AN INQUIRY

INTO THE CAUSES OF RESPIRATION;

OF THE

MOTION OF THE BLOOD;

ANIMAL HEAT; ABSORPTION;

AND

MUSCULAR MOTION;

WITH PRACTICAL INFERENCES.

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LIVERPOOL.

SECOND EDITION.

LONDON:
LONGMAN, REES, ORME, BROWN, GREEN, & LONGMAN.

MDCCCXXXIII.

PREFACE.

In consequence of a change in the arrangement, by which some repetition, otherwise necessary, is avoided, and additional perspicuity, it is hoped, attained; a change in the title of this edition became indispensable. The discussion of the mechanism of Respiration is made to precede that of the Motion of the Blood, which constituted the leading article in the former edition.

The doctrines, supported in the first edition are, in this, more amply discussed; are supported by additional proofs; and are relieved, it is hoped, from the objections which have been raised against them, by satisfactory replies.

The number of the articles treated of in this edition, has been considerably extended. In addition to Respiration and the Motion of the Blood, three other articles have been introduced, Animal Heat, Absorption, and Muscular Motion. The four first articles in this volume are so connected and interwoven, that an acquaintance, with every succeeding article, throws some new light upon all those which preceded it. Many points, in the first article even, could not receive their fullest elucidation, without arguments, which could not be introduced previously to the discussion of the last. It is requested of the reader, therefore, that if he should meet with any doctrine, or supposition, which might appear to him untenable, he will suspend his final decision respecting it, until he shall have perused the whole of these articles.

The fifth article, that on Muscular Motion, is unconnected with the others, and may be perused with every advantage separately.

Since the publication of the former edition of this work, the subjects investigated, and chiefly in consequence of that investigation, have attracted no common share of attention, both in Europe and America.

The Author, of course, has not been inattentive to that discussion; and has introduced into this edition whatever seemed to be important that was disclosed by it, or that has resulted from his own frequently repeated reflections, during the long period that has elapsed between the former publication and the present.

Most of the additional subjects contained in this volume, have appeared in other publications, so that they are not altogether new to the public.

The opinion has been advanced, and prevailed to a considerable extent, chiefly among those, who are disposed to form their judgement according to the assertions of others, that the doctrines which I have advanced, not being supported by experiment, are in a great degree speculative. This opinion has probably arisen, in some degree, from the situation of the Author, as a private practitioner, in a provincial town, debarred from those sources of information, and facilities of investigation, which are only afforded at the great seats of science, and with the co-operation of the learned, and which seemed to be necessary for the successful prosecution of such inquiries, as he has ventured to undertake. I do not admit this inference to be just. I have in every instance carefully ascertained the stability of the foundation upon which my reasoning rests ; and I have yet to learn, in what instance that stability has been proved to be insecure. For that stability, indeed, I have looked more to the field of observation that has been long open to all ; and to the extension made to that field by the experiments of others rather than by my own. No less reliance, it is hoped, will be placed upon the deductions drawn from experiments, made for a purpose different from that which they are now brought to support. In the present state of physiological science, new experiments did not seem in many instances to be required. A vast number of well attested facts are to be found in physiological and anatomical writers ; scattered and unconnected, indeed, and often seemingly incompatible. To discover the bearing and end of these disjointed and incongruous materials, seemed to be more required, than an extension of their number. In the language of the law, a summing up of the evidence adduced, seemed to be more wanted, than additional witnesses.

Brilliant and successful examples of this mode of pursuing physiological and other researches, are readily supplied. All the facts on which the doctrine of the Circulation of the Blood are founded, were known long before the days of Dr. Harvey. But in those parts of my Inquiry, which could not be pursued without fresh experiments, as they related to points altogether new, I have not shrunk from the task of making them. For ascertaining the power generated by the elasticity of the lungs, that engine, widely and variously effective, which lurked unknown and unsuspected in the fabric of man, and of all those animals whose constitution is considered the most perfect; and which has been proved to perform an essential part in the process of Breathing; in the Circulation of the Blood; in the supply of that vital fountain; in the formation and regulation of Animal Heat; and in the discharge of the recrement necessarily engendered by the rapid renovation of the living body; and which, it is probable, may be found to extend to other offices, in a way not yet discovered; my experiments, both numerous and laborious, are recorded in the Philosophical Transactions for 1821, and are, I believe, universally admitted to be conclusive, with respect to the object for which they were devised.

I have not, however, been insensible to the disadvantages of my situation, and have repeatedly endeavoured to render it more favourable. With that view chiefly I became a candidate for a medical chair in the university of Edinburgh, on the death of Dr. Gregory; and subsequently, and with more reasonable prospects of success, as the medical professors of that Institution, by whose opinion the chancellor might have been supposed to have been guided, were generally favourable to my pretensions; in the university of Glasgow, on the death of Dr. Freer. What influence a success in any one of these applications would have had upon the present production, it is in vain now to conjecture.

Some writers, who have given what it would appear an unwilling assent to the leading proposition, the influence of atmospherical pressure on the motion of the blood, have disputed my claim to originality, maintaining that the same doctrine had previously been advanced by others, particularly by Dr. Wilson, formerly a physician in Newcastle upon Tyne. I trust that it will not be deemed impertinent, if I allude briefly to the origin of my opinions on the subject, and endeavour to estimate how much may be due to Dr. Wilson.

In the course of my medical education, I became a visiter to the Royal Medical Society of Edinburgh, at the introduction of my friend, Mr. John Murray, who afterwards became so celebrated as a writer and lecturer on chemistry: for I never became a member of any of the Medical Societies at Edinburgh, having served what may be called my society-time, in the literary and theological societies of that place, and being indisposed to enter again the arena with students, generally by several years younger than myself. At this visit a paper was read, advocating thc inutility and even danger of an acquaintance with mathematics and the physical sciences, as a part of a medical education. I disapproved of the doctrine, and intended to have given it my reprobation, a privilege which was allowed to a visiter; but I waited to hear the sentiments of the members of the society, and while I was still hesitating about the time of addressing the society, and considering perhaps what I had to say, the president declared the debate to be closed. I retired from the meeting, not at all well satisfied with myself, for allowing such pernicious doctrines to pass without receiving the condemnation they deserved. The subject took exclusive possession of my mind; and in the fancied oration, in which I was refuting the doctrine, and in which I passed a sleepless night, the first born of a numerous progeny from the same parent, I selected the circulation of the blood as a part of a medical education, to the understanding of which the condemned sciences were necessary. After successive unsuccessful trials to get the blood round according to the scheme of Dr. Harvey, I began to discover the vestiges of powers which had not been noticed by physiologists, and which appeared necessary to the accomplishment of the blood's circulation. I, early in the morning, repaired to the residence of my friend Mr. Murray, pronouncing the *Heureka*, not aloud I believe, but as enthusiastically as it had been done by the ancient sage. I told him that I had discovered the causes of the motion of the blood, and explained to him my views, which he considered very plausible. I told him at the same time that I should make it the subject of my Thesis. He smiled and dryly replied, that he was afraid the doctrine would not be considered sufficiently orthodox for the college,—an expression of which I knew the meaning better afterwards.

I accordingly made the causes of the motion of the blood the subject of my Thesis. I found the prediction of Mr. Murray, respecting the doctrine advanced not being deemed sufficiently orthodox, fully verified. Some of the most eminent members of the medical faculty, into whose hands the Thesis fell after it had been printed, condemned it in no measured terms, and plainly intimated that, if the nature of the production had been earlier known, I should not have received a degree at that term, or perhaps at any other, from that university. The condemnation which my production received, and the discouragement I met with from that quarter, were more than counterbalanced by the consolations and approbation of a fellow student, who was also my most intimate friend, who took a degree in medicine on the same day that I did, and whose future career has fully justified the high value I then placed on his opinions, I mean Dr. Thomas Thomson, the present illustrious professor of chemistry in Glasgow. He bade me not to be disheartened, as the Thesis was a production that would do me no discredit, and strongly urged me to pursue the subject as it related to a branch of medical science, that in his opinion was still involved in great obscurity.

The few days I passed at Edinburgh after I had taken my degree, I employed in ransacking the libraries of that place, for such productions as might throw a light upon the subject, to which I had from this time devoted myself. In the library of the Royal Medical Society of Edinburgh, I found bound up with a number of other pamphlets, the treatise of Dr. Wilson, of Newcastle, who had taken, in many respects, the same view of the causes of the motion of the blood in the veins that I had done. It is impossible to say, whether I was more mortified or gratified, by the perusal of this work of Dr. Wilson's; mortified at finding that I had been in some respects anticipated in my views; and gratified that those views were not so unreasonable, but that they had been entertained by some other person. The name of Dr. Wilson, I had never heard mentioned by any lecturer, nor seen in any publication; nor since that time, and previously to 1815, though I have been constantly on the watch for it, have I discovered it to be noticed, excepting by the industrious and candid Haller, who states that Dr. Wilson, an English physician, had contended that the blood in the veins was returned to the heart by atmospherical pressure, a doctrine, the celebrated German observes, not to be despised. So completely unknown had the treatise of Dr. Wilson become, that, if I had been so disposed, I might have been silent about its existence, without any chance of the dishonourable concealment being detected. Dr. Wilson says, that the heart pumps the blood from the veins, but he assigns no cause for the expansion of the heart that was required to make it act as a pump. On this account the doctrine seems to have made no impression. Dr. Brown Langrish contended that the heart was dilated independently of the force of the returning blood, and assigned as the cause of that dilatation, the action of the muscles of the heart, which were supposed to be antagonists to those by which it was contracted. The existence of such muscles was refuted and the doctrine rejected. In my Thesis, I have assigned a cause for the expansion of th heart which must be allowed to be efficient, so far as it goes, in all animals, and in some classes perfectly so; but it must be admitted, that in the class of animals in which it was supposed to operate, its influence must be feeble, and by no means adequate to the effect ascribed to it. It is believed that if the doctrine had been left in the state in which it was placed by the treatise of Dr. Wilson, and my own Thesis, it would still have been unheard of. It was not till after the publication of the former edition of this work, in 1815, that the doctrine began to make any impression on the public mind.

But neither the hypothesis maintained by Dr. Brown

Langrish, which was of a date more ancient than the time even of that Philosopher, nor that of Dr. Wilson, had any reference to the mechanism of respiration. On this subject my views, so far as regards the mammiferous animals, have not, it will be admitted, been anticipated by the most distant or obscure allusions of any preceding writer,

It was my intention, as I have announced at the commencement of the following work, to have made a more extensive application of the physiological doctrines, to the treatment of disease, than it will be found I have done. Various causes have concurred to limit this application. In the first place, the physiological discussion, has, in several instances, extended to a greater length than I expected. So that the limits assigned to the work, were nearly filled up by that discussion. I was still, I confess, fearful of the consequences of the proposal of untried remedial methods, considering practical injury less excusable than speculative error. I was sensible also, that to those who understood or approved of the principles, a practical application of them would readily occur as occasion might offer ; and to those who did not understand or rejected the principles, no recommendation of a remedial nature could be advantageous or safely entrusted.

In some instances, I have ventured to fulfil the plan I had contemplated. One of these I particularly recommend to the notice of the Medical Profession, that is the method of treating consumption detailed at the end of the article on Respiration. In a paper which I read many years ago, in the Literary and Philosophical Society of Liverpool, and which was subsequently printed, and had a local circulation, I stated, that a surgical operation held out the only prospect of curing that disease, and described the nature of the operation, which I supposed would be required. The operation was performed according to the plan which I proposed. The causes of its want of success were clearly ascertained. To obviate these causes, a modification and extension of the operation were projected. The operation was attempted according to this new plan ; when new difficulties presented themselves ; not however it is hoped of an insuperable nature. But I dare not venture to proceed further on my own responsibility; but shall wait, not without hope, till I know whether the profession will deem my suggestions worthy of their countenance and support.

The lamentable ravages annually made by this disease, among the flower of the human race, will be an apology, I trust, for any proposition, however ineffectual it may prove, that may be made with a view to its cure.

RESPIRATION.

Respiration is that function of animals by which a quantity of air is alternately admitted into, and expelled from, the lungs. It is synonymous with breathing. The process by which air is admitted is termed inspiration, and that by which it is expelled, expiration.

The mechanism of inspiration consists in the enlargement of the thorax, which is increased both in diameter and in axis. The axis of the chest is increased, or its cavity is deepened by the movements of the diaphragm. This tendinous and muscular flooring divides the trunk of the body transversely, and separates the chest from the abdomen; it assumes at all times in health a form more or less conical ; having its apex or convex side turned towards the cavity of the chest, and its base or concavity facing the abdomen. It is by the decrease of the convexity of the diaphragm, or by its approach to a plane, that the axis of the chest is augmented or its cavity deepened. The contraction or shortening of the muscular fibres produces the change just mentioned in the form of the diaphragm ; they are brought by that means nearer to the direction of straight lines ; the altitude of the cone is lessened ; and as the area of its base necessarily remains the same, its apex is rendered more obtuse, or its convexity is decreased. The dimensions of the cone formed by the diaphragm are diminished, and the capacity of the chest is extended in an inverse proportion.

The chest is increased in diameter by the contraction of the intercostal and pectoral muscles. In consequence of this contraction, the ribs are drawn nearer to each other, and, as the position of the superior row of ribs is fixed, the inferior rows are all drawn upwards. From the peculiar nature of their articulation with the back bone, the assent of the ribs is necessarily accompanied with an extension of the arches in an outward direction, and, in consequence, with a general increase of the diameters of the chest.

During the process of expiration the muscular fibres are relaxed. The diaphragm assumes its former extension and shape; the ribs return to the position from which they had been drawn, and the capacity of the chest is reduced to its former dimensions.

As during these changes a free communication at all times subsits between the cavity of the chest and the external air through the windpipe; and, as the other inlets into this cavity are all subjected to regulations which prohibit the admission of a greater quantity of matter into it at one time than at another; to preserve the equilibrium of pressure between the air within and that without the chest, a quantity of this fluid must be alternately expelled and admitted.

The diminution of the altitude of the cone formed by the diaphragm, usually termed the descent of this partition, is universally acknowledged to follow of necessity the contraction of its muscular fibres. The causes by which the altitude of the cone is again restored are not so evident.

In our attempt to investigate the causes of so important a movement, it will be proper to examine previously the efficacy of those powers to which it has generally been ascribed.

The simple relaxation of the fibres of so extensive and thin a substance could unquestionably have no share in the production of this effect. But, for restoring the altitude of the diaphragm, the relaxation of the muscnlar fibres is necessary to the favourable operation of other powers.

The agents by which the diaphragm is supposed to be elevated (to use the language commonly employed to describe that movement) in respiration, are the abdominal muscles; and the manner in which they are believed to produce the effect is generally explained as follows.

When the diaphragm is contracted, as is acknowledged to be the case in inspiration, the oblique and transverse muscles of the abdomen, are supposed to be in a state of relaxation; and, by permitting the abdominal viscera to protrude, to allow an easy descent to the diaphragm.—When, however, the diaphragm becomes relaxed either with or withont the consent of the will; the abdominal muscles either in consequeuce of sympathy, association, or of the stimulus of volition, contract in their turn, and by pressing the abdominal viscera against the lower or concave surface of the diaphragm, cause it to assume a higher elevation in the cavity of the chest.

The abdominal muscles and diaphragm are considered in this explanation to act as antagonists; and, it is by the alternations in the contraction and relaxations of these muscles and the diaphragm, that the function of respiration is supposed to be performed.

That the actions of the abdominal muscles should perpetually and invariably alternate with those of the diaphragm, through sympathy, habit, or association, and generally without the intervention of the will, is a doctrine which appears so incomprehensible, that it will not slightly obtain the assent of the inquisitive mind. Great support has been supposed to be derived to the hypothesis of such a connexion subsisting between the muscles in every respect distinct, from the alternation in the movements of the ventricles and auricles of the heart. But the aid, which this doctrine is supposed to receive from the analogy of the heart is in this case fallacious; for, by referring to the account which will be given in the succeeding part of this work, of the action of this organ, it will be found that the alternations to which allusion has been made, are not produced through sympathy, habit, association, concatenation, or any of that tribe of causes, but that they are the result of a mechanical necessity.

But this doctrine, so improbable in its own nature, receives little or no support from observation. The muscles of the abdomen are not generally during expiration in that tense state, which their contraction to the extent supposed would render necessary. Nor do the abdominal viscera appear to sustain greater pressure during expiration than at any other period.

In cases of great inanition and collapse of the bowels, the contraction of the abdominal muscles, by bringing those muscles nearer to the direction of straight lines, passing between their attachments—the brim of the pelvis and the lower ribs, would not push the viscera of the abdomen against the diaphragm ; but by enlarging the cavity of the belly to a capacity beyond that which these viscera required, would draw them from the lower surface of the diaphragm. When the bowels are found protruded in consequence of extensive wounds in the belly, respiration proceeds for some time without more interruption than may be supposed to be derived from pain. But the diaphragm in these cases could not be elevated by the pressure of the abdominal muscles, which, during their contraction, would more easily displace the bowels to a greater extent than elevate the diaphragm. In cases of impaling and in the numerous experiments which have been made upon the lower animals, it is found that respiration may be performed for a considerable time, after the abdominal muscles have been divided, or altogether removed. After death even, when, in dissection, the abdominal muscles have been divided, and the viscera have been taken from the cavity of the belly, the diaphragm is observed to preserve a great convexity, attended with a drum-like tenseness, towards the chest.

If we consider how improbable it is, that a perpetual and invariable alternation in the actions of the abdominal muscles and diaphragm should proceed from the supposed causes ; that a doctrine, so incredible in itself receives little support from observation ; and that the effect, which is supposed in all cases to arise from this cause alone, is certainly produced in circumstances in which that cause cannot operate ; we may conclude with the utmost safety, that the contraction of the abdominal muscles is not necessary to the elevation of the diaphragm, and that, in general, it has no share in that effect.

As the causes to which the ascent of the diaphragm is attributed in the following pages, are altogether different from those to which that movement has hitherto been ascribed by physiologists ; as those causes are supposed to discharge an important part, not only in the process of breathing ; but also, in the circulation of the blood ; in the production and regulation of animal heat, and in other functions of the living machine ; and as these are subjects, about which it may reasonably be supposed that many persons, not of the medical profession, may feel a strong interest, but about which they are at present surprisingly ignorant and indifferent, in consequence chiefly it is supposed of the unsatisfactory nature of the received doctrines themselves, and of a hopelessness of reaching these subjects through a language exclusively professional, and singularly obscure; it seems necessary to give a more minute description of the parts concerned, than may appear requisite to the medical student, and to study the use of a language as plain and vernaculer as the subject will admit of.

It is hoped that any reader, who is acquainted with the structure of any ordinary piece of machinery, will have little difficulty in understanding the following pages.

The thorax or chest, denominates the upper part of the trunk of the body. The structure of the thorax is chiefly of bone; the ribs and the intercostal muscles form its lateral defences; and the breast bone and spine, to which the ribs are connected by cartilages and articulations, guard it before and behind. This bony circumvallation is widest at the bottom, and becomes gradually and pretty uniformly narrower as it approaches the top. It is, therefore, properly said to resemble a cone. Strong muscles and ligaments, passing across from the upper ribs to the cervical vertebræ and to the head,. close the contracted apex, leaving only a passage for the trachea, or windpipe, œsophagus, or gullet, and blood vessels, which are at all their entrances into the chest securely attached on every side to the surrounding structure. The floor of this cavity is formed by the diaphragm, a broad muscle which bisects the trunk of the body transversely and separates the chest from the abdomen or belly. The œsophagus again passes out of the chest on its way to the upper orifice of the stomach through the diaphragm, which is also penetrated by two large venous and arterial trunks. Both the œsophagus and the blood vessels are on all sides firmly bound to the diaphragm in their passage through it. The cavity of the thorax, therefore, is every where securely enclosed and is pervious only through the bores of the blood vessels and other tubes which enter it at its apex and base·

The structure of the whole of the diaphragm is muscular, excepting a circular portion in the middle, extending to the anterior edge, which is tendinous. In the living and sound body, and after death, except when that has been occasioned by peculiar diseases, the diaphragm swells up a considerable way into the chest, exhibiting a great convexity on the upper surface and a corresponding concavity on the side facing the abdomen.

The heart which will be more fully described, is inclosed in a strong tendinous undilatable bag, called by anatomists the pericardium. The surface of the heart is every where free and unattached ; but the pericardium is connected to the tendon of the diaphragm on which it rests by a broad and strong adhesion. The trunks of the large blood vessels, in proceeding from the fundus of the heart, penetrate the pericardium, become firmly bound to it all around, and receive a covering from the extension of its internal membrane. As no substance intervenes between the pericardium and the heart, that envelope necessarily assumes the form of this organ.

The other contents of the chest are the lungs, the thymus gland which is almost obliterated in the adult, the œsophagus or gullet, to which it affords a passage from the throat to the stomach, blood vessels, nerves and some membranes.

The windpipe immediately after it has passed from the neck into the confines of the chest, divides into two branches, one of which pursues a direction to the right and the other to the left side. Each of these branches subdivides into a number of smaller branches, which again ramify, and again become stems of fresh ramifications ; till at last they become extremely minute and multiplied. The ramifications of the divisions of the trachea are called bronchia or bronchial vessels ; they are also called air vessels. The first ramifications of the bronchial vessels continue for some time to preserve the structure of their original trunk, the windpipe; they are formed by a succession of cartilaginous rings, which are connected to each other by strong elastic membraneous tubes, and which are so constituted that the lower ring will pass into the circumference of that immediately above it. The rings are not cartilaginous all around, but at one part of the circle are composed of elastic membranes, and may easily, therefore, become of a larger or a less diameter. The ramifications of the bronchia, at length, as they become minute, drop their cartilaginous structure altogether and become solely membraneous and terminate in cells. These cells generally form clusters which are called lobules, and which each belong to some division of the bronchia. Inflation proves that the communication between the cells of each lobule is free, but that between the cells of one lobule and those of another difficult, though not impracticable. A cellular substance occupies the interstices between the bronchia and cells, and surrounds each of the lobules.

The two original divisions of the trachea form, with their ramifications and connecting substance, distinct and separate lungs, called from their situation right and left, or right and left lobes of the lungs. Each of these lobes is surrounded with a cellular substance, and, as has been remarked, is inclosed in a duplicature of the pleura, the membrane which lines the internal cavity of the chest. The two lobes of the lungs are, besides, divided from each other by another folding of the pleura, which is called the mediastinum, and which is placed vertically in the middle of the chest between the breastbone and the spine. The two laminæ or plates of which this particular folding of the pleura is formed, are connected at their insertions into the spine and breastbone, but in the middle and towards the bottom they separate and inclose the pericardium and its contents. This inclosure is formed chiefly by the diverging of the left plate of the mediastinum. There are, therefore, in the chest three distinct compartments, one on each side of the mediastinum, containing the right and left lobes of the lung, and the third in the middle forming a space for the heart. The lungs, therefore, are separated from the pericardium only by a single plating of the mediastinum, a thin transparent, flexible membrane, easily dilatable, scarcely elastic, and on the leftside, especially, lying loose upon the pericardium and surrounding it like a shield. As far, therefore, as the affording on one side or the other, any support against pressure, or defence against injury, may be considered, the laminæ of the mediastinum in contact with the pericardium may be regarded as nothing.

The shape of the lungs is not unfitly compared to that of the foot of an ox reversed. They are firmly bound to the top of the chest, but are at all other parts free and unattached. As no substance intervenes between the external surface of the lungs, and the walls of the thoracic cavity, they may be said to be suspended in vacuo.—They are exactly adapted to all the inequalities, and to the constantly varying dimensions of this cavity, by the weight of the atmosphere pressing upon the internal surface of the bronchia through the trachea.

The substance of the lungs is powerfully elastic. This is admitted by all anatomists. If a piece of the substance of the lungs be taken while its structure is still perfect, and stretched into double or treble its ordinary length, it will, upon the removal of the distending force, resiliate briskly into its former dimensions. This property was confidently to have been presumed from the cartilaginous and ligamentous structure of the bronchial vessels.

From the moment that the first inspiration is drawn by the infant to the close of life, and after it, so long as the structure of the frame is uninjured, the lungs are swelled into a forced or unnatural state of expansion; that is, they are distended into dimensions far more ample than those which they would assume, if all force or restraint was withdrawn from them. The power by which they are kept in a constant state of forced dilatation, is unquestionably, what has been stated, the weight of the atmosphere.

As it is of the utmost importance in this inquiry, that the opinion now advanced respecting the ordinary condition of the lungs, should be clearly and satisfactorily proved, it will be necessary to enter fully into the arguments by which it may be supported.

The lungs at or soon after birth, in consequence of the increase of the thoracic cavity, occasioned by the depression of the diaphragm and other changes which take place, are dilated into much larger dimensions than those which were assigned them before birth. It was to have been presumed from the structure of the lungs, that this augmentation of bulk would be accompanied by a stretching of the elastic substance beyond its original, and in all probability, natural state.

This presumption is fully confirmed by experience. If an opening be made into the cavity of the chest, through any part of the walls, the lungs immediately collapse, and return nearly to the dimensions which they would have occupied before birth; only making allowance for their actual increase by growth of substance, which may be supposed to correspond with the difference between the bulk of the body at the time the opening may be made, and that of the unborn fœtus. By such an opening, both the external and internal surface of the bronchial vessels, are equally subjected in every position to the pressure of the atmosphere; and the lungs have, therefore, full freedom to assume the form and dimensions which their structure may have made natural to them.

The collapsing of the lungs is, in this experiment, generally imputed to the admission of the weight of the atmosphere upon the external surface of these organs; and this is certainly true. But unless there were some other cause, the effect could not be produced. For the pressure of the air is equal both internally and externally. The specific gravity of the substance of the lungs, may in some conditions of the body, have a certain share in the production of the phenomenon, but it must evidently be very small. Indeed, the collapse is equally complete when the chest is erect, and when the specific gravity of the substance of the lungs must favour the distention of those organs.

In order to ascertain the strength of the collapsing effort of the lungs, the following experiments were devised.

On the 30th of March, 1815, the windpipe of a bullock which had been newly slaughtered, was laid bare from the larynx to its entrance into the chest. It was divided transversely near the top, and dissected from the other parts as far as the chest. The bullock was placed upon his back, with his shoulders higher than his buttocks. The windpipe was then bent into a pitcher full of water, so that the top of it reached ten inches below the surface of the water. An opening was then made into each side of the chest to admit the air. The sound of the air passing through the orifices into the chest, was distinctly heard for some seconds, without the surface of the water in the pitcher being in the least agitated. In my impatience I raised the windpipe about two inches, when bubbles of air issued from the surface of the water, and in a few seconds subsided, after which another strong ebullition prevailed for a few seconds more.

On the 4th of the succeeding April, the windpipe of a sheep, which had been just killed, was treated exactly in the same manner with that of the ox, in the above experiment. The sheep was supported on his hind quarters, with the chest in an erect posture. A piece of lead was tied to the end of the windpipe, and sunk in a pitcher full of water. The windpipe was then fixed in the water, so that its top was immersed under the surface of the water, to the depth of seven inches. Two orifices were then made into the chest, one on each side. After the air had been distinctly heard issuing through the openings for about 20 seconds, a bubble of air was observed to escape from the surface of the water in the pitcher, into which the top of the windpipe had been sunk, and in about four seconds more another rose. As the sheep was very fat, and the orifices small, the air did not enter into the chest freely. The openings were enlarged, when immediately the surface of the water in the pitcher was violently agitated for a few seconds, as if it had been boiling strongly.

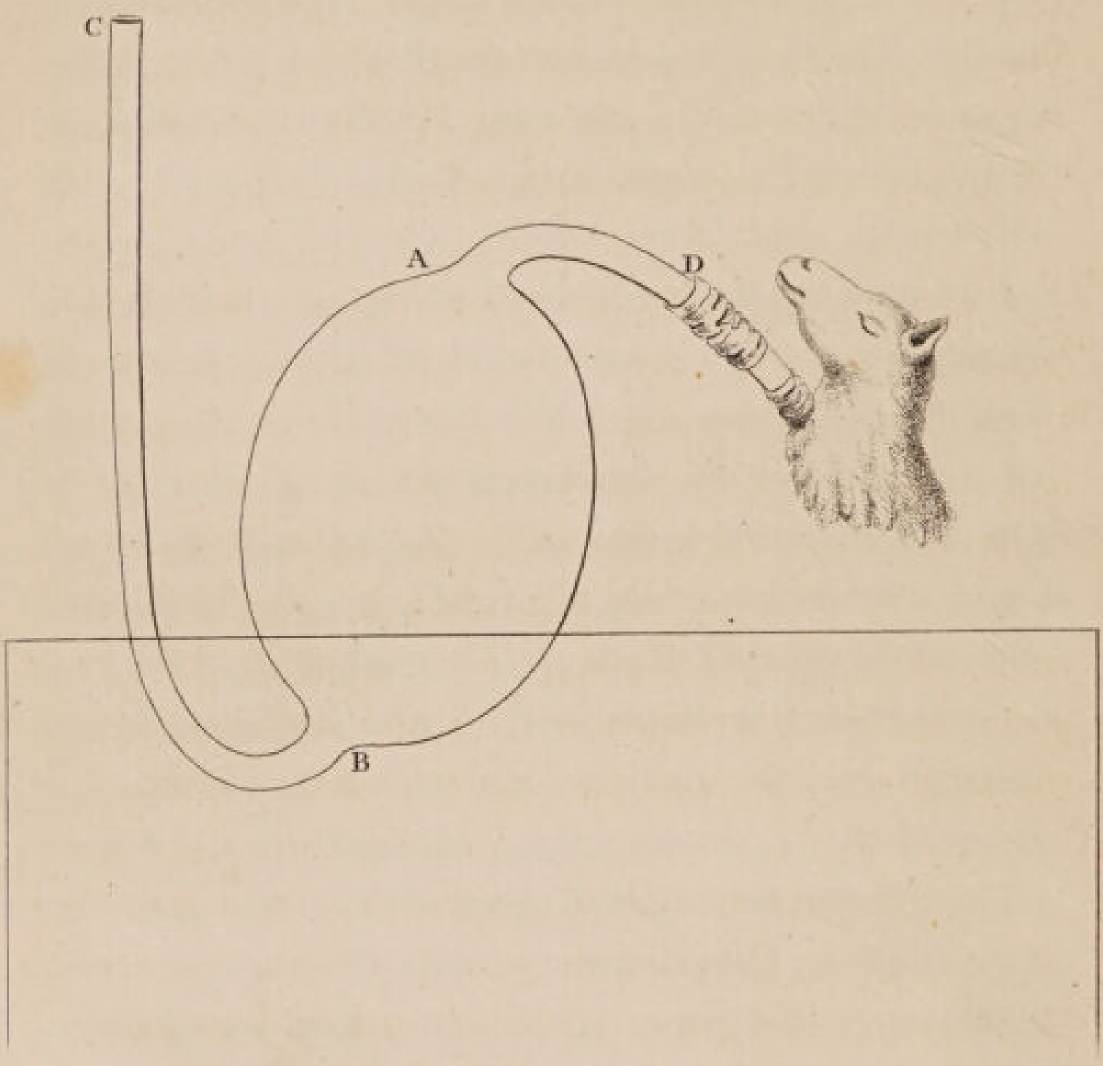
On the 12th of the same month another sheep, newly slaughtered, was treated in the same manner, only that the end of the divided windpipe, instead of being inserted into a pitcher of water, was firmly tied so as to prevent the escape of any air from the lungs. Openings were then made into the chest; the lungs collapsed briskly as in the other instance, but it did not recede within their lowest collapsing dimensions; for after some time the ligature was removed; air escaped; and the lungs gradually collapsed still further.

These experiments decidedly prove that, in any situation in which the fabric has not been injured by disease or violence, the lungs are always in a state of forced expansion ; that, when relieved from the agency of those powers by which they were expanded, they recede into their natural and comparatively narrow limits, in opposition to no inconsiderable resistance ; and that they evidently derive the power of contraction, as it is retained long after the extinction of life, from a property inherent in their structure.

The weight of a column of water of seven inches in height, which the contractile power of the lungs balanced in these experiments, though it may be considered as valuing the elastic force as it exists in the lungs towards the termination of its long range, is not by any means to be taken as the measure of the momentum with which the lungs would resiliate at the stages of fuller dilatation and particularly at the stage of full insipration. For the powers which are required to stretch any elastic substance must bear some proportion to the extent to which that substance is to be stretched. Greater force would no doubt be necessary to stretch such a substance to treble than to double its ordinary length, and the momentum, with which it resiliates, would bear, at the different stages of its extention, a certain proportion to the multiple of its original length at those stages. The measurement of the resisting force, which the lungs possess while the walls by which they are enclosed have beer. uninjured, is not ascertained by these experiments ;

for the air upon which this force acted, is a fluid easily compressible; and, before such a degree of elasticity, as would support a column of water a few inches in height, could be given to the air in the lungs by compression, the volume of this fluid must have been greatly diminished; the dimensions of the lungs in consequence proportionably decreased; and the intensity of the elastic force greatly reduced. These experiments therefore, do not afford any measurement of the force with which the lungs collapse at the comencement of that action, or, in other words, the proportion of the weight of the atmosphere which is employed in dilating them to the state in which they exist in life; but, from the degree of elastic power shown by these experiments to be possesed by the lungs towards the termination of the long range of that elasticity we may infer its very high amount at the utmost extent of that range.

The experiments now detailed, though they prove the existence of a power of considerable extent, derived from the elastic or resilient effort possessed by the lungs in the condition in which they are placed in the living and sound body, supply therefore no grounds from which the extent of that power may be estimated. At the distance of several years from the publication of the first edition of this work, I instituted a series of experiments, by which it was hoped the amount of the resilient power of the lungs of various animals might be correctly calculated. The attainment of this object appeared the more desireable, as I found that very erroneous deductions had been made, from the experiments which have been detailed; deductions which, at the time of the publication, I suspected might be made, and endeavoured, though without the success I wished, to prevent. The weight of a column of water of seven inches in height, which was proved to be overcome by the ebulition which took place on the surface of the water, in which the windpipe of an ox had been sunk, while the lungs were allowed to collapse by the admission of air into the chest, was held by writers of eminence to fix the amount of the elasticity of the lungs of that animal. The power demonstrated was stated to be so inconsiderable as not to warrant the effects that had been imputed to it, in the "Treatise of the Motion of the Blood, and on the Process of Respiration;" and was not more than was rationally to have been expected to arise from the specific gravity of the lungs in certain positions of the body, without any regard to a resilient property. It must be remembered that the air contained by the lungs, and, in the experiment in question, enclosed between the confines of the lungs and the water into which the windpipe was inserted, was an elastic fluid; before, therefore, it could balance the weight of a column of water of seven inches in height it must have been compressed to a great extent. It is the amount of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the compression alone that is measured by the experiment above detailed, and not the amount of the force by which that compression was produced; for the same amount of compression would support the same column of water from whatever cause that compression was produced. The existence of a resilient effort in the lungs is proved by these experiments, but the extent of it is left by them entirely unascertained.

It appeared to me a matter of great importance to ascertain, if possible, the resilient effort actually possessed by the lungs in the state in which they exist in the living and sound body in different animals and in different stages of its existence in the same animal. A simple, but as I conceive a very effectual, method of attaining this object, was contrived in the year 1817. An account of these experiments was transmitted to the Royal Society and was printed in the Philosophical Transactions for 1820.

For the purpose of these experiments, an apparatus of glass, of the following simple construction, was used. An oblong glass globe, containing nearly two quarts, had tubular openings at each end, A and B. (Plate.) A glass tube, nearly three feet in length, and bent at one end, was joined by the blowpipe to the opening at B, and is represented by B C. To the other opening at A, a shorter tube was joined in the same manner, and in the same form A D. A free passage was established from D to C, where the tubes were both open. To D, a piece of dried gut of some small animal was bound, of a few inches in length. The other end of the gut was fixed to a cylindrical tube of bone, metal, or wood, also a few inches in length, and of a diameter corresponding with the diameter of the windpipe of the animal which was to be the subject of the experiment. The windpipe of an animal, which had been recently killed, was divided across near the throat, and separated by dissection from the rest of the neck, nearly to the top of the chest.

