She took both almost without hesitation, though the salt was very imperfectly concealed. Some of the green pigment taken from the placenta of a bitch, was also given with some of the same kind of meat. This pigment was previously examined with test paper and proved to be neutral; it was, however, rather putrid, having been taken from the uterus of a bitch killed more than a week before. This pigment had been found not to change colour by the addition of either alkali or acid. The fragments of meat in which this pigment was given were taken with great hesitation, unless considerable pains were taken in concealing it, or the piece caught in the air and bolted. The administration of these different articles lasted about three-quarters of an hour, and was completed about one P. M.; about ʒij of magn. calc. had been given, and nearly three-quarters of an ounce of soda sub-carb. She afterwards lay quietly. She was killed at about half past one o'clock by a blow on the head. The abdomen was quickly opened. The contents of the stomach and small intestines were decidedly alkaline, as shewn by restoring the blue colour of reddened litmus paper. In dying, the animal passed a copious watery digestion, which appeared also alkaline. The chyme was coloured by the green matter which had been given, and which was not altered by the juices of the stomach. The lacteals, more particularly those from the upper part of the intestinal tube were filled with perfectly white chyle, which possessed decidedly alkaline properties. The thoracic duct was also turgid with the same fluid, about two or three drachms of this were collected perfectly pure. It was quite white, and had the appearance, as to consistence, of very rich milk. In little more than half an hour it coagulated into a thick grumous mass, and acquired a just perceptible shade of pink or lilac. The blood from the mesenteric veins was collected in large quantity; it coagulated in a short time—no streaks of chyle could be observed in it, but an appearance resembling this, was in one instance manifestly produced by the division of a small lacteal vessel, which passed over the vein at the part where it was opened. This blood was alkaline, both when examined as it flowed from the vessel, and when coagulated, and separation had taken place; but it was less so than the chyle—the serum was remarkably limpid and colourless. A little acetic acid diluted with water was thrown into an emptied portion of intestine, included between two ligatures, but vitality was too far gone for this to be considered part of the experiment.

The result of this experiment, which I do not offer as conclusive, is, at least, favourable to the idea that the lacteals are disposed to receive alkaline fluids. The complete rejection of the colouring matter accords with the almost uniform result obtained by all recent experimenters. This part of the subject may be pretty much regarded as settled, since the chyle, of the mammalia at least, examined after a meal of their ordinary food, is universally described as white, or very nearly so, or slightly tinged with red; whilst the food or chyme, from which it was produced, may have presented every variety of colour. Two slight exceptions are indeed related by Fiscinus and Seiler, in which a trace of yellow was perceived after the administration of turmeric, and in another instance, a similar effect was produced by rhubarb. A similar exception is already stated, with respect to rhubarb, in the Thesis; and another in the case of the colouring matter of bile; it would seem, therefore, that some yellow colouring principles are decidedly susceptible of lymphatic absorption. The cause of their forming an exception to the general rule, seems to merit farther inquiry.

Page 371. I have stated, that some neutral salts are taken up, both by lymphatics and veins. I find this observation confirmed by additional authorities. Those which have metallic bases, appear particularly of this number — lead, iron, silver and arsenic, having all been detected, both in the chyle and blood. Prusiate of potass appears almost constantly to enter the veins; yet, by several experimenters, it has been occasionally detected in the chyle, or lymph; but it would appear, that a considerable interval of time must elapse for this to take place. Hence, it may be questioned, whether the salt may not have found its way into the lymphatic system, through some communicating vein, or artery, and not in the ordinary course of absorption.

The absorption of fixed oils, by the lacteals, seems to be fully confirmed by Tiedmann and Gmelin, in their elaborate work on digestion, although their rivals, Leuret and Lassaigne do not appear to have arrived at the same conclusion.

The absorption of acid substances, by the veins, has been proved by various authorities, which have come to my knowledge since the publication of the Thesis.

Dr. Stephens has satisfactorily shown, that it is the common effect of all acids to blacken the blood by admixture with it out of the body, and the observations, both of himself and others, show that the same effect is produced when they are administered to the living animal. The alteration that takes place in the transition from arterial to venous blood, which I could only offer as a probable confirmation of this view, is now demonstrated to be owing to the accession of carbonic acid. Acetic acid has been shewn, by Franchini, to be taken up by the veins, not indeed in a free state, but forming an acetate with the alkali of the blood.

I do not pretend to lay much stress on the following observation, as it was made some time after death; but it tends to show that sulphurated hydrogen, which in some respects is allied to the class of acids, has a stronger tendency to be taken up by the veins, than by the lacteals. A patient in Guy's Hospital, had for a considerable time been labouring under constipation, occasioned by a stricture near the termination of the colon. For three weeks before his death, nothing passed from his bowels. On inspection, the intestines, small and large, were found prodigiously distended with fluid fœcal matter, and abundance of gas; the latter, which was allowed to escape from a small aperture, was proved to be sulphurated hydrogen, not only by its insufferable odour, but by the characteristic blue flame with which it caused a lighted taper to burn. A small quantity of blood taken from the mesenteric veins of this subject, was placed unmixed with any substance upon a piece of clean paper and conveyed to a distance, to be smelt by different persons who had not been present at the inspection, and who immediately recognized the smell of sulphurated hydrogen. A similar trial was made with lymph from the thoracic duct, but no smell was perceived in it.

Page 374. *Respecting the question as to the causes by which absorbed materials enter the vessels.* — I do not know that I can allude more suitably than in this place to the views of Dr. Barry, respecting the circulation through the veins, although their application to the subject of absorption forms only a part of their interest. It was far from being a new idea, that the blood returned towards the heart, in consequence of some other force besides the vis a tergo derived from the impulse given by the left ventricle of the heart; yet, I believe, no one before Dr. Barry, had had the merit of proving, although Huxham had suspected*, that the venous blood was, as it were, pumped towards the chest. Dr. Barry has shewn that the pressure of the atmosphere, is the agent by which this is effected. It is well known, that when an animal extends the capacity of its thorax, by the elevation of its ribs, and the descent of its diaphragm, the atmospheric air penetrates through the larynx and trachea, and fills the pulmonary cells. But it is not only the cavities which are destined to receive air which are filled — the pulmonary veins, and the branches of the cavæ within the chest, are similarly, though not equally dilated. The resistance of the flow of venous blood towards the heart being removed, this blood is sent forward by the pressure of the atmosphere, which becomes a powerful assistant to the remaining influence of the heart.

The following experiment of Dr. Barry, seems to place this beyond a doubt. A spiral glass tube was fixed, with the intervention of a gum catheter, by one extremity to the jugular vein of a horse, whilst the other was immersed in

* See Marshall Hall on the circulation.

a coloured fluid. At each inspiration of the animal, the coloured liquid was observed to rise in the tube whilst it was stationary, or even descended during expiration. This experiment, as well as several others, essentially the same in principle, though variously modified, was frequently repeated by Dr. Barry, in the presence of the members of the commission appointed by the Institute, and many other distinguished physiologists, several of whom have given their public testimony to the uniform and unequivocal results of these experiments. When the hand of the operator was introduced into the abdomen of a living horse, and applied to the vena cava, that vessel was found to be sensibly distended during expiration, and partially to empty itself during inspiration. The Doctor was not satisfied with proving, that fluids were pressed along towards the heart, in veins, and in tubes connected with them, whenever the chest was dilated by inspiration; he showed, that the same effect was produced in tubes communicating with the cavities of the pleuræ and the pericardium. In making the application of this view of the pressure of the atmosphere, as an agent in carrying on the circulation of the blood, Dr. Barry is by no means disposed to limit it to the venous trunks and larger branches. He believes that through their intervention it is felt in the most minute branches, where he believes it becomes important, as the means by which absorption is effected. He represents this fact as practically known to the ancients, who, as Celsus informs us, were in the habit of employing cupping glasses to obviate the effects of poisoned wounds. The Doctor performed may experiments, with a variety of active poisons, on several species of animals, and found that the removal of the pressure of the atmosphere, by means of cupping-glasses, was a certain means of suspending the operation of the poison; whence he concludes: first, that neither sound nor wounded parts of the surface of a living animal can absorb when placed under a vacuum; secondly, that the application of the vacuum by means of a piston cupping-glass, placed over the points of contact of the absorbing surface, and the poison which is in the act of being absorbed, arrests or mitigates the symptoms caused by the poison; thirdly, that as the veins communicate more freely and directly with the chest than the lymphatic system, they must be the more active absorbents. He thinks that the absorbing powers of the different tissues are in proportion; first, to the pressure to which their veins are exposed; secondly, to the freedom of communication with the thoracic cavities; thirdly, to the permeability of the mouths and coats of the veins; and, lastly, to the number of the veins. Although I have no hesitation in admitting, that Dr. Barry has performed an important service to physiology, in demonstrating the part which atmospheric pressure performs in assisting in the circulation of the blood; yet, I must confess a doubt, as to its having any material influence in promoting absorption, except indirectly through the medium of the circulation. His experiments, connected with this part of the subject, are neither questionable nor unimportant, but they admit of an explanation different from that which he has given. The pressure of the edge of a cupping-glass, under which a partial vacuum has been produced, is not only an effectual interruption to the circulation in the part, but, likewise, cuts it off from nervous influence. That it is the production of this pressure, rather than the removal of that of the atmosphere, is almost demonstrated by the experiments of the late Thomas Ellerby, by which he shewed, that a cupping-glass, firmly pressed upon a part without removing the atmospheric pressure, afforded the same impediment as the exhausted glasses used by Dr. Barry. Even a simple ring, or open cylinder, produced the same effect. Experiments, nearly similar to those of T. R. Ellerby, have since been performed in Philadelphia, by Dr. Pennock, but with this exception, that he employed the pressure of a solid body over the spot to which the poison was applied. He found that the action of the poison was suspended, so long as the pressure was continued.

Imbibition, or the soaking of fluids through the inert pores of the coats of vessels, and other tissues, has been considered by Magendie, as constituting the essence of absorption. I have briefly noticed in my Thesis, an objection to which this idea appeared liable. The subject has since been carefully investigated by Fodera; and many physiologists, amongst whom may be mentioned Panizza, are supporters of this doctrine.

It seems, therefore, necessary to offer a brief account of it, as well as of the observations upon which it is founded. In this view of the subject, absorption and exhalation are regarded as imbibition and transudation. That is to say, merely the effect of capillarity. It is shewn by an experiment of Magendie's, repeated and confirmed by Fodera, that if a portion of vein, or artery, be detached from the neighbouring parts, and stripped of its cellular membrane, but suffered to carry on the circulation, and some active poison be applied to the surface, the specific effect of the poison will be produced, although the utmost care may have been exercised to prevent the application of the poison to any other part. The fact of the imbibition of the poison, is not inferred from its effect alone, but from its actual presence in the interior of the vessel. The converse of this experiment was tried by including the denuded portion of artery between two ligatures, and filling it with the poison which produced its effect in this instance also. A portion of the intestine of a living rabbit was included between ligatures; the mesentery was likewise tied, that there might be no vascular communication between the animal and the portion of the intestine so included. A spirituous solution of extract of nux vomica was then introduced into the portion of intestine, which was then returned to the abdomen. Poisoning took placé as before. This experiment was repeated in a more conclusive manner, by introducing a convolution of the intestine of one animal, containing poison, into the abdomen of another animal. This second animal died from the poison, although there could be neither nervous nor vascular communication with the cavity in which it was lodged. He substituted sulphuretted hydrogen for the solution of nux vomica, and found its effects produced on the animal—proving, that gases, as well as liquids, are capable of penetrating through the animal tissues. These results were confirmed by numerous experiments upon recently killed animals, until Fodera, and his friends, were fully convinced, that this kind of imbibition really took place. He placed different fluids in different cavities of the body; as for example, a solution of prussiate of potass in the thorax, and a solution of sulphate of iron in the abdomen: it was not long before a blue colour was observed in the chest and abdomen, and in different parts of the body, showing, that not only had both fluids transuded, but what appeared more remarkable, that transudation and imbibition were taking place simultaneously, and through the same membranes. This curious fact was put to the test in several experiments, by which it appeared to be indisputably confirmed: at least I see no reason to call it in question in the cases which Fodera has related. There is, however, one objection to the application of this principle, which, though not overlooked by Fodera himself, seems to be possessed of more importance than he has ascribed to it. This objection is the same that was long since urged against experiments of the same nature, as we may learn from the quotation from Celsus already cited :—"Non quicquam esse stultius quam quale quidque vivo homine est, tale existimare esse moriente, imo jam mortuo."

The experiments of Fodera being, in most instances, tried upon dead, or dying animals, or when living animals were employed, with substances so completely foreign to the animal economy, that we may fairly suspect the vitality of the parts to have been somewhat impaired; may very reasonably be supposed to have allowed the inorganic function of transudation and imbibition to have been in operation, to an extent which the unimpaired powers of life could not have tolerated. The following considerations lead me to believe, that this suspicion is correct—if there were a constant and reciprocal transudation and imbibition going forward throughout the tissues of the living body, the result would necessarily be the presence of every principle in every part of the body, and a far greater uniformity in the chemical composition of all the fluids and solids, than actually exists.

It is well known that those animals which secrete the most deadly poisons, are by no means exempt from the fatal effects which follow the introduction of the poison into a wound. For example, the sting of the scorpion is as fatal to his own, as to any other species; yet, each individual carries his reservoir of poison about with him with perfect impunity, although it is only cut off from the rest of his system by a membranous sac. The existence of many partial dropsies, may be urged as another illustration of the limitation which life sets to transudation and imbibition. It is well known that during life, the large intestines are often distended with sulphuretted hydrogen, without the neighbouring parts appearing to suffer from its proximity; yet, after death, a short time is sufficient for these tissues to acquire, to a considerable depth, the peculiar leaden hue which it imparts. The yellow tinge, which the parts in the neighbourhood of the gall bladder commonly receive after death, has long been pointed out as a striking example of the difference between dead and living matter, with respect to imbibition. The investigation of this subject, requires the application of the principle to which I have more than once referred. The tendency to transudation and imbibition, doubtless exists in the living, as well as in the dead structures; but in the former, it is controuled by a mysterious power which we can only judge of by its effects, and which we cannot better investigate, than by examining the mode in which it resists other powers with which we are better acquainted. The experiments of electricians have so fully confirmed the fact, that electricity, or galvanism, has a powerful influence on the internal movement of fluids, and their transmission through almost impervious substances, that it is by no means surprising that this agent has been referred to; yet, it is very important that we should remember, that we may have only similarity, and not identity of action, and consequently, only employ the facts as assisting our analogical reasoning. The investigations of Dutrochet, a brief account of which is given in a subsequent part of this Appendix, appear to point to a principle, which, whether electricity be concerned or not, is in all probability most intimately connected with many vital phænomena, but more especially with absorption, nutrition, and secretion. These researches not only prove the strong tendency which some fluids have to pass through porous bodies — they also confirm the observations of Fodera, concerning the simultaneous existence of opposite movements, and what appears to be of no small importance, they show how much these movements of imbibition, and transudation, are influenced by the nature of the porous body, and by that of the fluids in contact with it. Instead, therefore, of regarding imbibition, and transudation, as phænomena that do not take place in organized beings until life is extinct, we may regard them, when variously modified, as concerned in most of the essential functions of life.

The difficulty of admitting a power of selection possessed by different sets of vessels is very much done away with, and our ideas respecting the molicular changes attendant on nutrition, secretion, and absorption simplified; when, instead of referring them to supposed and imaginary mouths, or extremities of vessels, we may regard them as taking place through the sides of the most minute vessels throughout their course. The difficulty respecting the removal of solid parts, is in particular very much removed. I have repeated in my Thesis, with reference to this point, the remark of Bichat; that entire decomposition takes place prior to absorption, in consequence of which, they may pass into the circulation in a fluid form. May we not conceive the solid parts of the body, such for example, as the most minute fibrillæ of muscles, nerves, cellular membrane, &c., which the most powerful microscopes can place within the limits of our vision, and the more amorphous elements of some of what are called parenchymatous structures, constantly, as it were, washed by the fluids just transuding from, or about to enter the minute vessels which pervade them, and giving up to them those elements which are to be thrown off from the system, and receiving others which are to be deposited in their place. The continual succession of new parts will therefore depend, as I have already suggested, not only on the joint operation of depositing and absorbing vessels, but also on the nature of those particles which they are destined to reinforce or succeed. See p. 377. I am aware here that I have been tempted into a mere speculation, and I ask the reader's excuse for intruding it on his attention. Whatever be the precise mode in which the vessels influence the fluids they contain, or by which they are surrounded, and whatever be the changes taking place in those minute structures, to the support of which these vessels and their fluids are subservient, it is manifest that they are liable to various influences and changes, very distinct from anything which takes place in materials not possessed of life. Physiologists, almost by common consent, point to the nervous system as the medium through which this mysterious influence is conveyed. Notwithstanding the very important advances which have been made in the physiology of the nervous system, it is still the department in which most remains to be done. Justice seems to require, that I should not omit this opportunity of mentioning the laborious, but neglected and almost unknown work of Bellingeri, who appears in part, at least, to have anticipated our ingenious and meritorious countryman, Sir C. Bell, in the distinction which he has made between the respective functions of particular nerves. He plainly distinguished that portion of the fifth pair which does not belong to the semilunar ganglion from that which does so, and pointed out the former as a nerve of motion, as Palletta had previously done. He also described the seventh pair as supplying motive influence to the same parts which receive the ramifications of those branches of the fifth, which proceed from the semilunar ganglion; but as he makes the seventh pair subservient to the functions of animal life, he did not separate the motive from the sentient nerves.

See the Inaugural Dissertation of C. F. J. Bellingeri, published at Turin in 1818.

For the knowledge and use of this extraordinary thesis, I am indebted to my friend S. D. Broughton.

With reference to this interesting subject, I cannot forbear to mention another circumstance, to which I can offer my personal testimony, and which, I trust, will contribute to allay the jealousy that has been excited in the minds of some.—I passed the winter of 1821-22, in Paris, and was frequently present at the meetings of the Institute. On one of these occasions, I had the pleasure of hearing some of the experiments and inductions of our distinguished countryman, related by Professor Magendie, who passed a just encomium on their author, and admitted the importance of the views which they opened. It can hardly excite surprise, that when this act of justice had been performed, the distinguished physiologist of Paris should himself enter the promising field that was laid open to him, and that his skilful hands should have collected some of the fruits, without abstracting, as has been too gratuitously suspected, from the labours of his predecessor.

Although most important steps have been taken in the physiology of the nervous system, not only in the establishment of the distinction between the nerves of sensation, and the nerves of motion, but also in the improvement of our knowledge of the anatomy of the brain, and the approach to the determination of the functions of several of its parts, which we owe to Dr. Foville, Flourens, Desmoulins, Magendie, Mayo, and others; yet, there are several points respecting which, we still continue in almost perfect ignorance. This is particularly the case with the part which the nerves are supposed to perform in secretion, and other vital phænomena of a chemical character. This influence, whether real or merely imaginary, we may designate, for the sake of convenience, by the term, chemical influence of the nerves. Many distinguished philosophers and physiologists have adopted the idea, that this influence is identical with galvanism. Dr. Young says, "We may imagine that at the subdivision of a minute artery, a nervous filament pierces it on one side, and affords a pole positively electrical, and another opposite filament, a negative pole."