The first experiment was made on the 27th of August, 1817, on a cat, which had been hanged the day before. A small cylindrical tube of bone attached by gut to the end of the glass tube A D, was inserted into the windpipe, which had been prepared in the way described, and which was tied to the cylinder so tightly, that no air could pass between the external surface of the tube and the internal surface of the windpipe. An open and secure passage was thus established between the glass apparatus and the windpipe, and of course the lungs of the animal. Water was then poured into the apparatus at C, until it stood in the upright tube C B, at the height of eleven inches above the level of the water in the glass globe. An opening was then made through the chest of the animal on each side, and the air admitted into contact with the external surface of both lungs. The water instantly sunk about two inches in the upright tube and the lungs were gently pressed out at the openings. Hence it was inferred, that the spring given to the air by the pressure of a column of water nine inches high, was stronger than the elasticity of this animal's lungs expanded to the dimensions of the chest. To my surprise and disappointment, the water began to sink still lower in the glass tube, and stood at last at the height of an inch above the level of the water in the globe; and the lungs at the same time gradually collapsed. Water was again poured into the upright tube, till it stood for a few seconds at the height of nine inches above its level in the globe, and the lungs again filled the chest. Upon applying the ear to the openings, the cause of the collapse of the lungs was discovered. The sound of air was distinctly heard pressed from the lungs at the openings. Hence it was concluded, that the pleura pulmonalis had been wounded in opening the chest.

On the 28th of August of the same year, a bullock recently slaughtered was made the subject of experiment. The same apparatus, using only a pipe of larger diameter inserted into the larger windpipe of the animal, was applied as in the preceding case. Water was poured into the upright tube at C, until it stood at the height of one foot above the level of the water in the globe, and at that of four inches from the top of the tube. Openings were then made in the chest to admit the air. The water instantly rose in the tube two inches higher, and remained stationary at that point. The lungs, to appearance, were nearly collapsed to the usual degree. It was evident, from the ascent of the water in the upright tube, upon the chest being perforated, that the spring given to the air by being compressed by a column of water twelve inches high, was not sufficient to balance the elasticity of the full dilated lungs of this animal. Water was poured into the apparatus till the tube remained filled. The lungs, with this additional pressure, still continued much shrunk. As the height of the tube was not great enough to ascertain the extent of the pressure necessary to balance the resilience of the lungs of animals of this size in the state of their usual expansion in the living body, an alteration in the apparatus became necessary.

On the 11th of September, 1817, a bullock was made the subject of experiment, with an apparatus of the same kind, but with a taller upright tube. Water was poured into the apparatus till it stood in the upright tube twelve inches above its level in the globe. The thorax was then opened. The water instantly ascended an inch and a half, and remained stationary. More water was then poured into the apparatus ; but when it had risen an inch higher in the tube, the globe was found to be full. A further alteration therefore in the apparatus becomes necessary, before the resilience of the fully distended lungs of an ox can be ascertained; for the lungs continued shrunk to a considerable degree, in opposition to the spring of air compressed by a column of water of fourteen inches in height.

On the same day another bullock, with a less capacious chest, was made the subject of experiment with the same apparatus. Water was poured into the apparatus, as in the preceding instances, till it stood sixteen inches above the level of the water in the globe. An opening was then made into the abdomen. A fold of the fleshy part of the diaphragm was then drawn down on each side, and care being taken that no part of the lung was included in the fold, it was cut into. The sound of a current of air pressing through the openings into the chest was distinctly heard, and the water in the upright tube rose instantly to the height of eighteen inches above the level of the water in the globe. The diaphragm, before the openings, was still tense, and slightly concave towards the belly ; but after that, it became lax, wrinkled and flat. Some additional water was poured into the apparatus ; but we were prevented in this case, as in the preceding, from ascertaining the amount of the force requisite to distend the lungs to the dimensions which they usually occupy in the living animal by the deficient capacity of the glass globe.

By these experiments I think it is clearly ascertained, that the spring of air compressed by a column of water of a foot and a half high, is not equal to the resilience of the lungs of an ox, at the usual stage of their dilatation.

In all these experiments, the oxen were placed upon the back with the shoulder raised a little above the rest of the body: some share of the collapsing effort of the lungs might be imputed to their specific gravity. But when the levity of the lungs is considered, and also that they were observed not to shrink more from the breast to the spine, than from the diaphragm to the neck, in opposition in this case to gravity, and that their dimensions were not increased, nor their form varied by any change of position; little of the resistance which the lungs made to the spring of the contained air, is imputable, I think, to their specific gravity.

On the 16th of September, 1817, the apparatus was applied to a calf. When the water in the upright tube had reached the height of fourteen inches above its level in the globe, the lungs appeared to be distended to the full capacity of the chest. Upon cutting off the communication between the apparatus and the animal, the water instantly fell to its level, and the lungs shrunk into very small dimensions. The animal, in this experiment, was placed with the chest erect, so that the shrinking of the lungs upwards from the diaphragm to the neck, and which was observed to be as great as in any other direction, must have taken place in opposition to the specific gravity of the lungs.

In almost every experiment in which the chest was perforated by a sharp instrument, the lungs were found to be wounded, and the object in a great measure defeated. In the experiment about to be described, and the last which will be detailed at this time, great care was taken to prevent the accident now mentioned.

On the 31st of October, of the same year, the apparatus was applied to the prepared trachea of a dog, which had been hanged on the preceding day. Water was poured into the apparatus until it stood in the upright tube at the height of six inches above its level in the globe. The abdomen of the animal was opened, and the diaphragm freely exposed. A part of the muscular substance of the diaphragm was drawn down in a fold, which was done without difficulty ; and care being taken that no part of the lung was contained in the fold, an incision was made into it. This was done on both sides. The water ascended instantly about an inch in the upright tube, and the lungs were found to have receded from the openings. Water was poured into the apparatus, until its level in the tube stood above that in the globe at the height of ten inches. The openings in the diaphragm were carefully extended along the chest, and the lungs exposed freely to view. They were now apparently dilated to the boundaries of the chest. In this experiment, the water in the tube remained steadily at the same height, and the lungs continued fully dilated. In those experiments in which the water was observed to descend slowly, and the lungs to collapse gradually, it is very evident, that the lungs must have been wounded, so as to allow some air to escape through the pleura.

The appearance which the lungs exhibited in this situation, was novel and interesting, and was no doubt the same which they would have exhibited in the living body, had it been possible to bring them into view. Their surface was smooth and polished, and their edges rounded, without any of those corrugations and sharp angles which they usually exhibit. Their color was red, and life-like. They felt firm to the touch. The heart appeared like a bird nearly covered by its nest.

The state of the diaphragm in this experiment was worthy of remark. Before the incisions were made into it, it still exhibited a degree of concavity towards the belly, but not with the same degree of tenseness which it is generally observed to possess; for a fold of it was taken with ease, which in ordinary cases, before the chest has been opened, is done with difficulty. As soon as the openings were made into the diaphragm, it became lax, flat, and corrugated.

Frequent repetitions of these experiments, and much care and accuracy in conducting them, would be required, before the amount of the resilience of the lungs, in all the conditions in which it may be supposed to exist, could be estimated. But defective as these experiments in some respects are, the object for which they were instituted, seems to have been fully attained by them. In the Inquiry into the Causes of the Motion of the Blood, it was contended, that the elastic substance of the lungs in consequence of the degree to which that substance was stretched in the living body, generated a permanent power of great extent, and that this power was employed by nature to circulate the blood, and to carry on the process of respiration. The existence of this power was inferred, from the elastic property of the substance of the lungs themselves; from the space which those organs must fill in the living body; from the phenomena exhibited upon opening the chest and admitting the external air; and from the ebullition on the surface of the water into which the inverted wind-pipe of an animal had been inserted, as soon as the lungs were allowed to collapse. In the various examinations which my opinions have undergone, the existence of this power has been admitted, and the claim to priority in the detection and application of it, freely conceded to me; but it has been contended, that the amount of it, in some instances, is inconsiderable, and consequently that the effects ascribed to it have been greatly over-rated. By these experiments, the power has been proved to be greater, than my anticipations even made it, and fully adequate to all the important offices which I have ventured to assign to it. From a defect in the apparatus, the extent of the power in question, could not be ascertained in the lungs of oxen aud animals of their size; but it was proved to exceed considerably, the force necessary to support a column of water of a foot and a half in height above its level. In calves, sheep, and in large dogs, the resiliency of the lungs was found to be balanced by a column of water, varying in height from one foot to a foot and a half; and in rabbits and cats, by a column of water varying in height from six to ten inches.

It has been stated, that, during the life of the animal, and after death, until an opening shall have been made into the cavity of the chest, the diaphragm assumes the form of a cone, and that the causes of this phenomenon would be afterwards pointed out. The brief explanation which is now to be given of this appearance, will afford at the same time, a perspicuous view of some of the important purposes to which, in my opinion, nature has turned the elasticity of the lungs.

While the chest is in a sound state, a balance of atmospherical pressure ponderates against the external surface of its walls ; or these are pressed inwards more than they are pressed outwards, by a given weight. The shell of the chest possesses sufficient stability to resist this pressure without changing in any considerable degree its form and capacity at all parts, except at the base, or diaphragm ; which being muscular, pliant, and of a more extensive area than that of the transverse section of the chest, is, in consequence of the greater weight sustained by its outward or inferior surface, necessarily pressed, or, in popular language, sucked upwards in the form of a cone. The extent of this cone will be necessarily regulated by the extent of the area of the diaphragm, compared with that of the area of the transverse section of the chest. But the contraction of the muscular fibres of the diaphragm diminishes its area, and reduces it to a nearer equality with the area of the transverse section of the chest, and thus diminishes the magnitude of the diaphragmatic cone, and in an inverse proportion enlarges the boundaries of the chest. But the diaphragm, at the succeeding relaxation of its fibres, is restored to its former dimensions; becomes capable of being swelled into a larger cone; and, by this encroachment, reduces the boundaries of the chest to their former limits.

Two powers are therefore concerned in regulating the movements and in varying the dimensions and form of the diaphragm, the elasticity of the lungs, and the contractile power of the muscular fibres of the diaphragm. Of these powers the one is permanent and equable, the other variable and exerted at intervals. The contractile power of the diaphragm, when fully exerted, is evidently much stronger than its antagonist, the resilience of the lungs; but the latter, not being subject to exhaustion, takes advantage of the necessary relaxations of the former, and rebounding, like the stone of Sysyphus, recovers its lost ground, and renews the toil of its more powerful opponent.

Breathing is in a great measure the effect of this interminable contest between the elasticity of the lungs and the irritability of the diaphragm.

The cause of the successive contractions of the diaphragm, in those cases at least in which the will is not concerned, seems to admit of the following explanation. A permanent and invariable load is sustained by its lower surface. By this load the relaxed muscular fibres become stretched to a degree which at length becomes painful and stimulating. To relieve itself from this irksome burden, the diaphragm is roused to contraction; but this contractile power, agreeably to the laws of muscularity, is soon exhausted, and falling into a quiescent state, allows the painful and stimulating distension of the relaxed fibres of the diaphragm to be again renewed. From the irksomeness of this condition it relieves itself by a fresh contraction. Thus, by the alternated superiority of two powers, on the balancing of which life itself depends, the chest is successively enlarged and diminished, and air alternately expelled and inhaled.

Such is the process of respiration, so far as it depends on the movement of the diaphragm; but according to a provision very common in the animal machine, this important office is not intrusted to one class of organs alone, but has a double class assigned to it. The objects obtained by this provision are, in the first place, that by the united action of both classes, the function may, in the most healthy state, be more efficiently performed; and, in the second place, in case of the destruction or deficiency of one class, it may be still continued

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wholly or chiefly by the other. The second class of organs, as they may be called, discharge their office by alternately increasing and diminishing the width of the chest. This class comprises the intercostal muscles, or short muscles, which pass obliquely between the ribs, and have one insertion in the lower edge of one rib and the other in the upper edge of that next below it, and several of the muscles of the chest; and the antagonists of these agents, the cartilages, which unite the ribs to the breastbone or to each other, and the lungs.

By the contraction of the intercostal muscles, the ribs are moved nearer to each other; and, as the highest row, or tier of ribs, is fixed immoveably, the whole extent of motion madė by the contraction of the intercostal muscles, placed between the first and second tier of ribs, must be made solely by the ascent of the second tier. It is evident, therefore, but highly worthy of notice, that the range of movement performed by each tier of ribs, increases as they descend, according to an arithmetical progression. For the first tier being fixed, the second tier, by the contraction of the intervening muscles, is drawn nearer to it; the third tier is at the same instant drawn nearer to the second; and, if the second had been fixed, the extent of movement made by the third, would have been that only which is measured by the difference of the distance between the two ribs; but, as the second is at the same moment drawn upwards, it is evident that the third tier has to move through a space that is equal, not only to the difference between the second and third ribs, but to the difference between the first and second and second and third united. The same thing holds with each tier of ribs in succession, so that the lowest tier of all has, in the process of inspiration, to move through a space that is equal to the spaces united, through which all the higher tiers move in the same process.

In consequence of the manner in which the ribs are hinged on the back bone, it follows that, when they are drawn upwards in the manner that has been described, by the intercostal and other muscles, they are necessarily turned outwards, or their convex sides are turned more directly outwards; by which the space they inclose is enlarged. The extent of the movement in this direction increases as the tier descends. It is greatest, consequently, in the lowest or false ribs, or those short ribs which are not united by cartilage to the breast bone, but to the ribs next above them.

Since the lower ribs perform the greater movement which they have to make, in the same time in which the higher ribs have to make the shorter movement, it follows, that the lower ribs ascend and descend with greater velocity than that with which the upper ribs move. The consequence of which is, an increase of power, or an augmentation of the force with which the lower ribs act. In inspiration, therefore, the chest is more powerfully and extensively dilated as it descends. Any one may be satisfied of the truth of this practically, by placing the hand upon the chest while he inspires.

This view of the action of the intercostal muscles upon the ribs, has suggested the existence of a property belonging to muscles, which appears to be of great importance; but which, so far as I know, has not hitherto been noticed by physiologists. Every muscle which, at one end has a fixed attachment, and at the other a yielding one, must increase in the velocity of its action, and consequently in the force of that action, as it approaches the yielding attachment, according to an arithmetical progression. When a muscle contracts it is shortened; every ultimate particle approaches nearer to that which is next to it, than it did in a state of relaxation; the particle in contact with the fixed attachment, approaches nearer to that attachment; the particle next in succession, approaches nearer to the first, but in making that movement, it has not only to advance through the space measured by the difference of distance between the first and second particle in its contracted state, compared with the distance between the two particles in a dilated state, but in addition, through a space equal to that through which the first particle had moved. This holds with respect to every succeeding particle. So that the movement made by the last particle in a line of particles, is composed of the movements made by the first particle and the sum of the relative approaches made by all the succeeding particles. The velocity and power with which a muscle acts, will be augmented in certain situations, according to the length of that muscle. A power, similar and equal to that of a lever, is thus bestowed upon muscles placed in situations often opposite to that in which the advantages of levers are obtained in dead matter.

This subject will be prosecuted at greater length in a subsequent part of this work, when the causes of muscular motion will come to be investigated.

As a contrivance, well calculated to give a more extensive movement to the ribs, and, consequently, a more ample expansion to the chest, the oblique direction of the intercostal muscles deserves to be noticed. In consequence of this obliquity, the same proportion of contraction draws the substances, between which the contracting muscles are placed, nearer to each other, than would have been done by muscles placed at right angles to these substances. This property was first, perhaps, discovered, and exhibited in a popular, but probably in a sufficiently satisfactory way, by the late Dr. Monro, but was first demonstrated strictly by Sir Gilbert Blane.

It would appear from the preceding reasoning, that, in consequence of the obliquity of the intercostal muscles, and necessary increase of their length, the same proportion of contraction is made with greater velocity, than would be by perpendicular and shorter muscles; hence the contraction of the intercostal muscles, in consequence of their oblique direction, produces in the ribs, upon which they act, a greater range of movement, while that movement is made with greater velocity, and, of course, power.

It has been stated that a double set of organs belong to respiration. It happens very frequently in this country, perhaps more than in any other, that the lungs become diseased to such a degree, as to be deprived of their elasticity. This may happen to one lung and not to the other, and to one portion of a lung, while the rest remains sound. When both lungs are deprived of their elasticity, the diaphragm is deprived of the power, by which, in ordinary circumstances it is elevated, and from that time ceases to have any share in the process of respiration. In this case, which occurs when the lungs become completely tuberculous and schirrous, respiration is performed solely by the intercostal and other muscles of the chest, and their only remaining antagonist, the elasticity of the cartilages of the ribs. It occurs occasionally, particularly in old age, that the cartilages of the ribs lose their elasticity, by being converted into inelastic bone. In that case, breathing is performed solely by the play of the diaphragm. In some animals, one set of organs are used almost solely, or more extensively than the other. In animals of the chase, respiration is chiefly performed by the movement of the diaphragm. This is evident from the violent action of the flanks. The lungs of these animals are large, powerfully elastic, and little subject to disease. In the human species again, the second set of organs are much employed, which is evident from the fascinating heaving of the female chest.

In consequence of the descent of the diaphragm, and simultaneous elevation of the ribs, it is evident, that a part of the pleura pulmonalis, that was opposite to, and in contact with, a part of the pleura costalis in the stage of expiration, will not be opposite to, and in contact with, the same part in the highest stage of inspiration, but will be opposite to a part considerably lower in the chest, and this difference between the parts of apposition in these stages, will increase with the descent of the chest. Hence arises a sliding motion of the surface of one pleura on the surface of the other pleura—a motion, which will be more extensive the nearer an approach is made to the diaphragm. Hence it happens, that in the healthy state of the organs of respiration, particularly in the healthy state of the lungs, no adhesions can take place between the pleuræ.

The lungs themselves, like the organs of respiration, of which they constitute an important part, are double. So that if one lung be rendered inefficient by disease or otherwise, respiration may still be performed by the aid of the other lung only. Hence it happens, that one lung may be completely collapsed without much apparent inconvenience to the animal. I found by some experiments which I made on animals some years ago, that the air may be admitted freely upon the lung of one side, without the destruction of life, and indeed with perfect safety. This proves that the mediastinum, or membrane placed between the two lungs, and dividing the chest vertically, is sufficiently tense and strong to be a wall to either of the lungs, too powerful to be overcome by the resilient effort of that lung, and to secure to it a space sufficiently ample to enable it to perform the purposes of respiration. A very striking and satisfactory proof of this, and indeed as conclusive a one as has yet been adduced, of the extent of the power generated by the elasticity of the lungs in a state of health, is supplied by the manner in which the Jews kill the animals intended for their food. It is well known, that this people will not eat the flesh of any animal that has the blood in it, or that has had any disease, or mal-formation in its organs. A mode of killing animals different from that in use with christians, and a peculiar examination for ascertaining the health and perfection of the organs of the animal, are practised by them. To separate the blood as completely as possible, the operation of slaughtering commences with bleeding, which is done by dividing all the vessels of the neck, including the windpipe. In this situation the animal bleeds to death, a tedious, and apparently a very cruel process. The operator, who is generally the priest, or some person appointed to this sacred duty, then proceeds to the examination of the viscera. The abdominal cavity is first freely opened, and examined. He next proceeds to examine the state of the thoracic organs. For this purpose, he makes a large incision through the diaphragm on the right side. On the first occasion of this kind at which I was present, an ox happened to be the victim. Upon the incision being made through the diaphragm, a loud sound, like that produced when a large drum is struck, was heard, for which I was not prepared; and which startled me like the unexpected firing of a pistol. This sound was occasioned by the rapidity with which the air rushed through the orifice to occupy the space left by the collapse of the lung. The sound being in this instance, rather louder than usual, was regarded by the operator as a proof of the soundness of the thoracic viscera. He next perforated the diaphragm on the left side; a sound, though not so loud as that following the first incision, was distinctly heard at some distance. The extent of the sound in the first instance, proved the force and rapidity with which the lung collapsed. The sound in the second instance, of diminished extent, proved that the mediastinum had yielded to a certain degree, but that it had preserved its position so far as to check the force derived from the elasticity of the lung upon its left side, after that lung had been slightly collapsed, and to preserve that lung in a condition fitted to carry on the process of respiration.

It is chiefly to the motions of the diaphragm, that the respiration is to be ascribed ; though, as has been stated, in some animals, and especially in man, a considerable share of this office is to be attributed to the intercostal muscles. Those animals which are remarkable for swiftness and perseverance in the race, scarcely employ the intercostal muscles, but use the diaphragm almost solely in breathing.

The action of the abdominal muscles, though not necessary, may occasionally be useful in respiration. When any substance lodges in the bronchi which is too tenacious to be separated, or too ponderous to be elevated by the powers commonly employed in respiration, the contraction of the abdominal muscles will increase the prèssure against the lower superficies of the diaphragm, and augment the velocity and the force with which the air passes through the bronchi and windpipe.

There may be also many diseases of the chest, in which the action of the abdominal muscles may be serviceable in respiration.

In no circumstances almost, during health, can it be supposed that the aid of the abdominal muscles is required to elevate the diaphragm ; as the elasticity of the lungs is of itself sufficient for that purpose ; and, unless it were counteracted by the residue of contractile energy subsisting in the muscular fibres of the diaphragm even during their relaxation, the cause might often be found too potent. In the act of sneezing, when the diaphragm, after a full contraction, is suddenly seized with a kind of paralysis, it is probable that the unresisting fibres are protected from the injury which they would be in danger of incurring, in consequence, of the violence of the jerk to be sustained in their relaxed state from the inequality of pressure, by the synchronous and almost involuntary contraction of the muscles of the larynx, fauces, and mouth. By this contraction the free exit of the air is resisted ; the air in the chest confined and compressed, checks the collapse of the lungs, and of consequence the ascent of the diaphragm.

This defence however is not always effectual. Sneezing has been known to be suddenly fatal in consequence, as has been supposed, of the rupture of a blood vessel in the chest, but more probably of the rupture of the diaphragm itself.

The great object of respiration seems to be, to bring a quantity of atmospheric air, for a given time, in contact with the internal surfaces of the air vessels of the lungs. The most perfect respiration, therefore, would appear to be that, in which the extent of this surface differs most considerably between the lowest stage of expiration and the fullest inspiration. This object will be most perfectly accomplished by augmenting in inspiration the air vessels, both in diameter and in axis.

Those air vessels which run in a longitudinal direction, must be lengthened in axis, by the depression of the diaphragm. There are two ways in which the same vessels may be increased in diameter. The first is by increasing generally the diameter of the chest. As the contents of the blood vessels must be at all times nearly the same, and as the other parts are fixed and unchangeable, the widening of the chest at any section must be attended with a proportional increase in the calibre of the air vessels at that section. So far as the general diameter of the chest is augmented by the elevation of the ribs, in the same proportion must the diameter of those air vessels, which run from the apex to the base, be generally increased.

But there is another way in which the same effecti s produced, and which is particularly necessary in those animals which breathe through the play of the diaphragm alone.

The chest forms a cone with its apex towards the neck, and its base facing the belly. As the lungs are at all times in contact with the internal cavity of the chest, and, as they are firmly attached to it at the apex, but at every other part free, the depression of the diaphragm must be accompanied with a lengthening of the lungs to the same extent; and this extension, it is reasonable to suppose, must be distributed equally through the whole length of the lungs; or every small portion of those organs must enjoy its own proportion of the increase. Any transverse section of the lungs, parallel to the base of the chest, must occupy a lower position in the chest, during full inspiration than it did during the lowest stage of the preceding expiration. The area of this transverse section will be larger, and of course, the air vessels which run longitudinally, will be wider at that section during the former, than during the latter state, in consequence of the conical form of the chest. The air vessels, therefore, which run in a longitudinal direction, will be widened as well as lengthened by the depression of the diaphragm; and those which run transversely will be lengthened as well as widened by the same cause.

Hence we plainly discover the important purposes which are served by the shell of the chest being in a conical form.

It is evident that immediate death, or death deferred so long only as it can be under the total extinction of breathing, is the consequence of openings admitting the air freely and uninterruptedly into both cavities of the chest. This conclusion, which is not only clearly deducible from the structure and action of the organs concerned, but is supported by all the experiments which I have myself made, or with which I am acquainted, has nevertheless, been lately disputed, or rather denied. The denial is founded chiefly in this case, upon some experiments that were, some years ago, made by Dr. Williams and Dr. Traill, two eminent physicians in this place, and were instituted in consequence of what had been published, or advanced in conversation by myself. It was contended by these gentlemen, as a result of their experiments, that the expansion of the lungs in the living system, is independent of any difference of pressure upon one surface of the lungs ; that, in a word, the lungs possess the power of dilating themselves. This doctrine is rendered, in the first place improbable, by the structure of the lungs, which is of a powerfully elastic nature, as any one may be readily satisfied, by taking a piece of lung, of that large, irregular, pale red, spongy mass, daily seen in the shambles, suspended by the windpipe, and stretching it; the lung readily resiliates into its former length, as soon as the stretching power is withdrawn: and from the form of the lungs themselves, which are globular, and which supply no agent for expanding this globe, against the tendency which it possesses to become of a less diameter. But it as been urged, that though reasoning may have weight when applied to the dead lung, it may be quite otherwise with respect to the lung when pervaded by the principle of life; and that, however plausible our reasoning may be, that reasoning must yield to the more powerful testimony of experiment.

With respect to the nature of the principle of life, and to the manner in which it sets its agents to work, we know nothing; but it is contended, that whenever a mechanical effect is produced by it, it acts by the employment of physical agents. But agents fitted for the supposed purpose, are no where discoverable. Among the experiments detailed was one, and indeed the only one, from which any fair conclusions could be made, respecting the matter at issue, at which some gentlemen were present, who had become abettors of the doctrines advanced by me. In this case, large orifices were made between the sixth and seventh rib on each side, and the ribs were kept separate, by interposing the fingers between them. The animal, which was I believe a dog, died in two minutes; that is, in the time during which, as has been stated, an animal totally deprived of breathing could live. What observations were made at the time, I do not know; but in the published account of this experiment it was contended, that this was not a fair experiment, in so far as that the auxiliary powers of respiration were withheld by the forcible separation of the ribs. These auxiliary powers are not specified; but they must have been the intercostal muscles, the integuments of the chest, and the elastic cartilages of the ribs, and it was for allowing these auxiliary powers to interpose, that all the other experiments were, for the purpose for which they were contrived, perfectly nugatory.

In the experiment, in which a communication between the external air and the surface of the lungs was maintained, by blowpipes inserted into wounds made in the sides, an inlet for the air was supplied by three passages, the windpipe, and a blowpipe on each side. When the chest was quickly and extensively dilated, air would rush impetuously through those passages. Some resistance would be opposed to the entrance of air through the windpipe by the resilience of the lungs, which however in that state of the organs would be slight. But the calibre of the blowpipes being small, a sufficient quantity of air could not be introduced through them into the chest, in the time required, without great impediment to it, from friction against the inside of the blowpipes. When the resistance to the entrance of air through the blowpipes was greater than that through the windpipe, or, when the obstacle to the entrance of air, derived from the friction against the sides of the blowpipes was greater, or indeed was not less, than that derived from the elasticity of the lungs, united to the friction of the air, through the more extensive opening of the windpipe, the air would follow the least obstructed course; would, in that case, dilate the lung to a certain extent in opposition to the slight elasticity which it possessed at the commencement of its dilatation. Hence arises the extension of the lung, which was ascertained by its swelling up against the end of the blowpipe, and the other appearances; which, in these experiments, were taken to be evidences of a self-expanding property in the lungs.

Another offspring of these experiments, bearing, it must be confessed, a great family likeness to the former, and giving indisputable proofs of its parentage, is that the lungs, in the sound and living system, never completely fill the chest; that there is always a space occupied by some other substance, or rather, it would appear unoccupied, between the pleura pulmonalis and pleura costalis. The same opinion was advanced by one of the cotemporaries and opponents of Haller, and was held to be proved by the same experiment which has lately been published as original. One great advantage of this opposition was, the eliciting from the illustrious Haller an examinatiou of the question, during which that physiologist has adduced such proofs of the pleura pulmonalis and pleura costalis being at all times, in the sound state of the system, in contact, as seemed to have set the subject forever at rest. In these days, when the nature of atmospherical pressure is so much better understood, and when the extent to which respiration is impeded, either when water, or air, or any other substance, finds a lodgment in the sacks of the pleura, the resuscitation of the doctrine appears not a little surprising.

As I have, in another work, already examined the experiments and deductions alluded to, I should not have noticed them again in this place, if I had not observed that great importance was attributed to them by some recent writers, particularly by Dr. Copland, in the notes affixed to his translation of Blumenbach's Physiology, who has regarded them as in a great measure subversive of the doctrine, that a power of great influence in the respiration, and in the circulation of the blood, is generated by the elasticity, or resilient property, of the dilated lungs.

It is remarkable, how slight a thing is required to satisfy minds even of a superior cast, when its tendency is, the removal of any galling or mortifying opposition to doctrines which have been long received ; or, perhaps taught publicly, as irrefragably true.

No two propositions in the whole range of physiological science, are more indisputably ascertained, than that the lungs possess no inherent property of dilatation, and that in health these organs are at all times in contact with the wall of the cavity in which they are lodged.

As satisfactory proof was supplied by repeated experiment, that an animal could live by using one lung only in respiration, it was imagined that the knowledge of this might be applied to the cure or relief of some diseases, particularly of consumption. It is generally believed that the ulcers, from which matter is discharged in consumptive cases, are prevented from healing, as well as the same kind of ulcers would do in other parts of the body ; by the agitation to which they are exposed in respiration.

In addition to that cause, another powerful one was supposed to be derived from the structure of the lungs, and from their position in the living body. In that state, the elastic substance of the lungs being always on the stretch, would be distended beyond its natural limits; if, therefore, the fibres of which the lungs were composed, were divided, they would resiliate from each other and the wounded surfaces could not again be brought into contact.

It appeared to be practicable to remove those two causes of the impediment to the cure of wounds, by reducing the diseased lung to a state of collapse. This would evidently be done by admitting the air into the cavity of the chest, by an opening through the side, and preserving the passage to it free for a sufficient time. The lung would, by those means, be reduced to a state of quiet collapse, and the elastic fibres separated by the wound would be brought together. The lips of the divided surface would be brought into contact. The disease, if in one lung only, as frequently happens, would be cured by this process alone.

It was found, that if a lung which had been reduced to, and kept in, a state of collapse for some time, and if the opening, through which the air had been admitted for reducing it to that state, were allowed to heal, the lung would, in consequence of the absorption of the air contained between the two pluræ, be again dilated to a contiguity with the internal surface of the chest; and that if the other lung were diseased, it might then be with safety reduced to a state of collapse; depending upon the lung now healed, for the performance of respiration ; and treated as the former had been. A paper containing those speculations was read in the Literary and Philosophical Society of Liverpool, where it produced a considerable sensation, and raised hopes, which, alas ! have not been realised, both in several members of the medical profession, and other members of the society. An opportunity occurred some time after, without any solicitation on my part, of putting the matter to the test of experiment.

James Sloane, Esq., an eminent merchant of Liverpool, the last of five brothers, the other four having died of consumption a few years before, had returned from the West Indies, to which he had gone for the purpose of trying what a change of climate might do in his case, in the last stage of consumption, which he knew to be incurable from any known remedies. Soon after his return, he heard of the paper to which I have alluded, and soon became determined of having the operation, which I had suggested as a possible means of giving relief, performed in his own case. It was done on the 26 of Sept. 1822, by Mr. Bickersteth, an eminent surgeon of this place, in presence of the late Dr. M'Cartney and myself. An incision calculated to admit air freely into the chest, was made between the sixth and seventh rib. As the sound usually heard upon an opening being made in the chest, and produced no doubt by the rapid passage of the air through the opening, was not perceived in this case, it was suspected that the lung did not collapse, and that an adhesion had prevented the entrance of the air. It was not deemed advisable to make a further examination for that purpose at that time; a tent was inserted into the wound to prevent an adhesion of thc sides, by the first intention. At our visit next day, when the wound was examined, our suspicion of adhesion between the lungs and the chest at the place, was confirmed at the first aspect; the discharge from the wound had not found access into the cavity of the chest, which it ought to have done in the position of the body, but was oosing out of the wound; an examination by the finger ascertained an adhesion of the lungs to the ribs, both above and below it; a probe found admission to a greater length across in both directions, but at length met with an obstacle. Mr. Sloane suffered no inconvenience from the operation, but thought his chest somewhat relieved. This might happen from the adhesion being rendered by it less tense. The orifice was kept open, and he suffered no inconvenience from it till the day of his death, which happened about a month after the operation, and which had been deferred fully as far as it was suspected it could be, at the time he arrived from abroad. Examination after death, shewed adhesion to have taken place between the pleuræ completely at the diaphragm, along both sides of the mediastinum, and along the chest, every where except at the spaces between the ribs, where the fingers might be admitted as into the fingers of a glove. The lungs filled the chest, and shewed no tendency to collapse. Two large abscesses were found, one in each lung. The substance of the lungs was tuberculous and hard throughout. Adhesion, so universal, did not in this case appear to arise from any previous inflammation, to which these adhesions are generally attributed; for, though I attended him regularly in every complaint to which he was subject for many years, I never had reason to suspect any inflammation of the chest; but, in my opinion, from the tuberculous affection of the lungs, by which they were deprived of all elasticity and of consequence of that sliding motion above described, which occurs in health between the pluræ, and which I conceive to be the chief cause of the absence of adhesion between these membranes. The diaphragm was deprived of its antagonist, the elasticity of the lungs. The customary movement of that organ must long have ceased, and in its quiescent state it had readily grown to the pleura pulmonalis. It was observed long ago, by that great ornament of the medical profession, Boerhaave of Holland, to be in phthisical affections a bad symptom, if the abdomen did not protrude in inspiration, or in other words, if the diaphragm did not descend. The cause of the unfavorableness of this symptom, of which Boerhaave could have no just knowledge, can now be satisfactorily explained.

After the death of Mr. Sloane, the attention of myself, and indeed of the medical profession of Liverpool, was directed more earnestly to the condition of the lungs in consumptive cases ; and it was found universally when patients had died of tuberculous phthisis, that the lung affected did not collapse in its whole extent, and that adhesions had uniformly taken place between the pluræ. It often happens that only one lung is diseased ; in that case, no adhesion will be found to have taken place in the side occupied by the sound lung. If the lungs were tuberculous, but not altogether solid ; if some elasticity still remained, the adhesions would be confined to the upper part of the chest, where the sliding motion is at all times slightest, and leave the lower part of the sides and the diaphragm free. Adhesions in the chest, I would conclude then, do not at all times arise from inflammation, but perhaps most frequently from a disease in the fabric of the lungs. It is very evident, that the operation which was performed on Mr. Sloane, could have no good effect in the case of tuberculous consumption. Whether there are any other kinds of phthisis in which it might be serviceable, will afterwards be examined. Let us inquire, now, whether any thing further could be done in such a disease as that of

Mr. Sloane ; I am of opinion that something more might be done, and that a dependent opening might be made through the ribs, for the discharge of the matter. If it is supposed the operation had not stopped at the division of the pleura costalis, but had made an opening into the abscess through the substance of the lung itself, no injury could evidently arise to respiration by such an operation. In this state of the lung, it signified nothing whether the air passed through the windpipe or through holes in the sides. The lung could not be collapsed. I anxiously waited for an opportunity for making this operation ; I resolved not to make the proposal, but would encourage it in case it came from the patient himself, or some of his friends. An opportunity of this kind occurred in an intelligent merchant of this town, of the name of Johnstone. He was well acquainted with the nature of the operation that had been performed on Mr. Sloane. I explained to him what appeared to me requisite to give it any chance of success. Finding himself on the brink of the grave, he proposed that the operation should be performed to the extent that I wished. Mr. Bickersteth, who, it is only fair to state, had shewn as great zeal in this scheme as if he had been the original projector, was selected as the operator. The patient's own surgeon, Mr. Samuel M'Culloch was, at the request of the patient himself, present. It was agreed that the operation should be proceeded in after the same manner that it had been done in Mr. Sloane's case, and if matters were found in the same condition as they were in that case, to cut freely into the substance of the lungs, taking care that the large blood vessels were avoided. When the first part of the operation was performed, matters appeared to be exactly as they were in Sloane's case. This was explained to the patient, but he declined from having any thing more done at that time. At a subsequent visit of Mr. Bickersteth, at which I was not present, the patient consented that the operation might then be completed, which Mr. Bickersteth, taking advantage of the present disposition of the patient, instantly complied with. An incision was made into the lung. A pretty profuse hemorrhage took place, which the operator had considerable difficulty in stopping, No abscess had been reached, no purulent matter was discharged, the disease pursued its course, and in a few days terminated with the death of the patient.

The appearances, on dissection, were exactly the same with those which had been exhibited in Mr. Sloane's case. The lungs adhered every where to the substances that were in contact with them, except at the interstices between the ribs. The incision which was made on the right side, had nearly reached a large sack full of purulent matter. It may be asked, what benefit could have arisen to the patient if this sack had been opened and an outlet afforded to its contents through the side? I answer, great. In the first place, a dependent opening would have been afforded to the matter in the sack, from which it would have been discharged as soon as formed, instead of accumulating, lodging and constantly filling the sack. It is well known how advantageous such openings are in parts externally situate. The matter, so far as that sack was concerned, would have been discharged without coughing, the painful and exhausting nature of the process required for discharging it in this manner, is well known. The sores in that lung would have been placed in a situation, as favorable for healing as the same sores could have been in any other part of the body. Nor would it have been necessary to have confined the operation to one side. The breathing on the side on which the operation had been made, would have been rendered less imperfect, insomuch as it would have been relieved from a load of morbid and acrid matter.

As I have before remarked, since the lung in this situation could not collapse, it matters not whether the air entered into the chest through the windpipe or through the hole in the side. If coughing and a discharge of matter by that distressing process still continued, it was with certainty to be inferred that there was a collection of it in the other side. An outlet to the matter on this side might be easily afforded, in the same manner as had been done in the other, without increase of danger, and with the same relief to the respiration. If no matter were discharged by cough, it was to be inferred that the other lung was sound, and no further proceeding necessary.

It will readily occur to any one, and appear as an objection to the operations which have been stated, that the matter of a diseased lung is not contained in one sack, but in a multitude of tubercles, without having communication with each other. It may be remarked, that the matter from each of these sacks, finds its way sooner or later into some of the ramifications of the windpipe; and as those ramifications have a communication with each other by their common stem, a communication between this stem, or indeed any of its branches and the outlet in the side, would secure the discharge of matter as certainly through this outlet as through the windpipe. And a communication being once established, it might be rendered more free, as the discharge was encouraged to take that course.

In cases of empyema on one side of the chest, a dependent opening for the discharge of the matter contained between the pleuræ, would be of indisputable benefit; and as it might be ascertained by symptoms rendered more plain by the use of the stethescope, on what side the matter was contained; and as empyema arises frequently in constitutions, in which the lungs are otherwise little diseased, complete cures might be often expected. I remember the case of a lady, who fell a victim to this desease, who I think, might with certainty have been cured by an opening made into the chest. She discharged periodically, I think thrice a day, about a gill of matter. This matter was raised at once by a violent paroxysm of coughing, with great sense of suffocation. In the intervals she was tolerably well. After death the body was examined. The left sack of the pleura was filled with matter. The left lung was small and dense. It was supposed by the surgeon that the lung was wasted by the discharge of matter, but I suspected at the time, that the smallness of the lung was occasioned by its complete collapse, for a long time the air being totally expelled from the bronchi. The matter found its way into the windpipe through its left branch, and when all the chest was full, to the extent of its reaching this orifice, it was discharged in the manner stated. She was a lady of a very sound constitution. The disease had, originally, been the effect of a simple inflammation of the chest.

No disease has been inflicted upon mankind that is more to be deplored than consumption; whether we consider the number of its victims, the length, and frequently the severity, of their sufferings, the age at which they are usually selected, or the moral and intellectual qualifications with which they are commonly endowed. It crops the flower of the human race. In no way, therefore, could man be supposed to approach more nearly to the exercise of the divine functions, or to render a higher and more endearing service to the world, than by discovering a cure for this disease.

The experience of every age and country, too well attests the insignificancy of all our efforts to arrest even for a moment, the slow but steadily onward pace of this fatal malady ; and if any addition to the proof of its incurability under every method of treatment hitherto proposed were required, that addition would be supplied by the views, supposing them to be just, that have been exhibited of its nature.

As, indeed, the mechanism of the lungs, on which the peculiar character of the disease is believed to depend, has only been discovered of late, no rational plan of treating it could, according to our hypothesis, have previously existed ; and far distant from the desired mark must every project have fallen, made at random or with a mistaken aim.

It has long been my opinion that if ever this disease is to be cured, and it is an event of which I am by no means disposed in all cases to despair, it must be accomplished by mechanical means, or in other words by a surgical operation. Whether the method proposed will be found practicable, or, if practicable, to the desired extent beneficial, or whether, as will be supposed by far most probable, it may amuse for a moment, and then like all its predecessors sink into deserved neglect, are questions which must be left to the decision of time. Whatever may be the event, I shall have this consolation, that I incur no risk in this case by any proposition that may be made, of diverting the current of inquiry into a channel that shall be less productive than any of those in which it has hitherto run.

Some other diseases of the chest appear susceptible of elucidation, from the views which have been presented, of the mechanism of respiration ; and might be expected to be examined at this place ; but these diseases are so much influenced by the actions of the heart, that that examination, it appears, will be more advantageously made, after the circulating system has been discussed.

ON THE MOTION OF THE BLOOD.

Two Centuries have now nearly elapsed since the Circulation of the Blood was first taught by Dr. Harvey. This theory has long surmounted the opposition against which it had for a period to contend, from inveterate prejudices and jealous ambition ; is universally acknowledged to have been fully proved by its author ; and is deservedly ranked among those discoveries, which are supposed to confer the greatest honour upon human nature.

Although, however, the course, which the blood pursues in its march through the animal frame, has been ascertained to the conviction of every inquirer ; differences of opinion are still found to prevail, respecting the causes by which it is moved in that course. Considering the immense progress which the mechanical sciences have made since the days of Dr. Harvey, and the attention which so important a subject must have at all times engaged, it might at first view have been expected, that the causes of the motion of a fluid, of which the path had been for ages so fully known, must long ago have been ascertained, with that mathematical preeision which is incompatible with difference of opinion. When, however, it is considered, that the machinery employed in the motion of the blood, is carefully concealed from the view; that every attempt to unveil the hidden springs of this machinery necessarily destroys it; and that the delicate and exquisitely appropriate movements of living organs cannot be imitated by any artificial process, the surprise, excited by the subject being still involved in obscurity, will soon be removed.

With respect to the sentiments which are at present entertained, concerning the causes of the motion of the blood, physiologists may be divided into two classes. The first maintain that the blood is transmitted from the heart to the extreme vessels, and from the extreme vessels back to the heart again, by the projectile power of this organ, aided by the vibration of the arteries. The other class contend, that the force impressed upon the blood by the heart and arteries is expended, or nearly so, at the terminations of the arterial system; and that the motion of this fluid, while in the veins, arises from causes distinct from those by which it had been produced in the arteries.

The more recent physiologists have generally arranged themselves in the latter class; but though they have succeeded in opposing insuperable objections against the supposition of the blood in the veins being solely returned by the joint action of the heart and arteries, conveyed to it through the extreme vessels, or in the language of the schools, by a *vis a tergo*; they have hitherto failed to explain its motion in this part of the sanguiferous circle upon any other hypothesis.

At an early period of my medical studies, an accidental occurrence fixed my attention more particularly on the circulation of the blood. In the course of that investigation, I was struck with the wide diversity, and even opposition, in the opinions entertained by writers respecting the causes of the motion of the blood, especially in the veins; the inadequacy of these causes, in many instances, to the effects ascribed to them; and the generally unsatisfactory and defective explication of the phenomena deducible from any hypothesis.

At length I thought I perceived, obscurely marked on the motion of the blood, the vestiges of powers, the influence of which had been unnoticed, little valued, or erroneously estimated by physiologists; and I ventured to explain my sentiments respecting their importance in the circulation, in the Inaugural Dissertation which I published at Edinburgh, in the year 1799.

Since that period, I have frequently, after intervals, returned to the consideration of the subject; and though I have been able to discover abundant errors and great deficiencies in the Dissertation, I have been more and more convinced, by every succeeding reflection, that the sources of the motion of the blood, while in the veins, have not been detected; and that an important share of this motion belongs to the powers to which I have alluded.

The following pages will contain,—

I. An enumeration of the causes to which the blood has hitherto been attributed, accompanied with an exhibition of the proofs by which their inadequacy to that effect is supposed to be established.

II. The true causes of the motion of the blood.

III. An explication of phenomena.

I. Before we proceed to enumerate the agents which have been supposed to contribute to the motion of the blood, or to attempt to estimate their share in the operation, it seems proper to describe the organs in which this fluid is contained, and the parts more intimately connected with them; but the account to be given of these in this place need only be concise and general, as it may be required to describe them more minutely afterwards, when the uses of each shall come to be investigated.

The heart, the arteries, and the veins, the organs in which the blood circulates, demand our first attention.

The general appearance of the heart is familiar to every one, and is nearly the same in all animals. It is usually said to resemble a cone, with an obtuse apex, a rounded or oval base, and at the sides more or less swelling or convex. Externally the heart is every where smooth, polished, and free, except at the base, where two rough, notched, and ear-like appendages, hence denominated auricles, are attached; and where a number of vascular tubes penetrate it, and connect it to the surrounding parts. Internally it is hollow and divided into four chambers, of which two, denominated ventricles, are contained in the body of the heart, and the other two in the appendages just mentioned are termed auricles. It is, besides, divided into two parts, between which there is no direct communication, by a septum or middle partition, which has on each side of it one ventricle and one auricle; those upon one side receiving the epithet of right, and those on the other of left. The right auricle opens into the large venous trunk, called the vena cava;

and communicates by another passage with the right ventricle, which, besides the outlet common to it and the auricle, has a communication with the great pulmonary artery. The auricle on the other side of the partition, communicates by distinct openings with the four pulmonary veins, and also by another passage with its corresponding ventricle; which, besides this mutual outlet, is penetrated by the large artery called the aorta.

The ventricle and auricle, therefore, on each side of the middle partition, besides their mutual communication, have outlets into the trunks of the blood vessels; the auricles communicating with the roots of the veins, and the ventricles with those of the arteries.

The structure of the heart is muscular and tendinous. The walls, or substance inclosing the different cavities, vary greatly in thickness. The walls of the ventricles are much thicker and more substantial than those of the auricles, and those of the left ventricle than those of the right. The thickness of the walls of the respective ventricles is pretty nearly uniform; but the walls of the auricles are extremely unequal, being composed of muscular and tendinous ridges, which especially project inwards, and which are connected only by a thin, transparent, expansive membrane.

Valves are stationed at all the openings into the ventricles. Those at the passages between the ventricles and auricles, usually called the auricular passages, are framed to favor the free current of a fluid from the auricles into the ventricles, but to obstruct its return from the ventricles into the auricles. Those at the roots of the arteries are calculated to prevent the return of any fluid from the arteries into the heart, without impeding its free passage from the heart into the arteries. No valves are stationed at the openings by which the auricles communicate with the large venous trunks. The chambers of the heart, in a man full grown, are each supposed to contain about two ounces and a half of fluid.

The arteries arise in two trunks from the ventricles, one from each. They are hollow cylinders, and at a short distance from their origin, branch into other vessels of the same description. Every branch becomes the stem of a smaller order of branches, and this process continues till the ultimate ramifications become so minute, that their bore will scarcely admit a single hair, and so extended and numerous as to penetrate almost every particle of the body.

The rule observed with respect to the dimensions of these cylinders is this :—the bore of the stem is always greater than that of any of the branches, but less than the bore of all the branches united. Hence the arteries have been properly said to constitute a cone, of which the apex is at the heart, and the extended base is measured by the united capacities of the extreme capillaries. The bore of the trunk of the aorta compared with that of the branches united into which it has ultimately divided, has been attempted to be determined, but the conclusions to which these attempts have led, have been often widely different. According to Keil, the bore of the aorta is to that of the extreme vessels united as 1 to 44,500. According to Helvetius and Sylva, as 1 to 700: as 90,000 to 118,490, according to the calculations of Senac. Martin maintains that the diameter of a trunk or stem is equal to the cube root of the diameters of its immediate branches united. Calculators have not been able to arrive at any certain conclusions on this subject. Some imperfect notion may be formed of the difference of the capacity of the aorta and that of its ultimate ramifications united, by considering the different velocities of the blood in the aorta and in the extreme vessels; as the square of the diameter of the aorta will be to that of the diameter of all the last ramifications in the inverse ratio of the velocities of the blood at these parts. From the observations that have been made respecting the motion of the blood in the extreme vessels of transparent parts, it is evident that the capacity of all the ultimate branches must in a vast degree exceed that of the arterial trunk.

The arteries are of a strong and firm structure. They are composed of different coats, each of which seems to confer upon them peculiar properties.—These coats are generally reckoned to be four, but anatomists are not altogether agreed respecting their number. The coats most particularly deserving of notice in this place, are the muscular and tendinous, and these are universally allowed. The muscular fibres are very numerous and generally pursue a circular or spiral course round the cylinder, though they are frequently found to run in a direction nearly parallel to its axis. The muscular coat confers upon the arteries the property of irritability; as the tendinous structure gives them that of elasticity, which they unquestionably possess in a considerable degree; for, when taken out of the body and empty, their transverse section forms a complete circle.

The veins, as has been stated, arise in different trunks from the auricles. They are also hollow cylinders, and with respect to their course, form, and laws of ramification, agree with the arteries already described. They are generally larger and more numerous than the concomitant arteries; so that the capacity of all the veins is supposed to be to that of all the arteries as 2 to 1, or

3 to 2. Their structure is very different from that of the arteries. They have no muscular coat, at least the muscular fibres are few; they therefore can scarcely be said to be irritable. They are much thinner than the arteries of the same size; are very dilatable; but possess little elasticity; their transverse section does not form a circle. But though they are thin and readily yield to a slight distending force, they are not easily ruptured; indeed they are found to sustain, before they break, a greater force than the arteries which accompany them.

The internal surface of the veins differs in one remarkable particular from that of the arteries. In the arteries it is uniformly smooth and equal. In the veins there are frequent projections formed by the foldings of the inner coat. These projections are circular segments, and are formed in pairs or triplets at a place. They are very numerous in the veins that run nearest the surface of the body, but are not found in many of the veins of the internal viscera. They are supposed to serve the purpose of valves; obstructing the current of a fluid from the heart to the extremities, without impeding its course in the opposite direction.

The veins which arise from one stem frequently communicate with each other and with those of other stems by intervening vessels, which are called by anatomists anastomosing vessels. These anastomoses are found in all parts of the venous system, but are particularly numerous among the smaller vessels. We shall have occasion to treat of these more particularly in a subsequent part of this inquiry.

The mode of communication between the ends of the arteries and the ends of the veins has been long a subject of dispute among anatomists, and is still in some degree undetermined. It was long warmly maintained that the extreme arteries and extreme veins communicated through the intervention of cells. Hence the *corpora nodosa* of authors. It is certain that in some parts of the body the arteries pour their contents into cells from which it is taken up by veins, as in the corpora cavernosa penis, in the mammæ, in the uterus, &c. This structure is generally considered however to be peculiar to particular parts, and by no means an example of the manner in which the arteries always, or even generally, communicate with the venous capillaries. It is now usually supposed that the ultimate arteries enter the ends of the veins immediately, but in such an oblique manner that the side of the penetrated vein forms a valve, opposing the course of a fluid from the vein into the artery.

The arteries, besides their communication with the extreme veins, have other terminations. They often terminate in glands by which peculiar fluids are secreted. They become exhalent vessels opening on surfaces, and pour out a transparent fluid for the lubrication of parts. The veins also, there can be little question, often terminate on surfaces and act as absorbents.

An artery is generally accompanied by one or two veins, which with a nerve are inclosed in a common sheath, and are embedded in a cellular substance.

The heart is inclosed in a membraneous bag of a very strong and firm texture. This bag is called the pericardium. It is no where attached to the heart, which is every where free ; and is only connected to it by the roots of the large vessels which penetrate it soon after they have issued from the heart, and to which in their passage it communicates a covering. The pericardium is firmly bound to the tendon of the diaphragm on which it rests. This envelope is found in all animals.

Particular appellations are given to different divisions of the circulating vessels. The aorta and all the vessels of which it is the original trunk, are called the system of the great aorta, or aortic system. The vena cava and its ramifications are termed the system of the vena cava or simply cava. In like manner the pulmonary vessels are divided into the system of the pulmonary artery and system of the pulmonary veins.

In the brief description about to be given of the march of the blood through the body, every term is purposely avoided which might convey theoretical notions respecting the action of any of the circulating organs.

Let it be supposed that the blood has reached some particular stage in the circulation, and let that stage be the ends of the arteries of the aortic system. Having been poured in infinitely small streams from the innumerable ends of the capillary arteries, the blood passes into the ends of the capillary veins, which are at least equally numerous and minute. In these vessels, like rivulets descending from their sources, the small streams frequently unite and form larger, till, after a long succession of confluences, the blood reaches the right auricle of the heart, contained in a single channel, formed by the meeting of the two branches of the vena cava. The right auricle, as soon as it has been filled from the cava, is successively emptied into the right ventricle. The blood, therefore, does not now enjoy the continued smooth course with which it had flowed in the veins, but is transmitted in divided portions with regular succession, into the right ventricle, which successively, as it receives the contents of the auricles, conveys them to the pulmonary artery. In following the ramifications of this artery, the blood is distributed through the whole substance of the lungs, subject throughout to alternate accelerations and retardations. From the ultimate terminations of the pulmonary artery, it passes into the ends of the veins of the same system, where it resumes the equal continued flow which it had enjoyed in the system of the vena cava; and, by the frequent junction of the smaller veins, is conveyed into larger and less numerous channels, till it reaches the left auricle, issuing from the mouths of the four pulmonary veins. From the left auricle it is forwarded to the left ventricle, and from this ventricle into the great aorta, in the same manner in which it had passed through the right side of the heart; and is distributed in a regularly retarded and accelerated motion in the extensive ramifications of this system, through all parts of the body, except the lungs, till it reaches the ends of the arteries, the stage at which it had, according to this description, commenced, to repeat again and again the same unvaried round, often undisturbed by one alarming pause through the whole course of a long life.

Such is the course which the blood describes according to the Harveian theory; a theory which must form the foundation on which the true principles of the science of medicine, if it shall ever attain the rank of a science, must for ever rest.—It is foreign from the purpose of this inquiry to examine the proofs by which this theory has been established. It is sufficient to state that they are universally admitted to be conclusive.

It appears then, that the two sides of the heart, though they may have no direct communication, have yet an intercourse through the intervention of the blood vessels. The extremities of the arteries, arising from the right ventricle, do not communicate with the extremities of the veins which branch from the corresponding auricle, but with the extremities of the pulmonary veins. In like manner, all the ramifications of the aortic system pour their contents into the veins belonging to the system of the vena cava. The blood, therefore, before it returns to any given stage in the circulation, washes the walls of the heart twice; passing through the chambers of one side of this organ, on its advance; and those of the other side, on its return. In completing the whole of its course, the blood describes the figure 8; the heart being placed at the point at which the two circles touch, or their lines decussate. It evidently performs two circulations; the one through the lungs, in consequence of the shorter circuit, is termed the less circulation; which was known and described with considerable exactness by

Servetus, long before the days of Harvey; and if the life of that eminent person had not been shortened by the persecuting Calvin, England, there is every reason to suppose, would not have had the honor of giving birth to the discoverer of the other, which, as it embraces the extreme parts of the body, is termed the greater circulation.

The human heart, therefore, is a double organ, having two chambers for the reception and two for the discharge of blood. But this two-fold property is by no means necessary for the performance of its functions; nor is it to be found in all animals. Those animals commonly termed the cold blooded, as fishes, reptiles, insects, *&c.* have only a single heart consisting of one auricle and one ventricle, one chamber for the reception and one for the discharge of blood; with one system of vessels belonging to each, one system of veins and one system of arteries. This view of the circulation is more simple, but not less perfect. Take again the termination of the arteries as the stage at which the blood commences its march. It passes from the arterial into the venous system, through the terminations of each; is conveyed through the converging venous system in a placid continuous flow to the auricle; which, at intervals as it is filled, empties itself into the ventricle. By the ventricle, in like manner, the blood is transmitted into the root of the arterial system, in pursuing the diverging branches of which, it is brought to the stage at which its motion was supposed to have commenced.

In the following pages it may be necessary, to avoid circumlocution, and for the sake of perspicuity, to regard the human heart as a single organ, consisting of one ventricle and one auricle, with one system of veins, and one system of arteries; in which case, the larger circulation will be understood.

We are now prepared to consider the action of the circulating organs, and its influence in promoting the movement of the fluid they contain.

It may be necessary in this place to promise, that any investigation of the causes of muscular motion or of that property of muscles, in consequence of which, upon the application of certain stimulating substances, or in obedience to the will, or through sympathy with other muscles, they are contracted, is not aspired at in this inquiry. We are, therefore, relieved from the consideration of those opinions of which all ages have been so fruitful respecting the cause of the action of the heart; and which as they are purely hypothetical, may fairly be presumed to have been in every instance erroneous.

As, further, the effects of the actions of the circulating organs upon the motion of the blood could not have been ascertained, with any degree of correctness, before the direction of that motion was known, it seems unnecessary to allude to any opinions that were entertained respecting these actions previous to the discovery of the circulation.

It had been observed, from a very early period, that different parts of the heart were alternately contracted and dilated. It has been found that the body of the heart, or that part of it in which the ventricles are contained, is all contracted or dilated at the same instant; that the two auricles or appendages are also synchronous in their movements; but that the dilatation of the auricles corresponds in point of time with the contraction of the ventricles, and vice versa. The similar conditions, therefore, of the two ventricles are synchronous, but alternating with those of the auricles which are also synchronous. These conditions of the heart are expressed in the language of anatomists. The state of expansion or dilatation is called their diastole, while the opposite state of contraction is termed their systole.

Let the description to be given of the movements of the heart commence with some supposed state of that organ, and let that state be the diastole or state of greatest distension of the ventricles, and, of course, the systole, or state of utmost contraction of the auricles] In this situation the ventricles are distended with blood while the auricles are empty or nearly so. In consequence of a stimulous derived from the blood and from the distension of the muscular fibres, the ventricles are roused into action ; they contract forcibly upon the blood with which they are filled, and propel it through whatever outlets may occur. These outlets we have seen are two in each ; one into the great artery and another into the corresponding auricle. The blood however is prevented from flowing into the auricle, which is at this moment empty, by valves ; which are stationed at the opening into it, and which, as was observed, are so formed as to prevent the blood flowing from the ventricles into the auricles ; but there is nothing to impede its course into the artery ; it therefore flows with great rapidity through this vessel. The ventricle being now in the systole, the auricle becomes dilated. Whence is derived the blood by which the auricle is to be filled ? The auricle is penetrated by two openings, one common to it with the corresponding ventricle ; the other proceeds from the large vein. It can receive no blood from the former opening in consequence of obstructing valves. It must therefore be filled from the vein. The auricle being in its turn dilated and filled, contracts upon the contained blood pouring it into the ventricle which is, consequently, again distended and filled. The blood, with which the ventricle is filled afresh, must necessarily be derived from the auricle; for, at the only other entrance into it, that from the great artery, valves are stationed which prevent the return of any fluid from that vessel into the ventricle. The actions of the ventricles are successively repeated in the same manner and with the same consequences that have been described.

At one part, this machinery appears to be imperfect. No valves are to be found at the passages between the veins and the auricles. It was to have been supposed, that during the contraction of the auricle the blood would have flowed as readily into the vein as into the ventricle; and, as the latter was to have been dilated by the force of the blood from the auricle, that it would have found the course into the vein more open and less resisted. But it is supposed that the force of the blood on its return to the heart is so great as to form a buttress to the blood in the auricle, and to direct all the power of this chamber against the ventricle.

The systole of the heart was easily admitted on all hands to have been produced by the peculiar property of muscles, that of contracting their fibres upon the application of stimuli. How are the cavities again enlarged? The muscles of the heart being hollow have no antagonist by the action of which, as is the case with the generality of other muscles, the contracted fibres could be again distended. The simple relaxation of its fibres, it was supposed, could not dilate the cavity but only place those fibres in the situation most favourable to be dilated by other agents. It was contended by Dr. Harvey and his followers, and the opinion still prevails, that the auricle is dilated by the force of the blood returning to it from the veins, and that the ventricle is dilated by the force of the blood impelled against the internal surface of its walls, by the contracting auricle.

Whence does the blood at the root of the vena cava receive such a quantity of motion, as is not only sufficient to carry it with great velocity along the channel of the vein, but, in addition, to impower it to resist the contraction of the auricle, and to distend the cavity of that chamber? Dr. Harvey, and many of his followers, contended that this power is derived from the left ventricle of the heart, extending through the aortic and venous systems back to the right auricle. Hence arose a question that for a long time occupied the ablest mathematicians of that brilliant period, "What is the power of the heart?" Mathematicians in all countries vied with one another for ascertaining the power of the heart. But the amazing diversity in the results, to which the candidates for the solution of this question were brought, proves, in this instance, the futility of their labours. While the calculations of Borelli advanced the power of the heart so enormously as to render it equal to the weight of 180,000 pounds; those of the equally learned and ingenious Keil reduced it to a force measurable by the weight of 8 and in another instance 5 ounces. Different weights as measuring the power of the heart, between those two extremes, have been contended for by Jurin, Hales, Robinson, and others.—The problem still remains unresolved; and physiologists are at this day obliged to confess, that there exist no rules by which the power of this organ can be estimated; but the want of this solution is not so much to be lamented, as will afterwards appear, as may be at first view supposed.

Although, however, the failure of the attempts to estimate the power of the heart, and the extreme diversity in the results to which the different candidates for the solution of this question have been led, are matters of little importance, so far as respects the question itself; their consequences have been truly baneful to the medical profession. They early gave rise to an opinion which prevails even at the present day, that the mechanical sciences are useless or unsafe, in their application to physiological subjects. The attempts at such an application of them have even been attacked with ridicule, which has generally been pointed with an allusion to the results of Keil and Borelli, respecting the power of the heart. The consequence has been, that these studies have not possessed their due importance in a medical education; that the science of Physiology has been retarded in a wonderful degree; and the character and usefulness of the medical profession lamentably degraded.

But however unlimited the power of the heart may be supposed, it will be allowed that the effects of this power, must, like those of all other agents, be liable to expenditure; and that the motion, communicated by it to the blood, must be again surrendered by that fluid, according to laws applicable to it in the circumstances in which it may be placed.

It is plain that, in any given time, the same quantity of blood is returned to the heart by the veins that is discharged from it into the arteries. It follows also from this that, in the same times, equal quantities are transmitted through any sections of the aorta and pulmonary artery before their bifurcation, or through any sections of their branches taken together, at the same relative distances from the heart, at whatever distances those sections be taken, as from the heart itself. That the adequate supply may be provided in time at the heart, it follows necessarily that equal quantities must, in a given time, pass through any sections of all the branches of the vena cava and pulmonary veins, taken relatively at proportional distances, that passes through, the sections of the trunks of the vena cava and pulmonary veins before their bifurcation.

To render this argument more perspicuous, let us for a moment return to the supposition of a single circulation as is to be found in the more imperfect animals. And let it be further supposed that the arterial and venous systems form only a single canal; that this canal is of the same length with the arterial and venous systems united, but that its capacity, at the commencement and termination, or at the two ends, is equal to that of the trunk of the great artery and great vein; that, at any intermediate section, it varies according to the joint capacities of the branches, at corresponding sections of the arterial and venous systems. From the description which has been given of the arterial and venous vessels, it is evident that this canal will be constituted by two cones joined to each other at their bases; that these cones will be of equal length, but varying in width or capacity; that representing the venous being larger than that representing the arterial vessels.

Let such a canal be supposed to be represented by the figure A C B D, (fig. 1.) As this canal is supposed to be every where filled with blood ; and, since as much is supposed to be discharged from one end of it as has, in a given time, entered at the other ; it must follow, that the same quantity of fluid will pass in the same time through all the sections of this canal. In any given time, as much will pass through the section EG as enters the canal at AL. The same may be said of CD, HK, or BF, or wherever any transverse section may be supposed to be taken.

The velocity of the blood must vary in different parts of this canal ; it will be greatest at the commencement and termination, and least at the middle. It will be greatest at AL and BF and least at CD. The velocity at the different parts will be in an inverse proportion to the areas of the transverse sections at those parts.

The quantities of motion possessed by the blood in different portions of this canal will be, in given times, as the length of those portions ; and in equal portions the quantites of motion must be equal. Let AE, EC, CH, and HB be supposed to measure equal lengths of the sanguiferous canal. The quantity of motion which the blood, in the same time, possesses in these different portions will be equal.

Upon the supposition, that the blood is circulated by the impelling power of the ventricles alone, this effect must be produced in one of two ways. Either, 1, At every contraction of the ventricle, the heart must charge every individual particle of blood issuing from it at that time, with such a momentum as shall be necessary to carry it to the end of its course with the momentum which it is known to possess there. Or, 2, The sanguiferous canal having been already filled with blood, and an additional quantity having been forcibly thrown into one end of it by the heart, and prevented from returning by the semi-lunar valves, an equal quantity is in consequence of the action of the heart necessarily displaced from the other end of the canal. Let us consider briefly each of these propositions. Let it be supposed that as much force is impressed upon every particular discharge of blood as shall be capable of conveying that blood from one end of the canal to the other, or from the heart through the arteries and veins back to the heart again. This supposition, so far as the individual blood projected is in question, must, from the nature of fluids in motion, and the concomitant circumstances, at the first view, be evidently inadmissible. For the canal into which the blood is projected by the heart, being already in a great measure filled with blood; a communication of motion between the particles forcibly discharged, and those already in the canal, and nearly at that time at rest, must necessarily take place; and such a resistance be immediately opposed to the former, as nearly to destroy the motion they had received from the heart, or even from the collision, to drive them partly in a retrograde direction.

But it may be maintained, that the effect may be equally produced by transmission of motion from the particles sustaining the original impulse to those against which they are impelled in a more advanced part of the canal; and that those again communicate a motion to others in a still more advanced situation, and so on, till the blood, which happens at the time to be in the roots of the cava, is urged against the internal walls of the auricle, in consequence of a power transmitted to it by succession from the ventricles of the heart. Suppose that the heart has thrown into the canal AG at AL a quantity of blood so forcibly as to produce in the portion of the canal AG a certain quantity of motion in a given time. According to supposition, the motion existing, in the same time, in the next equal portion of the canal, has been generated by the impulse sustained from the blood in AG. But from what has been stated, the quantity of motion in ED in the time given is equal to that in AG. Further, the quantity of motion in the next equal portion of the canal, CK, is equal, in equal times, to that in ED. But the motion in CK is, by supposition, all derived from ED. And so of the motion of the blood in every other equal portion of the canal. The heart, therefore, is supposed to produce an effect amounting to that of communicating to a body a certain quantity of motion, and at the same instant the property of retaining, without further aid, that motion undiminished through a long course, in which the moving body is subjected to various powerful resistances. But this, from the established laws of nature, is known to be impossible.

It may be further urged, that the blood already in the vessels at the time that a fresh impulse is communicated to it from the heart, is not at rest but in motion, and that in consequence of the new motion added to that which is already possessed, the blood is impowered to transmit the supposed motion to the blood in the next portion of the canal. But if a quantity of motion belonged to the blood already in the canal, at the moment at which an additional impetus was given to it, by the renewed action of the heart, that motion, after the transmission of a quantity equal to that just received, must be admitted to have remained in whole or in part, or to have been entirely annihilated. If it remained in whole, as much had been transferred to the succeeding pulsation as had been received from the preceding, and no actual aid obtained. If in part, that part must decrease after every pulsation, till it is reduced to nothing, and the blood be brought to rest between the impulses; and the state is only for a short time deferred, to which it would be immediately reduced by its instantaneous and complete annihilation.

All the motion existing in the whole vascular system during the time intervening between the commencement of one pulsation and the commencement of the next, must be admitted, upon the supposition now made, to be the effect of a single impulse of the ventricles. We are, therefore, unavoidably brought to the same conclusion; that a motion has been generated of such a description, as to continue undiminished, in opposition to gravity and many other causes of retardation.

To avoid the mechanical absurdity necessarily involved in this conclusion, Harvey, and many of his most able followers, maintained, that one wave of blood having become slower, is overtaken by another more rapid, and by it impelled forcibly forwards. This, however, is only repeating, in other words, the supposition which has been already refuted. Even though the portions of blood, between which the collisions are supposed to be made, were perfectly elastic, no new motion could in that way be generated; and as much motion would be lost by the impelling as had been gained by the impelled body. But as the blood is a tenacious, inelastic fluid, it is evident that a great loss of motion must result from such a collision between its particles.

But granting even that the heart were capable, by some unknown mode, of communicating the requisite motion to the blood in the roots of the cava, there is still the further admission necessary, that the vessels be always filled to that degree of distension which would give them the property of rigid undilatable tubes.

2. This suggests the consideration of the other mode by which the blood may be supposed to be circulated in consequence of the impelling power of the heart alone; which is, that the vessels having been already fully distended with blood, and an additional quantity having been thrown into one end of the canal, and prevented from returning out of it by valves; an equal quantity, to afford room for that injected, must necessarily be displaced from the other end of the canal;—out of the roots of the cava into the right auricle.

Though we are not acquainted with any data from which the power of the heart can be calculated, there must exist, nevertheless, certain limits, within which it must reasonably be supposed to be confined. If we consider that the quantity of blood in circulation, is nearly one-fifth of the weight of the whole body; that this great mass is spread over an immense surface; that it is, therefore, subjected to great resistances from friction, especially in the small vessels, where each globule is to be rolled over a fixed surface, and often pressed into an oblong shape, before it can pass through canals of which the diameter is less than its own ; that the currents, in consequence of anastomosing branches, are perpetually flowing in opposite directions ; and that attraction must powerfully prevail between the blood and the small vessels ; when we consider the mass moved, the motion with which it is moved, and the resistance opposed ; it is impossible to imagine that this labour could have been performed by the propelling power of the ventricles.—Besides, all this immense force must have been sustained by the root of the large artery, which would be constantly strained to a vehement degree, and would at every pulsation incur the danger of rupture.

But admitting, contrary to all probability, that the power of the heart was capable of effecting the motion of the blood in the way supposed ; the structure and condition of the vessels in which it flows, evidently prove that it is not circulated by this power alone. Before any blood could be displaced from the end of the canal, in point of action most distant from the moving power, it is necessary to suppose that the vessels have, at every intermediate section, been filled to that degree of distension, which would be occasioned by the weight of a column of blood whose height is equal to the distance between this section and the right auricle of the heart, (taking the measurement according to the track of the blood) and whose base was equal to the area of the transverse section of the vessel at this distance. The distension which the weight of such a column would produce would be immensely increased by the resistances above enumerated. But the blood vessels do not appear to sustain any considerable degree of lateral pressure. The veins, in particular, vessels, which are thin and eminently dilatable, even in those cases in which gravity acts in opposition to the moving power, are by no means dilated generally to their largest diameter; as may easily be observed by surrounding the leg or arm with a ligature.