Rolando, who was disposed to dispute the palm of merit with Sir Charles Bell and Magendie, as a successful investigator of the nervous system, attached considerable importance to his having represented the cerebellum as a sort of electromoter, and formed a theory for the explanation of the nervous influences, upon the basis of electricity. Dr. Wilson Philip has particularly distinguished himself, as a staunch advocate for the electric character of the nervous agency. Nevertheless, if we except the experiments which have been adduced by the last mentioned author, we shall find that we have little more than a shrewd suspicion, sanctioned by high authority, for the adoption of the theory. The observations of Dr. Philip, as well as those of Prevost and Dumas, who have trodden in nearly the same path, have been called in question, or attempted to be variously explained away. Hence, it may not be amiss briefly to examine what is actually known, and distinct from conjecture, in connexion with the subject. It is by no means easy, effectually to separate even a part of the body so completely from the influence of the nervous system, as to render the experiment conclusive in this respect, without at the same time so completely interfering with the circulation, as entirely to vitiate the experiment by its complexity. A considerable portion of the body and limbs may be completely paralyzed, as to sensation and motion, and considerable wasting may take place, in consequence of the want of activity of the muscles, and an inferior supply of blood. The process of nutrition is nevertheless carried on, and a certain degree of irritability remains, as shown by the paralyzed part being still susceptible of the influence of various agents—blisters will rise, and eruptions and ulcers may form, and also heal. Moreover, when this kind of paralysis has been most completely produced by the division of the spinal marrow, or of nerves near their origin, the effects of active poisons applied to wounds in the part are not intercepted, although it has been shown by the experiments of Dr. Addison and J. Morgan, already noticed, that the operation of the poison, is not to be ascribed to its introduction into the circulation. To explain this difficulty, recourse has been had to branches of nerves derived from the sympathetic system, and supposed to accompany the branches of the vascular system. Still, however, nothing is known of these branches, and we have no direct experiments to throw any certain light on their functions. The observation respecting the influence of these nerves on the function of absorption, founded on the experiments of John Hunter, and others, offered at p. 371 of the Thesis, may, perhaps, be regarded as an approach to an experimental inquiry into the subject. The intestines being solely supplied by nerves of the sympathetic system, and the facilities which they offer for the division or the ligature of these nerves, render them rather peculiarly adapted for this investigation. Though no positive conclusions can be drawn without the assistance of a greater number and variety of experiments, yet those which have been adverted to, seem, at least, to indicate the probability, that the vessels, when unassisted by the nervous influence, lose much of their power of selection. Numerous experiments have been made upon the division of the eighth pair of nerves, with the hope of obtaining some light on the subject of nervous influence. But although these nerves offer some manifest advantages for experiment, from their size and distinctness, and the readiness with which they can be divided, with comparatively little injury to other parts, and the character and importance of the functions of those parts to which they are distributed, yet it appears to me, that they are liable to some decided objections. It is evident that they have so complex an origin, that it is impossible to say, whether, at the point at which division is to be made, the fibrillæ belong more to the motive, or the sentient nerves. The gangliform enlargement, which is sometimes very evident at the upper part, would seem to place them amongst the sentient nerves, and favour the suspicion as to their function, noticed at p. 350. On the other hand, this nerve did not appear in the experiments of my friend, S. D. Broughton, to be possessed of sensibility; and the experiments of Drs. Wilson Philip and Hastings, and those of Dr. Milne Edwards, and others, exhibit this nerve as acting by virtue of an influence directed from the brain to the branches, and in this respect more allied to the motive, than to the sentient nerves. Dr. Holland, although an opponent to the views of Dr. Philip, at least, agrees with him in this respect, and regards this nerve as communicating an influence to the lungs, by which the circulation of the blood through them is promoted. The disturbance of respiration, and digestion, accompanied by an accumulation of mucus in the lungs, and the alteration of the secretions of the stomach, which succeed the division of the eighth pair of nerves, he regards as secondary to the interruption of the circulation through the lungs, which is a direct effect of the division. The destruction of different parts of the brain, and spinal marrow, does not appear to have thrown any certain light on the influence which the nerves exert upon the circulation through the capillaries, in which their chemical agency is chiefly exerted. It appears evident, that this destruction produces a disturbance in this part of the circulation; but the experiments of Dr. Marshall Hall have shewn, that the same effect is produced by the sudden destruction of other parts; as for example, the stomach. This interesting fact, as well as the operation of poisons inserted into a wound, seems to point at a kind of sympathy, or consent of parts, with which we are at present wholly unacquainted.

Dr. Foville has recently advanced some highly interesting observations and speculations, concerning the joint agency of the extreme branches of vessels and nerves, which have been greatly admired by some of the ablest physiologists in France, and which will, I hope, before long be laid before the public.

Page 378. That the production of black matter is promoted by the retardation, or stasis of blood in the vessels, as suggested in the Thesis, appears to be strongly confirmed by the effects of inflammation. It not unfrequently leaves the small vessels of the part affected, distended with blood, after the activity of the circulation through them has ceased; this is strikingly the case with the mucous membrane of the alimentary canal, in which the production of black matter from this cause is by no means unfrequent. In some instances we may notice the different shades of colour which attend the transition from red or purple to black. This colour, and various shades connected with it, have been regarded by some distinguished foreign pathologists as evidences of the existence of chronic inflammation, I would rather regard them as proofs that inflammation had subsided. Though it is not my intention to pursue this pathological subject further in this volume, I wish to correct an error into which I have been led, in representing this black pigment as a frequent occurrence in the spleen. Though I made this statement in consequence of my own personal observation in dissecting-rooms, the numerous opportunities which I have since had of the inspection of recent subjects, have convinced me that, except to a limited and partial extent, the deposition of this pigment in the spleen, is by no means common. Although I retain unchanged the opinion, that the black matter of the lungs is generally produced in the system itself by the alteration of the blood, in opposition to the view of Dr. Pierson, who ascribed it to inhaled carbonaceous matter, I cannot omit to notice a fact which has been since observed by my friend, Dr. Gregory of Edinburgh. He found the lungs of a patient, who had been long engaged as a coal-miner, unusually loaded with black matter. A specimen of this matter was subjected to a careful analysis by Dr. Christison, the result of which rendered it almost certain, that the black matter was in part, at least, composed of minute particles of coal.

Page 380. That the circulation of the blood is promoted by the way in which it is disposed of in the extreme vessels, is a view which I am still disposed to entertain. It seems to receive the support of analogy from the observations of Dutrochet, respecting the motion of the sap in the roots, and branches of vegetables.

OF THE PHÆNOMENA TO WHICH THE NAMES ENDOSMOSIS AND EXOSMOSIS HAVE BEEN GIVEN BY H. DUTROCHET.

Since the publication of some of the preceding views respecting transudation and inbibition by Magendie and Fodera, the subject has been very carefully investigated by Dutrochet, who, in examining the transmission of different fluids through different kinds of thin and slightly porous septa, has thrown much light on the phænomena attending this transmission, and exhibited what appears to him to be a new physical force distinct from ordinary capillary attraction, electric agency, and hydrostatic pressure ; to the movements resulting from this force, he gives the names of endosmosis or exosmosis, according to the direction.

Whether this force be altogether distinct and *sui generis*, or a modification of some principle, with which we are already partially acquainted, it seems to operate very generally throughout living and dead, and organic as well as inorganic matter, and as an agent intimately connected with some vital phænomena, it must not be wholly passed over in this volume. If we take a membranous sac or cavity, as for example, the cœcum of a fowl, or the air-bladder of a fish, and having put a small quantity of fresh milk into it, and secured the mouth by a ligature, we shall find on immersing this sac in water, that in the course of a few hours it will become quite full, and eventually turgid. This turgidity is not permanent, and after a few hours more have elapsed, the sac will again be flaccid. If the sac be now opened, it will be found to contain curdled and putrid milk ; if the sac be cleansed from this offensive substance, and again partially filled with milk and immersed in water, we shall find a repetition of the phænomena, but the sac will neither become so turgid nor so long remain full as in the first instance; this may be repeated several times, but with diminished effect. It does not make any sensible difference to the experiment, whether the sac be inverted or not, or whether the mucous or peritoneal coat be removed. If instead of milk some other fluid be employed, similar phænomena may be observed, but by no means in the same degree in all; in fact, very striking differences may be observed, depending on the nature of the fluids within and without the sac, as well as upon the texture through which they have to pass. All these points have been carefully investigated by Dutrochet. In this enquiry, instead of a fowl's cœcum, or fish's bladder, he employed an instrument, to which he has given the name of an endosmometer, which it will be necessary briefly to describe. It consists of a cylindrical tube of glass, to which is fitted at one extremity, a sort of moveable funnel, or cupola, having such a rim, or lip, as will admit of a firm attachment of the membrane, or other material, by which this aperture is to be closed, and though the transmission of endosmosis and exosmosis is to be examined, the other extremity of the tube is left open, and a graduated scale is applied to the tube itself: the fluid of which the power of producing endosmosis is to be tried, is placed in the reservoir formed by the closed funnel, after which the tube is applied and the funnel immersed in water. If endosmosis take place, the fluid will rise in the tube above the level of the water surrounding the reservoir, and the amount of this elevation may be read off from the graduated scale. In this way Dutrochet discovered a very considerable difference in the power of different fluids in inducing endosmosis, and it appeared that in general this power was greater in dense than in thin fluids. Solutions of sugar and of gum arabic possessed this power in a very remarkable degree; the former, when of the strength of one part sugar to three of water, producing an elevation in a column of quicksilver of 258 millimeters, or 45 inches 9 lines, French; and the latter, when of the same strength, an elevation of more than 28 inches. Saline and alkaline solutions have also considerable endosmodic power. Dutrochet, at one time, conceived that the acids were unfriendly to endosmosis, or rather that they produced exosmosis, and he was in consequence induced to attribute the phænomena of this kind of transmission to electric, or galvanic agency; and this opinion seemed to be strengthened by the results of various experiments, which he instituted to endeavour to ascertain the fact. Further experiments led him to abandon the idea of electric agency, and he discovered moreover, that most of the acids really possess some power of producing endosmosis; sulphuric acid however continued to form a marked exception.

Dutrochet likewise tried many of the animal fluids besides milk, and he found that they also possessed considerable energy in producing endosmosis, until, as in the cases of the milk in the preceding experiments, they had become putrescent. In this state he found that all the animal fluids were opposed to endosmosis. By varying his experiments and employing other fluids free from animal matter, but containing sulphurated hydrogen, as for example, the hydro-sulphuret of ammonia, he discovered that this principle, like sulphuric acid, is decidedly opposed to endosmosis; but he states, that we are at present completely ignorant of the mode in which these two principles, the only known sedatives of endosmosis, act. In observing the differences in the phænomena of endosmosis presented by different fluids, there are two points to which Dutrochet turns his attention, viz., the force and the rapidity of endosmosis, both of which may be made the subject of actual measurement. The rapidity he appreciated by the number of millemeters through which the fluid in the tube ascended in successive hours. In order to estimate the strength, he employs an endosmometer of a form somewhat different from that before described. It is constituted on the same principle as the apparatus by which Hales estimated the force exerted in raising the sap of the vine. The reservoir of this endosmometer is similar to that used in the former experiments; but the tube is bent so as to form a syphon with its convexity upwards; the descending leg of this syphon fits one of the legs of another, which has its convexity downwards. Mercury is put into this second syphon, and the force of endosmosis being exerted on the quicksilver in one leg, produces a corresponding elevation on the mercury in the other, when it may be read off on a graduated scale. By trying the rapidity and force of endosmosis on different fluids, by the use of these instruments, Dutrochet found, as might have been anticipated, that those fluids which acted with the greatest rapidity, likewise acted with the greatest force; and he also found, that the power of endosmosis, in these two respects, increased with the specific gravity of the fluids; provided that we estimate this only by its excess above the specific gravity of water.

The following are some of the most remarkable differences depending on the nature of the substance through which the transmission takes place. It may be seen from the experiments performed on milk, and the intestines of a fowl, that at each successive repetition of the experiment with fresh milk, but the same portion of intestine, the amount of endosmosis continued progressively to decrease: this appeared to depend upon decomposition taking place in the membranes themselves, by which they became infiltrated with a fluid, containing one of the principles which has been already remarked to be negative of endosmosis. Conducted by this fact, Dutrochet was led to the observation, that the transmission of fluid through a septum, by the influence of endosmosis, was very materially influenced by the fluid with which it happened to be pervaded. When the septum was dry, and the pores consequently filled with air, endosmosis was obstructed. This obstruction was removed as soon as the septum was saturated with water, and it became greatly increased, when, instead of water, a fluid favourable to endosmosis, such as a solution of sugar, or gum, occupied the pores of the septum. Various materials, besides the membranous parts of animals and vegetables, were employed to close the funnel of the endosmometer, such as very thin plates of sand-stone, plaster of Paris, lime-stone, burnt slate, and the biscuit of earthenware; with some of these substances, the endosmosis was carried on with considerable energy, whilst with others, it seemed totally inactive: this evidently did not depend on the mere porosity of the material employed, and satisfactorily showed that the phænomena of endosmosis must not be confounded with capillary attraction; very little, if any, endosmosis was observed to take place through plates of sand-stone, whether the most porous, or the least porous were employed, but the presence of a little ferruginous matter in one of the specimens was observed to favour it. To ascertain, that in instances in which endosmosis took place, there did not exist a physical impediment to the transmission of fluids, Dutrochet tried them both with the pressure of a column of fluid, and with the galvanic current, and in these cases he observed transmission to take place. The septa of lime-stone, though sufficiently porous to allow the passage of fluids and to exert a capillary attraction upon them, were found to be extremely unfriendly to endosmosis; but those of burnt slate, and baked clays, though but little promising, were observed to exhibit, endosmosis, strongly.

In some of these experiments it was remarked, that after the endosmosis had been carried on with considerable activity in several trials with the same septum, a diminution in the transmission took place, indicating that some impediment had interposed itself; this was discovered to proceed from an accumulation of the gum, sugar, or other principle in solution upon the surface of the plate or septum.

As respects the fluid, in which the reservoir of the endosmometer is immersed, it would seem, that there is no fluid more favourable to endosmosis, and at the same time so convenient for experiment, as pure water. The examination of this fluid, after it had been for some time employed, afforded convincing evidence of a very curious fact. Notwithstanding the copious and forcible transmission of water through the septum, occasioning in some instances an elevation of several inches in the tube, there is likewise, at the same time, a transmission of fluid in an opposite direction; thus, if a solution of muriate of soda be employed in the endosmometer, it will not be long before traces of this salt will be found in the water surrounding the reservoir of the instrument.

Dutrochet has been remarkably ingenious and happy in the application which he has made of the principle of endosmosis and exosmosis, to the explanation of many curious points of vegetable physiology. The cause of the motion of the sap, which has led to so much interesting investigation and controversy, he attributes, in a great measure, to a structure situated near the junction of the root and the stalk, in the minute cells of which, a powerful endosmosis is exerted. The peculiar firmness which forms so striking a distinctive character between a fresh and a living leaf, or other vegetable tissue, and the permanent flaccidity of that which is dead, he designates by the term turgid state, and refers it to endosmosis filling the cells of the vegetable structure, and induced by the nature of the fluid in these cells. Hence, a faded, but still living plant, is rapidly restored by immersion in water, and this experiment may be repeated until the death of the plant, has allowed such an alteration of the fluid remaining in the cells, that endosmosis is no longer provoked. Even when this condition has been arrived at, Dutrochet has succeeded in procuring an artificial turgid state, by causing the cells to imbibe solutions favourable to endosmosis. The forcible ejection of the juice from the fruit of the elaterium is referred, by Dutrochet, to the progressive operation of endosmosis, in conjunction with a peculiar arrangement of the cells, of which the substance of the fruit is composed. Many curious speculations have been formed respecting the cause which determines the direction of the roots and the stalks of plants. That which has been offered by Dutrochet, is, perhaps, the happiest application of endosmosis, which he has yet pointed out. From a difference in the arrangement of the cellular structure in the stalks and the roots, it seems to follow as an inevitable consequence, that the turgid state induced by endosmosis, will cause the first to form a curl with its concavity upwards, and the latter with its concavity downwards, until the one has acquired an ascending, and the other a descending direction.

Dutrochet thinks, that the same principle of motion may be applied to the explanation of the curious phænomena presented by the *balsamina impatiens*, the *hædysarum girans*, and the *mimosa pudica ;* but in the movements of this last especially, we are at fault for another yet unexplained power by which the movements are called into play, even supposing that endosmosis is, in some way, immediately concerned in producing the temporary turgidity of the cellular organs, placed at the angles formed by the moving joints of the plant. But if, as Dutrochet very candidly admits, something be wanted in the explanation of the movements of the sensitive plant, this difficulty is much more strongly felt in the movements performed by animal life. Their nature and rapidity seem completely at variance with any explanation founded on endosmosis; they are opposed by the experiments of Blanc, Burzoletti and Prevost, and Dumas, upon muscles in action, and by the most careful and minute microscopic observation of the elementary fibre, in which nothing approaching to the vesicular, or cellular structure, imagined by Dutrochet, can be discovered.

I am far from believing, that endosmosis and exosmosis, are not actively concerned in many of the phænomena of animal life; but in applying them, we must be extremely careful not to ascribe too much to them, to the neglect of other forces, which nature employs in living animals to restrain and modify their influence. I have had occasion to notice a similar error into which, I apprehend, both Magendie and Fodera have been led, with reference to the phænomena of transudation and imbibition. It is doubtless extremely difficult to decide, how much may correctly be attributed to any of the different forces which take a part in the various and complicated operations of animal life, and it is only as a conjecture, that I notice the following instances, in which it seems probable, that the principle pointed out by Dutrochet may be concerned. When we consider the great increase in the quantity of urine, which takes place in conjunction with the production of sugar, in those who are labouring under diabetes, and consider the strong power of endosmosis, which Dutrochet ascribes to sugar, may we not attribute the accumulation of a part of the superabundant fluid to this cause, and even suspect, that not in the kidneys alone, but throughout the urinary apparatus, endosmosis may add to the bulk of secreted urine? This idea, as respects the bladder at least, has, prior to the publication of Dutrochet's views, had the sanction of some distinguished physiologists, who have contended, that even in the healthy state, much of the fluid passed from the bladder is so collected.