Life would, in this case, have been a very insecure tenure. Every considerable loss of blood would have been followed by a permanent stoppage of the circulation. For, unless the vessels were always filled to a proper extent, no blood would be found at the trunk of the cava to dilate and fill the auricle. The heart would be left dry by every profuse hæmorrhage.

Had the blood been circulated by the heart alone, this organ might have been expected to bear, in different animals, some proportion to their size; but this is not the case, the heart of an ox does not bear nearly the same proportion to the bulk of his body, that the heart of a dog bears to his.

The emptiness of the arteries and the fulness of the veins after death, prove that the blood, which had been in the arterial system, must, at or before the time of death, have been influenced by some power distinct from the heart.

Many other arguments might be advanced in refutation of the doctrine, that the blood is circulated by the heart alone; but it seems unnecessary to notice them; especially as it is now generally allowed, that the heart is powerfully assisted in this operation by the blood vessels and other agents.

It was at all times observed that the arteries in many parts of the body at least, were characterised by a regularly repeated, sharp stroke, which in common language was termed the pulse. Various explanations have from an early period been proposed of the causes of this phenomenon. When the circulation of the blood became known, it was generally admitted that this beating of the arteries was primarily produced by the impulse of the heart; and after the following manner.

The heart, during its contraction, was supposed to throw, with great force, a quantity of blood into the arteries, and to communicate an impulse to the whole blood in the arterial system. The coats of the arteries necessarily have to sustain this impulse; but, being elastic and dilatable, they give way and become enlarged in diameter, as well as lengthened in axis; and, in consequence of the increase of dimensions, thus suddenly obtained, impart a stroke to the bodies that may be in contact with them. But this stroke could not be constantly repeated without an opposite action. There must be a recovering as well as a distending power. This antagonist action was easily deducible from the properties of the arterial structure. The impelling power of the heart having terminated; the arteries, now roused by the stimuli of the fresh blood, by the excitement arising from the over distension of their muscular fibres, and by the elasticity strongly operating, in consequence of the unnatural distension of the elastic substance, in their turn becoming predominant, recoil upon the blood; and as rapidly as they had been distended, recover their former diameter and axis. The systole of the arteries having thus been restored, a quantity of blood, as it is an incompressible fluid, equal to that which had been injected into their cavity by the heart, must necessarily have been again displaced. Valves prevent the return of any fluid back into the heart during the recovery of the arterial systole. It must, therefore, have been discharged by the only other outlets,—the extreme orifices of the capillary arteries into the capillary veins.

The arteries, by thus restoring to the blood the force that had been expended in dilating them, were supposed to have contributed greatly to the motion of that fluid through their cavities. But it does not appear that any actual aid could be given to the power that originated the motion by the arteries acting in the manner now stated; for, as was judiciously observed by Baron Haller, the same power which was required to dilate the coats of all the arteries, would have been sufficient to have transmitted the same quantity of blood through the arteries, if undilatable tubes, in the same time.

Objections, afterwards started, to the explanation just given of the beating of the arteries, suggested at length different views of the action of these vessels, and of the manner in which they contributed to the motion of the blood. Weitbrecht, a physician of St. Petersburg, first remarked, that the quantity of blood thrown into the arteries during the contraction of the ventricle, could not, when divided through the whole arterial system, produce such an extension of these vessels as to enable them to communicate a perceptible impulse. The left ventricle of the heart contains nearly two ounces and a half of blood, and is not entirely emptied during the contraction. But admitting that two ounces of blood are thrown by each contraction of the ventricle into the arterial system, this small portion is evidently insufficient to increase, by its addition, the bulk of the whole mass in a perceptible degree. It has been calculated that the radial and temporal arteries, which are each about three lines in diameter, and in which the beating is very strong, would not be augmented in diameter, by their share of the addition, beyond the twenty-sixth part of a line; an increase of capacity too small, however rapidly, to communicate any sensible impression.

It might have been added that, from the dilatable property of the arterial trunks, it was not to have been expected that the whole blood in the system could have been affected by the small quantity projected into them by the heart. For the impulse must necessarily have been sustained, in the first place, by the coats of the arteries nearest to the impelling power; and the portion of those vessels which, when dilated, exceeded the capacity of the same portion in a state of contraction, by as much as was equal to the quantity admitted, must alone sustain the shock of the heart. From the dilatable property of the vessels, and the strong resistance opposed by the blood already in the arteries, it is presumable that this portion could not be very extensive.

The pulse having been observed to be of different quickness, at the same time, in different parts of the body, seemed to confirm the opinion, that the pulsations of the arteries were not all the immediate effect of the heart.

From the days in which Weitbrecht flourished, it has generally been admitted by physiologists, that the dilatation of the whole of the arterial system could not be produced by the propelling power of the ventricle. The circulation through the arteries is now generally supposed to be performed in the following manner.

By the contraction of the left ventricle, a quantity of blood is thrown forcibly into the aorta, and an impulse communicated to the blood contained in that vessel. From the known property of fluids in motion, this impulse is necessarily sustained by the sides of the vessels in which it flows; and, as those are elastic, stretches them and enlarges the cavity formed by them. The impetus of the heart, therefore, is expended in dilating through the medium of the blood, a part of the arterial system. But the arteries, being irritable and elastic vessels, now stimulated and stretched beyond their natural condition, recoil upon the blood, by which they were distended; and, by the rapid recovery of their former diameter and axis, restore to it the impetus that had been expended in dilating them. By the contraction, therefore, of that part of the arterial system, which had been dilated by the immediate action of the heart, a quantity of blood, equal to that which has been ejected by the ventricle, is, as valves oppose its return into the heart, transmitted with an equal force into another more advanced portion; and, in the same manner as had been done in the first, dilates and stimulates the sides of this portion. The same series of actions, with corresponding effects, is repeated to the end of the system.

Thus, after a succession of dilatations and contractions, a quantity of blood, equal to that discharged at a single impulse from the heart, is poured, at the same time, out of the innumerable terminations of the capillary arteries; and a power equal to that of the heart, is thus transmitted to that ultimate portion of the arterial system, the extent of which will be determined by the equality between the difference of its diastole and systole and the bulk of the blood projected by a single effort from the heart.

Other circumstances necessarily concur to give a direction to the motion of the blood.

The excess of capacity of the branches united, over that of the trunk from which they divide, and the continuance of that ratio to the ends of the system, must have a considerable influence in directing the course of the blood from the roots to the branches, upon the supposition of its being affected by the vibrations of these vessels. For if a fluid be compressed strongly by the diminishing capacity of the vessel containing it, and if orifices of different sizes exist in the sides of this vessel, the quantity of fluid discharged from these orifices, in the same time, will, *cæteris paribus*, be as the squares of the diameters of these orifices. Upon the supposition of a synchronous contraction of all the parts of the arterial system, the quantity of blood discharged in consequence of that contraction, from the terminations of the arteries, must greatly exceed that which would be returned to the ventricles, even if there were no valves to oppose it; the quantity which would be discharged from the former, would be to that returned to the latter, as the square of the diameter of all the capillary arteries of the aortic system, to that of the diameter of the aorta alone. But it has been clearly ascertained, that the arteries constitute a cone, the apex of which is at the heart, and its broad base formed by the ends of the arteries.

The motion given to a body impelled by any point in the contracting sides of a cone, will not be in the direction of a perpendicular to the axis, but will observe an obliquity to it proportionate to the extent of the angle formed by the bisection of the apex. The direction of motion will form an obtuse angle with the axis on the side facing the base. Therefore the blood will not be impelled towards the axis directly, but obliquely towards the extremities, by the contraction of the arteries.

The proportions of the cone will necessarily vary in the different states. During the systole, any section of the cone near the apex will be a less circle compared with that, formed during the same state, by a section near the base, than the former with the latter during the diastole. For a greater diminution of cavity in proportion will be required to transmit a quantity of blood through a section near the apex of the cone, than would be required to transmit that quantity, in the same time, through a section near the base. Therefore, the arteries while recovering their systole, assume a condition still more favourable to the transmission of the blood from the roots to the extremities, in consequence of this action.

If, however, the contractions of the arteries, as would certainly appear to be true, are not simultaneous, but rapidly successive, these vessels would, in that case, be put in a condition still more adapted for transmitting a fluid from the heart to the extremities, in consequence of those contractions. For, at the instant at which any portion of the arterial system begins to recoil upon the blood, the portion next to it, on the side of the heart, will be approaching the state of greatest contraction, with its contracting *nisus* unfinished ; while the vessels on the other side of the contracting portion, will be in a state of relaxation and easily dilatable. The undulating successive manner of the arterial action, commencing at the heart, and proceeding with amazing rapidity through the arterial system, must evidently promote the course of the blood from the trunks to the branches.

From the circulating and spiral direction of the fibres, composing the muscular and ligamentous coats, it is probable that, in particular circumstances, the arteries possess the power of dilating themselves to a certain degree. For these fibres by relaxation must enlarge their circle, and, in consequence, augment the cavity formed by them. So far, therefore, the arteries, while they are relaxing, may possess an active influence upon the motion of the fluids they contain.

The length of the part of the arterial system, sustaining the direct impulse of the heart, might be calculated, were we acquainted with the quantity of blood projected at one impulse from the ventricles, and the difference between the capacity of the arteries in their contracted and their dilated state ; for it must extend precisely to that point, between which and the heart, the difference of the arteries in the diastole and systole is equal to the mass of fluid discharged at one impulse from the heart. Every portion, then, of the vibratory system, which receives into its cavity a quantity of blood, in addition to what is already contained, equal to that discharged at a single contraction of the heart, sustains and communicates a force equal to that of the heart.

It is not to be inferred, however, that the portions of the arterial system, though their actions are not admitted to be simultaneous, are supposed to be definable by a particular point. The movements of the different portions, though not synchronous, succeed with such rapidity, that the succession cannot be discerned by the senses. The contractions of the successive portions of the arterial system will be so blended together, that the whole will assume the character of an undulation, commencing at the heart, and advancing with the greatest rapidity to the ends of the arteries.

It has been stated, that two powers of a different nature, the muscular and elastic, combine their efforts to produce the systole of the arteries. The arteries, perhaps, could not have been constructed with a muscular coat of itself sufficiently strong, without becoming of an inconvenient and cumbersome thickness, and without being, in a great measure, deprived of their flexibility. Nor could the elastic power of these vessels singly have been sufficiently strong; for nature does not supply a substance that is perfectly elastic. Had the coats of the arteries been perfectly elastic, that is, had they resiliated, from their elasticity alone, with a force equal to that by wnich they were distended, there might, perhaps, have been no occasion for their partaking of the muscular structure. But this being necessarily not the case, their irritability is, with wonderful design, formed to supply the want of perfect elasticity.

Unless the actions of the different portions of the arterial system had been thus equally balanced, the circulation of the blood through them could not have been regularly performed. For, if any portion of this system had acted either more forcibly or more feebly than another portion, the blood would either have been discharged from that portion, or accumulated in it; and, in either case, the circulation would have been interrupted. To prevent, however, the recurrence of such a condition, the constitution is possessed of a corrector in itself. For, suppose that any portion of the artery should not recoil upon the blood with the same force with which it had been distended, and that its diameter in the systole should be greater than it usually is in that state; in the next impulse that it was to have sustained from behind, the unusual quantity of blood would have been thrown into this portion, the cavity of which would, by that means, have been distended to an unusual degree. The consequence of this unusual degree of distension would be a greater excitement in the muscular fibres, and a more vigorous resilient effort in the elastic; and, of consequence, a more vehement contraction; and in this manner would the unhealthy accumulation be removed. A similar corrective process would follow a too energetic action in any portion.

Since the appearance of the first edition of the Inquiry into the causes of the motion of the blood, a very ingenious and elaborate work on the arterial pulse, has been published by the late Dr. Parry of Bath. That author contends, that the stroke felt by the finger, when applied to the artery on the wrist, usually termed the pulse of that vessel, does not arise from any increase of the diameter of this artery, but in consequence of its being at that point pressed by the finger against a solid substance lying behind it; and that the stroke is occasioned by the renewal of the motion of the blood produced by the impulse of the heart through the diminished channel; that the arteries are at all times filled to the same extent in the same state of health, and of course possess the same calibre; that they are the same as rigid tubes, in all respects, except in that of their pliability; and that it is in consequence of their tonicity as well as elasticity, that they maintain this condition. This doctrine, Dr. Parry maintains to be proved by an observation of arteries exposed to view while the circulation is going on, and by the measurement of these vessels in the same condition.

The theory of the arterial circulation exhibited by Dr. Parry, implies a great facility of action, and the accommodation of such relief to the heart and arteries in that action, as correspond well with the simplicity of nature, and with the safeguards which she usually affords against the effects of overstraining action. It must also be admitted, that Dr. Parry's theory affords no uncongenial supplement to the view of the venous part of the circulation contained in the following pages.

Whatever may be the objections which may be opposed to particular parts of this explanation, or whatever may be the shades of difference which may still exist among physiologists, respecting the powers by which the blood is circulated through the arteries, and their manner of acting, it is certain, that as much blood is discharged from all the capillary arteries as is propelled from the ventricles of the heart in the same time; and that, therefore, the momentum of the blood issuing out of the capillaries of the larger circulation, is equal to that of this fluid at any section of the aorta itself. For, supposing the resistances in each case the same, the same force would be required to move the larger stream, with the velocity it possesses in the capillaries, as would be required to move the same extent of the less in the aorta, with the velocity which it there possesses.

Let it now be supposed, that the blood has reached the ends of the arteries, and is ready to be discharged into the concomitant veins. It is now to be circulated in a way in many respects different from that in which it had been in the arteries. The machinery and mechanism are here changed. The vessels are not of the same structure, nor have they the same properties; and their position, with respect to the motion of the fluid they contain, is reversed. The veins are neither irritable nor elastic; they are very dilatable, but have no re-action. They, therefore, can offer no resistance, except what may be derived from friction and position, to the blood that is thrown into them, till dilated to a great extent. What would be the effects of the action of the heart and arteries, as just described, upon the blood in the vessels in which it is now about to flow? By repeated contractions, the arteries might continue to throw blood into the veins, until the column of fluid in these vessels should balance the force of the last portion of the arteries. There are no data from which this force can be calculated. It would appear to be equal to that of the heart. To what extent a force, equal to that of the heart, distributed in due proportion, and acting upon the blood in the innumerable venous capillaries, would promote the motion of the blood in the veins, without the co-operation of any other agent, is a question we cannot certainly determine.

There are, however, certain reasons from which it may be concluded,—1. That the *vis a tergo*, or force derived from the heart and arteries, is insufficient to balance and keep in motion, the whole blood in the venous system.—2. That from various phenomena accompanying this part of the circulation, the motion of the blood as it exists in the veins, could not be produced by any power alone, however strong, that was so directed.

1. If we take into consideration the quantity of blood in the veins, it will appear too great to be sustained and kept in motion by the contractions of the last part of the arteries alone. The veins are supposed to contain at least twice as much blood as flows in the arteries. But the weight of this mass, upon the supposition of its being balanced and advanced by an impulse from the ends of the arteries, is enormously increased by the form of the vessels in which the fluid to be moved is contained, and by the position of these vessels in re-

lation to the moving power.—The veins, as has been mentioned, ramify from trunks which arise from the heart, after the manner of the branching of a tree. The area of the transverse section of all the branches united continually increases, the further this section is taken from the heart; and, at the ends of the veins, at which the impulse must be made upon the blood, the area of the transverse section is the largest of all. The blood, therefore, in approaching the heart is constantly passing into a narrower channel. Its motion is expended, not only on the column of blood before it, but upon the contracting sides of the channel along which it moves. So that, in fact, the impulse, necessary for advancing the blood in the veins, would not have the weight of the blood before it alone to support, but the weight of a cylinder of blood, the base of which is equal in area to the transverse section of the veins at their ends, and the height is the distance between this section and the heart. The form and position of the veins are, therefore, the most unfavourable that can be conceived to the motion of the blood, upon the supposition of its being totally advanced by a *vis a tergo*.

Formerly the blood vessels were supposed to constitute a cone, with the base at the heart, and the apex formed by the extreme vessels. From this structure, Muschenbroeck attempted to account for the apparent easy motion of the blood in the veins. His argument was, that, as the blood flowed from a narrower into a more ample channel, a weaker power from behind was required to advance it. The force transmitted from the heart and arteries, would be in proportion to the square of the diameter of the small vessels, which were supposed to constitute the apex of the cone (and therefore in them smallest) multiplied into the distance of the section from the heart. But subsequent discoveries in anatomy have proved that this reasoning, though just, is founded upon erroneous principles. For it has been found that the transverse section of all the branches united exceeds greatly the transverse section of the trunk ; that in reality the heart is the apex of the cone ; and that the terminations of the extreme vessels form its extended base.

The veins form frequent anastomoses. By the collision of currents meeting at considerable angles, the momemtum of each is to a certain degree expended.

2. But, allowing that the heart and arteries were sufficient to advance such a quantity of blood under all the impediments to which it is subjected, the veins are evidently not fitted for their supposed office. They must be allowed to be always in an extreme degree of distension. In the lower extremities, particularly in the

erect posture, they would necessarily have to sustain such a degree of lateral distension as their coats could scarcely be supposed to resist. The valves, with which these vessels are furnished, admitting them to be as perfect as possible, could not remove this pressure at all times ; for they must be opened by the force of the blood advancing to the heart, when the vein would have to sustain at any part the weight of a cylinder of blood, of which the base was equal to the square of the diameter of the vein at that part, and the height to its distance from the heart. But the veins, even in the lower extremities, do not appear to sustain any considerable degree of lateral pressure. When slightly pressed, they swell on the side of the point of pressure farthest from the heart, to a considerably greater size. They certainly, therefore, in their ordinary state are not in the situation of rigid tubes ; which they must be admitted to be, upon the supposition of the blood being advanced through them by a force impressed upon this fluid at their distant terminations. Besides a vein wounded in these circumstances, would never cease to flow while there was any blood in the part of it between the orifice and the heart, which would be nearly as long as there was blood in the system.

Many other phenomena might be stated, perfectly incompatible with the supposition of the venous blood being circulated by a force acting from behind, but these we shall omit to notice at present, both because sufficient proof has been already adduced, that the projectile force of the heart and vibrations of the arteries are not the only agents employed in returning the blood from the extreme vessels to the heart; and because a better opportunity will occur of considering those phenomena, after the other powers, supposed to be engaged in the circulation, have been explained.

It may be concluded, therefore, from what has been said, that a certain degree of the motion of the blood in the veins is produced by the force of the arteries; or, in other words, by a *vis a tergo*; but how great we cannot certainly estimate;—that, in consideration of the mass of fluid to be advanced, and of the obstacles of various kinds and of powerful efficacy which are to be overcome, the supposition of the whole motion of the blood in the veins being the effect of an impulse received from the terminations of the arteries appears most extravagant and altogether untenable; that the structure and position of the veins are not fitted for circulating a fluid, in consequence of a cause acting at the commencement of its motion alone; and that we are convinced that the venous blood is far from being wholly circulated by such a cause from various phenomena attending that circulation.

Some physiologists still contend, that the coats of the veins are contracted and dilated alternately after the manner of the arteries; but that, as the blood flows with much less impetuosity in the veins than in the arteries, the small imperceptible vibrations of the former are sufficient for the accomplishment of an effect proportionably less.

In the first place, what is assumed in this argument is erroneous, that the quantity of motion belonging to the blood in the veins is less than that belonging to the blood in the arteries. For, it has been already demonstrated, that the quantity of motion in equal portions or lengths of the circulating system, supposing all the branches included, is equal in equal times, from whatever part of the circulation those portions may be taken. Now, as the distance from the ends of the arteries to the heart, may be presumed to be equal to the distance of the ends of the veins to the heart, it must follow, that the quantities of motion, in the two systems embraced between these equidistant points and the heart, are universally equal in equal times.

Supposing the motions in the two systems independent on each other, and the resistances the same in both, the powers required to produce those motions would be equal. The venous circulation receives indeed an impulse from the arterial, the extent of which impulse we cannot ascertain. But on the other hand, it must be admitted, that the formation of the veins, and their position with respect to the moving powers, are less favourable to the motion of the venous blood than those of the arteries to the arterial, upon the supposition of each being produced by an impulse impressed upon it from behind, or by the contraction of the coats of the containing vessels. We cannot compare the impetus received by the venous circulation from the arterial with the additional resistances to be overcome in the veins, but, I presume, it will be found to be erring in favour of the arterial impetus, if we suppose them equal; and that there still would remain a motion in the veins requiring powers equal to those by which the whole arterial blood is moved. If such vigorous and perspicuous causes are required to produce the arterial circulation, can we suppose an equal effect, the venous circulation, to be produced by causes of the same kind, and operating in the same manner, but so much less in degree as to be perfectly imperceptible?

It is admitted that vibrations are perceptible in the larger arteries only, but it is assumed that the smaller or capillary arteries also vibrate; and that, in consequence, they contribute as well as the larger arteries to promote the passage of the blood through their cavities.

May not, it has been asked, the capillary veins also vibrate, though imperceptibly, to the same extent with the capillary arteries; and equally promote the current of the blood while it flows in them?

As the capillary veins are the continuation of vessels which certainly do not vibrate, it is presumable that they are possessed of the same structure and properties. Inflammation, while it affords the strongest proof of the vibration of the capillary arteries, seems to dispossess the veins of that property. For if the capillary veins were excited by stimuli in the same manner with the capillary arteries; and if, when excited, they affected the blood in the same manner that the capillary arteries in that condition are allowed to do, the veins of any part strongly stimulated, would have transmitted the blood as rapidly through them as it had been received from the excited arteries, and there would be no accumulation, and of consequence, no inflammation in that part.

But admitting that not only the capillary veins, but also that the whole venous system vibrated, what would be the effect of these vibrations?—Undoubtedly to send the blood from the heart to the extremities; from the apex to the base of the cone; to reverse the course of the blood, not to promote it.

The quickened circulation and strong beatings of the heart and arteries, which attend vehement bodily exertion, readily indicated muscular action to be connected with the motion of the blood; and have caused it, in the general opinion, to be ranked among the powers produced by nature for the circulation of this fluid. The following is the manner in which this power is supposed to contribute its aid towards the accomplishment of this important end. A muscle, in its contracted condition, pressing upon the full veins that run beneath it, drives the blood from the parts of the veins that are under the influence of its pressure. The blood will tend to flow in all directions from the centre of pressure, but is prevented from returning towards the extremities of the veins, by the valves with which these vessels are furnished; and is necessarily directed towards the heart, in which course it meets with no obstruction. As the muscle soon relaxes, the vein is again relieved from pressure; becomes an easy receptacle for a new supply of fluid, which by the mechanism of valves can only be drawn from the extremities, and which the succeeding contraction of the muscle directs in the same forcible manner towards the heart.

Let us consider for a moment more closely the effects of muscular action upon the motion of the blood. Admitting that the valves, with which the veins are furnished, are sufficiently perfect, which they certainly are not always, to prevent the retrograde motion of fluids, it appears doubtful whether the circulation of the blood be directly promoted by muscular action. The same agent, which drove the blood, flowing between the center of pressure and the heart, with augmented impetuosity towards that organ, would close the valves against the blood that was coming from the extremities, and bring for a time to rest the whole current that was in motion between the shut valves and the ends of the veins.—Reasoning, therefore, *a priori*, we should be led to expect that the action of muscles would impede the circulation in one part while it increases it in another, and that the immediate influence of their action would be to render the circulation unequal in force and interrupted, not uniformly stronger and quicker.

This argument receives considerable support from the frequently observed effects of violent muscular action. In those spasmodic affections unaccompanied with any increase of the natural heat, as epilepsy, hysteria, and the like, where the muscles are contracted and relaxed with great rapidity, and in a violent manner, the motion of the blood does not appear to be increased; the pulsations of the heart and arteries are found to be unequal both with respect to strength and quickness, not generally stronger or more frequent.

After long voluntary exertion, and when they have been so frequently repeated as to produce fatigue, muscular contractions cease to have any influence in augmenting the motion of the blood.

During voluntary bodily exertion, two causes seem to aid at least the direct influence of muscular action in increasing the motion of the blood. By the stronger, though interrupted, impulses of the returning blood, the heart sustains a degree of violence, the auricles are perhaps more than usually distended and stimulated, and the whole organ roused into a more vigorous action. The increase in the energy of the heart, which will speedily be communicated to the whole circulating organs, continues, though the cause that produced it may be withdrawn. Hence we find, that a strong momentary effort is often succeeded by violent pulsations, which frequently continue for a considerable time, though the cause that produced them be not repeated, and though bodily exertion of every kind, be entirely suspended. Chemistry has lately supplied another cause, why the circulation should be increased by violent muscular action. In making any great bodily effort, the chest is naturally enlarged to an unusual degree, the inspirations are fuller, and often more frequently repeated. Hence a greater quantity of atmospheric air will be received into, and transmitted from the lungs in the same time, than when the body is at rest. The heat, therefore, of the system will be increased, the blood rendered more arterial and stimulant, and the general irritability augmented. A stronger and more rapid circulation will necessarily be produced by a combination of causes increasing at the same time the irritability of the circulating organs, and the energy of their appropriate stimulus.

In fevers and inflammatory diseases, while all the muscles, excepting those employed in respiration, are at rest, the heart and arteries often beat as powerfully, and as rapidly, as they are ever observed to do under the most violent exercise.

Since, therefore, the blood often circulates with its greatest force and regularity, for a considerable period, during which, nearly all the muscles of the body, but especially those which by their action could affect the vessels, are at perfect rest; since a quickened circulation is not always the consequence of a violent and very general action of the muscles; and since, even in those cases in which muscular action is attended with a more vehement circulation, this effect is chiefly to be ascribed to causes different from the immediate action of the muscles upon the subjacent vessels; it may be concluded with the most perfect certainty, that the action of the muscles super-incumbent upon the veins is not necessary to the circulation of the blood. Nor was it indeed to have been expected, that nature would have left any share of an operation, that cannot be interrupted scarcely for a moment, without the suspension or loss of life, to a cause so accidental and precarious.

The important uses of muscular action in removing impediments, in restoring the balance, and in correcting errors in the circulation, might easily be explained; but as these are only occasionally required, and presuppose the existence of powers adequate in ordinary circumstances to the motion of the blood, it is foreign from the purpose of this inquiry to dwell upon them.

A cause of the motion of the venous blood, more constant in its operation than muscular action, but similar in its manner of application, is supposed to be derived from the vibration of the arteries. It will be remembered, from the account that was given of the structure and arrangement of the blood vessels, that the arteries are generally accompanied by their corresponding veins. The arteries, by dilating, are supposed to make a depression into the sides of the accompanying veins, to diminish their cavity, and to promote the course of the blood contained in them towards the heart, in the same manner that it has been believed to be promoted by the pressure of a contracting muscle.

Nothing could more strikingly demonstrate the difficulties to which physiologists have been reduced, in attempting to account for the motion of the blood in the veins, than the efficacy which has generally been acknowledged to belong to this agent. As we may fairly suppose that an artery in its diastole becomes distended equally on all sides, the difference of the length of its semi-diameter in its diastole, and of that in its systole, will measure the whole depth of the depression, which, by its dilatation, it can make in the vein. As both the artery and the vein are cylinders, and mutually recede on each side of the line of contact, the diminution produced in the general cavity of the vein, by so imperceptibly shallow and narrow a depression, must be extremely small. But to allow any effect, in the way contended for, to this impulse, it is necessary to suppose the veins to be in a state of rigid distension, which they certainly, for the most part, are not; and if they were in such a state of distension, would not the dilating artery be more likely to displace the vein slightly, which in general is not difficult, than to make any impression upon its sides? But we have hitherto been supposing the artery to be throughout in close contact with the vein. This is not generally the case; and if they were in contact, would it not be as reasonable to suppose that the artery receded from the line of contact in its systole, as that it advanced beyond it in the diastole? Granting that the artery, when dilated, made an impression upon the vein, throughout its whole length, the effect of this impression would be as much against as in favour of the motion of the blood in the vein. For in fluids, the momentum is equal in all directions, from the centre of pressure. It will scarcely be contended, that the valves could have any influence in this case. Before any alteration could be produced by them in the direction of the impelled fluid, it is necessary to suppose them to have been closed; and to close them the blood must have been driven on one side in a retrograde course. But these are effects to which the slight impulses of an artery are, in any supposeable circumstance, altogether inadequate. Besides, neither in the quiescent vein exposed by the dissecting knife, nor in the uniform stream, flowing from an orifice made into its cavity, can the senses recognize any marks of the pulsations of the arteries.

But it may be urged, that, though the reasoning here pursued, is confirmed by observation, and conclusive so far as the larger vessels are concerned, the extreme vessels are so minute, so multiplied, and so accumulated, that a capillary vein may be acted against by several capillary arteries at the same time. In reply to this observation it may be stated, that between the capillary ramifications, if we judge from analogy of structure, some muscular, cellular, or nervous, substance in all probability always intervenes, by which the vessels are protected against the actions of each other. But in a case in which observation cannot be appealed to, and where argument may be opposed to argument without producing conviction, let it be admitted that the venous capillaries are acted upon by the arterial to the extent and in the manner here supposed. What would be the result of these pulsations? The general cavity of the venous capillaries is supposed to be diminished by the pressure of the dilated arteries. In what direction would the greater part of the blood expelled from the venous capillaries flow? The discharge, supposing no other power concerned, would be in proportion to the dimensions of the outlets afforded by the vessels under pressure. But, as the veins form a cone, the apex of which is at the heart, the outlets would be greater beyond the pressure, or at the ends of the veins, than on this side of it. Therefore, by the supposed pressure acting singly, a greater quantity of blood would be driven in a retrograde direction, than towards the heart. The momentum, consequently, communicated to the venous blood *a tergo* by the impulses of the heart and arteries, would be diminished by the supposed lateral pressure of the arteries. This argument opposes with equal force the supposition of the venous blood being aided by the proper vibrations of the veins. Admitting, still, that the venous capillaries, either by their own action, or by the borrowed impulses of the accompanying arteries, contributed in some unknown way, to the passage of the blood through them ; all those arguments, which have been advanced in refutation of the opinion of the blood in the larger veins being circulated solely by the force of the heart and arteries, prevail equally against the supposition of its motion being all effected by these organs, aided by the proper or borrowed action of the capillary veins.

Capillary attraction is another power to which no small influence in advancing the venous blood has been attributed. It is certain that fluids will rise to a considerable height in tubes by what is termed capillary attraction ; and, as the small extreme veins are in capacity similar to capillary tubes, it is supposed by some that the blood is circulated through the veins partly by this cause. Various hypotheses have been proposed respecting the cause of the ascent of fluids above their level in capillary tubes ; but, without waiting to examine these hypotheses, there is no doubt that the effect is produced by an attraction in some way exerted between the sides of the tube and the fluid. It is therefore plain, that any force which tends to transmit the blood through these small vessels must act in opposition to any attraction between the sides of the vessels and the fluid that is propelled through them, and that, of consequence, that force, which is admitted to proceed from the arteries and affect the blood in the small veins especially, must be resisted by capillary attraction. It is evident, therefore, the capillary attraction, if it contribute any assistance to the passage of the blood through the small veins, is the only power concerned, and that it cannot co-operate with any other. But further, as both ends of these capillary tubes are inserted into, or arise from, vessels filled with blood, what reason is there to suppose that the blood should always be moved in one direction by this power? Might it not be apprehended that its motion would be occasionally inverted by it? After these vessels have been all filled with blood, by what power are they to be again emptied and fitted for the reception of a fresh supply. Besides, in the circumstances most favourable for the operation of capillary attraction, fluids can be raised by it only to a limited height above their level.—Capillary attraction, therefore, instead of promoting the motion of the blood through the small veins, appears to be a principal cause of its retardation.

The motion of the blood has been supposed to be influenced by gravitation. The argument usually urged upon this subject is, that this power operates upon the blood as it does upon all the substances with which we are acquainted, that gravitation will sometimes favour, and sometimes retard, the motion of the blood according to the position of the vessels in which it moves, and that if this power should favour it in one part of the course, it is likely to retard it equally in another, or in vessels in which its current is reversed.

Dr. Darwin, after acknowledging that the projectile influence of the heart and arteries does not extend beyond the ends of the arterial system, maintains that the veins take up the blood from the arteries by absorption, in the manner that a sponge imbibes water. But this celebrated author has not waited to reply to a question that seems to follow of necessity—What is the cause of this absorption?

Dr. Wilson, formerly a physician of Newcastle upon Tyne, published a very curious and ingenious pamphlet on the causes of the motion of the blood, especially in the veins. This physiologist conceives that the heart is dilated by a property inherent in itself, and not by the impetus of the blood either issuing from the roots of the veins into the heart, or from one chamber of this organ into another, and that, in consequence of this dilatation, a cavity or vacuum is produced, by which the blood is pumped from the veins into the heart. The question respecting the dilatation of the heart had been a subject of controversy for a considerable period after the discovery of the circulation of the blood. It had been urged against the opinion now renewed by Dr. Wilson, that the natural state of the muscles was that of contraction; that the heart being a hollow muscle and having no antagonist, the constant tendency of its own action must be to diminish its cavity; and that, even admitting the diastole to be the natural condition of the organ, no cavity or vacuum could be formed but simply a relaxed extension of the sides, placing them, at most, in a situation more favourable to be influenced by the impetus of the returning blood. Dr. Wilson has ascribed to the heart the property of self-dilatation without supporting his assertion by adequate proof, or without refuting the arguments which had been urged against it, and which had long been admitted in the schools of medicine as irrefragable and conclusive. In consequence, the hypothesis of Dr. Wilson does not seem to have attracted much attention; and the doctrine that the cavities of the heart are dilated by the force of the blood rushing into them, continues to be received without hesitation, and almost without inquiry.

The vitality of the blood, and of the vessels in which it flows, independent of the action of those vessels, has been supposed by some writers, not only to generate an increase in the motion of this fluid, but so to modify its character, as to emancipate it, in a great degree, from those laws to which the motion of dead substances is subjected. "It is in vain," says a recent author, "to attempt to apply the laws of hydrostatics to the blood, a living fluid moving in living tubes." The late Mr. John Hunter supposed that the blood was possessed of life in a more extended sense than had been done by former writers; and has attributed to it faculties, which necessarily imply the existence of a certain degree of consciousness, judgment, and even locomotion. An opportunity may occur in the course of this inquiry, of conidering of Mr. Hunter's doctrines at greater length. No physiologist, nevertheless, so far as I know, has ventured to describe how the passage of the blood could be immediately promoted, or its motion modelled by its vitality. The assertion of a late writer, indeed, implies the belief of a voluntary effort on the part of the blood itself, since he enumerates among the causes of the circulation of this fluid, the frequency with which it had travelled the same course before. But admitting the truth of all that has been said, or fancied, respecting the vitality of the blood, there would still remain insuperable objections to the supposition of the motion of this fluid being promoted by it in any degree, or being regulated by peculiar unknown laws. Should the property of locomotion be ascribed to single particles, or to any combination of particles, there would still be required a general mind to direct the wills of all those particles, or combinations of particles, composing the whole mass, to one common object.

With respect to vitality considered as a cause of fluidity, or of that particular combination of the constituent parts, by which the uniform consistence of the blood is preserved, and by which it is fitted for motion in vessels of minute calibre, or as assisting in the formation of those stimulant qualities, by which the heart and arteries are excited, and the motion of the blood indirectly promoted, the particular point of the question now under discussion, is not at all concerned.