Some proof that the excessive quantity of the secreted urine is influenced by the presence of sugar, is afforded by the fact, that if by strict exclusion of vegetable matter, the production of sugar may be suspended for a time, the quantity of fluid evacuated will exhibit a corresponding diminution, notwithstanding that the disposition to the complaint remains unabated. Another instance in which the operation of endosmosis will, perhaps, be more readily admitted, since the structures concerned are adventitious, and, consequently, less perfectly organized, is furnished by the enormous cysts, which are not unfrequently formed in or near to the ovaries, constituting what is generally called, ovarian dropsy. As these cysts are furnished with no special glandular apparatus, it is not unreasonable to refer the production of the fluid which they contain, to the whole internal surface; although, for reasons which I have endeavoured to explain in a short essay on the anatomical character of some adventitious structures, and which, I need not here repeat, it is very probable that the production of fluid is not equally rapid from every part. The short space of time in which some of the largest of these sacs are refilled, after the operation of paracentesis, is no less remarkable than the character of the fluid so produced. It is well known that these fluids are copiously charged with a muco-albuminous substance, which, like sugar, must, on Dutrochet's principles, be greatly favourable to endosmosis.

If endosmosis be admitted to take a part in those functions, in which there is a high degree of life and organization, it must be admitted, *a fortiori,* in other instances, in which organization is greatly inferior, and life nearly or quite extinct. Hence, I apprehend, we may be allowed to have recourse to its assistance to explain the changes which take place in structures which appear to be devoid of organization, or which having lost the life and organization which they once possessed, are yet still retained in the system. The copious impregnation of such substances with earthy salts, constituting what are termed petrefactions, examples of which are met with in old tubercles of the lungs, in the mesenteric glands, in the coats of arteries, and in various other situations, may, I believe, be referred to this principle.

ON THE MICROSCOPIC CHARACTERS OF SOME OF THE ANIMAL FLUIDS AND TISSUES. BY J. J. LISTER AND DR. HODGKIN.

THE researches of Prevost and Dumas, respecting the microscopic appearances of the blood, are alluded to in the work of Dr. Edwards. The very superior compound achromatic microscopes of my friend J. J. Lister, who has devoted much of his leisure time to the cultivation of this branch of optics, have enabled him and myself to correct some of the illusions into which the indefatigable physiologists of Geneva, to whom I have alluded, were unwittingly led by the imperfection of their instruments. Hence, it appears, that there would be an obvious advantage in reprinting in this work, with some few additions, the observations which have already been published in the Annals of Philosophy, and in the Catalogue to the Anatomical Museum of Guy's Hospital.

Any approaches towards a more accurate knowledge of the intimate structure of organized beings, may reasonably be looked to as collateral aids to our acquaintance with the influence of physical agents on life.

Very soon after the invention of the microscope, it was ascertained, that the blood, instead of being homogeneous, consisted of a fluid with coloured particles suspended in it. This discovery is attributed to Malpighi; but it does not appear that his inquiries into this subject were pushed to any great extent.

Observations similar in their result, but far more numerous and minute, were made nearly at the same time by Lewenhoeck, apparently without any connexion with those of Malpighi.

This indefatigable Micographer, describes the coloured particles of the blood as circular, or spherical, while at rest, but elliptical when in motion. Those of fish, he states to be flat and elliptical; and he remarked, that in the fluids of some insects, the particles were of a green colour. He believed rather than demonstrated, that each globule of the blood was composed of six subordinate globules.

For a considerable time the opinions of Lewenhoeck were generally received, and physiologists and micographers, amongst whom we may notice Fontana, taught that the particles of the blood were globular.

Haller in one part of his works concurs in this opinion, but doubts their form being susceptible of change from motion. In another place, he describes the red particles of the blood as flattened, and compares them to lentils.

Senac has taken the same view of them.

De la Torre, who employed in his observations single globules of glass possessed of very high power, but defective in point of clearness, recognized the flattened form of the particles, but mistaking the shaded spot in their centres for a perforation, he described them as rings. He believed them to be jointed, and to break regularly into seven pieces.

De la Torre was soon followed by Hewson, who, together with improved instruments, brought a large stock of ingenuity and perseverance into the inquiry.

To obviate the confused view, which the large proportion of the particles in undiluted blood is very apt to produce, he introduced the plan of mixing it with fresh serum, being well aware of the change of form produced by the addition of water.

He states that if, after this change is effected, a drop or two of a neutral solution be added before the bursting of the vesicles, the flat figure will be restored.

He found no central particles in the blood of the splenic vein.

Without entering into the numerous and in many respects accurate observations of Hewson, since we shall have occasion to refer to them in a subsequent part of this paper, it will be sufficient to recall to memory the results which he drew, as to the nature and figure of the particles.

He satisfactorily shewed that they are not globular, but flattened, as well when circulating in the vessels of the living animal, as when drawn from the body; and he also proved the fallacy of De la Torre's views with respect to a central perforation.

He believed the dark central spot to be a solid particle, contained in a flat red vesicle, whose middle only it occupied, and whose edges were hollow and either empty, or filled with a subtile fluid. He observed the flattened vesicles to become spherical, by the addition of water, and at the same time to be contracted in their diameters.

He states, that the middle particle may be seen to fall from side to side in the hollow vesicle like a pea in a bladder, or sometimes to stick to one part of the vesicle. The middle particles are less easily soluble than the flat vesicles which contain them, and a little time after the proper quantity of water being added they disappear, leaving the middle particles which appear to be very small.

He states them to be larger in the immature young, than in the perfect animal.

He likewise observed them to be of the same form, when circulating in the vessels during life, as when escaped from them, and denies that they alter from resistance during circulation.

This view of the structure of the particles, was founded on the examination of the blood of the skate of the larger size, and elongated form, of which he was perfectly aware. He admits, that it is more difficult to gain a sight of these appearances in the blood of man, but tells us that he had, notwithstanding, distinctly done so with the help of bright and clear day-light.

Falconar, by whom Hewson's Observations were repeated and published; and also Dr. Wells, entertained similar views respecting the figure of the particles of the blood.

Cavallo believed, that they consisted of double spheres.

The concise but pertinent observations of Dr. Young, claim particular attention and respect. The particles of the blood of the skate, from their superior size, are considered by him, as they had been by Hewson, as the best suited for the commencement of the investigation.

He describes them as exhibiting an oval and flattened form, and containing a nucleus generally round, but sometimes a little irregular, which occupies a nearly permanent position in the centre of the particle. It often remains distinctly visible, while the oval part is scarcely perceptible, and as the portion of blood dries, becomes evidently prominent. This nucleus is about the size of an entire particle of the human blood, the whole oval being about twice as wide, and not quite three times as long. The nucleus is very transparent, and forms a distinct image of any large object which intercepts a part of the light by which it is seen, but exhibits no inequalities of light and shade that could lead to any mistake respecting its form.

Having given these remarks respecting the particles in the blood of the skate, he proceeds to those of human blood, of which he says, that if placed under similar circumstances in the field of the microscope, near the confine of light and shade; although they are little if at all less transparent, one immediately sees on the disk an annular shade, which is most marked on the side of the centre on which the marginal part appears the brightest, and consequently, indicates a depression in the centre; but the Doctor was not quite decided, whether this apparent depression might not depend on some internal variation in the respective density of the particle. He thought the axes about $\frac{1}{3}$ or $\frac{1}{4}$ of the diameters, but the particles never appeared to him to be "as flat as a guinea." He never observed a prominence on the outline of the particles of human blood, but he remarked, that when they had been kept for some time in water, and a little solution of salt was added, their form and structure are more easily examined, and that they appear to resemble a soft substance with a denser nucleus, not altogether unlike the crystalline lens with the vitreous humour as seen from behind; but with respect to a central particle detached within a vesicle, like a pea in a bladder, he is satisfied that Hewson was completely mistaken. The colouring matter, according to the Doctor's view, does not appear to be a mere superficial layer, but imbues the substance of the particle from which water extracts it, and occasions such a loss of specific gravity that they remain suspended instead of sinking in that fluid. In this state they easily escape observation, which circumstance, together with their passage through filtering paper, has led to the monstrous assertion that they are soluble in water. When they have been long kept in water, and even after putrefaction has taken place, they do not appear to become constituent parts of an homogeneous fluid.

In the memoirs of Sir E. Home and Bauer relating to the subject before us, we find the globular figure of the blood again maintained.

The colouring matter appeared to them not to be contained in the particles, but rather to envelop them. They describe it as separating very readily, and flowing from all parts at the same instant. "To examine them," say they, "in their coloured state, a very small quantity of blood must be examined at once, and this must be spread as thinly as possible that the moisture may instantly evaporate, they then remain of their full size and colour, perfectly spherical. They seem to consider the flattened form as the effect of a change which takes place after death; for Bauer observes, in opposition to the assertion of Hewson, that in the skate, the particles during life are of the form of an egg, but that almost immediately after death they are flattened.

If the quantity of blood under examination be sufficiently large to retain its moisture only half a minute, the colouring matter in a few seconds begins to separate and form a circle round the globules. If the blood be diluted with water, the separation is instantaneous. They give the diameter of the globule, enveloped in its colouring matter, as $\frac{1}{1700}$, and when deprived of it, as $\frac{1}{2000}$. Elsewhere they state the proportion of the colouring matter to the globule to be as three to one. They describe the globules, when separated from the colouring matter, as being mutually attracted and coalescing with some disposition to linear arrangement, which is not the case so long as the coloured envelope remains attached; they further describe globules in pus, in muscular fibre, and in the substance of the brain, identical in point of size with the uncoloured globules of the blood.

The more recent and extended researches of Prevost and Dumas tend in some respects to confirm the opinions of

Hewson, while in others they more nearly coincide with those of Sir E. Home and Bauer.

They represent the particles of the blood as circular in all the mammalia, and elliptical in birds, reptiles, and fishes, but flattened in all, though a little prominent at the centre. Their size is uniform in the same animal, but differs in different species, from $\frac{1}{35}$ of a millimetre in the salamander, to $\frac{1}{120}$ in the callitriche, or $\frac{1}{288}$ in the goat. They regard them as consisting of a central colourless globule of one uniform size, $\frac{1}{300}$ of a millimetre, in all classes of animals, like those of chyle, milk, and pus, and inclosed, as before stated by Hewson, in a coloured membraneous vesicle, on which depends the difference in the form and size of the particles.

In those animals whose blood has elliptical particles, the nucleus appears also elliptical until muriatic acid is added, by which they conclude, that the surrounding matter is dissolved and removed. The nucleus has then the same appearance as that of the mammifera.

By repeated examination of the blood whilst in the course of circulation, these physiologists satisfied themselves that the particles possess the same size and form whilst in the vessels, as they do when recently drawn from the body. They deny that they perform a movement of rotation on the centres; but in describing the effect produced on the form of the particles from occasional resistance which they meet with, they confirm the remark of Lewenhoeck as to the elongation of the particles during circulation.

In our examination of the particles of the blood, we have in vain looked for the globular form attributed to them, not only by the older authors, Leeuwenhoeck, Fontana, and Haller, but still more recently by Sir Everard Home and Bauer. Our observations are also at variance with the opinion long since formed by Hewson, that these particles consisted of a central globule inclosed in a vesicle composed of the coloured part, and which, though refuted by Dr. Young, has since, in a modified form, been revived by Sir Everard Home and Bauer in this country, and by Prevost and Dumas on the Continent. We have never been able to perceive the separation of the colouring matter, which our countrymen have described as taking place in a few seconds after the particles have escaped from the body; nor can we with Prevost and Dumas, consider the particles as prominent in the centre.

The particles of the blood must unquestionably be classed amongst the objects most difficult to examine with the microscope; partly from the variations of form to which their yielding structure renders them liable, but still more from their being transparent and composed of a substance which, as Dr. Young has remarked, is probably not uniform in its refractive power.

These causes of error we have endeavoured to counteract by varying the mode of observation. We have viewed the particles both wet and dry, both as opaque and as transparent objects, under great varieties of power and light, and we lay no stress on observations which have not been confirmed by frequent repetition.

To us the particles of human blood appear to consist of circular flattened transparent cakes, which, when seen singly, appear to be nearly or quite colourless. Their edges are rounded, and being the thickest part, occasion a depression in the middle, which exists on both surfaces. This form perfectly agrees with the accurate observations of Dr. Young, that on the disks of the particles there is an annular shade, which is darkest on that side of the centre on which the margin is brightest. Though the Doctor drew the obvious conclusion that the disks were concave, he does not consider the fact as demonstrated; since the appearance might be produced by a difference in the refractive power of different parts of the corpuscle.

This objection we think completely met;

1st. By their transmitting the erect image of any opaque body placed between them and the light, precisely as a concave lens would do.

2dly. By the appearance presented by the particles when viewed dry, as opaque bodies. When illuminated by the whole of a Leiberkuhn, the entire margin is enlightened, and in most of the particles there is besides a broad inner ring of considerable brightness; whilst the centre, and the space between the two rings, is completely dark. On half the Leiberkuhn being covered, the rings are reduced to semicircles, the outer one being opposite to the light side, and the inner to the darkened side of the speculum.

3dly. When fluid blood having been placed between two slips of glass, a single particle happens to be at right angles to the surfaces of the glass, so as to be seen in profile, the two concave surfaces are visible at the same time,* or alternately, but more distinctly, if the particle slightly vacillates.

The concavity of the disks is, however, extremely trifling; and under particular circumstances, in a few of the particles, the surface is to all appearance quite flat.

Notwithstanding the great uniformity in the size of the particles of the blood, so long as they retain, unimpaired, the form which they possess on escaping from the body, their real magnitude has been so variously estimated, that we judged it worth while to attempt a new measurement. In doing so, we adopted a method somewhat different from

* This happens notwithstanding the interposition of the edge, when the centre of the particle is accurately in focus, owing to the large pencil of light admitted by the object glass.

those hitherto employed. A camera lucida is adapted to the eye-piece of the microscope in such a manner that the distance of the paper being ascertained, the object may be drawn on a known scale. Tracings of several of the images being made, they were applied to, and compared with, the images of other particles until their accuracy was established.

The diameter of the particles obtained in this manner may be pretty correctly stated at $\frac{1}{3000}$ of an inch.

The following measurements by former observers are given for the sake of comparison.

Jurine	$\frac{1}{5240}$
Jurine in a 2d measurement ..	$\frac{1}{1940}$
Bauer	$\frac{1}{1700}$
Wollaston	$\frac{1}{5000}$
Young	$\frac{1}{6060}$
Kater	$\frac{1}{4000}$
Ditto	$\frac{1}{6000}$
Prevost and Dumas	$\frac{1}{4076}$

The thickness of the particles, which is perhaps not so uniform as the diameter of the disks, is on an average to this latter dimension as 1 to 4.5

The form and size of the particles of the blood of other animals have frequently been compared with those of man. Many observations were made for this purpose by Hewson; but while some of them appear tolerably accurate, others are decidedly far from the truth. Those which have recently been made by Prevost and Dumas, are the most extensive and complete which as yet exist. Our attention having been chiefly taken up with the blood of man, we have not as yet carried our investigation of that of other animals so far as we design doing; we have, however, examined the blood in all the classes of vertebrated animals, and in different species of most of them. Our observations completely accord with those of Prevost and Dumas, as to the particles having a circular form in the mammalia, and an elliptical one in the other three classes. There are varieties both in the size and proportion of the particles in different species. Thus for example, in the pig and rabbit, the particles have a less diameter, but a greater thickness than in man. We have hitherto invariably found the elliptical particles larger than the circular, but they are proportionably thinner. In birds, the particles are much more numerous, but smaller than in either reptiles or fishes.

There are numerous interesting phænomena which present themselves when the particles lose their integrity and assume new forms. Changes of this description are occasioned by the spontaneous decomposition which the blood undergoes a longer or shorter time after its escape from the body, by mechanical violence, and by the addition of various substances, which appear to exert a chemical action on the matter of which the particles are composed. To these appearances we have been induced to devote the more attention, from their seeming calculated to throw some light on the composition and structure of the particles. We were also desirous of not hastily or rashly denying the existence of those colourless central globules which have been strongly insisted on by Sir Everard Home and Bauer, and by Prevost and Dumas, and which have been regarded not merely by themselves, but by other distinguished and intelligent physiologists, as constituting by their varied combination the different organic tissues. The separation and detection of these globules is stated to be facilitated by some of the means which effect the changes to which I have alluded ; but, as I have already stated, we have in vain looked for these globules.

After blood taken from the living body has been kept a sufficient length of time for an alteration in the form of the particles to commence, and this according to circumstances will be from a very few hours to one or more days, the first change which we have noticed is a notched or jagged appearance of the edge of a few of the particles. The number so modified continues to increase: some of the particles lose their flattened form, and appear to be contracted into a more compact figure; but their outline continues to appear irregular and notched, and their surfaces seem mammillated. Hewson and Falconar appear to have accurately noticed this change, and have compared the particles in this state to little mulberries. When more time has elapsed, most of the particles lose this irregularity of surface and assume a more or less perfectly globular form, and reflect the image of an interposed opaque body as a convex lens would do. Some of the particles resist these changes much more obstinately than others.

If a small quantity of blood be placed between two pieces of glass, which are afterwards pressed together with some force, several of the particles, however recent the blood, will be materially altered. The smooth circular outline is lost, and as in the former case, they appear notched. A few seem to be considerably extended by the compression. When the surface of the particles has in this way been broken into, the ruptured part exhibits an adhesive property capable of gluing it to another particle or to the surface of the glass; but the particles in their natural state, seem to be nearly void of adhesiveness.

There is scarcely any fluid except serum which can be mixed with the blood without more or less altering the form of its particles, probably in consequence of some chemical change. In this general result our observations accord with those of Hewson and Falconar, whose experiments of this kind were very numerous. We differ from them, however, in a few particulars. There is no fluid which, when mixed with the blood, produces a more remarkable and sudden alteration in the appearance of the particles than water does. With a rapidity which, in spite of every precaution, the eye almost invariably in vain attempts to follow, they change their flattened for a globular form, which from the brightness and distinctness of the images which they reflect as convex lenses, must be nearly perfect.

Contrary to Sir Everard Home's remark, that the particles in their perfect and entire state are not disposed to arrangement, it is in this state only that we have found them run into combinations, which they assume with considerable regularity. In order to observe this tendency of the particles, a small quantity of blood should be placed between two slips of glass. In this way the attraction exerted by one of the pieces of glass, counteracts that of the other, and the mutual action of the particles on each other is not interfered with, as is necessarily the case when only one slip is employed.

When the blood of man, or of any other animal having circular particles, is examined in this manner, considerable agitation is at first seen to take place amongst the particles; but as this subsides they apply themselves to each other by their broad surfaces, and form piles or *rouleaux* which are sometimes of considerable length. These *rouleaux* often again combine amongst themselves, the end of one being attached to the side of another, producing at times very curious ramifications.

When blood containing elliptical particles is examined in the same manner, it exhibits a not less remarkable but very different mode of arrangement. Though they are applied to each other by some part of their broad sides, they are not so completely matched one to another, as is the case with circular particles; and instead of placing themselves at right angles to the glass, with their edges presented to its surface, they are generally seen nearly parallel to it, one particle partially overlaying another, and their long diameters being nearly in the same line. In the blood of the toad or frog the lines thus formed are subjected to a kind of secondary combination, in which several assume to themselves a common centre, whence they diverge in radii. It is by no means rare to see several of these foci in the field of the microscope at one time. The particles at these points appear crowded, confused, and misshapen. This tendency to arrangement is perhaps not to be wholly attributed to the ordinary attraction existing between the particles of matter, but is probably to a greater or less degree dependent on life; since we have not only observed that the aggregating energy is of different force in the blood of different individuals, but that in the blood of the same individual it becomes more feeble the longer it has been removed from the body. At the same time, we are very far from believing that these or any other mode of aggregation which the particles of the blood may be observed to assume, ought to be regarded as at all analogous to the process which nature employs in the formation of the different tissues.

I some years ago briefly stated this opinion, which I was induced to form *a priori* *; but I may now appeal to facts in support of it.

Besides the particles above described, and which are evidently very important and essential constituents of the blood, other particles, much smaller and much less numerous, may occasionally be observed in the blood. They are circular, and perhaps globular, but we have not made them the subject of much examination.

* See "Thesis de Absorbendi Functione." Edin. 1823; and this Vol. p. 377.

We have not made many observations with a view to discover any microscopic differences between the blood of healthy and diseased persons. We have in general seen no perceptible difference between blood obtained from a small puncture expressly for the purpose of observation, and a portion of a larger quantity drawn from the arm to relieve some affection requiring venesection. There is some difference in the tendency of the particles to combine in piles, or *rouleaux*, but of the cause of this, we can suggest no explanation. The absence of this disposition to combination was most remarkable, and indeed almost complete, in a small quantity of blood taken from the arm of a young woman labouring under strongly marked chlorosis. This blood coagulated with little or no contraction of the crassamentum, which was covered with a thin buffy coat of a remarkable colour, somewhat resembling weak coffee diluted with milk.

The striking appearance in the blood of patients affected with cholera, has recently rendered its examination an object of some interest. In a specimen of this blood, furnished me from the City Cholera Hospital, by my friend Alexander Tweedie, the particles possessed their form, and other characters completely unaltered, notwithstanding that the blood had unavoidably been drawn a few hours before the examination was made.

We have observed in the blood of some individuals, that several of the particles presented more or less of the notched or jagged appearance, in which they have been compared to mulberries, and which may be regarded as a symptom of decay, or disintegration; since, as has been already remarked, it is seen to take place after the blood has been for a considerable time removed from the body. In the instances to which I am now alluding, the blood was quite recent, and taken from tolerably healthy subjects. In the blood of a small dog, from which the spleen had been removed several weeks, and which appeared to be in an emaciated sickly state, the particles generally seemed to have lost their natural figure, and to have been somewhat reduced in size. They likewise bore an unusually small proportion to, the watery serum in which they were suspended. In the blood of a remarkably fine doe rabbit, from which the spleen had been removed some years before by my friend Dr. Blundell, the particles did not seem quite so clear and regular in their figure, as in the blood of another rabbit examined at the same time for the sake of comparison. We are far from inferring from these two observations, that the particles of the blood are formed in the spleen, as Hewson imagined. In the first instance, their altered form may well be ascribed to the peculiarly sickly state of the dog; and in the doe, it might in part be occasioned by the state of œstrum in which she happened to be. It would be highly interesting to discover where and in what manner the particles of the blood are produced. When we consider their great uniformity as to size and figure in each species of animals, and the undeviating precision with which the rule is observed regarding their elongated figure in oviparous animals, and their circular form in those which are viviparous, we cannot help admitting some simple, but very powerful cause. We might hope to obtain some light on this subject, from the examination of the blood of a chick shortly after incubation had commenced. The presence of red blood is very distinct in the small areola of vessels belonging to the membrane of the yolk, immediately around the punctum saliens, whilst as yet, the rudiment of the spine and head, with a faint trace of the eye, and a very imperfect heart, are but just perceptible. In the very recent blood, seen whilst in motion in these minute vessels, the particles seem to want that uniformity as to size and figure, which is so striking in the more advanced animal; yet it appeared, that they already exhibited a tendency to the elongated figure. This enquiry is one which we by no means consider as complete, but it induced a doubt in our minds, as to the accuracy of the remark of Prevost and Dumas, that the particles in the blood of an incubated egg, are of a circular figure, as in the mammalia, until the period when the existence of the liver becomes apparent. In very minute animals quite of the lower classes, in which the vascular system is neither extensive nor complicated, and in which the channels for the conveyance of the nutrient fluid are proportionably of large diameter, the particles are not only comparatively rare, but very irregular as to size and figure, and even in colour. Although we hardly feel ourselves warranted in admitting a conjecture as to the mode of formation of these particles, yet we cannot help suspecting that they are much more intimately connected with their motion during circulation, than with any particular organ specially devoted to their production.

The striking changes effected in the particles of the blood, by the addition of water and other fluids, seems to merit careful attention at the present period, when the copious dilution of the blood is had recourse to, with the hope of counteracting the formidable symptoms of cholera. The great, sudden, and general alteration, caused by the mixture of pure water, would seem to render this fluid one of those which are the most to be dreaded. Saline solutions, on the contrary, have the recommendation of producing very little alteration of figure in the particles.

Chyle.—We have as yet devoted very little time to the microscopic examination of this fluid. A specimen taken from the thoracic duct of a young dog, a short time after his eating a hearty meal, was of a tolerably good white colour when first drawn, but it rapidly acquired a light pink blush on the surface by exposure to the air. It also speedily coagulated, but the quantity obtained was too small to admit of a sensible separation of serum. A small quantity of this chyle placed between too slips of glass, as in the examination of the blood, exhibited an infinite number of extremely minute particles in a state of constant agitation.

From these two circumstances, it appeared impossible to form any precise and satisfactory idea of their figure, but they appeared pretty uniform as to size. Dilution with water and with saline acid and alkaline solutions had no sensible effect in rendering the particles more distinct; but it is worthy of remark, that whilst the solution of oxymuriate of potash manifestly heightened the pink hue produced by exposure to the air, both the acid and alkaline solutions discharged it, a fact which it may be well to consider in connexion with the observations of Dr. Stevens. A minute portion carefully obtained from one of the lacteals, before it had passed through a lymphatic gland, presented precisely the same microscopic characters as that obtained from the thoracic duct. It is to be observed, that the microscopic characters of the chyle bear no resemblance to those of milk, of which we are next to speak.

Milk.—In this fluid the particles appear to be perfect and very transparent globules. But, far from being uniform, they present the most remarkable varieties in respect to size. Whilst some are more than double, others are not a tenth part of the size of the particles of the blood, to which they bear no resemblance.

Pus.—As far as we have yet examined this secretion, its particles appear to be as irregular in size and figure as those observed in the brain, and bear no resemblance to those of the blood.

In proceeding to offer a very short sketch of the result of our inquiries into the microscopic appearance of some of the animal tissues, I do so with one painful feeling, which I shall perhaps be excused from expressing. It is, that I am under the necessity of differing from my excellent and intelligent friend Dr. M. Edwards. It was the knowledge of his talents and address, and of the patience and care with which he made those investigations, which he has related, which induced me to enter into the examination of a question, which I had already regarded as settled in the negative. And though J. J. Lister and myself, in repeating the observations of Dr. M. Edwards, have arrived at widely different conclusions, I am confirmed in the conviction, that he described what he saw, and that he only saw amiss through the imperfection of his instruments. The idea of the globular structure of the different tissues is however by no means peculiar to Dr. Edwards, and to those micographers to whom I have already frequently alluded. Dr. Edwards, in the papers to which I refer, has employed much erudition to show that similar views had been taken, with respect at least to some of the tissues, by Hooke, Leuwenhoeck, Swammerdam, Stuart, Della Torre, Prochaska, the Wenzels Dutrochet, and Clocquet.

Muscle.—The muscular tissue may be easily seen with the naked eye, or with the assistance of a comparatively feeble lens, to be composed of bundles of fibres, held together by a loose and fine cellular membrane, and these fibres are again seen to consist of more minute fibrillæ. It is difficult to push the mechanical division much further; for the softness of the muscular substance is such, that it either crushes or breaks off, rather than admit of further splitting. If a piece of one of the most delicate of the fibrillæ last arrived at be placed on a piece of glass in the field of the microscope, lines may be seen parallel to the direction of the fibre, which show a still further division into fibres.* Although no trace of globular structure can be detected, innumerable very minute, but clear and fine, parallel lines or striæ may be distinctly perceived, transversely marking the fibrillæ. In some instances they seem to be continued nearly or quite at right angles completely across the fibril; but frequently the striæ in one part are opposite to the spaces in another, by which arrangement a sort of reticulated appearance is produced. The striæ are not in all specimens equally distant, but this may perhaps be owing to the elongation or contraction of the fibre. We have discovered this peculiar and very beautiful appearance in the muscles of all animals which we have as yet examined; and as we have seen it in no other tissue, we have been induced to view it as a distinguishing feature of muscle.

Although this characteristic appearance may be unequivocably seen in the voluntary muscles of the smallest animals of the lowest classes; yet, with the exception of the heart in most animals, and perhaps also, the gizzards of graminivorous birds, we have been unable to detect it in any muscle of organic life, such as, *e. g.* in the muscular coat of the alimentary canal, and that of the bladder. We have not only examined these textures in their ordinary state, but also in many instances in which encreased exertion, or other causes, had greatly developed their power and bulk, as when the bladder has gained great thickness and strength from diminution of the perviousness of the urethra, and the coats of the intestine from stricture of the colon or rectum. This essential difference between the minute structure of the muscles of voluntary motion, and those of organic life, is not only suffi-

* When sufficiently moistened, either by their own juices or by the addition of water, a minute portion of muscle thus thinly extended exhibits a nacreous irridescence.

cient to warrant a more decided distinction between them, anatomically and physiologically, but is borne out by equally striking differences in the pathology of the two structures. I must not, however, enter further into this subject on the present occasion, pathology forming no part of the present work. The nature and function of the substance of the uterus has been made the subject of warm dispute: some physiologists regarding it as unquestionably and powerfully muscular, whilst others refuse it the title of muscle. It was therefore an object of no little interest to us, to avail ourselves of an opportunity of examining this structure in its state of extreme development at the full period of utero-gestation. We could not detect in it the slightest traces of that appearance which we feel warranted in regarding as universally and essentially belonging to true muscle. We could perceive no sensible difference between its fibres, and those of the middle coat of the bladder, but so far as we are warranted in speaking of the muscular coat of the bladder and intestines, are we strictly correct in attributing muscularity to the uterus. In fact, this organ in its state of complete development forms the most striking and powerful example of this distinct contractile tissue, which, in accordance to the views of Professor Blainville, we may regard as an appendage to the mucous membranes. The minute fibrillæ, which enter into the composition of the fasciculi of fibres, of which this tissue is made up, instead of presenting the transverse striæ, of which we have been speaking, are perfectly smooth. They appear to be continued to a considerable length of nearly uniform width. In some instances these fibres are nearly straight and parallel, whilst in others there is more interlacing, and we are not sure that the fibrillæ do not unite and divide amongst themselves. In health they seem much less soft and fragile than the fibres of voluntary muscles deprived of life, and they are manifestly susceptible of considerable distention. In some states of disease, however, these characters are completely changed. We have not been able to observe, either in the voluntary muscles, or in those of organic life, any confirmation of the ingenious theory of Prevost and Dumas, already exhibited in the Appendix in this work ; in fact, the subject of vital contractility appears to us to be as great a mystery as ever. It might have been concluded, that the beautiful transverse striæ universally met with in all voluntary muscles, was, in some way, essential to this function ; but their total absence in the fibres of the muscles of organic life, endowed with equally powerful and more extensive, though somewhat different contractility, throws a strong doubt this supposition. The views of Dutrochet, who has beautifully explained some of the movements of plants by the application of the principle of exosmosis and endosmosis, which he has so carefully developed, do not seem from our observations to be applicable to the muscular motions of animals. The remarkable movements of the *mimosa pudica*, of the *hædysarum gyrans,* and of the *balsamina impatiens,* are by Dutrochet referred to the turgid state of a specially constituted vesicular structure ; and he is disposed to attribute the muscular motion of animals to a similar mechanism.

Arteries.—The middle coat of these vessels being still regarded by some persons as muscular, we were desirous of discovering whether its minute structure was at all more favourable to such an opinion than its chemical composition. Its subdivision may be carried as far as that of any tissue ; and it evidently consists essentially of long, straight, very delicate and even fibres, which offer no more trace of those transverse striæ, which we have regarded as the peculiar characteristic of muscle, than they do of elementary globules.

The inner coat, when completely detached from other structures, and presenting the appearance of a very thin uniform and almost transparent membrane, is also, by the aid of the microscope, seen to be composed of fibres, which are extremely delicate, smooth and uniform, but very tortuous and matted together, in the form of an intricate plexus.

Nerves.—These appear to be essentially composed of fibres, but their structure is looser than that of muscle. Though the fibres of nerves do not form such intricate plexuses as those of some other tissues, their course is by no means straight. We have looked in vain for globules, as well as for any trace of medullary matter, which has been somewhat gratuitously supposed to be inclosed in the nerves.

Cellular membrane.—This tissue, when perfectly formed, appears to be composed of delicate fibres, assembled together without any apparent order of arrangement. They exhibit no more appearance of a chaplet, or string of globules, than any other of the animal tissues; they have nothing like the transverse striæ which characterize the fibrillæ of the voluntary muscles, but like those of the involuntary muscles and of nerves, present a smooth surface and nearly uniform width, though they are much less distinct than either of the last mentioned tissues. The demonstration of the fibrous structure of the cellular membrane, is opposed to the view which Meckel entertained respect- this tissue, which he has regarded as amorphous, and of a consistence approaching that of mucus, and only presenting cells, lammellæ and fibres, as the effect of the means which are taken to examine it. The opinion, however, of Meckel is not to be rejected as altogether void of foundation in correct observation; it is probably to be attributed to his having taken as specimens for examination, portions in an incomplete state, as they occur in newly formed false membranes, which, when just separated from the serous fluid which pervades them, present no determinate arrangement, and in other respects more nearly approach to Meckel's description.

Serous membranes.—The structure of these membranes, bears the closest resemblance to that of the cellular.

Mucous membrane.—We have not been able to distinguish any decided fibrillæ, or other determinate form, in these membranes; but our examination of this tissue has not been completed.

Brain.—If there is any organized animal substance which seems more likely than another to consist of globular particles, it is undoubtedly that of the brain. Our examination of it has as yet been but slight; but we have noticed that when a portion of it, however fresh, is sufficiently extended to allow of its being viewed in the microscope, we see instead of globules a multitude of very small particles, which are most irregular in shape and size, and are probably more dependent on the disintegration than on the organization of the substance.

The structure of some other parenchymatous parts appears equally indeterminate, presenting neither globule nor fibre.

ON THE USES OF THE SPLEEN.

From the Edinburgh Medical and Surgical Journal, Jan. 1822.

[In reprinting the following juvenile essay, I must be allowed to remark by way of apology, that the functions it ascribes to the spleen, and which I am still disposed to admit as a part of its office, bears an immediate relation to the influence of temperature, and atmospheric pressure, two important physical agents, upon the state of the circulation. At the time it was written I was not aware, although I made considerable search and enquiry, that any similar views had been previously advanced, nor am I yet aware that the grounds on which they rest had been fully stated. The view which I have taken has since been admitted by several physiologists, and a contribution to a recent periodical publication, has reproduced the principal arguments in support of it; but, I believe, without any knowledge of the article which is here reprinted. Two opinions have likewise since been advanced; the one by Sir A. Carlisle, that it supplies warmth to the stomach, an idea which is by no means new, but which, as far as I am aware, rests on no other foundation than the antiquity, or respectability of its supporters. The second view to which I allude, is that of Sir Astley Cooper, who has carefully investigated the structure of the spleen, and by different modes of preparation, in which he singularly excels, has demonstrated the capacity of its cells in various species of animals. It is suggested by Sir Astley Cooper, that it is a part of the office of the spleen to elaborate venous blood, and thus to assist the liver in the formation of bile. My principal reasons for doubting the accuracy of this suspicion, consists, first, in the fact, that whilst the production of venous blood necessarily attends the process by which every part of the body is nourished and warmed, it is in itself rather unfriendly than salutary, requiring the most uninterrupted provision for its removal: secondly, in the fact, that the supply of venous blood does not appear to be essential to the functions of the liver, since the vena portæ has been found passing directly to the cava.]

Amongst the objects which yet remain for our investigation, after the persevering research of a vast host of acute anatomists and physiologists who have adorned this country and the Continent of Europe up to the present day, the uses of the spleen may, I believe, still be numbered.

In entering on the consideration of this subject, Haller has said, "In meras hic conjecturas demergimur obscuriores quam fere alio in viscere." When I recollect the names of those who have already been engaged in this investigation, I am almost induced to fear that I shall be accused of presumption in attempting to meddle with it; but, on the other hand, I am tempted into a field, so far cleared by their labours.

It is, I apprehend, perfectly needless for me here to say any thing of the structure or situation of the spleen; but, before attempting to explain the office which, I conceive, this viscus is destined to perform, I may, perhaps, be allowed hastily to glance at the theories which have already been advanced. Some of them are, however, almost too ridiculous to deserve notice. One while it was considered the seat of melancholy, then of merriment; while some have held that it was connected with generation. Aristotle thought that it received vapours from the stomach, to convert them into different fluids. Franciscus Velinus imagined that, in it, the fluids of the stomach were converted into blood; Stukely, that it acted as a sponge, and that, at will, blood might be pressed from it either into the arteries or the veins, as well as that it furnished blood to the genitals. Harrison thought it furnished a mucilaginous fluid; and Rivinus, that its secretion lubricated the viscera of the abdomen. Some have attributed to it a muscular power, by which it can compress its vessels (Willis); others, that it supplied the stomach with warmth; that it balanced the liver; or, that it rendered the blood of the vena portæ more alkaline and fluid, in order to obviate any danger of obstruction and scirrhous induration, which its slow circulation, and the deterioration it had undergone in the omentum and mesentery, might induce; that the blood acquired new properties in the spleen, by delay in its cells and large veins, and by its proximity to the fetid contents of the colon. It was the opinion of Malpighi, and, from him, revived by many others, that it prepared the blood for the formation of the bile; while others have not been wanting who denied its being of any use.

More recently, that indefatigable physiologist and anatomist, William Hewson, conceived that, in the cells of this organ, the red particles of the blood were completely formed round smaller particles, the production of the thymus and lymphatic glands. His theory has now shared the fate of its predecessors. His ideas respecting the red particles have been proved erroneous; and, though the delicate injections of Dr. Haighton have clearly shown the cellular structure of the spleen, yet these cells are not such as William Hewson had conceived them to be.

Sir E. Home, in two papers read before the Royal Society, London, in 1807 and 1808, advanced the ingenious speculation, that the spleen relieved the stomach of superfluous fluids which passed into it by direct communication; though, in 1811, he withdrew this opinion, which therefore requires no further comment. I may, perhaps, be permitted to say, that the interesting experiments which he has related in these papers are not less valuable than if they had fully confirmed the views of their author. No arguments which have as yet been advanced, appear to me more strongly to support the idea, that some part of the process of absorption devolves on the veins. They likewise tend to strengthen an opinion which I have for some time entertained, and which I hope soon to bring to the test of experiment, that those fluids which are taken up by the veins are either acids, or stand similarly situated in Professor Œrsted's arrangement, while the absorbents receive those of the opposite class.