But it is contended that the organs, in which the blood is contained, and by which it is put into motion, may, in consequence of their vitality, impart to this fluid a peculiar motion, different in its properties from that of fluids propelled by inanimate agents. To such as argue in this manner, it will be sufficient to reply, that the motion of bodies, unconnected with the causes by which it may have been produced, is the simple object of mechanical philosophy, and that motion, in all cases, observes certain fixed laws, is as soon as produced, independent of the causes by which it had been generated, and can be in no degree afterwards modified by them, without the communication of a fresh impulse to the moving body. The hand from which the ball has just been discharged, has now no influence in quickening or retarding its progress through the air. In the same circumstances, the motion, that had been generated by any living powers, will not be expended more or less rapidly than the same motion generated by inanimate agents. Nor can the qualities of motion be separated. It is not possible for any power to communicate to any body one quality of motion, and withhold from it another. It cannot communicate to a given quantity of matter a certain velocity, and withhold from it a due proportion of momentum, nor momentum without a requisite velocity.

Opinions, nevertheless, contradicting these plain and well-established truths, have been maintained by men of eminence. The ingenious and accurate Dr. Hales even, having in the course of his valuable experiments to ascertain the force of the blood, found, that the momentum of this fluid issuing out of a divided vein, bore no proportion to that of the stream from its corresponding artery, (while considering the quantity of fluid in motion, and the velocity with which, in the living body, it must flow, in order to return a sufficient supply to the heart in the time required, the momentum of the stream from the vein ought to have been nearly equal to that of the artery,) concluded hastily, that the motion of the blood was not governed by those laws to which the motion of other fluids was subjected. But would it not have been more becoming and judicious in the philosopher to suspect that his experiments did not fairly indicate the relative forces of the venous and arterial currents, than that the laws of nature, so uniform and constant, should, in this instance, be transgressed? Before a doctrine, so contrary to experience in every other subject, so contradictory to reason, so discouraging and inauspicious to improvement, be received, it will be necessary for those who have advanced it, to shew that they are acquainted with all the capacities of the system, and that nothing can have escaped their penetration. For their argument is not positive but negative. It is founded upon the supposition, that every thing that is possible in the case is known; a species of reasoning which, particularly in physics, has ever been found to be fruitful in error, and which ought never to be relied upon except, perhaps, in the science of pure mathematics. On the contrary, it is hoped, that in the course of this inquiry it will be found, that the experiments of Dr. Hales and other eminent inquirers, do not sanction the opinion, that the motion of the blood is at variance with the received laws of hydraulics, but that, like that of every fluid, it is subjected to their government.

With the cause of muscular action we pretend not to be acquainted, but the effects of that action, as exhibited in the motion of material substances, are the objects of as certain calculation as the apple that drops from the tree, the ball that is projected from the mouth of a cannon, or the moon that revolves in its orbit ; and we are as little acquainted with the causes of gravitation, attraction, and projection, as with those of muscular action.

Many years after the publication of the Inquiry into the causes of the motion of the blood, Dr. Barry, now Sir D. Barry, read a memoir in the Institute of France, on the causes of the motion of the blood, particularly of its return to the heart in the veins. This memoir was referred to a committee, with directions to report to the institute respecting its merits, according to the usage of that society in such cases. This committee, which consisted of men of great scientific merit, made a remarkably flattering report of Sr. D. Barry's performance ; asserting that his views were original, and supported by evidence resting upon his experiments, which left no doubt of their truth. By the publication of this memoir along with the report of the committee of the institute, in French, English, and perhaps in other languages, Sir D. Barry obtained a sudden, extensive, and high reputation. As the doctrine advanced by Sir D. Barry, is only a modification of the opinions which I had published many years before, and as it will be a better time to ascertain whether this modification be an improvement or otherwise, of the original doctrine, I defer the examination of Sir D. Barry's memoir, until I shall have stated my own views in their proper place, in this work. The committee of the French Institute, who reported concerning Sir D. Barry's performance, were evidently at that time, totally unacquainted with the Inquiry into the causes of the motion of the blood, published in England in the year 1815.

Dr. Arnott, in a very popular work lately published on Physics, contends that the motion of the blood cannot be aided by the removal of any share of atmospherical pressure from the blood in the confines of the chest, in consequence of the pliant nature of the coats of vessels, in which it is returned to the heart. This author is of opinion, that the venous blood is propelled by the action of the capillary arteries. This doctrine, which the author admits is only speculative, with the objections advanced by him to the influence of atmospherical pressure, according to the views of Dr. Wilson, Sir. D. Barry, and myself, will also be examined at a more suitable place, in a subsequent part of this volume.

THE TRUE CAUSES OF THE MOTION OF THE BLOOD IN THE VEINS.

It has, I trust, been clearly shewn, that the causes enumerated in the preceding pages are inadequate to the whole motion of the blood; and that powers, to which an important share of this effect is to be attributed, remain to be explored. We proceed, then, in the second part of this inquiry, to the developement of those other causes, which appear to contribute to the motion of the blood; and which, co-operating with the former, seem adequate to that effect.

As the heart is unquestionably the most important organ engaged in the circulation of the blood, it will be necessary to resume the consideration of its movements.

It will be found, on reference to the preceding part of this inquiry, that the heart is divided into what is properly termed the body of the heart and the appendages; that each of those divisions, consisting of two chambers, assumes successively opposite conditions; and that the diastole of the ventricles is synchronous with the systole of the auricles.

It is necessarily inferred from the conditions of the circulation, that each of these chambers, in assuming the systole, discharges a certain quantity of blood, and in recovering the diastole, it receives into itself again a quantity, in ordinary circumstances, equal to that which it had discharged.

It was also stated, that physiologists are agreed respecting the causes of the contraction of the heart, and are unanimous in ascribing this movement to that property, which characterizes muscles (the class of substances to which the heart belongs,) of being contracted upon the application of appropriate stimuli.

The peculiar stimuli, by which the different parts of the heart are excited to contract themselves, are without doubt, derived from the fresh blood which they receive at every diastole, and perhaps in the most extensive degree from the uneasy distension sustained by the muscular fibres at the moment of full dilatation.

By what causes are the chambers of the heart dilated after contraction? This question has particularly engaged the attention of physiologists, from the period at which the circulation of the blood was discovered; has given origin, as we have seen, to various hypotheses; and, in the opinion of many, is still unresolved.

As the determination of this question is of the utmost importance in the present investigation, it seems proper in this place, although some repetition may be involved in the statement, to consider briefly the causes to which the expansion of the chambers of the heart have been imputed.

The doctrine maintained on this subject by the celebrated Harvey, beyond whose age it is unnecessary to refer, was, that the auricles were dilated by the force of the blood returning to them through the veins; and the ventricles by the momentum of the blood projected through the auricular passages, during the contraction of the auricles. But the opinions of Dr. Harvey, on this subject, have not been universally adopted. It has appeared to many an extravagant supposition that there should still belong to the blood in the venous trunks such a force, especially considering the supposed origin of it, as should be sufficient to expand any of the chambers of the heart; and, still more so, that the left ventricle should have for its antagonist so thin and slender a muscle as the left auricle. It has been contended by many that the property of dilatation, as well as that of contraction, is inherent in the heart itself, and that the blood does not force a passage into the auricles or ventricles, but enters them without opposition, or is solicited into them by their previous dilatation. Dr. Browne Langrish, is one of the most able supporters of this doctrine. This philosopher contended, that two kinds of muscular fibres are interwoven in the fabric of the heart; that by the contraction of one set of fibres, the cavities are diminished; that, as soon as these have ceased, the other set begin to act, and by their contraction, restore the former fibres to the distended state; and that thus the chambers are dilated.

It was urged in opposition to this doctrine, that the assumption of two kinds of fibres existing in the heart, and acting at different instances, was purely hypothetical; that the supposition of muscular fibres, apparently of the same structure and properties, forming the same muscle, and subjected to the same exciting causes, acting at different times, was unreasonable; and that the contraction of any of the fibres of a hollow muscle, must necessarily diminish the cavity still farther, instead of expanding it. These objections appeared to afford a satisfactory refutation of the opinions of Dr. B. Langrish; and, as has too frequently been the case, the overthrow of this doctrine was considered as an irrefragable proof of the truth of the other.

M. Dumas, a French physiologist of great celebrity, clearly discerned the insuperable nature of the objections which may be opposed against every hypothesis hitherto advanced on this subject, and has lately presented a new view of the causes of the expansion of the heart. He maintains that there constantly surrounds the blood in the living system, an expansive elastic emanation, or atmosphere, *(une expansion active)* so powerful, as to expand the cavities of the heart when they are in the state of relaxation which follows contraction; and that this expansive faculty is not only the true antagonist of the muscles of the heart, but the cause of the pulsations of the arteries.

But this elastic emanation, to which the philosopher has attributed such wonderful powers, is purely hypothetical; its existence, indeed, has been attempted to be proved, but by the most vague and remote analogies, and inapplicable and deceptive experiments. But admitting that it did exist, it must necessarily surround the venous, as well as arterial, blood; and, therefore, the veins must be allowed to be constantly in such a state of distension as they would be placed in by a power capable of dilating the left ventricle of the heart; a supposition contradicted by the slightest observation.

The doctrine generally delivered in the schools of medicine, at present, on this question, is, that the auricles are dilated by the force of the returning blood, and the ventricles by the impulse transmitted to them through the medium of the blood by the auricles; both ventricles and auricles being placed by the relaxation following contraction, in the situation most favourable to be dilated.

For the purpose of obtaining a satisfactory solution of the question—" What are the causes of the dilatation of the heart?"—a solution of the first importance in the present inquiry, it will be necessary to examine, with considerable minuteness, the structure and mechanism of the heart itself; its situation in the chest, and the relation it may bear to the neighbouring parts, which will involve in some degree the consideration of the structure and mechanism of those parts; the momentum of the blood in the venous trunks, and the sources from which that momentum is derived.

The heart, as has been stated, consists of two parts, the body of the heart, or that part containing the ventricles, and the appendages or auricles. The body of the heart is a strong hollow muscle. Many attempts have been made to explain the origin, course, and insertion of the fibres of this muscle. Dr. Alexander Stewart, a physician who flourished about the beginning of the last century, used a method well fitted for ascertaining the muscular structure of the heart,—that of preparing hearts by boiling. The hearts of bullocks, sheep, and other animals, he treated in this manner. According to Dr. Stewart, the body of the heart consists of a single muscle, which when expanded or stretched out takes the form of a semicircle, and of which the fibres are all parallel to each other and to the base of the semicircle. The hearts, which had been previously prepared for his purpose by boiling, he unfolded in the presence of the Royal Society; which, as is well known, was originally instituted for the purpose of making experiments, as well as for that in which it is now usually employed, in reading and hearing communications. He commenced at the raphe, and separating carefully the outer layer, which was easily distinguished from that beneath by the different direction of the fibres; and pursuing a course in which he was conducted, as it were by a thread, he easily unravelled the whole; and found that it resolved itself into the shape and structure just mentioned. He attempted to illustrate the mechanism more clearly by shewing how this muscle might be refolded into its original form. The manner of refolding the muscle may be represented by a diagram. Suppose (fig. 2.) A B C to represent an ox's heart that had been prepared and unfolded after the manner described. Let A B be the diameter and D the centre of the circle of which A B C is the semicircle, and let f f f f represent the fibres of which it is composed running parallel to each other and to the diameter A B. Let the radius D B be supposed to be folded over in such a manner that D B shall cross the fibres f f f f nearly at right angles and the point B fall upon the circumfe-rences at E; the part E D B of the semicircle forming a hollow cone, of which the apex is at D, and of which the base is inclosed by E D bent into a circle. This may fitly be compared to a funnel formed by the fold-ing of a piece of paper. Then by continuing to roll until the whole muscle is folded, two rotations are sup-posed to be completed. The radius A D, falling upon the external surface of the cone, will, in consequence of the conical shape of the figure, and of the swelling and convexity of the sides, be found to run very obliquely between the apex and base; and the point A will fall considerably within the circumference. The line A D will represent the direction of the raphe on the external surface of the heart. The left ventricle is inclosed by the first rotation of the muscle. After the second ro-tation has commenced, the muscle continues for a space separated from the first; but at length becomes attach-ed to it; leaving a space between the outer surface of the first rotation and the inner surface of the second for the formation of the right ventricle. The walls of the left ventricle at every place, except at the septum or portion forming the middle partition between it and the right, are strengthened by two convolutions of the muscle, while those of the right have every where only the support of one. Lower, an ancient anatomist of great celebrity, the same who inspired mankind with the hopes of exchanging the decrepitude of age for the vigour of youth, and even of immortality itself, by his success in transfusing the blood of one animal into another ; this anatomist bestowed great labour in the examination of the structure of the heart ; he pursued the same mode of investigation which was afterwards adopted by Stewart ; that of unravelling the fibres of the hearts of bullocks and of sheep, which had previously been prepared by boiling, and arrived at the same conclusion : "From which," says he, "it is plain that the fibres of the inner and outer walls are the same and continued, although they seem to proceed in a manner contrary to each other." Any person may soon be satisfied of the truth of this description by repeating the simple experiments of Stewart and Lower. One thing which seems to have been omitted in the account given of Stewart's experiments, and which appears in the plates of Lower, is, that some of the fibres composing the outer layer upon reaching the base, turn inwards and pursue their course round the mouth and the internal surface of the ventricles. The fibres are not all of the same length. Those which in the extended muscle were most distant from the base being shorter than those which were nearer to it. Some of the fibres appear not to have proceeded their whole course but to have turned inwards at different distances to form the fleshy columns which characterize the cavities of the ventricles. The fundus of each ventricle is furnished with a ligamentous substance, surrounding it and also running across and dividing it into two openings, the one into the corresponding auricle, the other into the trunk of one of the great arteries. The muscular fibres are firmly attached to this ligamentous substance.

Several important consequences are deducible from the form and position of the muscular fibres of the heart. In the first place, all the fibres every where, except perhaps the septum, are more or less bent into arches. The form which the fibres bear in the heart may be allowed to be that which is natural to them and that which each would preserve when separated from its fellows, without any injury having been done to its structure, and freed from the agency of foreign bodies. Let us consider then the effects of contraction and relaxation upon a fibre of this form.

But without hazarding any opinion respecting the causes of muscular action, it may, in the first place, be requisite to state certain properties which are known, or may be conceived, of necessity to belong to it. In the first place, the state of relaxation is the natural, and that of contraction, the forced state of the muscular fibre. This is generally admitted by physiologists and is allowed to be true, particularly with respect to the fibres of the heart, as will afterwards more clearly appear. It is certain also, that when a fibre is contracted, the centres of the contiguous particles must approach nearer to each other, and that they must again recede when it is relaxed. As every very small portion of a fibre possesses, as completely as the whole, the muscular structure, it must also possess completely the muscular properties, and must act according to the laws which regulate the movements of the whole, independently of any other part of the fibre.

Let A B C (fig. 3.) then represent the portion of a muscular fibre of the heart in its natural situation or dilated. Suppose A d, d e, e f, f g, &c. to mark very small portions of this fibre. Let the fibre be supposed to contract. All the minute portions A d, d e, e f, f g, act independently of each other ; that is, though all the other portions were removed, any very minute portion, possessing the same properties and subjected to the same influences, would act according to the same manner. The particles forming the two very minute portions A d, d e, would approach each other in the direction of a straight line passing through their centres, or in the direction of the line in which they are placed. The same will happen in the portions d e, e f, f g, &c. and after contraction, every particle will have the same position relatively to the particles contiguous, and to every other particle, that it had before it was contracted, and the whole A B C will become a similar portion of a less figure of the same species. A B C, if the segment of a circle, will become a b c, the similar segment of a less but concentric circle. In the relaxation of the fibre the particles or globules must recede from each other in the direction of the straight line passing through their centres. Upon the supposition that nothing opposed the natural tendency of the fibre which formed the segment of the circle, a b c by relaxing would become A B C. Whatever, then, may be the curve of which this fibre forms a segment, the effect of contraction would be to form a similar segment of a less curve of the same kind, and that of dilatation a similar segment of a larger curve of the same kind. The change in the fibre produced by dilatation may be illustrated by comparing it with the effects of heat upon a metallic hoop ; the segment of the hoop if heated equally will become the similar arch of a larger circle.

An elastic substance is in the heart every where mingled with the muscular, and no doubt contributes greatly to fix the form of the muscular fibres. The elasticity may be constituted in such a manner as either to co-operate with, or to resist the contractile effort of the muscle. For reasons which will afterwards appear, it is assumed that the resilient power is opposed to the contractile. As the natural position of the elastic substance in every fibre must be the same with that of the muscular, any alteration in the elastic fibre, produced by the muscular portion acting according to its natural direction, must be to contract or expand the elastic according to its natural direction. Therefore the elasticity, if it is an antagonist power to the muscular, must tend to restore the fibre to the same position relatively that it had before it was contracted.

In consequence of the parallel direction of the fibres of the muscle, by the convolution of which the heart is constructed, it necessarily happens, that every fibre composing the first rotation is concentric with all those, the circumferences of which lie in the same plane with itself; that the circumferences of all the fibres are placed in planes that are parallel to each other, and that the fibres of which the circumferences are in one plane are similar, and placed similarly to the fibres, the circumferences of which are in another plane. A straight line, therefore, drawn from the apex to the base of the cone, and passing through the centre of position of any one circular fibre which has performed the first rotation, will pass through the centre of position of all the fibres composing the first revolution of the muscle. The same must evidently be the case with respect to the fibres of the second rotation. As the muscle, when performing the second rotation, has its lower superficies adapted to the upper superficies of the first rotation, it follows that the straight line, which passes through the centre of position of all the fibres of the first rotation, will pass through the centre of position of the fibres of the second rotation. Hence it occurs, during the contraction of the muscle of the heart, that the change produced in the figure of any one fibre is accompanied by corresponding changes in that of the rest; that the movement of one fibre is no where counteracted by that of another, but supported by it; and that there is a general, free co-operation in all the fibres tending to the production of one effect, which is the diminution of the cavity of the heart. This is plainly maintained by all the anatomists who have investigated most carefully the structure of the heart, however far they may differ in some particulars, as Lower, Stewart, Winslow, &c. "We must see clearly," says Winslow, "that the whole structure tends to make an even, direct, and uniform contraction."

It follows, also, from this arrangement of the fibres of the heart, that perfect freedom is afforded to all the fibres to assume that position and form to which they are inclined by their natural structure. The natural tendency of the fibre to enlarge uniformly its circumference, is every where supported by a corresponding tendency in the other fibres.

There arises, therefore, from the circular direction of the fibres of the heart and the arched form of its walls, a very remarkable property, that of dilating or expanding its cavities by the simple relaxation of its fibres.

The conclusion, which appears so clearly deducible from the structure of the heart, is confirmed by observation. The hearts of frogs, newts, insects, and other cold blooded animals, which, as is well known, are more tenacious of life than the more perfect animals, continue to beat for some time after the extraction from the body. This action may be greatly prolonged by placing the hearts, which are the subjects of observation, upon a warm stone, or immersing them in warm water. The dimensions of the organ will be observed alternately to be increased and diminished. I put the hearts of some frogs just extracted from the body into water blood warm. They were thrown into violent action, and, upon some occasions, projected a small stream of a bloody colour through the transparent fluid. It was supposed that a stream of the same kind continued to be projected at every succeeding contraction, but that, after the first or second, it ceased to be observable, in consequence of the liquid supposed to be imbibed and projected, loosing its bloody tinge, and becoming transparent, or of the same colour with the fluid in which the heart was immersed. The organ was felt by the fingers to expand during relaxation. The ventricles are generally distended with blood after death. These observations prove not only that the heart is, to a certain degree at least, dilatable, in consequence of the relaxation of its fibres, but also that the relaxed state is that which is natural to it. If the contracted were the natural condition of the muscle, the heart, after it had been abstracted from the body, and therefore placed without the influence of those powers which might be supposed to dilate it, would for ever remain contracted; and the application of fresh stimuli would only tend to confirm it in that state. There could be no motion observable in the heart after its abstraction from the body, if the systole were its natural condition.

It may safely be inferred, that the ventricles of the heart, during the act of relaxation, necessarily become dilated or expanded, but to what extent, or with what force, we have no means of ascertaining. The power required to bring a curved fibre into a straight line, would no doubt be inconsiderable; yet as all the fibres composing the substance of the heart, are made to act together, and to conspire during all their action to one common effect ; and as, from the attachment of the fibres no one fibre can be displaced or altered in its figure, without at the same time deranging and pressing into a form adverse to their natural structure, a number of others, it must be admitted that the heart would retain its shape, and preserve the form and capacity of its ventricles during the state of relaxation, in opposition to no inconsiderable powers. The convexity of its external surface and arched form of the walls, inclosing the ventricles would add greatly to the capacity of preserving its shape unaltered by powers acting against its external surface, especially if those powers were directed equally against the whole of this surface. The faculty which the heart possesses of enlarging its cavities after contraction, in consequence of the causes we have been endeavouring to explain, must be increased by the spirit and energy of life, to a degree of which we can form no adequate conception by an examination of the dead fibre.

It will on all hands be admitted, that the peculiar arrangement of the muscular fibres which has been described, must place the heart, after contraction, in a condition of being much more easily dilated than would otherwise be the case, by powers acting in conformity with the natural tendency of those fibres.

It will be found, by varying slightly the angle made by the radius of the muscle with the parallel fibres across which it is folded, that not only will a great variety be given to the form of the heart, but a diversity to the angles which the fibres of one convolution will make in crossing those of another. Hence is explained the diversity that is found, not only in the shape, but also in the direction of the fibres of the hearts of different animals. This mechanism will give all that vast variety and apparent intricacy to the direction of the muscular fibres which are actually found in the structure of the heart.

The auricles, or what has been termed the appendages, are placed near the fundus of the heart. They differ greatly in appearance and structure from the ventricles. Their dimensions are small in comparison with those of the body of the heart. The auricles are flat, earlike, notched in the edges, depressed, not convex in the dead subject like the ventricles. Their substance is thin; small bundles of muscular fibres are placed in the form of rings, or portions of rings, inclosing each other, with furrows appearing particularly in the internal surface between the rings. At the bottom of these furrows, the substance is so thin as to be almost transparent. As the membrane between the muscular bundles is very expansive and yielding, the auricles are easily dilated by any force acting against their internal surface ; and upon the withdrawing of that force, readily fall into the collapsed flat condition. The auricles are not spherical but form a cavity by the dilatation of one side principally. From the position and form of the muscular fibres, it does not appear that their relaxation would of itself dilate the cavity in any degree.

There is no communication of substance between the auricles and ventricles. The fibres composing the former do not pass into the latter, nor *vice versa*. For, if the heart be boiled for a proper time, the auricles will drop off or be easily separated from the ventricles, without any rupture of muscular fibres.

At the openings between the auricles and ventricles, valves are stationed, the offices of which have been already described.

The heart is situate on the upper surface of the diaphragm, and exactly on the middle and anterior part, where the structure is tendinous. This organ is inclosed in a strong, tendinous, undilatable bag, called by anatomists the pericardium. The surface of the heart is every where free and unattached ; but the pericardium is connected to the tendon of the diaphragm on which it rests by a broad and strong adhesion. The trunks of the large blood vessels, in proceeding from the fundus of the heart, penetrate the pericardium, become firmly bound to it all around, and receive a covering from the extension of its internal membrane. As no substance intervenes between the pericardium and the heart, that envelope necessarily assumes the form of this organ.

The other contents of the chest are the lungs, which have been already described; the thymus gland, which is almost obliterated in the adult; the œsophagus or gullet, to which it affords a passage from the throat to the stomach, blood vessels, nerves, and some membranes.

It is evident, from what has been stated respecting the structure of the lungs, that there is a constant and powerful tendency in the lungs to collapse, and retire from the internal surface of the thorax, that this effort to separate from the confines of the chest will be measurable by the share of atmospherical pressure which the elastic power of the lungs is, at the instant, capable of supporting; but as this power is by no means equal to the whole weight of the atmosphere, and as no elastic substance intervenes between the lungs and the cavity of the thorax, no actual separation can be produced.

It is further evident, that all the parts which compose the walls or boundaries of the chest, are pressed, or, in common language, drawn towards the spaces from which the lungs have a tendency to retire, with a force which is commensurate with the elastic power of these organs, or with the share of atmospheric weight which that power is at the instant capable of supporting. The bony structure of the thorax is unyielding; the diaphragm is forced to assume a tense convexity towards the centre of pressure.

In what condition is the heart in these circumstances? It must necessarily be considered as constituting a part of the walls or boundaries of the chest; for there is a free communication between its internal cavities and the parts without the thorax, as well as the substance of the lungs, particularly with the mass of fluids through the bores of the large venous trunks. Between the external or convex surface of the heart and the surface of the lungs, membranes alone intervene. The divided laminæ of the mediastinum are thin, pliant, slightly dilatable, and withont yielding incapable of supporting, either on the upper or lower surface, any share of preponderating pressure. The pericardium, indeed, which surrounds the heart, and which is every where in contact with it, is a strong, firm, inelastic bag, and cannot be expanded beyond certain limits, without such a powerful force as would at the same time injure its structure, but of itself it will not resist any pressure that may rest upon its external surface. It is fitted to resist with ease the balance of unequal pressure, which may be laid upon its internal or concave surface, by the collapsing effort of the lungs. But before it can be brought to support any share of this balance, it must be dilated by its contents to the utmost extent. Until the pericardium has been extended to the extreme limits of its expansibility, the unequal balance which the boundaries of the chest have every where to support, must be resisted at this part by the walls of the heart. A force equal to the weight which the elasticity of the lungs, in their ordinary condition during life, is capable of supporting, must constantly tend to dilate the heart to the utmost limits of the pericardium, by ponderating against the internal surfaces of its chambers; and until these chambers are expanded in such a degree as would swell the general dimensions of the heart to an equality with the capacity of the pericardium, this force must all be sustained by the walls of the heart itself. As the pericardium cannot be distended further, through any other means than the expansion of the chambers of the heart, and as a full dilatation of the greater part at least of those chambers would be required to give to the heart dimensions commensurate with the utmost limits of the cavity which the pericardium is capable of inclosing; the successive dilatation of the different chambers of the heart after contraction is fully secured by an adequate power.

By removing, therefore, in consequence of their elasticity, a part of the pressure of the atmosphere from the convex surface of the heart, and by that means causing the ordinary pressure to ponderate unequally against the concave surfaces of the chambers, the lungs become the certain and powerful antagonists of the muscles of the heart. The contractile force of the heart is necessarily more powerful than the antagonist action derived from the elasticity of the lungs, but the former is transient and interrupted, while the latter is equal and permanent; and, co-operating with the natural tendency of the structure, restores the chambers of the heart to the state from which they had been forced, and at which the superior strength of the contractile power begins again to be exerted.

Is that balance of weight, which is to be sustained by the whole external surface of the walls of the chest, in consequence of the resistance removed from the internal surface of these walls by the resilient effort of the lungs to be supported at this part by the walls of the heart itself, or intermediately by the pericardium?

Before we proceed to inquire into the answer which it may be possible to give to this question, it is necessary to make some explanation of the language we are obliged to use in the statement of it. The heart has been said to constitute a part of the walls of the chest. What is termed, in relation to the above question, the internal surface of the part of the walls of the thorax formed by the heart, is external with respect to the heart itself; the internal surface of the chambers of the heart must be considered as external with respect to the thoracic cavity. To avoid ambiguity, the epithets concave and convex will generally be applied to distinguish the surfaces of the heart.

The degree to which the pericardium is dilated during life, and the dimensions which the heart occupies, compared with the extent to which the pericardium is dilatable, have not hitherto been determined by observation. For no observer has ever been so fortunate as to see the heart and pericardium in the condition in which they exist in the living or sound system. Whatever admits into the regions which these organs occupy, the vision or the touch, must have already admitted an influence destructive of the machinery. The springs of life must have been broken ; the invisible chain by which the heart and the lungs are connected must have been loosened ; these organs must have sunk from their condition and their place ; and, in the scattered fragments, we shall search for the forms of their pristine existence, with no better success, than the school-boy examines the few drops of spray for the fabric of the beautiful bubble that lately danced in the air, or inspects the cavity of the drum into which he had cut, for the purpose of discovering the origin of its sound.

If the balance of weight, ponderating at all times against the external surface of the boundaries of the chest, is to be sustained at one part by the pericardium ; then this envelope will be at all times fully dilated ; and the heart will always occupy the same dimensions ; dimensions commensurate with the cavity of the pericardium dilated to its utmost capacity. But if the weight is to be sustained at this part by the walls of the heart, then the pericardium will rest loose and easy upon the convex surface of this organ, to the varying bulk of which no fixed limits will be assignable.

Whether in general, the inequality of pressure, arising from the causes which have been explained, be ultimately sustained by the pericardium, or by the walls of the heart, is a question which, I fear, cannot, in the present state of our information, be certainly answered. But it is contended, that in the statement of one of those two suppositions, the true and natural condition of these parts must be accurately described. As the actions of the heart must, upon the first of these suppositions, in particular, being true, be modified by the pericardium in an important manner, it will be necessary to consider those actions under both suppositions ; and, by comparing the cases, we may be enabled, perhaps, to form a plausible conjecture, at least, of the true condition of these parts during life.

In the first place, let it be supposed that the preponderance of atmospheric pressure is sustained throughout all the movements of this organ, by the walls of the heart itself.

According to this statement of the case, it is evident, that the concave and convex surfaces of the heart must be at all times unequally pressed ; that the balance of this unequal pressure must rest upon the concave surface of these walls ; and that this balance is exactly equivalent to the elastic power of the lungs. Unless there had existed in the heart itself an antagonist and recovering power, it is evident that all the chambers of the heart must, in these circumstances, have been in a state of forced dilatation. The manner, in which the constant and uniform operations of unequal pressure are counteracted by the stronger but interrupted exertions of muscularity, has been already described.

As the ventricles are much thicker than the auricles, a greater power might be supposed to be employed in dilating the former than the latter ; whereas, according to this view, it is equal. But the difference in dilatibility, in the different parts of the heart, may be only apparent; and that inertness, supposed in the ventricles to arise from massiness of structure, may be more than counterbalanced by the figure and position of the muscular fibres; as, in consequence of these, a strong expansive effort, which must at all times be in proportion to the massiness of the structure, necessarily accompanies the simple relaxation of those fibres. Some aid may possibly be supposed to be contributed to the expansion of the ventricles by the projectile power of the auricles, which are at this moment resuming their systole; although the counteracting the resilient power of the lungs may be conceived to be at least as great a labour as these chambers are able to accomplish.

Whatever other difficulties may appear, in this view of the case, it must be allowed that, according to it, the full expansion of the chambers of the heart are abundantly secured.

Let us next consider the other supposition on this point, that the pericardium is always distended by its contents to its utmost limits, and that, therefore, the heart in the living system always occupies the same dimensions.

This supposition is not new. It has been supported by many able physiologists, particularly by the celebrated Lieutaud. The heart may easily be supposed to be at all times of the same bulk. As the conditions of the different parts vary, the dimensions of the whole may remain the same. When the ventricles are in the diastole the auricles are in the systole; when the auricles are in the diastole, the ventricles are in the systole; and, at any intermediate point, the ventricles and auricles are in reciprocal proportion, with respect to their degree of dilatation. It is evident that, in all these situations of the heart, the same quantity of blood, by which the difference in its bulk can alone be produced, may be contained by it.

Suppose, in this case, the ventricles to be in the diastole and about to contract. A quantity of blood is thrown by this action out of the confines of the pericardium. To preserve the equilibrium of pressure between the contents of the pericardium and the substances without this envelope, the capacity of the pericardium will necessarily either be diminished to an extent that will be commensurate with the bulk of fluid that has been expelled by the ventricles, or the same bulk of fresh matter must be introduced into this cavity. From the convex surface of the pericardium, a share of the weight of the atmosphere is removed by the collapsing effort of the lungs. But the blood in the arteries and veins, with which the cavities of the heart have a communication, is subject to the usual pressure. Valves, and the condition of the ventricles at the moment, prevent the return of blood from the arterial trunks. But the entrance of the venous blood is not opposed by valves, and is favoured by the condition of the auricles at the time. The quantity of matter, therefore, required to occupy the space which the expulsion of blood from the ventricles would leave, is necessarily supplied from the veins.

But the ventricle again becomes relaxed; and, in consequence, expansion to a certain extent takes place. Synchronous with the natural expanding nisus of the ventricle is the resilient or contracting effort of the auricle. The blood, therefore, while it is pressed upon by the contracting auricle, sustains a diminished resistance on the side of the ventricle, and necessarily flows all into that chamber. Proportionably as the auricle contracts, or is diminished, is the ventricle less resisted on its convex surface, while its concave surface is pressed with additional force; and thus the full dilatation of that chamber is secured.

During this movement, no blood is either received into the confines of the pericardium, or expelled out of them, but a quantity is only transferred from one part of these confines to another.

According to the last view of the condition of the pericardium and heart, the movements of this organ are evidently performed with greater ease, and the labour which each part has to perform, is more adapted to its apparent powers. There is in this, as in the preceding supposition, an evident necessity for the massy structure and great strength of the ventricles; as their office is not only to propel a quantity of blood with great force into the arteries, but also, in addition, to overcome the antagonist power of the lungs. But there is a point at which the ventricles may be conceived to have thrown off their load, and to be at rest. At the period of full dilatation, the force by which the ventricles are dilated, is removed from their concave surface, and supported entirely by that of the pericardium. An opportunity may thus be conceived to be afforded this organ for recovering itself.

Upon the last supposition, the aid that is afforded to the auricles during their movements is remarkable. The auricles, while contracting, are not required to overcome the collapsing power of the lungs, which at this period is removed from all parts of the heart, and supported wholly by the pericardium. As the equilibrium of pressure is perfectly adjusted between the contents of the pericardium and the parts without this envelope, during the contraction of the auricle, the whole power of this chamber is reserved for the dilatation of the ventricle. Nor is this power required to be considerable. For the simple relaxation of the muscular fibres of the ventricles, so far as it tends to dilate them, must, in these circumstances, act with a double effect. As the ventricular cavity is enlarged, the blood is drawn from the auricle; and, in consequence of the diminution of the auricle necessarily following, the pressure on the convex surface of the ventricle is diminished. Indeed, admitting that the muscular fibres of the heart could, by their relaxation simply, in circumstances the most favourable, resume the form and position that are natural to them, it is not necessary to consider the auricles as more than inactive receptacles provided for the convenience of the ventricles; as, in that case, the depletion and impletion of the former chambers would be the necessary effect of the actions of the latter. At all events the positive power of the auricles, whatever it may be, must in these circumstances have a double influence. For, by their projectile power, on the one hand, the pressure upon the concave surface of the ventricles is increased; while, on the other, by the diminution of their bulk necessarily accompanying that projection, the resistance on the convex surface of the ventricles is lessened.

The full dilatation of the ventricles would be, in this view of the case, perfectly secured, without the necessity of ascribing to the auricles a labour apparently incompatible with their structure; the alternate movements of the ventricles and auricles would be regulated with unerring certainty; and the salient movements of the heart would be well explained from the changes of figure and position induced in that organ, especially during the contraction of the ventricles.

It may be objected against this supposition, that a greater quantity of blood is propelled from the ventricles, by a single contraction, at one time than at another. But this may arise from the contractions being more complete at one time than at another. It is evident from the structure of the ventricles, that they can never, in any state of that organ, be entirely emptied.—There must at all times be a quantity of blood in them. But they may be more nearly emptied by one contraction than by another. Thus a greater quantity of blood may be circulated through the heart, by a given number of contractions, without any alteration in the general dimensions of the heart, or without its containing at any one instant a greater quantity of blood.

Although the pericardium may not generally be fully distended by its contents, but lie loose upon them, there can be no doubt, that, if at any time, by the accession of a greater quantity of blood, or by an enlargement of the solid parts, it should be completely filled, this envelope would then serve to regulate the motions of the heart, according to the manner just explained. Security is thus afforded by the pericardium to the ventricles and auricles against the danger of their being overcome and kept in a state of distension by the antagonist influence of the lungs.

The degree to which the pericardium is generally distended by the heart and its appendages, cannot be correctly established; but the argument, so far as it may be strengthened by a more exact correspondence of the actions with the means employed and the ends in view, seems to preponderate greatly in favour of the opinion, that the pericardium is always fully expanded by its contents, and that the heart is at all times of the same dimensions.