The theory which I believe at present meets with the most general support, is that of Dr. Haighton. After having proved that the blood drawn from the splenic veins does not sensibly differ, as Hewson had asserted, from that of other veins; and that the bile of an animal whose spleen has been removed, is not necessarily changed in any respect, as Malpighi has said,—Dr. Haighton advanced it as his opinion, that the spleen was subservient to digestion in another way, occasioning an increased secretion of the gastric and pancreatic fluids, at the precise time when they are most required. In explaining the mode in which this effect is to be produced, he agreed with Haller in the opinion, that the stomach, when distended with food, makes sufficient pressure on the spleen to supply its cells, and to direct the blood commonly sent to it to the stomach itself, and to the pancreas. The late lamented Henry Cline, junior, about the same time, was induced to come to a similar conclusion.

This speculation, which has deservedly gained many admirers, is not, as Dr. Blundel, the nephew and ingenious successor of Dr. Haighton, very candidly acknowledges, perfectly free from objection; noticing, at the same time, the fact, that Nature can, on occasion, supply particular organs with an increasing quantity of blood, without such a contrivance, *e. g.* the nipple, combs of cocks, &c.; that, in ruminating animals, the spleen is not connected with the digesting stomach, which ought more particularly to require its assistance; and he hence concluded, that though the office which Dr. Haighton had assigned to the spleen is probably one of the functions which it has to perform, others may also belong to it, which remain to be discovered. Besides these objections, which are admitted by Dr. Blundel, others I think may yet be urged against Dr. Haighton's theory. It appears to me that an elastic, easily distendible bag, like the stomach, filled with a soft pultaceous mass of masticated food passing into chyme, and enclosed by the yielding parietes of the abdomen, is ill adapted to expel the blood from the innumerable spherical cells of the spleen, or to resist the flow of blood through the splenic artery. Nor, were it capable of doing this, can I conceive that it would favour the healthy secretion of either the gastric or pacreatic juices. Hæmasemesis might more naturally be expected.

Having now endeavoured to show, that all the speculations as yet advanced respecting the spleen are liable to objections, it remains for me, as briefly as possible, to explain the opinion which I was induced to form concerning this organ, when considering the subject rather more than a year ago.

The structure and situation of the spleen — the different appearances it assumes, according to the circumstances under which death has taken place — the causes which derange it, and the effects which it produces on the system when deranged — together with the result of experiments made on inferior animals, conspire to induce me to believe, that the spleen performs in the animal system, a similar office to that which tubes and valves of safety do in various kinds of chemical and mechanical apparatus.

The comparison may, perhaps, be thought a strange one; but, I believe, it will make my idea sufficiently intelligible when I say, that I imagine that the office of the spleen resembles that of the middle tube of a Woulfe's apparatus. By this I wish it to be understood, that the spleen tends to obviate any inconvenience which might arise from a sudden disturbance of the proportion between the capacity of the vascular system, and the fluids which circulate in it. These disturbances must, I conceive, be frequently induced by various causes to which animals are continually exposed, and which operate more powerfully than the elasticity of the vessels alone can compensate for, and more rapidly than absorption, secretion, and excretion can, in every case, counteract.

It will now be proper that I should enumerate the reasons which, if I am not mistaken, support me in this opinion.

1. The structure of the spleen.

The cells of this organ appear admirably calculated to admit, with impunity, of a protracted congestion of blood in them, which, in other parts, would necessarily be followed by consequences of the worst nature. On the one hand, these cells are sufficiently minute to bring the blood so much into contact with the solid vital parts as to remove every danger of coagulation which takes place when blood escapes from its vessels; and, on the other hand, those changes are not likely to be effected in it, at least with the usual rapidity which takes place when the blood enters the ordinary minute terminations of vessels. In addition to this, these spherules have this advantage, that they will be less likely to yield and rupture under the sudden distension produced by the accession of a large quantity of blood.

2. The idea is not a little supported, as I apprehend, by the situation of the spleen. I have already said, that it seems improbable that it can receive from the stomach that degree of pressure which, according to the theory of Dr. Haighton, it is necessary that it should do; and here I may add, that it appears highly probable that the spleen is placed in contact with the stomach; for this very reason, that, of all the viscera of the abdomen, the stomach is the least likely to interfere with the variations of its dimensions, as well as for the sake of the protection which, in this situation, it receives from the ribs.

Not less consistent with this idea of the functions of the spleen, is its connexion with all the viscera of the abdomen, through the medium of the vena portæ, which, probably on this account chiefly, has not been furnished with valves.

3. The great variety which is met with as to the size of the spleen, has I believe been long observed, and has tended to add to the perplexity in which the function of this organ has been involved.

Should the limited observations, which it has been in my power to make with regard to this point, be confirmed by the experience of others, this variety will add support to the speculation which I have advanced. In persons who have died suffocated, I have found the spleen large and turgid; but, in a man who died from the bursting of a large aneurism, which extended from the origin of the cœliac artery quite into the pelvis, the spleen was small and flaccid.

Should such coincidence not be found to exist in all cases of death from similar causes, this circumstance alone will not amount to a refutation of my opinion; for we cannot but suppose, that, in different individuals during life, this function of the spleen must be more or less forcibly and extensively called into action, by which the growth of the organ must be affected to a degree which the circumstances occurring at death cannot, in every instance, be able to countervail.

4. Before proceeding to relate some experiments which have been made in relation to this subject, it will be as well to mention a few pathological facts which may be advanced in support of my opinion.

First, as to the causes which tend to produce disease of this viscus. Of these, none is more remarkable, and at the same time of more frequent occurrence, than intermittent fever.

In the cold stage of an intermittent, when the vessels of the surface are often suddenly diminished in capacity, and when, consequently, a large quantity of blood must be as suddenly thrown upon internal parts, an organ acting the part which I have here attributed to the spleen, must be called into unusually active service, at a time when the febrile state checks those secretions by which it is to be itself relieved, and the balance between the circulating fluid and its vessels is to be preserved. What else could we expect of an organ frequently called into excessive service of this kind, but first increased growth, and afterwards, should the cause continue to operate, derangement of structure? Those who die of ague are, it is well known, almost invariably cut off in the cold stage, and the spleen is either found prodigiously gorged with blood, or actually ruptured. More frequently, however, the complaint being less urgent, proceeds to the derangement of the spleen, and effects are produced which I shall presently have to consider.

Another complaint which leads to the derangement of the spleen, is amenorrhœa; and the mode in which it acts, admits, I apprehend, of as easy an explanation as the preceding case, provided the functions which I have supposed be attributed to the spleen.

Secondly, A very few words will suffice with regard to the effects produced by the structure of the spleen becoming so far deranged as to disable it from fulfilling its function. Either hæmorrhage or serous effusion is, I believe, the almost invariable consequence induced, as I conceive, by the small vessels being unable to sustain the shock occasioned by a sudden disturbance of the balance between the capacity of the vascular system and the circulating fluid; which shock, in the healthy state of the spleen, is in a great degree broken by it, assisted, no doubt, by the elasticity of those vessels which have not been exposed to the constricting cause.

5. I shall now relate a few experiments on inferior animals, and then conclude.

An experiment related by the ingenious and indefatigable B. C. Brodie, in his Lectures delivered last summer at the London College of Surgeons, appears to me to strengthen my opinions, though the object for which it was instituted was not connected with an inquiry into the office of the spleen. The vena portæ of a dog was obstructed by a ligature, just as it enters the liver. After some hours uneasiness the animal died; and, on examination, the abdominal viscera, as we must conceive, would necessarily have been the case, were found charged with venous blood. This, however, was most remarkably the case with the spleen, which was gorged with blood, and prodigiously distended.

Here the spleen seems to have performed its part to the utmost; but the relief which it in its turn requires, being cut off by the ligature on the vena portæ, no exertion of its function, however considerable, could preserve life.

Though it will be quite needless here to quote all the interesting experiments of Sir E. Home, to which I have before alluded, since they have now been long before the public; yet I cannot forbear availing myself of the support which I may derive from such respectable authority, by noticing a few of the circumstances which occurred in the performance of them. Infusion of rhubarb was repeatedly given to different animals, and invariably the spleen was found turgid, and its blood impregnated with the rhubarb; and this, notwithstanding that the thoracic duct and right trunk were secured, and that the chyle exhibited no trace of the drug. Nor did the part of the canal, from which the absorption took place, appear to cause any difference, as the same results are obtained, whether the infusion was lodged in the stomach, as in the experiments made on the dogs and rabbits, or in the larger intestines, as in the ass. I would here inquire, Does not this look like venous absorption?—for what other system of vessels is there, which, in this case, could communicate at once with the stomach, intestines, and spleen?

The experiments of Sir E. Home further prove, that this passage of the absorbed fluid into the spleen, is neither at all times necessary, nor invariably taking place; for the spleen, he has since shown, may be removed without preventing the rhubarb from finding its way into the system. But I would more particularly solicit attention to the following fact. In all the preceding cases, fluids had been injected, and the spleen was full and distended. When, however, the rhubarb was given in substance, and the animal deprived of water, which was done for four days, in the case of one ass which Sir E. Home subjected to his experiments, the spleen was found contracted to half the size of what it was seen in the former experiment, and its substance was also as much condensed as that of the liver; nor did the blood from it, in such cases, exhibit any very marked indication of the presence of rhubarb, at least not more than in the other parts of the body, though its presence in the urine proved that it had been taken into the system. Sir E. Home has remarked, "that the spleen is met with in two very different states, one of which may be termed the distended, and the other the contracted; and that, in the one, the size is double what it is in the other."

In the distended state, there is a distinct appearance of cells, containing a limpid fluid, distinguishable by the naked eye. In the contracted state, these only become distinct when seen through a magnifying glass; the distended state takes place when the stomach has received unusual quantities of liquids before the animal's death; and the contracted state, when the animal has been kept several days without drink before the spleen is examined. In a subsequent paper, he states his belief that this limpid fluid is the peculiar secretion of the spleen, which is conveyed to the thoracic duct by the lymphatics of the organ, which are, as he has said, larger in this than in any other organ. "The purposes," he adds, "that are served by such a secretion from the spleen into the thoracic duct, cannot be at present ascertained." If I might be allowed to offer an explanation of the preceding facts, it would be the following.

Consistently with the idea, that the spleen is to maintain the balance between the circulating fluid and the vessels destined to contain it, we cannot be surprised to find it in its turgid state, at a time when absorption is making large contributions to the former; and more especially if we admit, as I cannot help doing, that the veins take a share in the process of absorption. The contracted state is not less accordant with that state of the system which must be induced by secretion being continued, while the accustomed supplies are withheld. As to the fluid conveyed from the spleen to the thoracic duct, and which Sir E. Home has considered as the secretion of this organ, I must beg leave to hold a different opinion, and to regard it as merely the effect of ordinary lymphatic absorption actively going forward to relieve the spleen of the load which the other viscera are throwing upon it. The whitish corpuscules, I imagine, are intimately connected with the lymphatic system. I have found them particularly conspicuous in a cat, whose lymphatic glands, throughout the body, were remarkably large.*

If my speculations should prove correct as to the nature of the matter taken up by the absorbents and veins, it may account for the absence of any trace of rhubarb in the fluid contained in the absorbents, and also for the strong signs of it, which Sir E. Home so repeatedly observed in the splenic vein. William Hewson had also observed the fluid contained in the lymphatics of the spleen. The red colour which he observed in it must, I imagine, be attributed to some extravasation, the consequence of congestion, occasioned by the ligature which he had applied.

* Some experiments have been made on this subject by my friend Bracy Clark, a very ingenious and scientific Veterinary Surgeon, who has thrown much light on the feet of animals with undivided hoofs, and has investigated, with great success, the habits of *œstri*, as well those peculiar to other animals as to the horse. I have not yet been able to procure the detail of his experiments; but, from the sketch which I have received, I believe they will rather confirm me, than not. He has not himself advanced any opinion grounded upon them.

In the 8th month (August) last, after dividing the spinal marrow of a cat, I opened her abdomen, and immediately immersed her in cold water, taking care not to admit the water into contact with the viscera of the abdomen. The spleen became sensibly turgid. This seems to prove, as far as the result of one experiment can do so, that the spleen is, in the mode which I have hinted at, subservient, not merely to the vessels of the abdomen, but to those of the system generally. The derangement produced in this viscus, by intermittent fever, tends to the same conclusion.

I have not been able to learn whether any peculiar symptoms were discoverable in the two instances which are recorded, of the spleen having been extirpated in the human subject; but some of those which have been observed in dogs, from whom this organ has been removed, and which Haller has recorded, on the authority of Jolyffe, Boyle, Malpighi, Hunter, and some others, seem to indicate an increased stress upon the arterial extremities; as, *e.g.* an increase in the flow of urine—a greater degree of salacity—and the accession of plethora. In some the functions of the liver were disturbed, and this organ itself rendered turgid.

I have now given a sketch of the principal reasons which have led me to the opinion which I have here advanced, not by any means as a point which I as yet regard as demonstrated, but rather to be considered as a suggestion; nor shall I be surprised if some one, better acquainted with the subject than myself, should say with Haller, "Nondum tamen inde audeas aliquid firmioris theoriæ superstruere;" yet I am induced to trust, that there is enough of plausibility to secure me from censure, in having ventured to bring it forward.

It will, I have no doubt, be thought by most, that I should myself have made more experiments with regard to this question; to whom I can only state, in defence, that, besides having little opportunity for doing so, I conceived that, by availing myself of the experiments of others, of men of the highest celebrity, no imputation could arise of facts having been warped to suit theory, which, by the theorist, may sometimes be innocently done; and further, the performance of these experiments would be necessarily attended with a degree of cruelty, which would render them alike painful to myself to undertake, and to others to hear detailed.

THOMAS HODGKIN.

Edinburgh, 4th month (April) 1821.

NOTES.

PART I.

CHAPTER I.

Page 11.—In confirmation of the opinion advanced by Dr. Edwards, that venous blood, though greatly inferior to arterial, still contributes to produce the action of the nervous and muscular systems, may be adduced the extraordinary facts with which the epidemic cholera has familiarized us. We have seen individuals, whose dark and blue colour has attested the general circulation of venous blood, exercising, though with a certain degree of habitude, the intellectual and locomotive faculties.

Page 13.—Although in Dr. Edwards' experiments on the duration of the life of frogs inclosed in plaster, the animals do not appear in any instance to have survived more than six weeks; we are not on this account bound to discredit the statement of Hérissant, who kept them alive eighteen months, in boxes inclosed in plaster. My friend — Hodgson of Birmingham, has informed me, they have been kept alive for three years inclosed in globular masses of plaster, but that to effect this, it will be necessary to keep them in a cellar at a low temperature. I believe it is his intention to publish these, and some other interesting facts pointed out by a deceased friend of his.

Page 37.—The fact, that fishes cannot live in water deprived of air, and yet speedily die when removed from the water and exposed to the direct and unmixed influence of the atmosphere, is probably in some respects analagous to what occurs in the mammalia, who, though they cannot survive many minutes, when removed from the vivifying influence of the oxygen, nevertheless require that it be diluted with a large proportion of nitrogen, or hydrogen, and are soon cut off when restricted to an atmosphere of pure oxygen. I shall have occasion to advert to the experiments of Broughton on this subject in a subsequent note.

The following is a note by Dr. Marshall Hall.

* The results of the experiments detailed in this interesting chapter, do not appear to be all phænomena of the same kind.

It is plain that death is the result of asphyxia within certain limits of temperature. But as we approach that of 42° cent. (107° 6′ Fahr.) the animal is doubtless destroyed, not by asphyxia, but by the positive action of this elevated temperature upon the nervous and muscular systems; the nerves are deprived of their peculiar properties and the muscles become perfectly rigid.

The facts, however, together with those of chap. 2. sect. 1. p. 18, and that quoted from Legallois, p. 148, demonstrate, that exposure to a low temperature, both previous and immediate, enables the animal tribes to bear the privation of air better than exposure to a higher one.

What is the rationale of this fact? Mr. Edwards does not offer a conjecture upon the point. It is probable, that it depends upon augmented irritability. The effect of exposure to cold, when the temperature of the animal falls, appears to be, in a general point of view and within certain limits, diminished respiration and augmented irritability.

The experiments detailed, pp. 19 and the following, appear incomplete. It was found, that the temperature of 42° cent. (107° 6′ Fahr.) was alike immediately fatal to the bactrachiæ in summer and in winter; but as the experiments were first performed in the months of July and September, it was a question not only whether the temperature of 42° cent. (107° 6′ Fahr.) would be equally promptly fatal in November, but whether a lower temperature would not have been so.

It is probable, that all temperatures which reduce that of an animal, without destroying life, raise its irritability and lower its respiration, and render it, at once, less capable of bearing any increased stimulus, as that of heat, and more capable of bearing diminished stimulus, as the privation of air, or of food. This will probably be found a general law of the animal economy, equally applicable to the warm-blooded and cold-blooded animals. It is illustrated and confirmed by the fact quoted from Legallois, p. 148; and by the histories of persons or animals buried in the snow.*

PART II.

CHAPTER I.

Page 55.—I visited the Magdalen grotto, near Adelsberg, in 1824, and obtained some of the protei from the subterranean pools which exist in it. Only a part of the floor of the cavern was then occupied with water, and these animals might have enjoyed atmospheric respiration had they inclined to do so, to at least an equal degree with those which Dr. Edwards kept in an earthen vessel exposed to the air. They were, however, deprived of the benefit of light.

CHAPTER II.

Page 56. — I have been furnished with the following authorities for fishes being able to live in water at very high temperature, by my excellent and accomplished friend A. R. Dusgate of Paris, from which it would appear, that in a state of nature, fish not only live, but thrive in a temperature beyond the limit which they were able to endure in Dr. Edwards' experiments: may not this difference be in part referred to the influence of habit? Saussure, speaking of the hot springs of Aise in Savoy, says, "I have frequently examined the temperature of these waters at different seasons, and have always found it very nearly alike, viz. from 35 in that of Souffre, and from $36\frac{1}{2}$ to $36\frac{7}{10}$ in that of St. Paul. Notwithstanding the heat of these waters, living animals are found in the basins which receive them. I saw in them eels, rotifera and infusoria, in 1790. I discovered in them two new species of tremelles possessing spontaneous motion, of which a description may be seen in the 'Journal de Physique' for 1790, p. 401." See Saussure, Voyage dans les Alps, vol. vii. pp. 18 and 1168. Neufchatel edition in 8vo.

Sonnerat states, that in the island of Lugon, one of the Manillas, there is a hot spring, of which the temperature was so high as to raise Reaumur's thermometer to the degree of 60, equal to 86·25 cent. or 187·25 of Fahr. According to his account, one could not put one's hand in it, yet he distinctly saw fish which did not appear to be at all incommoded by the heat; and small plants, the *agnus castus*, flourishing in it.—Journal de Physique de Rosier, April 1774, p. 256. See also Rees's Cyclopædia and Pinkerton's Geography.

The *sparus Desfontaines* of Lacepede — the *chromis* of Cuvier, was found by Desfontaines in the hot waters of Cafsa in Barbary, in which Reaumur's thermometer rose to 30 degrees.—See the article 'Sparus' by Bosc, Dictionaire d'Histoire Naturelle, Deterville's edition, vol. xxxi. p. 550. My friend likewise received the same statement from the Professor's own mouth.

The following extract is from Bruce. "At Feriana, the ancient *Thala,* are baths of warm water without the town:—in these there were a number of fish, about four inches in length, not unlike gudgeons. Upon trying the heat by the thermometer, I remember to have been much surprised, that they could have existed, or even not been boiled, by continuing so long in the heat of this medium.

In opposition to the account of the hot springs at Manilla, given by Sonnerat, must be placed that of Dr. Abel, who accompanied Lord Amherst in his embassy to China. In his narrative of the journey, he notices visiting those springs, and remarks, that "Sonnerat has stated, and his statement has been copied by other authors, that a species of fish lives in these springs. It is scarcely necessary to observe, that I was unable to verify this observation. All the animals which I saw there, and I saw two, a small snake and a frog, were not only dead, but boiled;" but he adds, "a plant vegetates in them, and in this respect my experience partially accords with his. I found a small plant, apparently a species of arenaria, vegetating in a soil that raised the thermometer, plunged amongst its roots, to 110 degrees on the side of the spring, which was 120 degrees."—Fourth edit. London, 1818, pp. 246–249. (It will scarcely escape the attention of the reader, that Dr. Abel's account does not positively disprove that of Sonnerat. Though he appears only to have seen two dead animals, which probably came there accidentally, it is still not impossible, that other species, under the influence of habit, may support the temperature of some parts of the fountain.)

Shaw, after enumerating the thermal waters of Barbary, adds, "The *ain el houte* (fish fountain) and the springs of Cafsa and Tozer, nourish a number of small fishes of the mullet and perch kind."—Shaw's Travels in Barbary, folio edition, Oxford, 1738, p. 231.

The late lamented Baron Cuvier, whilst engaged in publishing his great work on fishes, was reminded of these observations by my friend, and in consequence wrote to M. Marcéscheau, the French vice-consul at Tunis, who, not only confirmed the fact in his reply, but sent him two long-tailed fresh-water turtles, from a basin of water at Utica, of which the water is at the temperature of 36 degrees of Reaumur, or 113 of Fahr. The vice-consul also sent some fishes from the hot waters of Cafsa and Tozer, which proved to be *chromis* or *spari Desfontaines* of Lacepede. These waters were said to be as warm as 62 degrees of Reaumur.

Breislak in his Institutions Géologiques, has an article on this subject. Amongst other facts, to those above noticed, he adds, that Dunbar and Hunter, in their journey made in 1804, along the Washila or Ouachita, a river of Louisiana, observed above Fort Meiro, on the frontiers of the United States, springs of the temperature of 40 degrees to 50 degrees of Reaumur, or 122 to 145 Fahr., in which were not only growing *confervæ* and herbaceous plants, but also shrubs and trees. They likewise found in them *bivalve molusca*.

Lamark, in his Histoire des Animaux sans Vertebres, states, that the *paladina muriatica* is found in Italy and in France, especially in the south, inhabiting in fresh water, and has been met with in water of the temperature of 34 degrees Reaumur, 109 Fahr.

To these instances presented by nature, of animal and vegetable life, maintained at high temperatures, my friend Dusgate adds the following extract from an article by Bosc, in the Dictionnaire d'Histoire Naturelle, tom. xxxi. p. 551. "The facts mentioned by Sonnerat, and other travellers, induced Broussonnet to make some experiments on the degree of heat which our river fish are capable of enduring. I have no detail of the result of his observations, although I took a part in them; but many species lived for several days in water, which was so hot that I could not bear my hand in it for a single minute."

I have not brought forward these curious facts, with the intention of disputing the general accuracy of the limits of high temperature assigned by Dr. Edwards, as consistent with the life of fishes. As exceptions to it, they may be apparent rather than real, since it is by no means impossible, that the heat of that part of the water in which the fishes were seen, might not be exactly the same as that in which the thermometer was placed. If they cannot in this manner be explained away, they afford a very legitimate object for further enquiry. The following fact is quite compatible with the limit given by Dr. Edwards, but it is interesting as shewing, that a very considerable degree of warmth is not only supported, but very congenial to some species of fish. It is well known, that in manufacturing districts, where there is an inadequate supply of cold water for the condensation of the steam employed in the engines, recourse is had to what are called engine dams or ponds, into which the water from the steam-engine is thrown for the purpose of being cooled; in these dams, the average temperature of which is about 80 degrees, it is common to keep gold-fish, the *ciprinus aureus;* and it is a notorious fact, that they multiply in these situations much more rapidly than in ponds of lower temperature exposed to the variations of the climate. Three pair of this species were put into one of these dams, where they increased so rapidly, that at the end of three years, their progeny, which were accidentally poisoned by verdigris mixed with the refuse tallow from the engine, were taken out by wheelbarrows-full. Gold-fish are by no means useless inhabitants of these dams — they consume the refuse grease which would otherwise impede the cooling of the water by accumulating on its surface. It is not improbable, that this unusual supply of aliment may co-operate with increase of temperature in promoting the fecundity of the fishes. My friend, Charles May, of Ampthill, to whom I am indebted for the fact just related, has communicated to me another fact in proof of the high temperature which vegetable life is sometimes capable of enduring. —John Daulby, brother to the curator at the Botanic Garden, at Liverpool, brought from Iceland, a short time since, a species of *Chara,* which he found flowering and producing seed in one of the hot springs of that island, in which he states, that he boiled an egg in four minutes.

PART III.

CHAPTER II.

The remarks of Dr. Edwards, respecting the hybernating mammalia, induced me to query, whether there might not be some species amongst the class of birds possessed of a similar constitution, as respects the influence of temperature. The migrating birds, which quit this country on the approach of cold weather, seemed the most likely to be of this description. The season was far advanced, and swallows had for the most part left the country, when this idea occurred to me. Through the kindness of a friend I obtained two individuals, which enabled me to perform the following experiment: —

With a small and very delicate thermometer, I ascertained the temperature of the swallows to be 106° Fahr., that of the laboratory in which the experiment was performed, being nearly 70°. One of the birds was then placed in a deep glass vessel, immersed in a mixture of ice and salt. Although the bird remained quiet, its respiration soon became greatly accelerated, and its temperature, in somewhat less than an hour, was reduced about 20 degrees. It is not easy satisfactorily to ascertain the temperature of so small animal as a swallow. In this instance the temperature was examined with a small thermometer, carefully placed under the wing, which was kept applied to the side. Although the extraordinary powers of flight possessed by swallows, and other migrating birds, generally enables them to avoid that degree of cold, which their constitutions are not calculated to resist; it is extremely probable, that if they survived detention in this country, after the setting in of cold weather, they would fall into a state of torpor. The brumal retreat of swallows, has long been a subject of speculation and controversy, and numerous anecdotes of their having been found in a state of torpor, are related by those who maintain the opinion that they do not quit the country, but retire to various places of concealment during the winter. Some of these statements may rest on questionable authority, but I am convinced that others are too well attested to admit of rejection. As an example of this kind, I might cite the following incident which occurred to my friend, James Browell of Guy's Hospital. I will relate it in his own words :—

"I will endeavour to give you as clear an account as I can of the circumstances relating to the habits of the swallow, which came under my notice at a period of life not very prone to philosophise on things seen. The impression on my mind is very vivid, though the distance of time is half a century.

"Residing with my parents in Hampshire, so near the sea that the high tides reached the walls of the house, after morning school, I was occupied in the boyish play of throwing up a ball in the back yard, which fell into a butt placed for receiving the rain water from the roof of the house. It was in the winter, though not a wet time, as the cask was only half full. On my getting up to the edge of the water cask, and leaning over into it, sweeping my hand round in search of the ball, my hand touched the bung cloth, a little under the water, and I felt something which induced me to move it, and found it, on examination, to be a bird in a torpid state, perfectly wet and to appearance inanimate. Whether I had heard any thing said on the subject, or what is not very probable at about nine years of age, had read about it, I cannot recollect; but I remember well, leaving the ball and taking my prize to the kitchen fire, which after drying and warming, I had succeeded in restoring to perfect animation my bird; when my mother found me there, and it was time for me to go to school; my entreaties to be allowed to complete my restoration were not attended to, and I received orders to let the bird fly when it could, and it was put opposite an open window, facing the sun, and on my return from school, no trace of my protegée could be found."

Dr. Marshall Hall has obliged me by furnishing me with his remarks respecting the subjects of this PART. *They are included between asterisks.*

CHAPTER I. III. IV. V. and VI.

* This interesting series of experiments, admits of a ready association and explanation, by a reference to the law, that the quantity of respiration is inversely as the degree of irritability, and the facts, that the activity of the animal is directly as the former, and that its tenacity of life under the privation of air, food, and other stimuli, is directly as the latter. The very young animal has a lower respiration, and a higher irritability than the adult. It has less power of evolving heat, and greater power of bearing the privation of air.

The adult animal has a higher respiration, and a lower irritability. It has greater activity, and less tenacity of life, under the privation of air and food.

The animal which maintains a steady given temperature, has a higher respiration, and a lower irritability, in winter than in summer. Animals, which do not maintain their temperature, have a lower respiration, and a higher irritability, in winter and summer. When the cold induces the state of torpor, these phænomena are observed in a still more marked degree, and the animal bears asphyxia, and the privation of food, with comparative impunity. The fact quoted from Legallois, p. 148, and the case of animals buried under the snow, already noticed, are sufficient proofs of this fact.

CHAPTER II.

It appears to me that the case of the young animal, is incorrectly compared with that of the hibernating animal. The former lose their heat whenever they are exposed singly to the influence of the atmosphere, even in moderate temperatures. The hibernating animal, on the contrary, maintains its temperature unimpaired, even when the thermometer is pretty low.

Besides this remarkable difference, there is another which has not, I believe, been pointed out. It is that *sleep* invariably intervenes, in the hibernating animal, between its power of maintaining its temperature, and the order of phænomena, of which the loss of temperature constitutes one, and one so remarkable.

I cannot, therefore, agree with the inferences of this Chapter, and of p. 155. Under ordinary circumstances, the hibernating animal maintains its own temperature. It has, therefore, the full power of evolving heat. The loss of this power is an induced condition, not hitherto noticed, and is observed in the ordinary sleep of these, and, indeed, of all animals, only in a less degree, than in true lethargy, or hibernation.

It is further obvious, that a due distinction is not made between hibernation, and the torpor which may be induced by cold in any animal, and especially the young. Legallois commits this oversight. (See p. 148, and the Œuvres de Lagallois, tom. i. 282.) The first condition is preservative, the second destructive of life.*

To the objections of Dr. Marshall Hall, already given in his own words, may be added those of Dr. Holland, respecting the temperature of young animals. The Doctors agree in this respect—that they regard the inferior power of resisting the influence of exposure to cold, which hibernating mammalia, and very young individuals of the class of birds, and mammalia, generally possess, as not indicative of a constant inferiority in the power of producing warmth; since under different circumstances, and a higher temperature, their animal heat exceeds that of the ordinary temperature of the atmosphere, as much, or more than is known to be the case, with those adult mammalia which possess the strongest power of resisting cold. Dr. Holland found the mean temperature of forty infants, aged from one day to eighteen months, to exceed that of the same number of adults, by $1\frac{3}{4}°$. Twelve children possessed a temperature of 100° to $103\frac{1}{4}°$, whilst in no instance did the temperature of adults exceed, and in one instance only did it reach 100°. The solitary observation made by myself on swallows, shows the high temperature which they are capable of raising themselves, in conjunction with a very feeble power of maintaining it. I do not consider that the objections of either of the physiologists, whom I have mentioned, undervalue the credit or importance of Dr. Edwards's observations on the relations which young and hibernating animals bear to external temperature. Without disturbing the practical inferences to be drawn from what has been stated by Dr. Edwards, they are of great value in themselves, not only by correcting one of his deductions, but as leading us some steps in advance into the inquiry concerning the physical and vital conditions, concerned in regulating the phænomena in question. Dr. Hall considers that a different degree of irritability, possessed by young animals, by hibernating mammalia, and in some degree even by other adult mammalia, which have been exposed to the continued heat of the summer season, is the constant associate of their inferior power of resisting external cold. †

I confess that I am much disposed to adopt this view, which, so far from arresting the progress of inquiry by interposing the mysterious agency of the nervous system as an insuperable obstacle, ought rather to assist our investigation concerning the functions of that system. Dr. Holland, in his experimental inquiries into the laws of life, takes a different view of the subject. He, as well as Dr. Hall, has ably pointed out the fallacy of some of the conclusions of Dr. Wilson Phillip, and others, with reference to the agency of the nervous system; but I cannot help suspecting, that he has carried his objections somewhat too far, and thereby been led to regard the functions he has examined, as more independent of

† See two papers in the Philosophical Transactions on Hybernation and on the Inverse Ratio which subsists between Irritability and Respiration, by Dr. Marshall Hall, 1832.

the nervous system than is really the case. Be this as it may, the view which he has taken has had one important influence on his investigations. It has led him to pay a very careful and undivided attention to the varied conditions of the circulation and respiration, functions, which certainly stand in the closest relation to the production of animal heat.

Dr. Holland lays it down as an axiom, on which he strongly insists, and which he makes the basis of several other principles, which it is his object to establish, that animal heat is in the inverse ratio to the quantity of blood exposed to oxygen in the lungs; and he opposes the opinion of Dr. Edwards, that it is in the direct ratio to the quantity of oxygen consumed. Dr. Holland insists on the opposite effect produced by inspiration and expiration. The latter tends to expel blood from the thorax, and to oppose its return thither, consequently, when the force of the expirations predominates over that of the inspirations, the quantity of blood in the lungs is diminished, and the production of animal heat is increased with the diminished proportion which the blood bears to the inspired air. He illustrates this by various kinds of exercise, by the effects of the exhilarating passions, and of some diseases. He is obliged, however, to admit the acceleration of the circulation, by which the mass of blood is brought more rapidly and frequently under the influence of inspiration, to be one of the causes of the increase of temperature.

A predominance of inspirations, he represents as producing precisely the opposite effect. It increases the quantity of blood in the lungs, and, consequently, its proportion to the inspired air. Hence it is followed by a manifest reduction of temperature. The depressing passions, bodily inactivity, and various diseases productive of congestion of blood in the lungs, but more especially asthma, are adduced as illustrations of this point of the Doctor's views. He attributes the diminution of the production of heat, occasioned by a sudden, or temporary exposure to cold, to be owing to the altered condition of the circulation — the surface of the body becomes pale, because the capillaries cease to be filled with blood — the internal organs are loaded at their expence, and the blood in the lungs bears a larger proportion to the inspired air. Though he differs from Dr. Edwards in regarding their young animals as capable, under favourable circumstances, of raising their temperature to as high, or even a higher degree than that of adults, he of course admits their inferior power of maintaining it, in spite of exposure to cold, and attributes this to the difference in the character of the circulation at different periods of life; that of the young animal, is what he calls external, the blood sent to the surface bearing a larger proportion to that with which the internal parts are supplied, than is the case with the adult, whose circulation possesses, what the Doctor calls the internal character; this change he attributes to the successive development of particular organs. In the young animal, at birth, the internal organs present their minimum of activity, digestion calls an encreased quantity of blood to the chylo-poietic viscera — the lungs continue to receive an increasing quantity of blood as the thoracic viscera are developed under the influence of respiration and exercise. The exercise of the functions of the brain makes a demand in that direction, and the last additional demand is made as the period of puberty takes place by the development of the sexual organs. The habits and passions of the young animal concur with its organization to give the external character to its circulation, whilst those of the adult concur with the organic changes which it has undergone, in producing the preponderance of the internal circulation. It is to these differences, which I have briefly sketched, that Dr. Holland refers the inferior power which the young, compared with the adult animal, possesses, of resisting cold. When cold has constringed the vessels of the surface, a larger quantity of blood is thrown upon the internal organs, which receive it, with a less proportionate capacity of vessels, than in the adult. The proportion which the blood in the lungs bears to the inspired air is increased, and the production of heat, according to the Doctor's hypothesis, is diminished as a consequence. Adult animals in summer have a circulation of a more external and juvenile character than in winter; hence their power of producing heat is liable to a similar reduction by the application of cold. Dr. Holland brings forward several interesting facts, and employs considerable ingenuity of reasoning to support of his views, and to exhibit the importance of their application; they do not, however, appear to contain the whole truth. Why does the vigorous adult maintain the ruddy colour of his well-injected skin in a temperature, in which the infant would be pale and benumbed with cold? Dr. Holland himself remarks, that the greater vigour of the adult enables him to resist cold better than the infant. The reader is left to form his own opinion of what this vigour consists, and I confess, that I think this is to be found in the different condition of the nervous system, which Dr. Holland appears not to take into account in his investigation of the calorific process.

I must return to his objection to Dr. Edwards's view regarding the production of heat, when we shall have to consider the changes of the air effected by respiration.

[*The following experiments, which the kindness of Sir Astley Cooper has allowed me to extract from amongst several recorded in one of his Memorandum Books, dated* 1790, *anticipate and corroborate some of the observations of Dr. Edwards. — They have not to my knowledge been hitherto published.*]

Experiment I. — A young puppy was immersed in warm water, at about 120° Fahr. for one minute and a half. It struggled violently, and during the latter part of this time threw out the air from its lungs. It then remained still for another minute and a half, when its struggles were renewed, at which time it voided its excrement. These efforts were soon over. After remaining still for three minutes, it was put into another vessel containing water of the same temperature. In this it gasped twice or thrice. In ten minutes after its first immersion I opened it — a slight undulation was observable at the lower part of the right auricle of the heart, and there was some motion in the intestinal canal. The action of both ceased in about a minute, and could not be reproduced by the irritation of touching and piercing it. Thus, then, the action of the heart was destroyed eleven minutes after its first immersion.

Experiment II. — A puppy, of the same age as the last, was immersed in water, at about 56° Fahr. For one minute and a half its voluntary motions continued violent — it expired the air from the lungs, and then was quiet. At the end of another minute and a half its struggles were renewed. Its excretions passed off. For two or three minutes after it continued to gasp. It was then thrown into another vessel containing water of the same heat. It gasped at the end of every minute, and ten minutes after its first immersion it was opened. The heart acted vigorously, and there was strong peristaltic motion in the intestines. The action of the heart continued strong for nineteen minutes after it was opened, when it began to undulate — it undulated for four minutes, when all action ceased. At the end of twenty-nine minutes, then, the heart of this animal acted as strongly as that of the first, which died in ten minutes.

Experiment III. — A puppy was immersed in water heated to 90° of Fahr. for a minute and a half. It continued to struggle violently another minute and a half — it remained motionless — when it began to struggle again, and for half a minute continued to do so. At the end of ten minutes it was opened. The lower part of the right auricle undulated, and continued to do so seventeen minutes after it was opened, when no motion could be produced by the stimulus of vinegar, or by the irritation of pricking it.