But whatever may be the extent to which the pericardium is generally expanded in the living system, the dilatation of the chambers of the heart, upon the supposition of its arising from the causes to which it has been ascribed in this inquiry, must produce the same effect upon the motion of the blood, both in its passage through the heart itself, and in the venous system generally.

In consequence of the pericardium being all around, except at its inferior flat surface, where it is attached to the diaphragm, invested by the lungs, and of the tendency which these organs must have of receding from the surfaces in contact with them, in the direction of the perpendicular to these surfaces ; the direction of the force derived from the resiliency of the lungs, as it affects the pericardium and walls of the heart, must be divergent. Hence the uniform expansion of these walls is secured. If the decrease of resistance had been sustained in one direction only, or if the walls of the heart had been pressed to one central point, however much the fibres may have been stretched, the chambers of the organ could not have been dilated, by that power.

The manner in which the blood, immediately in the chambers of the heart, will be affected by the movements of this organ, must already have appeared abundantly evident ; let us next inquire, How the blood in the venous system generally will be influenced by them ?

The evident and incontrovertible consequence of the abstraction of a certain portion of the general weight of atmosphere, from the convex or outward superficies of the pericardium, by the resiliency of the lungs, and of course from the convex surface of the auricles, either directly or intermediately, by the contraction of the ventricles, is, that the blood, at the time in the large venous trunks in contact with the heart, must be less resisted on the side of the heart than at any other point; and that, in obedience to the laws by which fluids are governed, it must flow towards the centre of pressure, or into the auricles, until these chambers have been dilated to an extent, at which their walls will afford a resistance equal to the resilient power of the lungs; or, until a quantity of blood is admitted into the boundaries of the pericardium equal to that which is projected out of them by the concomitant contraction of the ventricles. At every contraction of the ventricles, a quantity of blood is thus pumped out of the venous trunks into the heart. While any fluid is found in the roots of the veins in contact with the heart, a portion of it must, by the suction powers of this organ, be successively emptied into the auricles, the recipient chambers of the heart.

The next question to be considered, is, How far the suction power of the heart would extend its influence through the venous system; and how the blood in the remote vessels would be affected by it? It is plain, that the blood could not be raised by suction alone to the heart, through the veins, from parts at a distance from that organ, unless the intervening vessels were incompressible, or were so fabricated, that their tubular form would be preserved in opposition to a considerable balance of pressure ponderating against their external surface. But it is acknowledged, that the coats of the veins are extremely thin, pliant, and yielding; and that they collapse in consequence of their own gravity alone. The bores of those veins, in which the advance of the blood is not favoured by gravity, would, by the supposed actions singly, be obliterated near the heart, and the blood, beyond the point of collapse, would be left stationary, or become retrograde.

A portion of the motion of the venous blood is, unquestionably, derived from other causes. If a vein is surrounded by a ligature, the part of the vein beyond the ligature swells and becomes tense; and, if an opening be made into it, blood will continue to flow with an impinging stream, until the ligature is removed. All communication, between the swelled part of the vein and the heart, has been entirely intercepted by the ligature; and, in no circumstances could the elevation of pressure from the blood on the side next the heart, cause any fluid to be projected out of the cavity of the vein. The momentum, indicated by the swelling of the vein behind the ligature, and by the projecting stream in venæ-section, may be termed the positive momentum of the blood. Let us next investigate the sources from which this momentum may be derived.

It was stated in the preceding part of this inquiry, that the blood in the extreme veins sustained an impulse from the arteries successively, as fresh blood continued to be thrown by the latter into the former; that the aggregate amount of the force of the contractions of the extreme arteries was estimated to be about equal to that of the heart; and that blood would be continued to be thrown into the veins, by this power alone, until its projectile force was balanced by the weight of the column of blood in the veins.

No data, it was stated, have been discovered, from which the amount of the impulse, which the blood in the veins sustains from the arteries, can be calculated. If, however, we take into consideration the peculiar resistances which are opposed to the motion of the blood in the veins, from anastomoses which are particularly frequent in the small veins; from the converging dimensions of the venous system; and from the facility with which the coats of the veins are dilatable by lateral pressure, while no great share of such pressure is in ordinary circumstances discernible in these vessels, it may be concluded that the effects of the *vis a tergo* would not singly extend far into the venous system. It is admitted, indeed, that the momentum of fluids may be continued, either by apportioning the impelling power to the motion required while the resistances remain the same ; by diminishing the resistances without increasing the impulse ; or by a combination of those causes. The venous blood may now be said to be influenced in both ways. While the impulse is constantly renewed from behind, the resistance is diminished in front, and consequently the whole mass may be advanced. But the distance between the blood which sustains the supposed impulse, and that from which the pressure is originally removed, is, in many instances, so great, and the intervening vehicles so pliant and yielding, that it is impossible to conceive that these causes could operate in conjunction. Extensive intervals would be left, in which the blood would stagnate, or its motion be reversed ; especially if gravity, from the position of the body at the time, should oppose the reflux of the blood to the heart.

Besides, there are certain veins in which the blood cannot be conceived to be affected by a *vis a tergo*, and in which the positive momentum is nevertheless found to exist, in the same extent as in all the other parts of the system. If a ligature be put round the internal jugulars which collect the blood from the sinuses of the brain, the part of the vein beyond the ligature will swell, even when the position of the head is dependent. It will scarcely be contended, that any impulse could extend from the arteries of the brain to the internal jugulars, when it is considered that this impulse must be conveyed through the veins of the brain to the sinuses, and through them to the jugulars. Such a supposition must be made in defiance of all probability.

Gravity has been alluded to as a power, influencing, in certain circumstances, the motion of the blood. It is not a little surprising, that the effects of this power are not more visible in the venous circulation. A vein in the leg, for instance, in the sound frame, is scarcely observed to swell more, or become more tense, in the erect posture, than the same vein in the recumbent state of the body. This phenomenon is unsatisfactorily explained by the doctrine, that the weight of the tall column is sustained at different heights by valves.

To what extent may gravity be rendered available to the circulation in the venous system generally? Of this question, it is feared, that we are not prepared to give a full and satisfactory solution. The following observations may, perhaps, throw some additional light upon the subject.

The plain and universally admitted principle, upon which gravity influences the motion of fluids in tubes, is, that if a fluid of the same specific gravity throughout be placed in vessels between which there is a free communication, it ascends or descends, until the fluid in all the vessels becomes of the same level. Capillary attraction presents the only exception to this rule.

When treating of the structure and arrangement of the blood vessels, it was remarked, that anastomoses or vascular communications were to be found, not only between the branches which had arisen from the same trunk, but between those belonging to different trunks; and that these communications prevailed, particularly and to a great extent, in the venous system. The veins, therefore, which constitute a large proportion of the animal fabric, may be considered as ramifications of particular trunks, that these ramifications, in their progress, form an irregular net work of tubes, communicating with each other, in such a way that a fluid may readily pass from one tube into any other, without returning to the original stem from which these tubular ramifications arose.

In this view of the subject, the blood in the vena cava and its divisions may be supposed to be placed, with respect to that in its smaller ramifications, between which anastomoses generally prevail, and which on that account may be deemed a single vessel, in such a manner as to constitute an opposing or balancing column. As the fluid is in both of the same specific gravity, the height of it in the one column will, unquestionably, be regulated by its height in the other. Let it be supposed then, that the system of the vena cava has been filled with blood, and that this blood has subsided in all its parts to an adjusted equilibrium, and let this system, in the state supposed, be subjected to the action of the heart and arteries in the manner that has been described. By the stroke of the heart, a quantity of fluid is withdrawn from one end of the column, and by the synchronous vibrations of the arteries, an equal quantity is added to the other. While the adjusted level of the fluid is diminished on one side by the abstraction of a part, it is heightened on the other by the accession of fresh matter. A double derangement is the effect. By these actions a motion must be excited in the fluid generally to restore the equilibrium between the different parts of it. But the causes are perpetually renewed; therefore a perpetually repeated generation of motion must be produced through the different parts of the venous system by gravity, and this motion must be from the ends of the veins to the trunks.

It is immaterial from what part of the column in relation to height, the portion of fluid be abstracted; or to what part the addition be made; the derangement of equiponderance in the fluid generally, will evidently in every case be the same.

It appears, then, that, in this manner, gravity augments the impulse which the venous blood sustains from the heart and arteries, and, in all probability, produces a great proportion of that which has been denominated the positive motion of the blood in the veins, and which is indicated by the swelling observable in the part of the vein beyond the ligature.

The force communicated to the venous blood by the heart and arteries, assisted by gravity, and favoured by the natural structure of the vessels, may be supposed to be sufficient to preserve any part of the venous system from a permanent state of collapse; and thus, as the actions of the heart are renewed, to place a fresh supply of blood within the sphere of its influence. The blood, thus solicited and impelled, continues to flow, in an uniform continued stream, from the ends of the veins to the heart.

We are now enabled to discern the necessity that exists, for the structure of the veins being different from that of the arteries. We have already seen how well the arteries were fitted for the performance of their peculiar offices. Had the veins been either elastic to any considerable degree, or muscular, the tendency of the powers derived from that structure would have been to diminish the cavity of the vessel. But such a tendency must evidently appear, from the above reasoning, to have been in opposition to the motion of the blood through the veins.

The smooth equable current of the venous blood, exhibiting no marks of sustaining any impulse from any other substance, or of acting upon any itself, not even producing the slightest agitation in the flexible tubes in which it flows, through all the vicissitudes of its course, subject to rapid changes of direction, perpetually varying its elevation, and formed by apparently opposing streams, is a matter that is calculated to strike the mind with astonishment.

The motion of a fluid so rapid, but at the same time so imperceptible and quiet, as that of the blood in the veins, in vessels so flexible and easily distended, proves that no opposition can exist between the forces by which the different particles of blood are moved, but that every particle succeeds to the place of another with the same direction and force that the preceding particle possessed at that place. But by what cause can such a description of motion be produced? Certainly not by any force acting partially upon the blood, whether we suppose the impulses to be made upon it at one end of the column, as by the *vis a tergo;* or laterally, by the pressure of the muscles, or the distension of concomitant arteries.

The description of motion exhibited by the venous blood, seems to be deducible from the principles which we have attempted to establish. Every particle of blood sustains an equal share of the pressure of the atmosphere. If this pressure, as is contended to be the case, be to a certain degree removed from any part of this body of fluid, the particles on one side of which the pressure has been diminished, will yield to the greater influence on the other side, and advance; the next particles, losing the usual support of the preceding, and sustaining a greater weight on the opposite side, move into the place just left by their precursors, and in like manner the next until a motion is produced in the whole. The quantity of motion in the particles behind is regulated by that of those which preceded. Hence, at any section of the current of venous blood, there is a constant change of the particles, but no change in their number, direction, or force. The motion of such a fluid may be deemed internal, it has limits assigned to it beyond which it cannot pass, nor affect any other substance. In the tendency of the blood to fill those limits, its motion in a great measure consists. The veins, therefore, can sustain no unequal lateral pressure, nor undergo any sudden alteration in length, from the fluid which is transmitted through them.

That the limits occupied by different portions of the venous system may be preserved the same, at all times, in its natural state, it is necessary that a supply should be produced for every part equal exactly to its expenditure. The capillary arteries are well calculated to make this supply. These vessels communicate with the venous capillaries and pour their contents into them by regularly repeated discharges, at every distance from the heart. In consequence of the difference of distance, the times of the discharges also vary, or the last vibrations of all the arteries are not synchronous. Regularly repeated projections from an infinite number of orifices, varying with respect to the time and the distances with which those of each are made, but collectively pouring into the veins an equal quantity of fluid in equal times, will easily be conceived to produce uniform equal currents in the veins into which these discharges are made; and seasonably to apportion the requisite supplies. The anastomoses, which prevail so universally in the capillary veins, and which can so readily repair the deficiencies of one part from the surplus of another, must contribute greatly to the uniformity of motion in the venous circulation, and to the instantaneous supply of its demands.

A brief recapitulation of the causes by which the blood is supposed to be affected, combined with a summary explication of the manner, and, so far as that can be ascertained, the proportion in which each contributes its share, may be necessary for the purpose of placing the whole theory of the circulation at once before the view of the reader.

By every contraction of the ventricles a quantity of blood is projected forcibly into the great arteries. As the coats of the arteries are dilatable, these vessels give way to the impetuosity of the propelled blood ; and, to a certain distance from the heart, become of a more enlarged calibre. In consequence of the stimuli derived from the blood newly admitted into the arterial cavity, and (if so bold a metaphor may be used) from the pain of distension, the irritability of the muscular fibres is excited ; the contractile effort is roused ; and, co-operating with the elastic, restores the artery by a rapid movement to its former dimensions. As the blood is an incompressible fluid, a quantity equal to that projected from the heart, must now be displaced from that portion of the arterial system which had been dilated by the immediate impulse of the ventricles. The valves, which are stationed at the roots of the great arteries, and which are securely closed, during the contraction of the first portion of the arterial system, by the retrograde movement of the blood nearest the heart, and more effectually by the suction occasioned by the synchronous dilatation of the ventricles, completely prevent the return of any fluid from the arteries into the heart; while the more ample calibre possessed by the aggregate of the vessels beyond the contracting portion, and the distensible condition of their relaxed fibres at the moment, favour its advance into a more distant portion. This portion, now distended and stimulated by the same causes by which the first had been excited, recoils vividly upon its contents; and, by the rapid resumption of its former capacity, expels from its cavity a quantity of blood equal to the addition which it had received during its dilatation.—The blood, displaced by this movement from what may be termed the second portion of the arterial system, is directed into a more advanced part of it, by the different state of the coats of the vessels and the difference between the aggregate of their calibre, before and behind the contracting portion; because, in consequence of the rapidity of the vibratory undulation, the coats of the vessels between the contracting portion and the heart are still rigid, while those beyond it are relaxed and easily dilatable; and because the outlets on this side are greater, as the aggregate of the bores of the vessels increases with the distances from the heart. The same series of actions is repeated to the ends of the arterial system. A quantity of blood, therefore, equal to that which had been projected from the heart into the arteries, is propelled from the ends of the arteries into the veins in equal times. The power of the heart may thus be said to be transmitted undiminished to the end of the arterial system. (*)

No data were discoverable from which the momentum of the blood, discharged from the ends of the arteries, or the quantity of motion it would generate in the venous blood, could be estimated. But from the mass of fluid that was to be put in motion; from the velocity with which it was known to flow; from the form and position of the vessels containing it; from the resistances opposed by friction, tenacity and various other causes; and from the phenomena attending the venous circulation; it was concluded, with the fullest confidence, that the blood could not be circulated through the veins by the impulse it received from the arteries.

In searching for the causes by which the chambers of the heart were dilated after contraction; it was ascertained, that this condition of the organ was in part ascribed to the form and position of its fibers in conse-

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(*) This statement was written before the appearance of Dr. Parry's work on the arterial pulse. If the account of the passage of the blood through the arteries, given by that Author, and already noticed in this work, should appear to the reader, as it does to the Author, more reasonable, it may be substituted, without prejudice to the argument, instead of this, the explanation which was generally received, when the first edition of this work was published.

quence of which simple relaxation was accompanied by a certain degree of dilatation ; but particularly to the supporting of a part of the atmospherical pressure that would have rested upon the convex surfaces of the heart or its envelope, by the resilient or collapsing effort of the lungs. It was urged, that the abstraction of a part of the ordinary pressure of the atmosphere, from the convex or external surface of the heart or from the convex surface of the pericardium, was perpetual, and was therefore always ready to co-operate with the dilating faculty of the heart itself and that was alternately renewed ; and that the conjunction of these powers was fitted during the intervals of contraction to dilate the chambers, to the utmost extent or at least to the extent of the capacity of the pericardium.

In consequence of the dilatation of the ventricles by the causes which have just been stated, the valves at the roots of the arterial trunks, yielding to the greater pressure from without, become securely closed, and the resumption of blood by the heart from the arteries is completely prevented ; but the passages of blood from the auricular into the ventricular cavities are not obstructed ; the blood, therefore, by which the former chambers were dilated, pursues the less resisted course, and occupies the space left by the dilating ventricles. Any deficiency in the full dilatation of the ventricles, which in the healthy condition of these parts can scarcely occur, will readily be supplied by the projectile force of the contracting auricles. By the dilatation of the auricles, the valves, in the auricular passages, sustaining less resistance on their internal surface, become securely closed; but, at the other openings, those by which they communicate with the venous trunks, no obstacles are interposed. The blood, therefore, in the large venous trunks is relieved from a part of the ordinary pressure in the direction of the heart; it necessarily takes the course in which it meets with the least resistance, and continues to move in that course until the resistance is equalized by the full dilatation of the auricles.

The heart, therefore, acts at once in a twofold capacity. By the contraction of the ventricles it propels the blood through the arteries, and by the dilatation of the auricles it pumps it from the veins. It is at the same time a forcing and a suction pump.

The structure of the veins is not fitted for raising blood through them to the heart, from parts at a distance from that organ, by suction alone. For these vessels, being very thin and pliant, would immediately collapse and become impervious under such an influence. Other agents are required to preserve the permeability of the venous vessels, to give them as it were the property of rigid tubes, and to bring the blood which they contain generally within the sphere of the action of the heart. For this purpose, two causes are supposed to co-operate. The first, to which allusion has already been made in this recapitulation, is derived from the projectile power of the ventricles, transmitted by the vibrations of the arteries to the ends of the veins. This is the *vis a tergo* so famous in the schools of medicine. We were unable to estimate the share of the venous circulation that is to be attributed to this cause ; which, however, so far as it extends, is evidently well fitted to co-operate with an abstraction of resistance in front, and to preserve uninterrupted the communication between the blood in the remote parts and the heart.

The other power, which was supposed to assist in preserving the distension of the venous vessels in opposition to the suction influence of the heart, was gravity. By the anastomoses which so generally prevail among the veins, particularly among the smaller ramifications, the system of the vena cava may be considered as a single canal. By its retiform fabric, the communication between the blood in the different branches, by the aggregate of which, any portion of this canal is formed, is preserved as ready and free, as if the blood had flowed in that portion in a single unpartitioned channel. The position of the vessel is fixed. At the moment in which the equilibirium between the contents of the vessel may be supposed to have been adjusted, a quantity of blood is abstracted from one end of it by the stroke of the heart, while a quantity is added to the other by the synchronous contraction of the ultimate portion of the arteries. The balance between the opposing columns of fluid is disturbed ; to restore the equilibrium, deranged as the actions of the heart are renewed, a motion is generated in the blood by gravity from the ends of the veins to their roots.

In short, the motion of the blood while it flows in the veins is produced by the force of the heart and arteries urging it behind ; by the abstraction of a share of the atmospheric pressure from it in front, in consequence of the resiliency of the lungs, interposing in the intervals between the contractions of the heart ; and by gravity, which is rendered available in this case by the projection of the arteries and the diastole of the auricles.

Objections have been urged repeatedly, and from different sources, against the supposition that the venous blood is moved by means of the automatic or pulmonic expansion of the heart, in consequence of the pliancy of the veins ; as the suction or pumping of the heart, would cause a collapse of the sides of such vessels, and of course obstruct, not promote, the motion of the fluid through them. Notwithstanding the care with which I stated this objection in the former edition of this work, and the readiness with which I admitted the force which I conceived it to possess, various writers, who have honoured my opinions with their notice, have stated this objection as having solely occurred to themselves, and as having been entirely overlooked and omitted to be noticed by me. Dr. Wilson Philip was the first, who I think, stated it in this way; and of course displayed it as an insuperable objection to all my reasoning on the subject. A few years ago, Dr. Arnott, the author of a popular work on Physics, advanced the same objection; and in the same manner as had been done by Dr. Philip and others, claimed it as having originated with himself; and likewise concluded with great confidence, that it entirely destroyed the inferences that had been drawn from the supposed suction of the heart, by Dr. Barry and myself. Dr. Wilson Philip again, in a paper on the motion of the blood, printed in the Philosophical Transactions of 1831, repeats the same objection, with the same inference. The words of Dr. Philip are as follows: "that all, so far as I know, who have either made experiments with a view to prove the effect of these means on the circulation, (the suction of the heart) or who have sanctioned the inferences from such experiments, have overlooked the circumstance, of the veins being tubes of so pliable a nature that they collapse by their own weight. As far as the heart may possess such power, its tendency must be to cause the vessel to collapse, not to move the fluid it contains." Soon after Dr. Philip, in the first instance stated the objection lately repeated, an answer was made to it in a paper which is printed in the Edinburgh Physical and Surgical Journal, on the circulation of the blood in the head; and which, it is presumed, is hereby pretty generally known to the profession. So much for the care or candor with which writers read or state the contents of the works they undertake to criticise.

Although I have already, in the instances now quoted, and more fully in a paper published in the London Medical and Surgical Journal, in reply to Dr. Arnott, stated the extent and nature of this objection, and the cause of the phenomenon on which it is founded, it seems necessary to repeat the argument, not only in allusion to the circulation in the mammalia, but also in explication of the circulation of the blood in fishes, in birds, in crustaceous animals, and in the fætus in utero in which elastic lungs are either altogether wanting, or so situate that their elasticity can have no influence.

The obvious and universally admitted cause, why the sides of a pliant tube collapse, when an attempt is made to raise water through it by sucking from the other end, is the difference of the specific gravity between the water, the liquid intended to be raised, and the air, the medium through which it is attempted to be raised. For the water will not ascend in the tube, until a portion of the pressure of the atmosphere, greater than the difference of weight between the water and the air, shall be taken from the internal surface of the tube. But the whole weight of the atmosphere presses upon the external surface of the tube. If the sides of the tube are not rigid enough to support this difference, they must yield to the greater weight, and collapse. The sides of the tube will require to be more unyielding, the higher the heavier liquid is required to be raised. The heavier the liquid to be raised, compared with the liquid through which it is to be raised, the greater will be the portion of atmospherical pressure which must be removed from the internal surface of the pump ; the sides of which, to prevent collapsing, must be proportionably strong. It will therefore require a stronger cylinder to raise mercury through air, than it would do to raise the same fluid to the same height through water.

But if the liquid, to be raised by suction, be of the same specific gravity with the medium through which it is to be pumped, in that case, no portion of atmospherical pressure will be required to be removed from the internal surface of the pump, before the liquid can ascend in it; and of course, the sides of the vessel, however pliant they may be, will not collapse, except so far as that may be occasioned by their own gravity, or by the slight resistance which friction may make. As the veins are always immersed in a fluid, or at least in a substance, pervaded by a fluid of the same specific gravity with the fluid which they circulate, if a diminution of resistance should occur at one end of any of these vessels, the liquid, contained by it, would move towards the less resisted point, without greater pressure being required to be supported by the external, than the internal, surface of the tube; and, of course, without any collapsing or yielding of their pliable sides. This reasoning will apply, in its full extent, to the veins of those animals, the bodies of which are constantly immersed in a fluid of the same specific gravity with the blood, as of fishes, of the fœtus in utero, and nearly so of those animals which are incased in an unyielding shell or crust. But as the bodies of the mammalia are surrounded by an element of a much less specific gravity than the blood, the above reasoning, as it applies to them, will require to be considerably modified.

For the purpose of illustration, and with the hope of affording an estimate of the matter under review, sufficiently exact for our present purpose, the following mechanism has been conceived. Let two vessels of a cylindrical form be supposed to be placed in contact, that a frees communication be made between the vessels by bore or pipes, at different heights in the line of contact, and let these vessels be filled with water. It is evident, that in consequence of the communication just stated, the water in both of these vessels will be on the same level. Let it be supposed, that water be transferred from one vessel into the other by a pump, with a root reaching nearly to the bottom of the vessel from which the water is to be transferred. And in the first place, let it be supposed, that this pump works under the level of the water, but at its surface.

The transferring of water from one vessel into the other by a pump thus situate, will represent the circulation of the blood through the veins of fishes, and those of the fœtus in utero. If the piston of the pump is raised slightly above the level of the water, the pumping in this case will represent the venous circulation of those amphibuous animals which are incased in a shell, as the turtle, crocodile, &c.; for some part of the bodies of these animals being necessarily slightly compressible, a difference on this account will be required in the action of the machinery for circulating the blood of these vessels when out of the water, from what is required for that purpose in fishes. When, however, the piston of the pump is raised considerably above the level of the water, the action of pumping may be taken to represent the circulation of the blood through the veins of man, and of the other mammalia. The part of the pump in this case, between the bottom of the piston at its highest ascent, and the surface of the water in the vessels, will require to be of a structure sufficiently rigid to support a weight equal to that of the difference between the weight of a column of water and that of a column of air, of a height equal to the distance between the surface of the water in the vessels, and the bottom of the piston at its highest ascent, with the additional weight occasioned by the resistance given to the passage of the water through the stem of the pump by friction. With respect, however, to the part of the pump which descends below the level of the water, no such strength of sides will be required. The only difference between the pressure on the external, and that on the internal surface of the leg or legs of the pump, will be occasioned by the resistance given to the current of fluid along the internal surface by friction.

Such an apparatus will represent the venous part of the circulation of the blood of the mammiferous animals.

As the human frame is immersed, at all times, entirely in atmospherical air, a fluid of a less specific gravity than blood ; as it is not incased in bone or shell, but is soft and compressible to a certain extent by a light weight ; though its veins are generally sunk in a fluid, or in a substance pervaded by an unconfined fluid of the same specific gravity with that which is transmitted through them ; and though the heart is fitted to supply that fluid by which the veins are surrounded, and to keep up its amount ; and by that means places the veins, in a great degree, in the same situation with vessels constantly immersed in a fluid of the same specific gravity with that of their contents ; yet, as the veins run often on surfaces where they are partially invested by a fluid of a less specific gravity than the blood, and connected with substances that have the support of such a fluid only ; in the act of transmitting blood through vessels so situate, by pumping, or by taking off a share of the atmospherical pressure from the blood at one end of the tube, a degree of pressure, in addition to that occasioned by friction, will be made upon the external surface of the vein, from which its internal surface is free. The extent of this pressure is intended to be indicated by the height to which the pump is raised above the level of the water, in the apparatus. But it must be held in view, that the comparison between the transfer of the water from one end of the vessel into the other, by the means supposed in the apparatus, and the circulation of the venous blood, does not hold in one important particular. The degree of pressure, which the external surface of the veins has to support, in the situation supposed, more than that which is required to be borne by the internal surface, does not, as in the case of the apparatus, fall upon one part of these vessels, but is pretty equally distributed through the whole system.

Let us next consider, how the veins are qualified to preserve their tubular form, under this difference of pressure. The sides of a vein, when taken out of the body, and empty, collapse in consequence of their own weight. But veins, when placed in the living system, have a space assigned them ; and have a greater or less adhesion to the contiguous structure. The calibre of the veins, in these circumstances, cannot be diminished, without a forcible derangement in the position of the surrounding substances. The tubular form of the venous coats is favourable to the preservation of calibre, in opposition to pressure upon their external surface. This property will be greater, as the veins diminish in diameter, for in these minute vessels, the arches which form the tube are of a greater curvature, and of a thicker structure in comparison with the size of the vein. This is particularly the condition of the small veins which run on surfaces, and the tubularity of which is less supported by the connecting structure. The superficial veins of old people are often very prominent; but in that case, the coats of the veins approach to a state of ossification, and have become rigid. The tonicity, which the veins in the living subject are said to possess, is calculated to secure full effect from all these causes.

The veins, therefore, are fully qualified to form the limbs of a pump, by which alone the blood may be returned from all parts of the body to the heart.

It has been contended that the supposed use to which the elasticity of the lungs is converted, that of aiding the full expansion of the heart, is unnecessary, and therefore not likely to be so employed; at all events, that it cannot be considered indispensable; because the blood circulates in fishes, in birds, and in the fœtus in utero, in which lungs are either altogether wanting, or are not elastic; or, if elastic, are so situate, that their elasticity can have no avail in the way supposed.

With respect to fishes. As these animals are constantly and wholly immersed in a fluid of nearly the same specific gravity with that of the blood which flows in their veins, the mechanism for returning the blood to the heart, must be classed in that case of the apparatus in which the pump is below the level of the water. The power which the pump will, in this case, require for transferring the water from one vessel into the other, will be such only as is necessary to overcome the resistance given to the transit of the fluid through its limb or limbs, by the friction of the water against the internal surface. So in fishes, the only force with which the heart is required to expand, is a force sufficient to overcome the resistance occasioned by friction, against the internal surface of the veins. As the circulation in these animals is not, I believe, very rapid, this resistance must be inconsiderable, and such as the automatic expansion of the heart, the necessary result of the causes which have been already stated, will be admitted to be fully adequate to overcome.

The fœtus in utero is in exactly the same situation with the fish in the sea; it is constantly and wholly immersed in a fluid of the same specific gravity with the blood contained in its veins; though it is provided with elastic lungs, that great engine serving so many important purposes in the animal frame, their elasticity, is, during its residence in the womb, in an inactive state; but is complete, and in readiness for the new part required of it by the great change which the animal undergoes, in being removed by birth into a different element.

So far, therefore, as the circulation of the blood in fishes and the fœtus in utero is concerned, the want of elastic lungs will, upon due consideration, appear, not as an objection to the doctrine of the use that has been ascribed to them in the circulation of the blood, in the mammalia after birth, but will be considered as a very strong negative argument in its favour.

Birds, however, are not only placed in the same element with the mammalia, but are accustomed to ascend into regions in which the air is much rarified; and in point of specific gravity, still more remote from the blood which they carry in their veins, than that which the latter breathes. The reasons, by which the want of elastic lungs in fishes is accounted for, will not apply to them. An adaptation to the ærial residence which these animals have assigned to them, requires a peculiarity of structure, and a different series of powers. The same functions are necessarily performed after a different plan. To enable the reader to comprehend fully the view which we have of the method by which nature has contrived to supply the office performed by elastic lungs in the mammalia, would require a more detailed explanation than can be properly given at this place. I have therefore to request the reader to suspend his judgment, till the respiration, and other functions of birds, shall have been considered; assuring him, in the mean time, that it is confidently hoped that a satisfactory cause will be shewn, for the deviation from the structure of the mammalia; and that in that deviation, proofs will be adduced of some greater power being required, than that derived from the simple relaxation of the heart, for securing the expansion of that organ in all animals, in which a fluid circulates of greater specific gravity than that in which they are immersed.

Of the authors who have lately written on the causes of the motion of the blood, and who refuse their assent to any influence being derived in favor of that motion, from the suction of the heart, may be mentioned Dr. Arnott, in a work on physical science, already mentioned; and more particularly in a paper, published in the London Medical and Physical Journal for 1827, in reply to some observations of mine; and Dr. Wilson Philip, in a paper printed in the Philosophical Transactions for 1831. Dr. Arnott contends, that the blood in the veins is returned to the heart altogether by a *vis a tergo*, which is chiefly supplied by the capillaries. Although in my opinion, the structure, natural condition, and converging position of the veins, supply an insuperable objection to the supposition of the venous blood being moved by any powers, which act upon it in the way of *vis a tergo*, from whatever origin this may proceed, it may be proper, briefly to consider the views of Dr. Arnott. The learned author adduces what he considers to form a proof of the propelling power of the capillaries. One of the chief props of the doctrine of the irritability of the capillaries, and of the power which these vessels possess of transmitting the blood in greater quantities, is the blush which suddenly suffuses the cheek of the bashful youth. But is Dr. Arnott quite certain, that the redness is produced by the action of the vessels of the cheek? On the contrary, it will be found, I think, to have a very different origin; as will appear, when the peculiarities of the circulation of the blood in the head come to be considered. As well might the swelling of the corpora cavernosa penis be ascribed to the capillaries of that organ, as the increased flow of blood into the cheek, to the action of the capillaries, when the mind is invaded by a sense of shame. The other proofs adduced by Dr. Arnott, of the irritability of the capillaries, are unaccompanied with any evidence that they are rendered capable, by that irritability, of transmitting blood through them in a state of health. Veins, indeed, in some conditions, are observed to swell. When the feet or hands are immersed in warm water, the veins of these parts swell. The swelling of these veins has been attributed to the increased action of the capillaries, excited by the stimulus of heat. But the distension of the vein in this case, is more reasonably accounted for, from a part of the blood in the vein being converted into gas; or rather, from the gas already in the vein being rarified, by the increase of heat. That a cause, which has its origin in the ends of the veins, or rather in those intervening spaces, which exist between the terminations of the arterial and venous capillaries, and which is calculated to expand the veins, does exist, and may have at all times, some effect in preserving the calibre of the vessels in those remote parts, is not denied. But this cause is not derived from the action of the veins. It will be explained when the doctrine of venous absorption, and of the sources of animal heat, shall come under review.

The veins of old persons are often so dilated as to rise above the skin. As age advances, the venous structure becomes of a more rigid nature ; it verges towards ossification. When the system, in extreme old age, is deprived of that source of nourishment conveyed through the lymphatics, by the ossification of the thoracic duct, the body shrinks, and leaves the superficial vessels exposed. But such veins are nearly in the condition of rigid tubes. If compressed, they expand by their own elastic force. Some share of this expansion may, as will afterwards appear, arise from the cause to which I have just alluded, as having its origin at the ends of the capillaries.

Dr. Arnott contends, that, as the heart, according to my own admission, is capable of transmitting the blood to the end of its course, as is proved by the operation of phlebotomy, it is unnecessary and unreasonable to seek for any other cause. But it is certainly unreasonable to argue in support of the ordinary action of any organ, from the effects produced by it under such unnatural and violent circumstances. The passage of the blood through the veins, in this case, and which it is admitted is attributable to the heart alone, is occasioned by the veins being distended to that size which gives them the property of rigid tubes ; a condition in which they are never placed in the living system, and without which the power of the heart, however great, could not force the blood through them.

That my opinions should have gained so many adherents, appears to Dr. Arnott as a lamentable proof of a great deficiency in the medical education of the period in which we live. But I will venture to tell Dr. Arnott, and I cannot omit to tell him, though it may be accompanied with the certainty of giving him alarm, that the number of these adherents is increasing ; that it will increase from day to day and from year to year ; and that the rapidity with which that increase will proceed, will be in proportion to the extent in which the rising generation of physicians is educated in those sciences, to which Dr. Arnott alludes, and in which he is himself so great a proficient.

Dr. Wilson Philip takes a different view of the causes by which the blood is circulated. Like Dr. Arnott, this eminent physician is satisfied that the blood cannot be returned to the heart by suction, on account of the pliant nature of the coats of the veins. He is satisfied, however, that the force of the heart and arteries is all expended at the terminations of the arterial system; and the arguments, by which the influence of the heart and arteries on the motion of the blood in the veins is disproved, apply equally to the supposed action of the capillaries in promoting that motion.

As the suction power of the heart is supposed, from the structure of the veins, to be inadmissible, no other power is apparently left, but that derived from the action of the veins themselves. This source of power had been so often discussed and rejected, that it evidently required no small degree of courage to bring it again on the field. As a proof of the efficacy of the veins in transmitting the blood through their calibre, he states an experiment made on a newly dead rabbit, in which the circulation had been prolonged by artificial respiration. He laid thejugular vein bare to the extent of an inch and a half, and put a ligature round the denuded part at the end nearest the head. He caused the rabbit to be held up by the hind legs; he then tightened the ligature. Before the ligature was tightened, the vein was full; after the application of the ligature the blood in the part of the vein between the ligature and the heart was instantly and completely expelled, as the transparency of the vessel enabled him to perceive. The vessel itself was wholly collapsed, proving that all its blood had entered the heart. In the mean time, on the other side of the ligature, the vein had become completely gorged with blood.

The inferences which Dr. Philip draws from this experiment are, that the blood on the side of the ligature nearest the heart, had been advanced to that organ by the vessel itself; by its own natural action; all the other possible causes of its movement having been withdrawn; and that the gorging of the blood, in the part of the vein behind the ligature, arose from the same cause, the action of the blood vessels.