Experiment IV. — A puppy of the same age was immersed in cold water. The first minute and a half it struggled. It remained quiet for one minute and a half, then struggled again. It continued to struggle at the fourth and fifth minute for one quarter of a minute. At the tenth minute it was opened. The heart contracted strongly in every part, and the intestines moved. Eighteen minutes after opening the animal, the heart contracted more strongly than that of the third experiment did at first, and it continued to undulate six minutes after; so that it acted twenty-four minutes from the opening of the chest.

Experiment V. — A kitten was put into cold water, and after about two minutes ceased to struggle. For some minutes after it had convulsive motions. Twelve minutes after it was immersed, its abdomen and thorax were opened. The heart was contracting, and the intestines moving. The heart continued to do so for half-an-hour.

Experiment VI. — A kitten was immersed in water heated to 100° Fahr. Its efforts seemed rather more violent than those of the other in the fifth experiment. Its convulsions were sooner discontinued. Twelve minutes after its immersion it was opened — no action could be observed, either in the heart or intestines, nor could it be produced by stimuli or irritation.

Experiment VII. — A snake was opened after having been immersed in rectified spirits of wine in order to destroy it. It coiled itself up, became rigid, and was supposed to be dead. In opening it, it shewed some voluntary power, and its heart was found beating strongly. It was put into a vessel of cold water, and the action of the heart became languid and slow. It was then thrown into water heated to about 80° Fahr.—the action of the heart became quick and vigorous, and it began to move freely in the vessel, recovering its voluntary motions, although its body was opened. It was then returned into cold water—its voluntary power lessened, and its heart acted less frequently and vigorously.

Experiment VIII.—The atmosphere being at 61½° Fahr., a thermometer was introduced into the belly of a viper, and it stood at 76°.

Experiment IX.—Water was heated to 99½° Fahr. A viper was plunged in and kept there ten minutes. The thermometer stood in its belly at 92°, but soon fell.

Experiment X.—A viper was exposed to air heated to 102° Fahr., and kept there fifteen minutes. The thermometer stood in its belly at 96°.

Experiment XI.—A viper was exposed to air at 34° Fahr. It became torpid in a considerable degree—it was retained in this stupefaction fifteen minutes. The thermometer stood in its belly at 42°. Nitre and muriate of ammonia, in equal parts, were used to produce this degree of cold. From these last experiments it appears, that the viper is subject to great changes of its temperature by the surrounding atmosphere, which corroborates the idea of Dr. Crawford,—"for these reptiles have their blood but very partially heated; hence their power of resisting high and low degrees of heat must be weak."

Experiment XII.—I put a kitten six weeks old into water, which I kept at 32½° Fahr. by putting in it small pieces of ice. Its mouth was above water. It died at the end of sixteen minutes. During almost the whole of the first five minutes it laid quiet in the water, but its nose, lips, and gums, which were previously pallid, soon after immersion, became of a beautiful vermillion colour. It struggled violently during the second five minutes. Between the tenth and sixteenth minutes it laid quietly. It breathed first quickly, then laboriously, and lastly at long intervals, when it died. Upon introducing a thermometer, immediately after the last breath, into the chest, it stood at 52°. The heart did not act. The blood was of a florid red in the left side of the heart. The peristaltic motion of the bowels still continued. When touched, the heart acted; but was quite motionles, unless thus stimulated. An hour and thirty-five minutes after its apparent death, I poured warm water at 90° or 100° into the chest. The heart began to act, and continued to do so for more than two hours; therefore, four hours after the chest had been laid open. The blood in the mesentery was very florid.

Another experiment, almost in every respect similar, is related as tried upon a puppy a month old. It survived twice as long. Its temperature was less reduced, and its heart continued to act longer. Its lips, nose, and toes, became of a florid colour.

A few experiments on the immersion of kittens seven days old in water of different temperature. Made 28th August, 1832. *By T. Nunnelly.*

With water pumped up from the well at 57° Fahr.

No. 1. Struggled hard for a minute, when it apparently became insensible and passed the fæces; it exspired frequently and made violent attempts to inspire. Some involuntary motion was continued for two minutes and a half, when it was taken out, wiped dry, and placed before the fire at a temperature of about 80; it heated immediately and quickly recovered.

No. 2. Allowed to remain in the water four minutes; the effects were the same as in No. 1, except that it did not completely recover so soon.

No. 3. Allowed to remain under the water for five minutes; during the last minute the involuntary motion was very slight, when taken out, breathed, and recovered in fifteen minutes.

No. 4. Allowed to remain under water for six minutes; this kitten did not breathe for thirty seconds after being taken out, and was half an hour before completely recovering.

No. 5. Allowed to remain under water for ten minutes; for seven minutes and a half some very feeble involuntary motion could be seen, but not more than three minutes after the fifth minute. When taken out it was apparently quite dead. I opened the trachæa, and continued artificial respiration for ten minutes before it made any effort to respire, which for half an hour was very feeble; for an hour it lay in a comatose state, after which it gradually recovered, and in four hours looked much as the other kittens. I allowed it to live for two days, when I déstroyed it, as of course it was unable to suck owing to the opening in the trachæa.

With water at 100° Fahr.

No. 1, 2, 3. Effects much the same as with water at a temperature of 57°F.

No. 4. Remained under water six minutes; this kitten recovered, but was a longer time than that with the water at 57°, and lay for an hour as though asleep without moving, unless disturbed.

No. 5. Allowed to remain under water for ten minutes; motion was perceived for a longer time than in the similar experiment with water at 57°, viz. for nine minutes. When taken out it was quite dead. Dry heat was applied, the trachæa opened, and artificial respiration continued for an hour without success.

Water at 120° Fahr.

No. 1. Allowed to remain under the water for five minutes; it struggled hard as the others did for a minute, and involuntary motion could be seen for four minutes. When taken out, it was quite dead, and although précisely the same means were adopted as with the other cases, they were without success.

In all the kittens that recovered, in proportion to the time they had been under water, was the breathing at the first slow, it then became exceedingly quick and short, and it was some time before they regained their ordinary temperatures, quite as long, or rather longer, after the warm medium, as after the colder.

THOMAS NUNNELLY.

PART IV.

CHAPTER II.

Since the publication of Dr. Edwards's observations respecting the heat of young animals, some interesting researches have been made by his brother, Dr. Milne Edwards, in conjunction with Dr. Villerme. They not only prove the inferiority of the infant's power of resisting cold, but show in a forcible and striking manner, the great practical importance of bearing this fact in mind. It is the custom in France to convey infants, within a few hours of their birth, to the office of the mayor of the quarter in which the nativity took place, in order that the birth may be registered, and the child become posssessed of its civil rights. A careful comparison of the register of births, with the register of deaths, furnished statistical observations on so large a scale, that there can be no room to doubt the correctness of the results. It appeared that the proportion of deaths, within a very limited period after birth, compared with the total births, was much greater in winter than in summer, and that this difference of proportion, was much greater in the northern and colder departments, than in the southern and warmer. The details of this investigation are recorded in a paper which the Doctors have presented to the Institute. They have since continued the inquiry, and the following extract from a letter which I have received from Dr. Milne Edwards, will show the accordance of their results.

"In order to ascertain in a more positive manner than before, whether the mortality of new born children is increased by their being carried to the *maire* immediately after birth, we obtained from the minister of the interior, necessary orders to have the tables of mortality of infants made in a certain number of parishes, where the inhabitants are scattered over a larger surface of ground; and in others, where they are, on the contrary, agglomerated around the *maire*. It appeared evident to us, that if our opinion was correct, the increase of mortality during winters, must be much greater in the former parishes than in the latter, and such is, indeed, the result actually afforded by our tables."

CHAPTER IX.

I have received the following letter on the subject of cutaneous absorption, of which Dr. Edwards is a decided advocate, and, although, it combats the Doctor's opinions, I am induced to publish it, not only because the author, Dr. Corden Thompson of Sheffield, is an extremely well-informed physician, thoroughly versed in physiological reasoning and experiment, on which account his opinion is entitled to respect; but also, because it affords me an opportunity of meeting similar objections, which may be urged by others, against the conclusions maintained by Dr. Edwards, upon this subject.

"At the commencement of his observations on cutaneous absorption, Dr. Edwards states, that from comparative essays made in air and water, Seguin thought himself justified in concluding, that the latter fluid was not absorbed. But, he continues, 'the result of these experiments admits of being viewed in a different light, when we consider certain facts relative to the animal creation.' 'We have seen,' he observes, 'that the batrachia are capable of imbibing, by their external surface, a considerable quantity of water, which passes into the system at large. In such animals, as well as in man, the skin is bare, a condition highly favourable to absorption. The human skin, it is true, from the nature of its cuticle, is less fitted for performing this function, though it continues to exercise it in a high degree.' Such is the language of Dr. Edwards. Now, in the first place, I cannot, for a moment, admit the correctness of the analogy, here assumed, betwixt the exterior surface of the batrachia, and that of man. The former approaches the nature of mucous membrane, the absorbing faculties of which, no one for a moment questions; but, the latter, is essentially modified by the dry superjacent cuticle, which, even the Doctor confesses, is less fitted for for absorbing. To infer from mere analogy, that because the one takes up substances, therefore the other does, is in reality to take for granted, the very thing which ought to be proved. The analogy, in fine, is incomplete; the anatomical elements of comparison are not the same in each case; neither are the functions of the structures in question the same. Pursuing, however, a similar, and I should say, illusive mode of argument, we are told that it becomes impossible to entertain further doubts on the subject, when we witness what occurs in animals, the teguments of which, appear the least susceptible of transmitting fluids. Lizards, for example, possess, as all know, a hard scaly exterior, which should seem an effectual barrier to the passage of an aqueous fluid. Yet, could this be proved to take place, our author imagined he would be justified in concluding *à fortiori*, that a similar transmission exists in the human integuments. Hence, for the purpose of determining that point, he confined an animal of this description in water, in such a way, that the tail, posterior, extremities, and corresponding parts of the body, were alone immersed. The lizard, moreover, in order to excite the activity of the absorbents, had been previously kept in air for a few days; its weight being thus somewhat diminished, any subsequent increase would be rendered more notable. At different intervals of time the animal was weighed; and the weight gradually augmented until the loss it had previously sustained was restored. At this stage, the experiment was stopped, the Doctor being satisfied with its result. I cannot, however, help thinking, that he has overlooked several very important considerations; considerations, which, to me, at least, seem altogether to invalidate the conclusions drawn from this trial. In the first place, whether absorption by the skin do, or do not, exist, that by the lungs cannnot for a moment be doubted; neither can it be doubted, that it is of a most active and energetic description. Now, here we have an animal confined mid-way in water, in a glass tube, and surrounded therefore by a highly moist atmosphere, and yet no notice whatever taken of pulmonary absorption! This omission of itself is fatal to the accuracy of the experiment. But, again; if we revert to the nature of the integuments, such an experiment, we shall see, is not calculated to settle the point in question. The exterior covering of the body is an inorganic production, and like similar substances, capable of mechanical imbibition, after continued maceration in a fluid. This is very easily observed in those parts of the body where the cuticle is thick, as in the hands and feet. When thus saturated, it becomes white and wrinkled. In like manner, the inorganic tegument of the lizard would doubtless absorb a portion of water, after considerable maceration in that fluid, and thus the weight of the animal be increased, totally independent of any absorption, into the interior of the system. Here, then, is another source of fallacy. But, let us even suppose, after continued maceration of the integuments in water, and after having thus been saturated, that eventually a portion of the fluid finds its way into the system, would such an experiment warrant us in concluding, that under any ordinary natural circumstances, any such thing as cutaneous absorption exists? Surely not; and I apprehend we shall find it to be a frequent and radical error with experimentalists, to conduct their experiments under highly unnatural circumstances, and to assign the result, thus obtained, as a natural and ordinary function of the animal economy.

"If we except Seguin, whose admirable experiments on this subject most physiologists are well acquainted with, Dr. Edwards does not notice any other writers, or the facts which they adduce, in disproof of the doctrine which he maintains. Seguin himself, indeed, obtains but a cursory glance; nor are his experiments in any way refuted. Having established the existence of cutaneous absorption, as he conceives, by the evidence already mentioned, Dr. Edwards next proceeds to institute, what may be called, tare and tret computations, relative to the gain or loss sustained by the body when immersed in water. And here again the capital oversight is committted, of neglecting to take *pulmonary absorption* into the estimate. It is needless, therefore, to follow him in his observations on this part of the subject. On the whole, the evidence from which Dr. Edwards seems to infer the existence of cutaneous absorption, is either loose and vague on the one hand, or manifestly fallacious on the other. The doctrine *may be* true; but the reasons here produced, are insufficient to substantiate it."

The principal objection advanced by Dr. Thompson, appears to rest on the experiments of Seguin, and on the impediment, which he conceives, the epidermis must oppose to absorption from the surface of the body. There can, indeed, be no question as to the reality of the obstacle, but it is by no means evident, that it is insurmountable. On the contrary, I think it most reasonable to infer, that if this absorption, or imbibition, can take place through the dense and less pervious coverings of many reptiles, it must, notwithstanding Dr. Thompson's objection, be admitted in the case of the human epidermis. There is an obvious difficulty in bringing this question to the test of direct experiment, arising from the fact, that exhalation, or transmission outwards, is unquestionably taking place, and in general to a greater degree than we can even suspect it to take place in the opposite direction. This very objection, however, furnishes us by analogy, with one of the best arguments in favour of cutaneous absorption, since we have the authority of several investigators, and more especially of Fodera and Dutrochet, to prove that imbibition, and transudation, are reciprocal and simultaneous. Dr. Thompson points to pulmonary absorption, as the cause to which the supposed effects of cutaneous absorption ought to be ascribed. Many experiments, but especially those of Meyer and Magendie, leave no room to doubt the activity of the pulmonary absorption; but, except under very extraordinary circumstances, it does not seem probable that it can be exerted to any considerable extent, except upon the secretions of the mucous membrane itself. It cannot, therefore, be assigned as the inlet to any perceptible accession to the system. The fact, that the expired air is much more charged with watery vapour, than that which is inspired, tends to the same conclusion. If Dr. Edwards has omitted, as Dr. Thompson remarks, to notice the statements of several authors, who have called in question the reality of cutaneous absorption, he has likewise omitted to claim the support of many who have sanctioned it, amongst whom might be mentioned several of those who have written on the subject of diabetes.

Connection of rainy seasons with disease, exemplified in the cases passing through an hospital.

There fell at the Havannah, in seven years, $603\frac{3}{4}$ French inches of rain, viz. in 1821, $131\frac{1}{2}$ inches; in 1822, $53\frac{1}{2}$ inches; in 1823, 100 inches; in 1824, $79\frac{1}{2}$ inches; in 1825, 97 inches; in 1826, 74 inches; in 1827, $68\frac{1}{4}$ inches.

The distribution per month, was as follows: — average for January, $4\frac{1}{2}$ inches; for February, 3 inches; for March, $3\frac{1}{2}$ inches; for April, $2\frac{1}{7}$ inches; for May, $9\frac{1}{2}$ inches; for June, $23\frac{1}{2}$ inches; for July, $5\frac{1}{2}$ inches; for August, $6\frac{1}{2}$ inches; for September, $10\frac{3}{4}$ inches; for October, $10\frac{1}{2}$ inches; for November, $4\frac{3}{4}$ inches; for December, $1\frac{5}{7}$ inches.

From a corresponding table of 4028 sick, who passed through the Hospital, in the course of seven years, it appears, that the *months* in which the sick exceeded the monthly mean in number, were those from May to October inclusive, in which, (with two exceptions) the rain was above the general monthly average of seven inches. And the two exceptions lying between months which had rain in excess, and following the highest amount of rain, (in June,) seem to be accounted for on the principle, that disease may be continued by infection, after the atmospherical predisposing causes have ceased to operate. Again, the *years* in which the rain was above the annual mean of $76\frac{1}{3}$ inches, are mostly those in which the sick exceeded the average annual number of 575 cases.

In both accounts the *temperature* is not to be left out of the question. The annual mean, being 25·6° cent.; that of the winter, is 21·8°; of the summer, 28·5°. The number of sick in seven years is, on an average for the winter, 218; for the summer, 357. Yet it is not probable that heat alone, without the moisture, would cause such a difference, but rather, that the summer would in that case be the more healthy season. — Translated and abridged (with a remark annexed by L. Howard) from *Bibliotheque Universelle,* 16me année, p. 33.

Although it can scarcely be supposed, that any slight addition to the weight and pressure of the atmosphere can have any sensible effect on the animal economy, seeing, that not only changes of this description frequently accompany vicissitudes in the weather, without leading to any obvious consequences, and that by ascending to considerable heights and by descending into mines, we may greatly increase the extent of such changes with perfect impunity; yet, I cannot omit to notice a recent observation of Dr. Prout's, which seems to indicate, that even a slight increase in the weight of the atmosphere, may be the concomitant, if not the cause, of a highly deleterious influence. Dr. Prout had, for years, been in the habit of carefully examining the weight of the atmosphere, when, on the breaking out of cholera, he noticed a slight, but sensible increase of its weight, which maintained itself with constancy for six weeks, when circumstances occurred to suspend the Doctor's observations. He does not attribute any morbid influence to the mere increase of weight of the atmosphere, but rather regards this as the consequence of a deleterious and heavy gaseous principle, diffused through the lower regions of the atmosphere. For a detailed account of Dr. Prout's experiments and opinions upon this subject, the reader is referred to his paper, which is about to appear in the transactions of the British Association.

Those who descend to a considerable depth in diving bells, are subjected to the greatest atmospheric pressure to which it is easy for man to expose himself; but its effect is necessarily complicated with that of the deterioration, and want of motion of the air. Notwithstanding these combined sources of inconvenience, it is well known, that workmen may continue their operations, during several hours of the day, at a depth of many feet. From personal experience of the effects of this situation, during about half-an-hour, I may state, that the only painful sensation is that occasioned by the pressure on the membrane of the tympanum, which is felt on first descending; but soon ceases, when the equilibrium between the interior and exterior of the body is restored. The blood is very much driven from the surface of the body, producing in general extreme paleness. When the superficial capillaries are not emptied, which I observed to be the case with one individual, who had, what is styled a fixed colour in his face, they become completely livid. According to the theory of Dr. Holland, the production of heat ought to be reduced nearly to its minimum, since the proportion of the blood in the lungs to that of the inspired oxygen must be great. The heat, however, becomes oppressive. It seems by no means improbable, that increased pressure of the atmosphere may be one of the causes requiring the exercise of the function, which I have ascribed to the spleen.

CHAPTER XVI.

In considering the observations of Dr. Edwards on the changes of the air in respiration, there are two points which appear to be particularly interesting and worthy of attention. The experimenters who have succeeded him had arrived at different conclusions, more especially with respect to the consumption of oxygen, and the alterations in the quantity of nitrogen. As these differences could not be attributed to errors of observation, they tended to render the subject more complex and puzzling, until Dr. Edwards, by instituting a series of experiments, continued through the different seasons of the year, at once confirmed and explained the discrepancies of his predecessors, and made a valuable discovery respecting the influence of climate and season. The other point to which I have alluded, refers to the part of the body in which the changes in the respired air are effected. It had been a subject of question, whether the carbonic acid expired, was not formed immediately in the lungs, by the combination of the oxygen of the atmosphere with the carbon of the venous blood. According to another view, it was supposed that whilst a portion of the oxygen of the atmosphere was taken up by the blood, and carried with it in its circulation, and at the same time carbonic acid was thrown off from the lungs, having been previously taken up in the course of the circulation. Dr. Edwards appears to have settled this question, which seemed previously to be nearly balanced, by confirming the latter view. We have, therefore, in the function of respiration, not only a striking instance of the transudation, and imbibition of the gases through the membrane, but also of their simultaneous passage in different directions. In both of these respects, Dr. Edwards has anticipated Fodera and Dutrochet, whose observations have further elucidated them, and pointed out analogous phænomena in other parts and functions. Since the publication of Dr. Edwards's work, some further experiments on respiration have been performed by those careful and accurate operators, my friend William Allen, and his associate, W. H. Pepys; and others of equal interest, by my friend S. Broughton. Before I notice the facts, which these experimenters have either confirmed or added, it appears necessary that I should notice the discoveries and views of Dr. Stevens, which throw the most important light on the process of respiration. These views were not the offspring of speculation, which he has sought to confirm by subsequent experiments, but they forced themselves upon him, whilst he was investigating the changes of the blood, and the phænomena of fever; and it seems necessary that I should remark, to set aside any prejudice which may exist in the mind of the reader, that the Doctor's physiological observations respecting respiration, stand upon their own distinct merits, and are by no means compromised by his pathological and therapeutical doctrines. In order to keep the subjects distinct, I purposely refrain from offering an opinion respecting the last mentioned points; yet, I cannot withhold the expression of my admiration of the zeal, perseverance, and self-devotion, with which the Doctor has pursued his investigations respecting them, under the most arduous, and, perhaps, perilous circumstances. One of the most striking facts which the Doctor has brought into notice, is the powerful attraction which exists between oxygen and carbonic acid. It was so fully admitted amongst chemists, that carbon in carbonic acid is united with its maximum dose of oxygen, that the idea of attraction between carbonic acid and oxygen was rejected from *à priori* reasoning by several able chemists, to whom the Doctor mentioned the subject. The fact, however, is clearly proved by the experiments of Dr. Stevens. If a receiver filled with carbonic acid, and closed by a piece of bladder firmly tied over it, be exposed to the atmospheric air, the carbonic acid, notwithstanding its superior specific gravity, rapidly escapes, and does so without the exchange of an equivalent portion of atmospheric air; the bladder is consequently forcibly depressed into the receiver. If the converse of this experiment be tried, and the receiver, containing atmospheric air be tied over with a piece of bladder, or thin leather, and then be immersed in carbonic acid, this gas will so abundantly penetrate the membrane, and enter the receiver, as to endanger its bursting.

Dr. Stevens had repeated opportunities of verifying these facts, during a stay which he made at Saratoga, in the United States; the springs at which place liberate a large quantity of carbonic acid. In the high rocks it often collects in considerable quantity and purity, and experiments on dogs and rabbits are often made for the entertainment of strangers, as at the Grotto del Cane, near Naples. This rock stands by itself in a low valley, through which there run two currents of water, the one fresh and superficial, the other beneath, and charged with salts and carbonic acid. A current of this water rises to some height in a cavity of the high rock, which appears to have been formed by a deposition of earthy salts from the water. It has a conical figure, the base of which, is below the surface of the ground, is about nine feet in diameter. It rises about five feet from the ground, where it is truncated, and presents an aperture a foot in diameter. The cavity of this rock is conical, like its external figure—the water appears formerly to have overflowed the summit, but it now rises in general only about two feet above the ground. In the three feet above, the liberated carbonic acid collects, but it varies very much, both in quantity and purity, notwithstanding the sides of the rock are thick and impervious, and the superior specific gravity of the gas, which is constantly liberated in large quantity. The removal of the carbonic acid appears to be effected by virtue of that attraction, which Dr. Stevens has pointed out as existing between it, and the oxygen of the atmosphere. When the air is somewhat agitated by wind, a taper will burn in the cavity of the rock, almost as low as the surface of the water; but when the air is calm, the taper is extinguished much nearer the top of the rock. By luting a large funnel over the aperture, so as to exclude the influence of the air, the rock became filled with carbonic acid, which the Doctor collected for his experiments, at the mouth of the funnel. Dr. Stevens took advantage of the facilities afforded by this rock, to multiply and vary his experiments, the results of which, were not only perfectly satisfactory to himself, but to many individuals to whom he exhibited them. This attraction, which the

Doctor has pointed out, is not only to be regarded as an important agent in the function of respiration, but throws considerable light on the constitution of the atmosphere, since it accounts for carbonic acid, notwithstanding its greater specific gravity being found in equal proportions at every elevation to which we can ascend, instead of being collected at or near the surface of the earth.

Experiments similar to those of Dr. Stevens, and attended with the same results, have been published in an American Journal, by Drs. Faust and Mitchell, who have anticipated Dr. Stevens, in committing them to the press, without making any allusion to his discovery, although there can be but little doubt but they were in a degree acquainted with it, as the Doctor himself had related the result of his previous experiments, not only to other professional individuals in the United States, but even to the very editor of the Journal in which the American papers were first published. It is stated also, that this gentleman took a part in Dr. Mitchell's experiments. Dr. Stevens formed his views, respecting the attraction of the atmosphere for carbonic acid, and committed them to paper, in 1827, at which time, he resided in the West Indies. In 1828, they were mentioned, or shewn in manuscript, to several persons in this country: and in France, which the Doctor visited in 1829, more than one chemical philosopher was disposed to dispute the existence of such an attraction—Dr. Edwards himself was amongst this number.

Dr. Stevens went to the United States in 1830, in the seventh month (July). The American experiments commenced soon after, and were published before the end of the year. The reader, I trust, will allow the excellence of the principle, *suum cuique*, to be a sufficient apology for the introduction of this statement.

Although this mutual influence, between carbonic acid and oxygen, may not now be doubted, yet different views may be entertained respecting its nature. The views and discoveries of Dalton, respecting the admixture of gases and vapours, appear to bear the closest relation to this subject, but they do not seem to be adequate for the explanation of all the phænomena. Although the particles of a particular gas, or vapour, may repel each other, yet allow those of a different gas, or vapour, to come between them, and thus allow, what Dr. Mitchell styles, the penetrativeness of one and the elasticity of another gas, to promote their intermixture; yet, it is not very evident, that this theory can explain their energetic union, when a membranous septum has been interposed between them, and still less, why carbonic acid should be so much more powerfully brought into admixture with oxygen, than with nitrogen, or hydrogen, which are much rarer gases.

It has been supposed, that the phænomena pointed out by Dr. Stevens, are of the same nature with those which Dutrochet has described under the terms, endosmosis and exosmosis, but unless we are to regard every instance, in which one fluid diffuses itself through another, or passes through a porous body, as an instance of endosmosis, or exosmosis, an idea which Dutrochet himself would reject — there is, notwithstanding, some analogy — a striking difference between the phænomena in the cases of endosmosis related by Dutrochet; the intervening septum performs a very important part in influencing the movement of the fluids. This is most strikingly exemplified in the application which he makes of his principle to the circulation of the sap in the roots and branches of plants. In the phænomena pointed out by Dr. Stevens, the impulse resides in the gases themselves, and all we can say respecting the septum interposed between, is, that it does not prevent their union.

Although Dr. Stevens informs us, that Dr. Edwards offered some objections to his views, respecting the removal of carbonic acid from venous blood in the lungs, by virtue of an attraction for that acid, inherent in the inspired air; yet, I must confess, that after a careful consideration of the subject, the views of Dr. Stevens, instead of militating against the observations of Dr. Edwards, are in the most satisfactory accordance with them.

In order to understand the application of the attraction pointed out by Dr. Stevens to the function of respiration, it will be necessary to be aware of a few facts relating to the blood; some of these were more or less known prior to the experiments of Dr. Stevens, but he has the merit of greatly extending, as well as applying them. All acids impart a dark colour to the blood. With respect to most acids this colour remains, although the added acid be afterwards saturated. Carbonic acid forms an exception, for on the removal of this aerial acid the blood resumes its bright and arterial colour. Alkalies, like acids, darken the colour of the blood, but salts produce a bright and vermillion colour, when added to the colouring matter of the blood. The alkaline carbonates require particular notice. The acid is so feebly held by the base, that in some respects they conduct themselves as alkalies, and in particular, will restore the blue colour to reddened litmus. Dr. Stevens believes that this circumstance has led some chemists of great celebrity, to admit the presence of free alkali in the blood, whilst he takes an opposite view, and believes that in venous blood at least, there is a superabundance of free carbonic acid, which, however, is soon removed by exposure to the air. This opinion seems to be confirmed by the fact, that if water, holding carbonate of soda, and carbonic acid in solution be added to the blood, a deep and venous hue is produced. After a short exposure to the air the carbonic acid is removed, as Dr. Stevens believes, by the attraction already noticed, and the blood is reddened by the carbonate of soda, the influence of which, is no longer controuled by the redundant acid. The recently separated serum of venous blood has no effect on turmeric paper, although it has after a little exposure to the air.

There are many other phænomena connected with the blood which Dr. Stevens has noticed; for these, I must refer the reader to the Doctor's own interesting work. Those which I have already mentioned, will suffice to enable us to appreciate the light which the Doctor has thrown on the function of respiration. I must, however, take the liberty of remarking, with respect to the curious phænomena he observed with the help of a powerful solar microscope, that I believe some fallacy attends them, in consequence of the heat unavoidably applied to the object, brought into the field of the instrument in bright daylight. It will, I am sure, be understood, that I am not invalidating the Doctor's evidence, when I ascribe many of the globular appearances which came under his view, to the disengagement of gases effected by the heat in question. The best compound achromatic microscopes, not only possess a superior power, but are exempt from the objections which I am now urging against the solar microscope. The account given in this volume of the microscopic appearances of the blood, as seen through a compound microscope of the highest quality, does not coincide with the description given by Dr. Stevens. The explanation which I have offered, will, I believe, satisfactorily account for the difference. Though I do not regard the microscopic phænomena, described by Dr. Stevens, as affording a correct view of the structure of the blood, I am not disposed to reject them, but rather to query, whether they may not lead to some curious observations in the disengagement of gases from fluids.

To return to the subject of respiration. The views of Dr. Stevens accord with the opinions of those who reject the idea of the formation of carbonic acid as taking place in the lungs, by the immediate union of the oxygen of the atmosphere with the carbon contained in the venous blood. We have seen that Dr. Edwards is of this number, inasmuch, as he believes, the formation of carbonic acid to take place throughout the body. Dr. Stevens, however, does not regard the air as passively receiving the carbonic acid as it is liberated from the blood, which had not only held it in solution, but actually imbibed it. He considers that it is actively removed by the attraction existing between oxygen and carbonic acid, which overcomes the weaker attraction by which the acid was united with the blood. When the blood has lost its carbonic acid, it presents the bright vermillion tint which naturally belongs to its colouring matter, and salts, when combined. When the venous blood gives up its carbonic acid, it receives in exchange, a portion of the inspired air, which is chiefly at the expence of the oxygen. The proportion of this gas, abstracted from the inspired air, being very nearly, and often exactly, the same as that of the carbonic acid added to it, Dr. Edwards has pointed out the circumstances under which the quantities differ. We must not, however, suppose that it is only carbonic acid which is exhaled, or oxygen which is received by the blood and lungs. The experiments of Allen and Pepys, as well as those of Dr. Edwards, have proved that there is an interchange of other gaseous principles. The reddened and oxygenated blood having returned to the heart, is diffused over the system, imparting animal heat in proportion to the quantity of oxygen which it gives up for the production of carbonic acid. It receives this carbonic acid in exchange for the oxygen which it has lost, and is darkened by its presence, which counteracts the effects of its salts. This, I believe to be a concise sketch of Dr. Stevens's theory of respiration ; it is far from clashing with Dr. Edwards's observation respecting the disengagement of carbonic acid ; it seems, on the contrary, satisfactorily to account for cutaneous respiration, since, wherever the atmosphere is exposed to a vascular part, its oxygen must promote the separation of carbonic acid from the venous blood. If we apply this view to the respiration of animals who live in water, and admit that the oxygen dissolved in that fluid, separates carbonic acid from their venous blood, we have another argument in favour of an actual attraction existing between oxygen and carbonic acid, since the discoveries of John Dalton, and the penetrativeness of Dr. Mitchell, are quite inapplicable to the subject.

The experiments of Allen and Pepys to determine the changes produced in the air by respiration, which have been made subsequent to the publication of Dr. Edwards' work, are a continuation of their former researches, and were made solely on the respiration of birds. These enquirers conducted their experiments in the same method as that which they had formerly employed, and in no instance compromised the life or health of the animal. The birds were placed in a small glass chamber, which received its supply of air from one gasometer and parted with it into another at certain intervals. The most careful analysis was made, both of the gas supplied to the animal, and of that which it had respired; every calculation being made which the state of the barometer and thermometer, and the volume of air existing in the receiver containing the bird, and the tubes leading to it, could require; their results may be stated as follow :—

After one hour and twelve minutes respiration, the amount of gasses employed being originally

	oxygen	*azote*	*carb acid.*
Cubic inches ..	245·59	61·41	
There remained	195·61	90·11	21·27

shewing a loss of 28·71 of oxygen beyond the volume converted into carbonic acid, and a gain of 28·70 of azote. The head and other parts of the pigeon in which the state of its vessels could be seen, were of a bright red. In a similar experiment which lasted one hour and ten minutes, 24·74 cubic inches of carbonic acid were produced, besides which, 21·75 of oxygen were lost. In atmospheric air, 35·80 cubic inches of carbonic acid were produced in sixty-nine minutes. In a mixture of oxygen and hydrogen with a portion of azote, a pigeon in the course of twenty-six minutes, produced 17·62 cubic inches of carbonic acid; 35·48 of hydrogen were lost, and 35·23 of azote were added.

Allen and Pepys were not acquainted with the researches of Dr. Edwards, and as they inclined to the belief, that the volume of oxygen lost was replaced by an equal volume of carbonic acid, their delicate and accurate experiments form a valuable confirmation and supplement to those of the Doctor. The experiments of S. D. Broughton, relate to the same subject, but were performed in a somewhat different manner, and supply us with new and valuable facts. He placed a variety of animals in receivers of considerable capacity compared with their bulk; he filled them with different gases, in which he allowed the animals to remain until they were nearly or quite dead, when he examined their state and that of the gas remaining in the receiver. His most important and numerous experiments relate to the respiration of oxygen. He found, as Allen and Pepys had done, that animals at first bear this kind of respiration with apparent impunity, that the pure oxygen at first acts as a stimulus, and that all the parts of the body in which the state of the vessels can be seen are injected with bright arterial blood. Though this florid colour continues, the powers of the animal progressively sink, he falls into a state of suspended animation, and inevitably dies in the course of a few hours if suffered to remain in the gas; and even if taken out alive, the injury which he has received may be fatal. This effect is not to be ascribed to the deterioration of the air in the receiver, as in the case of an animal dying in a given quantity of atmospheric air. The remaining oxygen is still sufficiently pure to support the vivid combustion of iron-wire, and to produce a repetition of effects on a second and third animal similar to those described as occurring with the first. The animal is found to have all its sanguiferous vessels filled with bright arterial blood, and its temperature is found to have fallen several degrees, even when taken out before life is extinct. The fatal effects of the respiration of pure oxygen gas are confirmed by the experiments of Sir H. Davy, and by those of Drs. Prout and Magendie. The blood is observed quickly to coagulate after the respiration of this gas. S. D. Broughton tried the effects of the gaseous oxide of nitrogen, commonly known as the exhilarating gas; this can be respired longer than other gases, yet death takes place sooner than in pure oxygen. The blood continues fluid and thin. He found animals die very quickly in sulphuretted hydrogen; indeed it is impossible to conceive death more instantaneous than that which I have myself seen take place in a sparrow, which Professor Thenard introduced into this gas. This effect of sulphuretted hydrogen, appears to have been known to the ancients long before chemistry existed as a science, as may be inferred from the expression, *graveolens aornus*, employed by Virgil, as well as from some remarks of Pliny, respecting a fountain not far from Rome. He found carbonic oxide, though a fatal gas, to be less promptly so than sulphuretted hydrogen; and it is worthy of remark, that the interior of animals killed by it, was not only gorged with venous blood, but seemed unusually hot. The results of the preceding experiments, together with some others detailed in this volume, suggest the following observations:—

We have seen that Dr. Holland has objected to the theory maintained by Dr. Edwards, that the amount of animal heat is in proportion to the consumption of oxygen, and endeavours to substitute in its place, that it is in the inverse ratio of the quantity of the blood to the inspired air. It is evident from Dr. Edwards's own words, that by the consumption of oxygen, he means its conversion into carbonic acid, since he admits the absorption of this gas during summer, when even adult animals are considered by him, to lose a part of their power of producing heat.

The researches of Broughton have shewn, that when animals inspire this gas in its pure state, they sink in temperature; and the experiments of Allen and Pepys have shewn, that a larger quantity of this gas is consumed than is replaced by carbonic acid, the production of which, is diminished. It is also remarked, that those external parts, in which we can observe the state of the circulation, become manifestly injected; hence we must have that condition of the circulation, which Dr. Holland regards as the most favourable for the production of animal heat; yet we have seen that the results alluded to oppose this conclusion. I am inclined to believe, that the production of animal heat bears a close and necessary relation to the quantity of carbonic acid produced. I agree so far with Dr. Holland as to believe, that when the lungs are greatly loaded with blood, the changes in it effected by the air are impeded, and that the temperature may sink; but this I conceive to be the consequence of the diminished production of carbonic acid. On the other hand, the effects of pure oxygen evince a striking difference between animal heat, and that of ordinary combustion. The carbonic acid by which the blood is darkened, is strikingly removed; but contrary to what one would have suspected, *à priori*, its further production is impeded: hence, not only the diminution of temperature, but also, the universal redness of the blood. It has been shewn by some of the experiments of Sir Astley Cooper already related, that immersion in ice-cold water, had the effect of inducing a singularly bright arterial colour in those parts in which the blood is collected. This effect, like that of animals dying in oxygen gas, was more to be ascribed to the suspended carbonization, than to the increased decarbonization of the blood.

Without attempting to draw any express conclusions from the experiments of Dr. Edwards, with reference to temperature, season, and age, beyond those which the Doctor has himself offered, I cannot refrain from remarking, that there is no part of the Doctor's work which possesses greater practical importance and utility. In conjunction with the researches of Dr. Curry of Liverpool, they will afford the most valuable assistance in the regulation of clothing, of exposure to the open air, of confinement within doors, and of the application of the various forms of baths.

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