With respect to the first inference, it is not true that all the causes, external to the vessel itself, were withdrawn; the self-expanding power of the heart, accompanied with the suction occasioned by that power, was sufficient to empty the vein in the manner that is described. Had the vessel been emptied by its own contractile force, completely, as it was, it must have become a solid cord; but it appears it was transparent, which proves that its sides have been collapsed simply without contraction. To account for the part of the vein behind the ligature being gorged with blood, I must refer to what is said, in the subsequent pages, on the causes of the motion of the blood in the head, and on consequently of the motion of the blood in the jugular veins.

Dr. Philip does not fail, in these cases, to repeat the objection to my hypothesis, drawn from the collapsing of the sides of a pliant vessel, when a fluid is attempted to be raised through it by suction ; and to maintain that this objection had escaped the notice of all preceding writers.

I have already alluded to a work of Sir David Barry, in which a view of the circulation of the venous blood is exhibited ; and which may be considered a modification of the doctrine advanced on that subject in the preceding pages. Sir David contends, that the blood in the veins moves only during the period of inspiration ; that, during this period it flows into the chest, and is there accumulated in sufficient quantity to supply the demands of the heart, until the return of another inspiratory period. This doctrine, which is certainly repugnant to our preconceived opinions, and in apparent opposition to many known and well attested facts, Sir David contends to have established by experiments. This doctrine was introduced into the world under very favorable circumstances ; it appeared under the high patronage and approval of the Institute of France, and obtained by that means a reputation both sudden and extensive.

The experiments, by which Sir David contends that this view of the venous circulation is confirmed, are the following. A catheter was introduced into the jugular vein of a horse, and pushed onwards to the pectoral cava; the other end of the catheter, to which a spiral glass tube was affixed, was connected with a coloured liquid. This liquid was observed to rise contrary to gravity when the animal inspired, and to be stationary, or even retrograde, at any other period. This experiment did not exhibit the same appearances when the animal was in a standing position, it only shewed them when the animal lay on his back ; for, says Sir David, the horse is scarcely observed to breath when standing.*

* These experiments, in my opinion, by no means warrant the conclusions which Sir David Barry has drawn from them, and which were admitted to be legitimate by the committee of the Institute of France. The phenomenon, namely, the ascent of the blue liquid during the inspiration of the horse, admits of the following explanation.

The walls of the chest are secured in all directions, excepting through the windpipe and the bores of the large vessels, which enter it at the top and bottom. The dimensions of the chest are variable, being greatest at the period of full inspiration, and smallest at the lowest stage of expiration. These dimensions, in passing from the lowest to the greatest extent, require the introduction of matter into the enlarged boundaries. The sources from which this is most readily derived, are the atmospherical air through the channel of the windpipe, and the blood of the system, through the blood-vessels.

The introduction of air from the atmosphere is impeded by two causes, the friction which the air sustains against the internal surface of the windpipe, and the resistance offered by the elasticity of the lungs. The pectoral and abdominal cavæ are the other outlets by which matter can be introduced into the chest during inspiration. The introduction of blood in greater quantity, at the time that the dimensions of the chest are expanded, seems to be effectually prevented by the regulated actions of the heart. No great additional accumulation of blood can be made in the cava, for this vessel must be at all times distended to nearly the same capacity. It is im-

That the blood in the veins is returned through those vessels in one equable continued flow, has been an opinion long received, and supported by evidence too strong to be easily repelled. If a vein is laid bare and tied by

mersed in the elastic lungs, which tend constantly to resiliate from its external surface, and to draw after them the coats of the vessel. When a full inspiration is made with great rapidity a supply of matter, to provide for the increased dimensons of the chest, cannot be made in the time required. All the contents of the chest which lie behind the elastic curtain of the lungs, are at that moment relieved from a share of the pressure of the atmosphere. A greater expanding power is placed upon the internal surface of the coats of the cava, than upon the external surface of that vessel. The coats of the cava may be supposed to yield, to a certain extent, to this distendlng power, and afford room for an additional quantity of blood. This additional quantity is yielded by the jugulars with difficulty. At this moment, matter having access to the cavity of the jugulars, would be sucked into the channel and moved towards the heart. This would appear to be the effect of an extraordinary and unnatural effort, not the effect of ordinary inspiration. It would, I am of opinion, be found upon examination, that the result of the experiments made by Sir D. Barry, upon which so much stress has been laid, is to be attributed to the position of the animal at the time of making them. The horse was placed upon his back, with the head probably dependent. This position would appear to be very unnatural and irksome to that animal, as he never places himself in it for any considerable time. The circulation of the blood from the head and neck could not fail to be impeded by this position. This is the more probable, that the operation was not attended with the same result, or as it is stated, did not succeed when the animal was standing. The reason which Sir D. assigns for this is not a little curious; because, says he, horses are scarcely observed to breathe when they are standing. According to Sir D. Barry's doctrine, the blood of horses must not circulate when they are standing.

Sir David Barry is wrong in supposing, as he seems to do, that what is called, for the want of better language, a vacuum, a ligature, the blood instantly leaves the part of the vein between the ligature and the heart, without waiting for the period of inspiration. The cessation and renewal of the motion of the venous blood according to the periods of expiration and inspiration, which are necessary upon the supposition of Sir D. Barry, would exhibit marks of an agitation too evident to be mistaken. The blood advances, according to Sir D. Barry, in the veins during inspiration, and lodges in the large vessels near the heart, by which it is received as the necessities of that organ require. But where can this lodgement be made? The vena cava is at all times filled, nearly to the greatest extent that its coats will admit of. No alternating depletion and distension take place in the veins without the confines of the chest. That the motion of the blood is sometimes accelerated by inspiration, is evident from the vivifying effects of deep sighing, to which persons labouring under great depression of spirits have involuntary recourse, and which affords them relief. This is explained from the fuller and more vigorous expansion of the heart occasioned by the increased resilience of the lungs.

exists in the chest only during inspiration. The causes, by which it is produced, operate at all times, though not with the same force. For the lungs are always dilated, in the sound state of the system, far beyond their natural condition. It is only in consequence of the elasticity of the lungs, that the vacuum, discovered by the experiments of Barry, could be produced. If the lungs had not been elastic, but dilatable like a bag, the windpipe would readily have supplied all the matter requisite to fill the chest during inspiration.

The general agents which are employed in the circulation of the blood, and which themselves constitute a part of the animal machine, are the lungs, the heart, the arteries, and the veins. There are other agents which may be called general, but which do not peculiarly belong to the animal fabric. The chief of these are atmospherical pressure and gravity.

But, in addition to these, there are other powers or contrivances which may be called subsidiary, and of which the office is confined to particular parts of the circulation; among these may be ranked the machinery of the liver and the spleen.

The observations which I am now about to make will be confined to the subsidiary agents, the office of which belongs exclusively to the motion of the blood through the encephalon; and, in noticing these, it will be necessary to consider, generally, this part of the circulation.

The cranium is a hollow spheroid constructed of bone. Though the shell of this spheroid be formed of several bones, yet, in consequence of the exact and perfect manner in which these bones are united by what anatomists term sutures, it may be considered to be composed of a single bone continuously of the same structure.

This osseous crust, when examined after death, and when all the softer parts have been separated, exhibits many small perforations, affording, in the sound state of the parts, a passage for nerves, arteries, and veins, besides one much larger, by which a communication is formed between the contents of the cranium and those of the spinal canal, a cylindrical tube of the same osseous structure, and perforated in the same manner, and for the same purposes, as the cranium itself.

As a foundation for the arguments which may be advanced in this inquiry, it seems necessary to examine briefly the component parts of the encephalon, beginning with the vascular and membraneous parts of it.

The arteries, at their entrance into the cranium, through the perforations appropriated to that purpose, possess the same structure with those vessels in other parts of the body. They are described as taking at this place, a winding or tortuous course, which is supposed to retard the impetuosity of the blood when it is too violent. Here, as in other places where they perforate, they form, by their external coat, an adhesive communication with the membrane, which, in this case, is described as a continuation of the dura mater lining the passages.

After they have entered the cranium, they for some time run in grooves formed on the internal surface of the skull, sending off, as they advance, branches to the brain and membraneous parts of the encephalon. The arterial system in the head seems to differ little from the same system in other parts of the body. The capillary arteries of the head communicate with the capillary veins in all probability after the same manner in which these vessels communicate in other parts of the body, and which is at present altogether unknown.

The part of the venous system within the head, intercepted between the ultimate terminations of the arteries and the sinuses, is in no respect peculiar or different from the same system in other parts of the body. After this, a remarkable change in the venous structure takes place. The veins do not unite to form large branches or trunks of the same structure with themselves, but open, as is well known, into vessels of a peculiar fabric, termed by anatomists sinuses. These sinuses it will be necessary briefly to describe.

The internal surface of the cranium is lined by a strong membrane, termed the dura mater, composed of two plates, and adhering throughout to the skull.

Foldings of the internal plate of this membrane cut the cranium both vertically and horizontally, separating the brain into two divisions or hemispheres, and the brain, properly so called, from the cerebellum. The two plates of which these foldings of the internal lamina of the dura mater are formed, adhere firmly together, excepting at their approach to the skull, where they separate, and become attached to its internal surface at a distance from each other. The space formed by the separated plates of the dura mater, and the part of the cranium intercepted between the lines of the attachment of these separated plates, are the sinuses. Their transverse section forms a triangle. In consequence of the great strength of the membrane forming the sinuses, and of the stretch upon which it is always kept, the membraneous sides of the sinuses preserve their position in opposition to any moderate force directed either against their internal or external surfaces. They are vessels whose dimensions never vary. They are very numerous; are situated in all quarters of the head; they differ greatly in size; and have each a communication with all the rest, either mediately or immediately. Such are the vessels into which the veins, returning the blood from the brain and its membranes, discharge their contents. The sinuses, by passages through the cranium communicate with the veins of the neck; and, at this part of the sanguiferous circle, the ordinary venous structure is again restored. As these vessels pass through the cranium, their external surface forms a circular adhesion to the membranes lining the bony canals through which they pass.

With respect to the nerves, the other system of vessels of the head, it is only necessary to remark, that they also, at their exit from the cranium, form a similar attachment to that of the blood-vessels to the periosteum, lining the door of their exit. Besides the vessels now enumerated, there belong to the encephalon other receptacles which now require to be considered, and which will be found to hold an important place in the present investigation. I allude to the ventricles. The ventricles of the brain may be described as an irregular fissure, traversing the interior substance of the brain, varying greatly in breadth, and dilatable at different places, according to circumstances, into cavities of greater or less extent. The more extensively dilatable parts of this fissure have received the name of ventricles; and the narrow channels by which they communicate, have received the name of passages, or roads between the ventricles. The ventricles are generally reckoned to be four, and each communicates with all the rest, either directly or indirectly. These ventricles, when dilated, so as to form cavities, contain a watery fluid. It is still a matter of dispute, whether they are at any time altogether empty, or whether their opposing surfaces are not at all times, and at all places, separated by a portion of water. The water, which may exist in the ventricles, will, during the sound state of the parts, be distributed in a given ratio through all the ventricles. This seems necessarily to follow from the position of different parts of the brain, taking in its course the ventricles, while the other parts remained in the relation in which they exist in life, this section would exhibit a surface, on which the ventricles and communicating passages might be compared to the lakes and rivulets upon a map.

The contents of the cranium may be divided into three parts. 1. The blood contained in the arteries and veins. 2. The water in the ventricles. 3. The parenchymatous substance of the brain itself. The membranes, the coats of the blood-vessels, and nervous substance, we propose for the sake of argument, to place in the last of these divisions. As the limits of the cranium are fixed, and as the solid contents of it must be at all times the same, it is evident that the proportions of any of these divisions cannot vary without affecting the proportion of one or both of the other two. As the substance of the brain, during life, cannot undergo any considerable alteration suddenly, or without the operation of a slow process, and as this substance is incompressible, in a manner to be afterward explained, and as the quantity of water cannot be instantly increased or diminished, and is itself also incompressible, it follows, that the quantity of blood contained in the head must, on the supposition of the other two divisions of the contents of the cranium remaining unchanged, be at all times the same. Let it be supposed, that the quantity of water contained in the ventricles, and the quantity of brain, two of the three constituent portions of the contents of the cranium, remained unchanged, the quantity of blood, the remaining portion, could not be augmented by any force by which the cranium was not ruptured, or the cavity enlarged. No blood, in these circumstances, could be thrown into the head by the arteries, unless an equal quantity was at the same instant discharged from the cranium through some of its outlets. As the contractions of the arteries by which blood is thrown into the head, are synchronous, no blood could be returned back from the encephalon through one artery, at the moment that a quantity was thrown into it by another. The sinuses are the only outlets through which blood could be discharged. But by what means can any blood be displaced from the sinuses? If we consider for a moment the form and structure of these vessels, and the oblique manner by which they are perforated by the small veins leading from the brain and membranes, it is impossible to imagine that the effect could be accomplished by a force directly communicated from the heart and arteries, in the manner of a *vis a tergo*. But the system is provided with other agents by which the blood in the sinuses can be affected. These are gravity in certain positions of the head, and another still more effectual and permanent, the removal of a portion of the pressure of the atmosphere from the blood at the ends of the sinuses, communicating with the veins exterior to the head, by the dilatation of the heart and the resilience of the lungs constantly tending to increase the diameter of the veins within the chest. But this class of agents, if not so powerful as to outbalance the whole weight of the atmosphere, or if the parietes of the cranium did not yield, could not withdraw a single particle of blood from the head, unless a quantity equal to that to be extracted, was at the moment supplied by the arteries. But the abstracting powers, though singly insufficient to move the blood in the head, being permanent, are at all times ready to co-operate with the successive injections of the arteries. By the joint agency of these two classes of powers, an impulse is given to one end of a column of fluid, and an abstraction of resilience, or, in popular language, a suction is applied at the other; and thus an easy, equable, and duly proportioned motion is given to all the blood contained between the first entrance of the arteries, into the cranium and the outward ends of the sinuses; that is, to the whole blood contained within the cranium.

I do not know of any machinery with which this, for circulating the blood in the head, can in all its parts be compared, for the sake of illustration. The heart and sinuses resemble, in some degree, a succession of pumps, by which water is raised to one elevation, and then from that to another; with this difference, that it is on account of the great elevation to which the water is to be raised, that a second pump is required; but, in the case in question, the sinuses, or a first order of pumps, are not required on account of the elevation, but for the purpose of collecting the blood through a thousand channels from all parts of the brain, and of forming reservoirs to which the action of the second order of pumps can be more effectually applied.

The sinuses, and the veins of the neck, have been supposed to form a syphon. But it cannot be by this machine that they act at all times, at least in conveying the blood from the head. In the erect position of the head, they may form a syphon; but, in the recumbent, and, still more, in the dependent state of that part of the body, gravity, which is the cause of the motion of a fluid through a syphon, ceases to influence, and ever opposes the motion of the blood from the head; and the actual removal of a part of the pressure of the atmosphere from the blood on the side towards the heart, or some other equally effective power, is required. But no such power has been exhibited in any of the views taken of the circulation of the blood, excepting in that in which the elasticity of the lungs, and the automatic expansion of the heart, are supposed to have so great a share. Gravity, as has just been stated, may, in the erect position of the head, assist in returning the blood from that part of the heart. The co-operation of gravity with the other agents which have been mentioned, is in this case direct. But even in the pendent and horizontal position of the head, gravity, though opposed to the motion of the blood in the jugulars, has indirectly a considerable effect in securing an uninterrupted and free motion in these vessels; for, by the lateral pressure, which it in these circumstances gives to the sides of the veins, gravity prevents what may be termed a collapse of these vessels, which, if it took place, would intercept the suction influence, and, by that means, it more than counterbalances the opposition which it directly supplies to the return of the blood.

A singular phenomenon, exhibited in the motion of the blood from the head, receives a striking illustration from the views which have now been exhibited, and appears to me singularly confirmatory of their truth, and may with propriety be noticed at this place. The veins of the neck, particularly the jugulars, vessels which receive the blood from the encephalon, and the pectoral cava, into which they discharge their contents, contrary too the usual mechanism of the veins, pulsate. For the want of a better explanation, this effect has been usually attributed to the stroke communicated by the accompanying arteries to them, and transmitted by these vessels in a secondary way to the touch. The arguments by which the opinion of the pulsations of veins being attributable to accompanying arteries is, I think, refuted, have been stated elsewhere, and need not be repeated here. The simple, and I trust, satisfactory explanation about to be given of this phenomenon, is this. The arteries, which convey the blood to the head, throw this fluid into the cranium by synchronous jets. An equal quantity, according to the hypothesis contended for, must at the same instant be discharged from the cranium into the veins. This must also be by jets which cause successive currents, or in other words, pulsations in these veins. While the contents of the ventricles, and the substance of the brain, remain unaltered, it is evident, that the quantity of blood contained in the cranium must be at all times the same. No force could, in the state supposed, increase this quantity of blood ; and from this circumstance, a great security is given to the vessels of the brain, the rupture of any of which would be attended with fatal effects. Nor would the abstraction of any quantity of blood from the rest of the system, diminish that contained within the cranium. This consequence, which is so clearly deducible from the principles which

I have attempted to establish, has lately been confirmed by the observations of an ingenious and able writer, who bled a number of animals to death, and found the blood-vessels of the head all fully distended with blood. Although the quantity of blood contained within the head be at all times the same, in the circumstances stated, the distribution of it may vary. One part of the encephalon may at one time contain a greater quantity of blood than its ordinary proportion ; but some other part, in that case, must contain less. In what circumstances this occurs, or what are the consequences of it, are questions which, I suspect, are at present unanswerable. A partial inflammation of the brain, if inflammation of that organ be accompanied with the same phenomena accompanying inflammation in other parts of the body, must alter the distribution of the blood in the cranium. Blood would be accumulated in the inflamed part, and there would be a corresponding abstraction of it from another. From which of these states greater danger would arise, is altogether unknown. Though the quantity of blood belonging to the encephalon must, in the state supposed, be at all times the same, the quantity which passes through it, in any given time, will vary according to the velocity of the blood.

We have hitherto been considering the motion of the blood in the head, in the case in which the two other constituent portions of the encephalon are supposed to remain the same. We are next to examine how far the motion of the blood in this part of the circulation may be affected by any changes in the quantities of one or both of these other portions of the contents of the cranium, by their augmentation or diminution; and, first, with regard to the substance of the brain itself.

The late Dr. Alexander Monro of Edinburgh contended, that he had proved by experiments, that the substance of the brain was incompressible. What that learned Professor meant by the incompressibility of the brain, may require some explanation. If we take a piece of the brain into our hands and squeeze it, few substances can be found more yielding and compressible. But the meaning of the term, as employed by the Professor, was, that the ultimate particles, of which the brain was composed, were incompressible; or, that the same number of these particles would, under any different degrees of pressure, occupy the same space. Perhaps the idea may be expressed in other words, by saying, that the substance of the brain was inelastic. In this light, the muscular substance, and indeed most other parts of the body, may be considered incompressible. Previously to the time of Dr. Monro, and by many of the physicians of the present day, in the case of accumulation of water in the ventricles, the water was supposed to compress the brain into smaller bulk. Dr. Monro justly contended against this opinion, and maintained, that the water in these circumstances, by pressing against the brain, caused the absorption of that organ; and that, by this means, a part of the substance of the brain was consumed, or rather dislodged from the encephalon, to give room for this increased quantity of water. An opinion was held by Dr. Monro, and indeed by all the physiologists of his day, that the office of absorption was solely performed by a peculiar class of vessels called lymphatics. Though the search had occupied the eager and ambitious toil of anatomists during the last fifty years of the 18th century, no lymphatic vessels could ever be found in the brain. It was concluded from this, that water, when once deposited in the ventricles, or in any part of the encephalon, must remain there, as there existed no machinery by which it could be removed. In the case of the disease called Hydrocephalus, Dr. Monro was induced to recommend as the only, but awful alternative, the discharging of the water by the puncture of the brain. By what machinery the substance of the brain could be carried out of the encephalon, while there existed none for the removal of water from the ventricles, neither this eminent physiologist, nor any of his followers, have attempted to explain.

But the doctrine, that the lymphatics are the only vessels endowed with the faculty of absorbing, or of taking up fluids deposited in cavities, has been disproved. By far the greater part of the process of absorption, it is now admitted, is performed by veins. Indeed, the lymphatics, to which the sole office of absorption was attributed, owe, in a great measure, the power which they possess, in this respect, to their connexion with the veins within the chest, where the great lymphatic trunk is subjected to the exhausting powers derived from the elasticity of the lungs and the automatic expansion of the heart.

The important physiological truth, that the veins are fitted to absorb, is plainly deducible from the causes by which the blood is moved in its passage through the veins; and that they do absorb, has been fully established by the experiments of many physiologists. We are now, therefore, happily relieved from the disheartening conviction, that water, when deposited in the ventricles, can never be removed by any natural process. There can exist no doubt, also, that the brain, like every other part of the body, is perpetually undergoing a process of renovation; that its substance is subject to increase and diminution; that it may be augmented under the enjoyment of luxuriant health, as well as wasted by defective nourishment, or long consuming disease.

Let it be supposed, from some cause, that the expending process has been more active than the renovating; and that, in consequence, the substance of the brain has been considerably diminished. In this case, the quantity of one or both of the two other constituent portions of the encephalon must be affected. The space that would otherwise have been a void, must be filled up by an augmentation of the quantity of one or of both of these encephalic portions. Either a greater quantity of blood must be contained in the vessels of the head, or the quantity of water in the ventricles must be augmented, or both of these effects will, to a certain extent, be produced. The last of these conditions is, I believe, the one most usually found. On the dissection of bodies reduced to great emaciation by disease, the vessels of the head are found to be turgid, the substance of the brain to be soft, and to contain an unusual quantity of blood, and the ventricles to be greatly distended with water. Sometimes, in such cases, the quantity of water contained in the ventricles is very considerable; ten ounces is by no means uncommon. Suppose there had existed no receptacles for water, such as the ventricles, and the brain to have been wasted to that degree, by which room was afforded for the admission of ten ounces of water into the encephalon, the blood-vessels of the head must necessarily have been loaded with ten ounces of blood in addition to the quantity which they already possessed, and by which they appear to have been already too much distended. Long before they could have been distended to the capacity necessary for the admission of so great a quantity of blood, their coats must have given way, and a fatal hemorrhage ensued; or, at all events, they must have been too much surcharged, for the performance of their functions. Hence we readily perceive the important uses of the ventricles. By becoming the receptacles of a mild fluid, they, in certain circumstances, prevent the blood-vessels from being over distended. By their greater or less expansion, they become the grand regulators of the circulation of the blood through the head. Water in the ventricles, in such circumstances, instead of being considered a disease, is, in reality, the great remedy provided by nature for the preservation of life, in situations in which it could not otherwise exist. It is the defence set up by nature for the protection of the breaches or weak points which may exist in this part of her works.

The ventricles of the brain, in consequence of their irregular course, are admirably situate for enabling the substance of the brain to assume that variety of position necessary, as circumstances alter, to give due support to the vessels of the head, without sustaining at any point a disproportionate distension. But, to perceive this sufficiently, the brain itself must be examined.

These uses of the ventricles are most important, necessary, and as now described, so certainly true, as, if I am not singularly blinded, to require only to be announced to gain assent and acknowledgement of every unprejudiced inquirer. No physiologist has hitherto, I believe, even ever ventured to form a conjecture respecting the purposes for which the ventricles were designed by nature. The demonstrator generally passes them over with the observation, that they must be of some use, as nature never forms any thing in vain. That the ventricles may serve, besides those now explained, other purposes at present unknown, is not improbable; for nature, a rigid economist of her means, often accomplishes various ends by the same machinery.

We have been considering the state of the brain, in which its substance is unduly diminished. Let us notice, for a moment, the opposite condition. Little water will, in this case, find admittance into the ventricles; and if the substance goes on to augment, the space assigned for the blood-vessels will be encroached upon; and if the growing process continues, sufficient room for the transmission of blood in proper quantity will not be left. I think it extremely probable, that some of the species of apoplexy and of palsy are the consequence of an overgrowth of the substance of the brain. When no water is found in the ventricles, the substance of the brain will also be observed to be unusually firm and solid, showing little appearance of blood upon being cut into, and the vessels on its surface slightly distended.

As the quantity of blood distributed to the head may be supposed, from the calibre of the vessels which supply that quarter being fixed, to be nearly the same, if, from an overgrowth of the brain, the quantity of this blood transmitted through the encephalon be diminished, a greater quantity must be sent to the parts of the head exterior to the cranium. Hence that redness of the face and eyes, which so frequently attend great obesity, and which is so often the precursor of the apoplectic stroke.

In the cases which, in this head of the inquiry, have been supposed, as well as in all the former, it is very evident, that the abstraction of any quantity of blood from other parts of the system, could not diminish the quantity of this fluid within the cranium; and that no arterial force directed against the brain could, for an instant, place an additional quantity of this fluid within the same limits. Some change in the other two constituents of the contents of the cranium must take place synchronously with any alteration of the quantity of blood in the encephalon. If, however, the substance of the brain, or the water in the ventricles, or the blood itself, be at all compressible, the quantity of blood admissible into the head will be affected by changes in the pressure of the atmosphere. If any, or the whole of the three constituent portions of the encephalon be compressible in the way that has been explained, then a greater quantity of blood will be contained within the head during a high, than during a low state of atmospherical pressure. It is probable, that the substance of the brain is in some degree diminished in bulk, as would happen, on the supposition that it was compressible, by an increase in the weight of the atmosphere; for, in that state, an unusual alacrity of mind and activity of body, as was long ago pointed out by Boerhaave, are enjoyed; while the contrary affections, both of mind and body, are at all times, I believe, to a certain extent, the concomitants of a very low state of the barometer, or a very light state of the atmosphere. If an increased quantity of blood should be admitted into the head during a higher state of atmospherical pressure, the addition would only be as much as was necessary for carrying on its circulation through the head.

It may naturally be asked, from what source is the water lodged in the ventricles derived, and how is the supply of it so regulated, that it should be at all times equal to, and never more than, the demand?

The water is unquestionably supplied from the blood, and is conveyed from those exhalent terminations of the arteries, which, in ordinary circumstances, carry a colourless watery fluid. The mechanism, by which the supply is proportioned to the demand, is a subject of considerable intricacy; but may, perhaps, be thus explained. Let it be supposed, that a certain proportion of the substance of the brain has been by some cause or other withdrawn, without being replaced by a fresh supply; or, in other words, that the quantity of brain has been diminished. The place of the portion of brain which has been withdrawn must be occupied by some other substance; indeed it could not be withdrawn without such a substitution being synchronously made. The augmentation of the quantity of blood distending the blood-vessels, is the direct and immediate manner in which the requisite supply of matter is made. But when the process is continued, the blood-vessels become enlarged to an unnatural degree; and in that case their coats make a resistance to further distension. A force is at this time generated by the resistance of the coats of the blood-vessels. Supposing that the contents of the cranium, as must always be the case, are all equally subjected to the pressure of the atmosphere, and that a force, generated by the resistance of the coats of the blood-vessels, existed, the necessary tendency of this will be, to remove a part of the pressure of the atmosphere from the rest of the contents of the encephalon.

What will be the effects of this tendency or effort, when applied to the mouths of the exhalents and absorbents opening into the ventricles? Certainly, to increase the discharge from the former, and to diminish the suction of the latter. The natural consequence is, an accumulation of water in the ventricles, which will go on increasing, until the force generated by the resisting coats of the vessels shall be removed; or until that force shall have been balanced by an equal force, generated by the resistance afforded to further distension by the sides of the ventricles.

If, as may easily be believed to be the case, the coats of the blood-vessels of the brain do at all times make some resistance to their contents, or tend, with some force, to diminish their calibre; in that case, according to the argument now maintained, a certain quantity of water ought, almost at all times, to be found in the ventricles of the brain. Hence the existence of water in the ventricles of the brains of persons who have been cut off in the enjoyment of perfect health, and the constant absence of adhesions between the sides of the ventricles of the brain, may be explained.

The inquiry has hitherto proceeded on the supposition that the solid dimensions of the capacity of the cranium are fixed; but, if it be pursued under a supposition of a change in these dimensions, either an increase or diminution of them, a new and extensive field opens before us. As I have already, I fear, extended these remarks to too great a length, I shall confine the observations which I have to make on this part of the subject, within very narrow limits. Any extensive and sudden increase or diminution of the solid dimension of the cranium must necessarily prove fatal; as the circulation of the blood must, in that case, cease before the contents of the cranium can be accommodated to the change; but if the enlargement or diminution of these dimensions be gradual and slow, they may take place to a very great extent, without the destruction of life.

The most frequent instances of the undue enlargement of the cranium occur in infancy. When explaining the structure of the cranium, it was observed, that it was constructed of several bones firmly united. In infancy, it frequently happens that the bones are only united by a membraneous substance, not yet in a state of ossification. This membrane, from various causes, is liable to rupture or distension; the bones are separated, and the dimension of the head consequently enlarged. To this imperfection of the *crania* of infants may, I think, be traced the great irregularities in the circulation, and the numerous diseases of the head, to which that age is peculiarly liable. If the shell of the cranium give way, and the dimensions of it become enlarged in the manner supposed, the same appearances will take place within the head as were stated to occur under a supposition of a diminution or waste of the substance of the brain. The proportions between the constituent portions of the contents of the cranium, are altered in the same way, though from a different cause. To give to the contents of the cranium that degree of pressure which is necessary for carrying on the circulation of the blood through them, in the manner that has been explained, water becomes accumulated in the ventricles. The blood-vessels of the encephalon at the same time are gorged, and the substance of the brain, though undiminished, becomes soft and flaccid, in consequence of the distension of the blood-vessels running through it. This is one of the species of that frequent disease, improperly termed hydrocephalus, or water in the head; for it evidently is not the water, but the enlarged capacity of the cranium, which is the cause of the malady. It is the collection of water in the ventricles which, in these circumstances, places the blood in the head in a state to be influenced by the powers employed for its circulation. The evident remedy for this disease, is to supply the imperfections of the cranium, and to reduce its capacity to its natural and healthy dimensions. Accordingly, Sir Gilbert Blane, physician to his late Majesty, having observed that the disease termed water in the head was always accompanied by a large bregma, or what is called an *open* in the top of the head, sagaciously concluded, that the disease in question was, in some degree at least, attributable to the want of complete ossification of the cranium, and boldly attempted to supply the deficiency by art. This he accomplished by the very ample means of compressing the head by a bandage. The effect of the treatment has been wonderful. It has not only been successful in this eminent physician's own hands; but we every day witness and hear of this disease, which two years ago was deemed incurable, completely removed by this simple, easy, and natural process. It may not be impertinent, perhaps, to remark, that several years ago, and sometime before the appearance of Sir Gilbert Blane's paper, I was led to the same treatment, in consequence of the views I had taken of the causes of the circulation of the blood in the head. At this time I was desired to visit a child two months old, a patient of Mr. Reay, surgeon, of this place. The disease with which the child was affected, was what is usually termed hydrocephalus externus, and had advanced so far as to leave, in the mind of that experienced practitioner and myself, no hopes of its recovery. The head was greatly enlarged, and increasing daily; a fluctuation was distinctly felt between the parietal and occipital bones; the child was affected with coma. The bringing together the too far separated bones of the head by gentle pressure, and retaining them in that situation by a bandage, was proposed; and the proposal was readily acceded to by Mr. Reay, who also applied the bandage. The effect of the treatment was surprising. After the first application of the bandage, which was daily renewed, the unpleasant symptoms began to disappear, and the child advanced, without interruption, to a state of perfect health, in which it at this day remains.

Fears may be entertained against this practice, lest injury be given by it to the tender structure of the brain But it must be remembered that the brain, at the time, is in a state of over-distention, and that the pressure is only calculated to restore it to the easy condition that is natural to it. That the brain, in early infancy, may be compressed without injury to life, we have sufficient proof, from the practice of various tribes of Indians, who compress the heads of their infants into a variety of forms, conformable to the false taste of beauty prevalent among them. It is never to be supposed, however, that this remedy is to be applied without the caution which an acquaintanee with the subject will not fail to supply.

There is still another state in which the ratio, between the brain and the capacity of the cranium may be deranged so as to give rise to disease. Though the ossification of the cranium be complete, its capacity continues to enlarge, and this it may be supposed to do in a quicker ratio than that in which the brain is augmented. If such a condition ever takes place, it would account for those cases of hydrocephalus which occur after the age of infancy, and after the ossification of the cranium has been completed. In such cases, the remedy proposed by Sir Gilbert Blane would evidently be inapplicable. When the cavity of the cranium is diminished by a thickening of the skull, or by any other means, the encephalon will be placed, in a situation, similar to that in which it is when the substance of the brain exists in too great a quantity, and therefore requires no further description.

The preceeding view, of the circulation of the blood in the head, was published originally in the "Edinburgh Medical and Surgical Journal," and is now printed without any alteration from that work. The following remarks may contribute to the further elucidation of the subject.

The contents of the cranium may be divided into four constituent parts. 1. The parenchymatous substance of the brain including the nerves. 2. The arteries and veins, to which may be added the membranes. 3. The ventricles of the brain. 4. The sinuses. On the sound state of these constituents, and on the proportion which they bear in respect of quantity to each other, will the healthly state of the head in a great measure depend. It has been stated that a frequent cause of apoplexy, is, an overgrown state of the substance of the brain; or an undue proportion of the parenchymatous part of the contents of the cranium. The manner in which this effect is supposed to be produced, has been already explained.

When the substance of the brain, on the other hand, is deficient, as will happen in cases of great emaciation, the space abandoned by this deficiency must be occupied by some of the other constituents of the encephalon, which will be increased in quantity for that purpose. The part of those constituents, which are capable of being augmented, are the blood vessels and the ventricles. One of these constiuents, or both of them, will necessarily be augmented when the substance of the brain is diminished. Hence an emaciated state, if I may so call it, of the brain, will be accompanied with an unusually large quantity of water in the ventricles, or an unusual turgidity of the vessels of the head; or, what, it is supposed, will generally be the case, both of these effects, will to a certain extent take place. A dilatation of the blood-vessels, when it takes place, will belong chiefly to the veins, as these vessels are more dilatable than the arteries; and as the action of the arteries tends to preserve to them a more equal calibre. The consequence will be an increased quantity of blood in the head; but the substance of the brain will not, in consequence of that, in the state supposed, sustain any undue compression. As the quantity of blood transmitted through the head in a given time, may be supposed to be the same; the whole quantity of blood, in this increase of it, will not be changed so frequently as when that quantity is less. A morbid condition will be the effect of this remora of blood in the head. Hence may arise that languor and want of activity, both corporeal and mental, which so often accompany an emaciated frame.

Is the water contained in the ventricles changed? Or does it continue the same? The pellucid and inoffensive state of the water in the ventricles, manifested upon opening the head immediately after death, and the sensible qualities of substances taken into the circulation, being in a short time discoverable in the water of the ventricles, would appear to prove that this fluid is constantly changing. It is reasonable to suppose, that this change would, as in the case of the blood, be completed more speedily when the quantity is small, than when it is in great abundance. A longer delay of this fluid in the ventricles would be accompanied with a change in its qualities, which it is reasonable to suppose might be deleterious. Morbid effects, would naturally accrue from such a change in the contents of the ventricles.

The water in the ventricles may be in undue quantity, while the parenchymatous part of the contents of the encephalon is not diminished. To admit of an increase of water in the ventricles in this case, the quantity of blood must be diminished. The space allowed for the occupation of the blood-vessels must be encroached upon. The same effects will arise from this state as were observed to be produced by an overgrowth of the substance of the brain. Apoplexy, coma, convulsions, and that disease called hydrocephalus internus, are the consequences of this erroneous distribution of the contents of the cranium. Some of the morbid effects of this condition, may be attributable to the deleterious quality of the contents of the ventricles, occasioned by the remora of the same fluid in them, for too long a space.

As there are no absorbents in the brain, the water in the ventricles must be renewed, by the absorbent property of the veins, The water in the ventricles, in this view of the case, constitute a part of the circulation of the head.

As the relative proportions between the constituents of the head are liable to change, the indispensable use of the ventricles of the brain is rendered evident. By the increase or diminution of the water of the ventricles inversely as the other constituents may increase or diminish, the relative position of all the parts of the head is preserved under varying circumstances, and every part of the contents, however slender, protected from unequal straining and rupture. The water of the ventricles varying in quantity as the other contents of the head vary, is as necessary for the protection of the easily lacerated substances of the encephalon, as the cranium itself. Without the contrivance of the ventricles, the blood could not be circulated in the head, if the parenchymatous part of the constituents of the encephalon, were at all subject to increase or diminution. Nature has so placed and shaped the ventricles of the brain, that by their expansion or diminution inversely as the other parts of the head expand or diminish, the relative position of all the parts is preserved.

Considering the bulk of the mass of which the brain is composed and its consistence, it appears surprising that the blood could be circulated through it by any means whatever. This indeed would be impossible were the brain placed in any other condition than that in which it is. No power could circulate the blood through it, were it not placed in the cranium. The strongest vessels belonging to the head would be ruptured by a far weaker power than that which would be required to propel the blood through it in any other situation. But as it is, the brain is uniformly held in a fixed state of expansion; and, of course, every vessel which passes through it, or which runs upon its surface, or which is placed any where within the cranium, has a fixed space allotted to it, which as long as the cranium is sound can never be increased by any propelling force, nor, without a change in some other constituent, incroached upon by the surrounding matter. The vessels of the head are in the situation of rigid tubes. No power is expended in dilating the vessels of the head. The only obstacles which the heart has to overcome, in transmitting blood through the head, are friction and gravity. The resistance derived from the first cause must at all times be considerable; and when gravity is added to it, more in many cases than the heart can overcome. In dyspepsia in that state of exhaustion that follows the too free indulgence in wine, in a hemorrhage, alvine fluxes, and in the sudden change from the recumbent to the erect position, particularly during the influence of the preceding diseases; giddiness of the head, faintings, stammering in the speech, and tottering walk take place. These affections are often taken as indications of too great a fulness of the head, and bleeding is improperly had recourse to, which only aggravates the disease it is intended to remove. The patient often faints, and in that situation the obstacle of gravity which, in the erect posture, the heart had to overcome, as well as friction, is removed ; and, so by the renewal of the circulation through the head, the fainting ceases, which otherwise might end in death. Affections of the mind would seem to have the effect of retarding the passage of the blood through the head, or of weakening the power of the heart. The sense of shame is accompanied with a flushing of the face. This appearance has been improperly attributed to the action of the capillaries of the face, and held as a proof of their influence in circulating the blood. But the true cause is the difficulty which the blood meets with in passing through the encephalon ; the usual quantity is not transmitted through it ; of course an increased quantity takes the external course and distends the vessels of the face and neck. To the same cause may be attributed the flushings which occasionaly assail the cheeks in the last stages of typhus fever, in hydrocephalus internus, &c. These flushings are, in the diseases that have been stated, the frequent concomitants of convulsions and other distressing affections of the head. They prove that the powers assigned for the circulation of the blood through the head have become unequal to the task. These flusings are often considered as a proof of too vehement a circulation of the blood, and of a determination of it to the head ; and bleeding is had recourse to, which increases the malady. It frequently happens that patients in typhus fever are lost by wine and other stimulants being withheld, in consequence of the flushing of the face being taken as a proof of too great a force of the circulation of the head, which it is supposed wine would augment.

That species of inflammation which is characterised in other parts of the body by redness and tumor and which ends in resolution, suppuration, or gangrene, can scarcely be admitted to take place in the brain. The inflammation to which the encephalon is subject, would appear to arise from the increased rapidity of a stream of blood, the stimulant qualities of which are augmented, at the same time that the sensibilty of the brain is increased. In such a case the most excruciating pain and outragious delirium would be the cousequence, which could not continue long without exhausting the irritability and ending in death. The evident means of subduing this disease, is to diminish the force and rapidity of the circulation by profuse bleeding, and by such regimen and medicines, as are calculated to diminish the stimulant qualities of the blood and the morbid sensibility of the system.

Though in my opinion, there can exist no general accumulation of blood in the head, or no increased determination of it to that quarter, by which I suppose is meant, a greater fulness of all the vessels of the head, it is quite possible that the distribution of the fluids in the head may, from certain unknown causes be unequal. There may be a morbid accumulation of blood in one part of the encephalon, which must be made at the expence of an equal quantity in some other part. Hence may arise a long train of painful, incapacitating, and incurable diseases.

Diseased changes may take place in the substance of the brain itself, and be altogether undiscoverable on dissection. When such changes do take place, we are equally ignorant of their causes, manifestations, and consequences.

I have stated that the sinuses form a fourth division of the contents of the cranium. As these vessels are necessarily at all times of the same capacity, and always contain the same quantity of blood, no deviations from the healthy state in other parts of the brain, can effect them, excepting so far as the velocity of the current they inclose is influenced. The coats of the sinuses are, like every other part of the body, subject to disease. Obstructions may occur in the sinuses themselves, which would necessarily have a great influence over the circulation of the blood in the head. Two young men each about 22 years of age died within a few months of each other, in Liverpool, with symptoms of water on the brain. They were examined after death. No disease was discoverable in the substance of the brain, in the vascular structure, or in the membranes, or in the ventricles. The sinuses were carefully examined. In the longitudinal sinus of each, a white febrine substance was found filling the sinus like a plug No blood was found in these sinuses, nor was any red colour observed in them. The other sinuses contained blood. It may be supposed that this was a coagulum formed after death, and that the blood, as far as it was fluid, had in consequence of position left the sinuses after the cranium was opened. This appearance I found in no other instance, though I have omitted no opportunity of searching for it since. If these obstructions existed before death, they are sufficient to account for the severe disease that terminated fatally. Sufficient attention has not hitherto been devoted to the state of the sinuses in *post mortem* examinations.

The sinues are passive receptacles of blood. As has been stated, a quantity of blood flows from them into the jugular veins at the instant that the same quantity has been thrown into the cavity of the cranium by the carotids. The bloood must enter the jugulars in jets corresponding with the pulsations of the carotids. The jugular veins indicate this mode of the blood's passing through them, by pulsations after the manner of the arteries.

When Dr. Wilson Philip tied the jugular vein of a rabbit in an experiment, which has been already noticed, the part of the vein between the ligature and the heart became gorged with blood. The Doctor considered this as a proof of the action of the vessels in circulating the blood through them. It only proves, that as blood was thrown into the cavity of the head by the arteries, it was received by the jugulars, or veins of the neck.

A very remarkable peculiarity in the circulation of the blood, occurs, in its passage through the liver. The causes and purposes of this peculiarity do not appear to have been satisfactorily explained. The liver is generally regarded to be a gland, and in that case must be deemed the largest in the body. It would appear to be, in this, different from most of the other glands that belong to the system ; that it is placed upon the sanguiferous vessels, while the others belong to the lymphatic system. The liver may, therefore, be termed a venous gland. The dimensions, position, and form of the liver, are well known. Its structure is porous or cellular. The cells are not easily compressed, and it maintains the same bulk, when removed out of the body, that it possessed in the living system.

One acknowledged office discharged by the liver, is, the secretion of bile ; but that cannot be supposed to be the only or chief use of the liver, for its size is quite disproportionate to such an office. Besides, the secretion of bile is not found in all animals that are supplied with a liver. The horse has a liver, but is without a gall bladder, or secretion of bile.

The blood which is returned to the heart from the middle and lower parts of the body, is collected before it reaches the heart, into two large channels. That which is supplied by the lower extremities, or what may be termed the solid parts of the lower half of the body, is collected from numerous branches into the vena cava abdominalis, and is conveyed directly to the heart. The blood which is returned from the abdominal viscera, from the stomach, namely, the alimentary canal, spleen, mesentery, &c., is conveyed by numerous vessels which unite into larger ones as they proceed, and at last form one trunk called the vena portarum. The blood, in-instead of flowing in an undivided stream after the ve-nous form to the heart, is distributed first into every the most minute part of the substance of the liver. For this distribution it is necessary that the vein should ramify after the arterial manner. Before it penetrates the liver, it divides into several smaller trunks, which taking different directions, become infinitely ramified, till the whole of the substance of the organ is supplied by them. It has been supposed by some physiologists, that the vena portarum, upon entering the liver takes on the structure, and performs the functions, of an artery. But this does not appear to be the case. The divergent form of the vena portarum, does not arise from the same cause which produces that form in arteries. When a fluid is discharged from any point, by a force impelled upon it at that point, it has a tendency to diverge; and if it is contained in tubes, it tends to occasion a ramification in these tubes. The arterial ramification may therefore be in some degree considered as the natural effect of the impulse given to the blood by the heart. Less force will be required to transmit the blood in vessels of the arterial form, as the fluid is conveyed in a way that is more conformable to the direction which projected fluids naturally take.

The liver is composed of small cells or receptacles, containing blood, and of vessels either for the supply or discharge of these receptacles. It is a reservoir of fixed dimensions, containing an infinite number of smaller reservoirs, the dimensions of which must also be fixed.

Each of the smallest of these reservoirs is supplied from a branch of the vena portarum, and from each of these reservoirs a discharging branch proceeds. These discharging branches being emptied by a power which is centripetal, the suction of the heart, naturally converge into larger streams, which uniting, form the vena cava hepatica.

No blood can enter the liver from the vena portarum until an equal quantity shall have been taken from it by the vena cava hepatica. And, *vice versa*, no blood can be taken from the liver by the vena cava hepatica, until an equal quantity shall have been admitted into it from the vena portarum. The demands of the vena cava hepatica are regulated by those of the heart.

When any part of the body increases from growth, the first rudiment of that increase is supplied by an artery. When the liver increases in the same manner, that increase is the product of a branch of the hepatic artery, but when one of the infinitely minute cells or folicles of which the liver is formed, is made, the cell obtains a supply of blood from the vena portarum. Blood is sucked by the cell from the nearest receptacle of the vena portarum, and thus an extension or new branch of that vessel is produced. The ramification of the arterial system, is the effect of an agency in which the artery performs an active part; the ramification of the vena portarum is the result of an agency, in which that vein is passive.

Of the purposes which may be served by the liver, several seem to present themselves. In the first place, it may serve as a reservoir to supply the demands of the heart. Its proximity to the heart would appear to fit it well for this office. In some animals the liver is much larger than in others, compared with the bulk of the body. In birds, this is the case in a remarkable degree. In this animal, its form also is different. It lies as much in the left side as in the right. It is contiguous to the heart. This animal has no diaphragm, the use of this organ being supplied by the breast bone. The heart is lodged in a cavity formed by a depression of the liver below, and by a corresponding depression in the breast bone above. The dimensions of this cavity are invariable. Hence it happens, that when the ventricles of the heart contract, the auricles necessarily become to the same extent expanded, to fill the space left by the diminished ventricles. When again the ventricles dilate, in consequence of the expansion which necessarily, as has been explained, accompanies dilatation, the blood flows into the ventricles from the auricles, in consequence of the diminished resistance it meets with in that direction, and also of the impulse of the contracting auricles. It is in consequence of this position of the hearts of birds, that elastic lungs are not required to produce the expansion of the heart. As the hearts of birds may not on account of the want of elastic lungs have so strong a suction power as the hearts of the mammalia, who are provided with such lungs, a larger reservoir is provided in the liver. It is chiefly from this fountain that the heart of birds have to draw their supplies. The heart of a bird may almost be supposed from its position, to be an appendage of the liver. The liver from this view must in all animals, but in particular in birds, afford great aid to the heart. It may be asked, by what power is the liver supplied with blood? I answer, by gravity. In the same manner that any porous and incompressible substance deposited in the earth is filled with water. The liver is a well from which the heart pumps a great part of its supplies.

Another purpose which the passing of the blood through the vena portarum may serve, may be the retardation of the blood returning from the abdominal viscera. As the part of the circulation occupied by the abdominal viscera is shorter than that which includes the lower extremities, the blood which supplies the viscera would be returned to the heart more frequently than that which is conveyed in the abdominal cava from the extremities. To give an equal return to the blood in each part, a more circuitous course would seem to be required by this circulation, without which it would be shorter and more frequently performed.

It is probable, that the blood, during its delay in the liver, may undergo some changes, which may fit it for discharging some important purpose in the animal economy. A part of it may be rendered more ready to assume the gaseous form, from a slight diminution of pressure, and by that means serve to regulate the heat of the system, in a way that will be afterwards explained.

It is evident, from all that has been stated, that the liver must maintain an extensive influence over the living system; that any derangement or imperfection in its functions must be attended with the most important effects; and that if the fabric and functions of the liver are impaired, the health of the system cannot long subsist.

In the Thesis, which I published at Edinburgh, in year 1799, and of which a reprint will be found at the end of this work, the causes to which the expansion of the human heart were attributed, are the same as those which are now assigned to the expansion of the heart of birds. It was contended that the human heart had certain dimensions assigned to it; that it at all times necessarily filled those dimensions; and that when one part of the heart was diminished by contraction, another part was enlarged to a proportional extent. The causes which I stated for this condition of the heart, did not at the time appear very satisfactory, but as I was satisfied that some power existed for dilating the heart, different from that usually asigned for that purpose, the force of the blood in the veins, I adopted them as the best I could find. I had not then, nor for many years after, any notion of the elasticity of the lungs, nor of the purpose to which that elasticity was applicable. Neither did I conceive that the heart by simple relaxation of its fibres, became dilated in consequence of its spherical form, and of the circular direction of its musculo tendinous fibres. The reasoning used in the thesis, though not applicable to the human heart in a healthy state, is nevertheless true with respect to it in some states of disease. When the elasticity of the lungs is impaired or destroyed, as in the case of their becoming a solid inelastic mass from scrofula, adhesions take place between the pleura pulmonalis and pleura costalis; and between the pleura pulmonalis and the mediastinum and diaphragm, walling in a space of fixed dimensions for the heart. The human heart is then in the same condition with that of a bird, and the expansion or contraction are occasioned after the same manner. Life is in this way continued, in consequence of the wonderful power, that the living system possesses of adapting its organs to the changes produced by disease. We ought therefore to be very cautious in adopting any explanation of the phenomena of life, which may be suggested by appearances supplied by disease.

We shall now attempt to deduce some of the most important phenomena exhibited by the animal frame, from the principles which are supposed to have been established in the preceeding Inquiry.

A remarkable phenomenon, connected with the circulation of the blood, is exhibited in the vacuity of the arteries after death.

The Harveian Doctrine of the circulation of the blood, may, I think, be divided in two parts. The first is the course of the blood; the second the explanation of the causes by which it is moved in that course. The arguments advanced by Dr. Harvey on the first of these points, the course of the blood, must, I think, couvince every candid inquirer that the blood is conveyed from the heart by the arteries, and returned to it again by the veins. But the illustrious discoverer has not been so fortunate in the second part of his great undertaking. In maintaining that the projectile power of the ventricles of the heart propels the blood through the whole of the arterial and venous canals, and, after having discharged this office, opens the auricular chambers by means of the returned blood, he lays claim to effects which are not warranted from the supposed causes, and which are inconsistent with the established laws of hydrostatics; laws by which the blood, as well as every other fluid, must be governed. But this part of the doctrine of Harvey, admitting it to be philosophically correct, must be rejected in its present application as affording no satisfactory solution of the phenomena. The followers of Harvey, adopting as a foundation for their argument his doctrine of a *vis a tergo*, have enlisted the arteries into the aid of the heart, and contended that the blood is circulated by the combined agency of these powers. But the difficulty is not removed by the supposition; it is only placed a little further on in the system, and the phenomena are not better explained.

It has often created surprise that a doctrine so simple in appearance as the circulation of blood, and pointed out so plainly as we now suppose by facts of daily occurrence, should have been reserved for the discovery of modern times. The knowledge of the circulation seems to have been retarded by one remarkable phenomenon. The arteries which are now known to constitute the channel of the blood for one half of its course, were uniformly found to be devoid of that fluid after death. That vessels in which no blood was to be found by the most careful examination after death, should be the constant receptacles of it during life, would be a supposition that would scarcely suggest itself to the anatomist, and if suggested would soon be rejected from the list of probable conjectures. The arteries were supposed to be the recipients of a vital aerial fluid. One fact of frequent occurrence seemed to be at variance with this belief; and, if the effects of it had not been defeated by an hypothesis, it must, we would suppose, have led to the truth. An artery, when wounded, was constantly observed to discharge blood from the living frame. But the ancient physiologists, unwilling perhaps to degrade the arteries from what they conceived to be their more refined office, and conceiving it impossible, that if these vessels contained blood during life, that they could be deprived of it by death, contended that the discharge of blood from a wounded artery was no proof that that vessel contained any blood before it was wounded; but that the pain and irritation given by the wound drew blood from other quarters into a vessel which contained none before; and that the impetuosity and obstinacy of the discharge arose from the conflict between this foreign intruder and the native aerial spirit. What gave greater plausibility to this supposition was, that the blood shed by the arteries being of a different colour from that discharged from the veins, seemed to be not the natural product of the body, but the factitious result of this imagined conflict.

The condition of the arteries after death was urged confidently by the opponents of Harvey as an insuperable objection to the doctrine of the circulation, upon its first promulgation; and was unquestionably one of the greatest obstacles found in the path of the discoverer. And, after all, the explanation given by this celebrated man of the powers by the operation of which the arteries are found to be empty after death, is most unsatisfactory. He says that the left ventricle, in the last struggles of life, continues to propel, after it has ceased to receive, blood; and that, by these final propulsions, the blood at the time in the arteries is chased into the veins; and he further asserts, in support of this explanation, that the arteries of animals which have been killed by submersion in cold water or by mephytic air, will be found to contain blood after death as well as the veins. But there can be nothing more evident, than that the heart, by these abortive impulses, could only drive blood through the more remote portions of the arterial system by some impinged medium; and that this medium, which, upon the hypothesis of Dr. Harvey, could only be blood, must still remain in the arteries. But defective as this explanation is, it has the further imperfection of being built upon an hypothesis that is altogether destitute of proof. This hypothesis is, that the heart continues to propel after it has ceased to receive blood. The heart, on the contrary, is generally found full of blood after death. The converse, therefore, of what is maintained by Dr. Harvey, would appear to be the truth, that the heart retained the capacity of receiving blood, after it had lost the power of discharging it. The statement also by which Dr. Harvey has supported his opinion, has not been confirmed by observation.

Dr. Harvey has endeavoured to support his opinion, by instances of sudden death. But in almost all kinds of death, however sudden, the arteries are found to be equally empty of blood with the arteries of animals cut off by lingering disease. The arteries of animals killed by drowning, are found to be full of blood. But this effect is not occasioned by the suddenness of the death, by which the power of the heart to empty the arteries by the last contractions, was supposed by Dr. Harvey, to be destroyed; but to other causes which will be stated in the sequel.—In deaths far more sudden than that occasioned by drowning, the arteries are found to be empty after death.

No new light has been thrown upon this subject by the followers of Harvey; and in general it may be observed that after the lapse of two centuries, and though a thousand volumes have been written, and thousands of animals slaughtered, to elucidate the subject, the doctrine of the circulation has descended to our times nearly in the same state in which it came from the hands of the great discoverer.

The complete failure of every attempt to explain this phenomenon, as well as many others connected with the circulation, has driven the followers of Harvey to the unphilosophical conclusion, that the motion of the blood is exempted from those laws, to which the hand of the Creator has subjected the motion of all other material substances. This inauspicious opinion originated, I believe, with the learned and ingenious and pious Dr. Hales, and was suggested to him by the result of his valuable experiments to ascertain the relative forces of the blood in the arterial and venous canals. Recommended by the great authority of its origin, and affording in many instances a pleasing apology for ignorance, it soon received numerous adherents, and has at length become so general that, since the days of Mr. Hunter, the language of the schools on this subject may be said to be, that it is in vain to apply the laws of hydrostatics to the motion of the blood, a living fluid flowing in living tubes. Physical investigation as applied to this subject, is termed by Sir Gilbert Blane, the vain parade of a science foreign to medicine. Mr. Lawrence who in other respects may be allowed the character of "*nullius in verba jurari magistri*," has in this instance joined in the general cry, and as usual brought forward in confirmation of the opinion the noted discrepancy between the estimates of the power of the heart made by the celebrated mathematicians, Borelli and Keil. Mr. Charles Bell in a recent publication which he has termed a Treatise on the Circulation of the Blood, but for what reason he has given it that title, I confess myself unable to discover, says, that the only use of mechanical philosophy to the student of medicine, is to convince him upon his entrance into the dissection-room, where alone, according to this Gentleman, nature appears in all her beauty, that he has new principles and new laws to learn. But no two physiologists agree in their description of those new laws; and, in this state of revolution and of anarchy, each enjoys the happy privilege of forming a code agreeably to his own fancy.

Mr. George Ker, an ingenious and learned surgeon, in Aberdeen, has taken a different course. He cuts the knot which he is unable to untie. Perceiving distinctly the defective and inapplicable explication of this phenomenon, the vacuity of the arteries after death, and of many others which he has stated, and biassed no doubt by an avowed persuasion in the infallibility of the genius of antiquity, in all its applications, has, in a recent publication boldly denied the doctrine of the circulation altogether, and with great ingenuity, earnestness and confidence, defended the opinions of the ancient physiologists respecting the location of the blood in the living system, and the uses of the arterial tubes. This author further maintains, that not only has physiology or medical science, taken a retrograde course since the days of Dr. Harvey, but that the healing art, which, if the doctrine in question had been true, must have been notoriously advanced by it, cannot boast of having received any great and conspicuous improvement from that source, but in many instances has been involved in greater obscurity, and rendered less certain and efficient by the long predominancy of a false doctrine.

The objections urged by Mr. Ker against the circulation had been stated by myself as objections, not against the doctrine of the circulation itself, which I believe to be founded on a basis never to be shaken, but in refutation of the causes assigned for the accomplishment of that effect, at least two years before the appearance of Mr. Ker's work, but unquestionably without the knowledge of that gentleman.

The causes which, in that Treatise, I have ventured to assign to the motion of the blood, will, in process of time, I trust, be found to have a real existence in nature; to afford a plain and satisfactory explication of the various phenomena, to answer fully all Mr. Ker's objections, and, in a word, to vindicate a theory, which does so much honour to our country, from all future opposition, and from the more baneful support which it has lately received from many of its abettors.

The objection principally dwelt upon by Mr. Ker, the emptiness of the arteries after death, did not pass unnoticed, as may be seen on reference to the Inquiry into the causes of the motion of the blood, but as I had not then had an opportunity of submitting my opinions on that point to the test of experiment, I did not state them with that confidence which I even then felt in their truth. I have lately had that opportunity, and I now propose to state the result, premising the process of reasoning by which I was guided in that experiment.

The chief, if not the whole, of the movements of the animal machine seem to be the effect of two powers acting either conjunctly or separately, These are elasticity and irritability. The elasticity of the parts which possess this property is inherent in the structure, and is independant of life. Irritability, which is the property of the muscular substance, is the concomitant of life, and ceases with it. The movements, which are the usual result of a combination of those powers, will not wholly cease at death. The elasticity will still continue to operate ; and the result will be different, either from that which would be produced by their combined agency, or from that which would arise from their synchronous destruction.

The motion of the blood seems to be the result of the contractions arising from the irritability of the heart and arteries, and of the resilience arising from the elasticity of the arteries and of the lungs. One class only of these powers is destroyed by death. The resilience of the lungs and of the coats of the arteries possess then an uncontrouled operation.

The resilience of the lungs removes a part of the pressure of the atmosphere from the internal surface of the chest, and perhaps from the external surface of the vessels by which they are penetrated. To restore to the parts within the chest an equality of pressure with that of the substances without it, the adjoining liquid and less fixed parts of the body will be pressed through every channel that offers into the chest. What is called a vacuum will in effect be made in the chest by the elasticity of the lungs. There will therefore be a draining from all parts of the body towards the chest, to fill up this vacuum. As thus the causes which return the blood to the heart continue to operate, after the heart, the great engine by which it is discharged, has terminated its labour, a greater quantity of blood will be necessarily collected in the neighbourhood of the heart after death than existed there before that event.

Various circumstances may intervene to fix the channels in which the blood will flow in its course towards the heart after death. The arteries are powerfully elastic, and when their coats are relieved from the distending force of the heart, become of a diminished caliber. Valves stationed at the roots of the arteries prevent the return of blood from these vessels into the chest. After the small part of the aortic system intervening between the heart and the confines of the chest shall have been, as it usually is found to be, filled with blood, the blood in the rest of this system will sustain no diminution of pressure on the side of the heart.

No obstacle exists in the way of the blood in its course to the chest through the veins. No valves are stationed at the roots of these vessels, and the blood finds an unobstructed course from the roots of the cava into the auricle, from that possibly into the right ventricle and into the pulmonary arteries and thence into the pulmonary veins. The heart, particularly the auricles, and the large venous trunks, the coats of which being inelastic and easily dilatable, being all placed within what may be called the vacuum of the chest, will be distended to their utmost capacity. The additional blood requisite for this purpose can only be drawn from the veins. The place of the blood taken from one part of the venous system will be supplied by that from another.

The termination of this process will be the emptying of the arteries into the veins.

If the preceeding argument be correct and founded upon true principles, it would follow that, were the elastic powers employed in the motion of the blood disengaged before the muscular powers has ceased to act, or synchronously with that event, a distribution of the blood would be found to exist after death, different from that which is now usually observed. The blood would not be found so extraordinarily accumulated in the right auricle, and in all the veins belonging to the system of the cava within the chest, and at the approaches to it, and the arteries and capillary vessels would contain the proportion which, upon the supposition of the Harveian theory, must have flowed in them before death. I have not been able to devise any method of annihilating before death the elastic influence of the arteries, and therefore some allowance must be made in the phenomena which are to be brought into view for that cause; but I have been successful in removing from all influence after death the elastic power of the lungs, by far the most efficient, by the manner in which the animals were killed in the following experiments. Death was in these cases effected by inducing a previous collapse of the lungs, which was done by making openings into the chest of the living animal, and exposing the external surface of its lungs to the free access of the air.

In the first experiments made with this intention, an opening was made of about an inch in length between a pair of the ribs on each side. I expected that sudden death would be the effect of these openings; but in this respect I was disappointed, and at first not a little perplexed. This disappointment I experienced particularly in the case of a large dog. This animal, as I supposed, after the collapse of the lungs, by pressing up the diaphragm by means of the abdominal muscles, and by depressing the ribs as far as possible, by the same means, and then by a rapid and forcible contraction of the intercostal muscles, accompanied by an equally rapid and forcible contraction of the diaphragm, was enabled to rarify the air contained between the external surface of the lungs and the chest, to such a degree as to occasion a partial dilatation of the lungs, and an imperfect expansion of the heart. Thus life was painfully prolonged for nearly twenty minutes. The sufferings sustained by this animal for so long a period, prevented a repetition of the experiment in the same manner on any other animal. The result was in other respects satisfactory. Though the death was tedious, it was ultimately produced by the collapse of the lungs. I had previously performed the same experiment upon a rabbit and a cat. In these the death, though not sudden, was neither so tedious nor to appearance so distressing as in the case of the dog.

The same appearances were, on dissection, exhibited by all. The muscles were remarkably red; and, when an incision was made into them, they poured out blood. The membranous parts exhibited the blood vessels as if they had been nicely and fully injected; forming anastomoses which appeared like a netting made of red threads. I was particularly struck with the coats of the intestines. Instead of exhibiting the usual pale smooth surface without the vestige almost of a single blood vessel, they appeared to be composed of a red coloured netting, the meshes of which varied greatly both in dimension and in form. The liver was like red morocco. The flesh of the rabbit, which is usually white, was in this case of a reddish color, and all the dissected parts became wet with effused blood. The heart contained little blood. When the chest was opened, and the large vessels it contained were divided, a small quantity of blood only was effused, not much more indeed than from the other parts of the body. The aorta and large arteries, in all the instances, were pale externally, while the accompanying veins were of a blue color. A part of the descending aorta, above the bifurcation of the iliacs, after its extremities had been secured by ligatures, was cut out, and was found to contain a small cylinder of blood generally coagulated. So it appears that the white color of the arteries did not arise from their being devoid of blood, but from the opacity of their coats. With respect to the vessels, which the stomach, the intestines, and the membraneous parts exhibited in so beautiful a manner, I do not pretend to say what part of them may have been veins, and what part may have been arteries. Supposing, however, that these carcases exhibited the distribution of the blood as it really existed in life, it is very evident that the blood, not only of the larger arteries, but of the smaller vessels, whether they be arteries or veins, must, in consequence of death produced in the usual way, be emptied into the large veins. I think it probable, however, that what are called the carpillary vessels, may, in consequence of this mode of killing the animal, be found to contain more blood than the share that belonged to them during its life; for the elasticity of the coats of the arteries, the effect of which, as I before stated, I had not been able to devise any plan of counteracting, by contracting the bore of these vessels, would propel a part of the blood that was flowing in them at the moment of death into the vessels, the coats of which were inelastic and dilatable.

For the purpose of comparing the appearances of two animals of the same kind, killed in different ways, two rabbits were killed on the 20th of September, 1819; one of them by causing the lungs to collapse before death, the other after a different manner. In the case of the first of these rabbits, the belly was opened freely from the scrobiculus cordis nearly to the pelvis, and the lower surface of the diaphragm exposed to view. An opening, fit to admit my two fingers, was made through the museular part of the diaphragm on each side. The sound of air rushing through the orifices announced the collapse of the lungs. As the amimal possessed no power of contracting the openings made in the diaphragm, as its struggles would probably tend to render them still wider; and as therefore the capacity of dilating the lungs, even to the smallest degree no longer remained, the animal instantly died. The appearances exhibited by the dissection of this rabbit, were precisely similar to those which have already been described of the bodies of animals killed by the previous collapse of the lungs. The vessels, particularly of the intestines, stomach and mesentery, were very distinct and full of blood, forming frequent anastomoses with each other in the way already described. The flesh was reddish, and, when cut into, bled. The heart and vessels about it contained only a moderate quantity of blood; for scarcely any blood was found to have effused, after the division of those vessels into the shell of the chest. The other rabbit was killed by thrusting a sharp instrument between the vertebræ of the neck. It died instantly and was immediately opened. Scarcely was the vestige of a blood vessel to be observed on the surface of the intestines or stomach, which had a pale appearance, excepting where they were tinged by the colour of their contents. The membranes scarcely exhibited any traces of vascularity. The flesh was white, and when cut into appeared to be dry, discharging at some parts a drop or two of blood. The liver was of a dusky brown. The trunks of the veins were swollen and rounded, whereas in the other rabbit they appeared flat, and to contain a thin layer of blood. A considerable quantity of blood was found in the shell of the chest, after it had been opened, and the large vessels it contains divided.

A few days after, a sheep was killed in the same manner as the first of the above-mentioned rabbits. When the openings were made through the diaphragm, the sound of air rushing into the chest, and a hollow groan from the throat announced still more plainly the fatal collapse of the lungs, and the last expiration. The animal, after making a few heavings with the chest, became lifeless. Several other sheep had been killed at the same place at that time, and there was an opportunity of comparing the carcase of this animal with those of the others. Scarcely any traces of the smaller vessels were observable in the stomach, intestines, peritoneum or mesentery of the other sheep, while in the same parts of this animal they appeared in great abundance, and as injected with red wax. The appearance was so remarkable as to strike the butchers, and other persons whose attention I directed to it, with surprise. The colour of the fat was browner than usual. The muscles of this animal being at all times red, did not exhibit so marked a difference as in the case of the rabbits, but when cut into, they discharged blood. The larger arteries, where I had an opportunity of seeing them, contained a small cylinder of coagulated blood. The flat and tape-like appearance of the large veins, which I had observed in the rabbit killed in the same manner, was in this case very remarkable.

The result of these experiments I think fully warrants the conclusion, that the difference of the distribution of the blood after death from that in which, according to the Harveian theory, it must exist in the living system, arises chiefly from the elastic power of the lungs; and that the emptiness of the arteries and of the smaller vessels observed after death, admits of a satisfactory explanation from the supposed operation of this cause, combined with that of the elasticity of the arterial canals.

In concluding this subject, I am anxious to express my hope that some benefit may result to anatomy from the examination of animals killed by the collapsion of the lungs; in particular a better chance seems to be promised by it of tracing the vascular system to its various terminations. According to the Harveian doctrine, the blood must flow from the minute and ultimate branches of the arteries into the corresponding branches of the veins. But the manner in which the vessels form the communication necessary for this purpose, is still a desideratum in medical science ; and, as in all ordinary modes of death, these vessels are always deprived of their contents ; and, as in these circumstance, the knife of the anatomist and the microscope, though guided by all the colourings which the art of injecting could supply, have been found incapable of bringing this union into view, it was likely to remain so. But the examination of animals in which the smallest vessels contain at least their full proportion of the red blood that flowed in them in the living body, seems to hold forth the hope of some satisfactory knowledge being attainable in this dark and mysterious part of physiology. And it is evident that, without a knowledge of the manner in which venous and arterial capillaries communicate, and of the powers by which the blood is moved through these capillaries, the momentous question, What is the cause of Inflammation, and, of course, of the majority of the diseases affecting the animal frame? must now be involved in darkness. On this subject, a foundation of adamant has indeed been laid by the immortal Harvey, and some additions of the same imperishable materials may have, from time to time, been made to it by others; but an edifice that shall be worthy of the indestructible basis still remains to be raised.

The animal frame is supplied with another system of vessels besides the sanguiferous, connected with the latter, and containing a fluid, the motion of which is influenced partly by the same powers by which the blood is circulated. This is the lymphatic system. The extreme terminations of this system open upon surfaces from which they imbibe the peculiar fluids which they convey, and uniting as they proceed towards their other termination, after the manner of the veins, form one trunk, which discharges its contents into the subclavian vein. The vessels of which this system is composed, though during life, constantly transmitting a fluid, are always found, like the arteries, empty after death. When Aselli, to whom the honour of discovering the lacteals is due, observed, to his great astonishment and joy, vessels containing a milky fluid running on the mesentery of an amimal, the abdominal cavity of which while living, he was examining for a different purpose, he killed the animal instantly to obtain, as he supposed, a better opportunity of examining the vessels, but no sooner was the animal deprived of life, then the milky streaks disappeared, and the most careful search for them proved unavailing. The causes which produce the emptiness of the lymphatics after death, are similar to those to which the emptiness of the arteries had been ascribed. The suction-influence, derived from the expansion of the heart, and the elasticity of the lungs, extends to the lymphatics, which are a ramification of the veins, and aided by the elasticity of the coats of the lymphatic vessels themselves, carries on the process of emptying, after that of supplying has been withheld. It is evident that the cause assigned by Dr. Harvey for the emptiness of the arteries after death, the projectile power of the heart, could have no influence on the movement of the chyle or lymph.

It is worthy of remark, that the same canse which retarded the discovery of the circulation of the blood, retarded the discovery of the lacteals and lymphatic system generally. The emptiness of the arteries after death made it appear impossible, that these vessels could be the channels of blood during life; but, without that supposition, the circulation of the blood was inconceivable. The emptiness of the lymphatics and lacteals produced by death, combined with the pellucid structnre of these vessels, withdrew them from the observation of the anatomist. It is remarkable also, that the discovery of the cause of these effects, has been long subsequent to the discovery of the operations which they concealed.

It was stated by Harvey, that the arteries of animals, killed by immersion in cold water and in mephytic air, retain blood after death. I have observed that the cause why, in animals, that are killed by drowning, the arteries continued to be full of blood after death, would be afterwards explained. This appears to be a suitable opportunity for that purpose.

When an animal is deprived of life by immersion in water, it has generally been supposed that none of the fluid enters the trachea. This opinion has been formed in consequence of no water having been found, after death produced by drowning, in the windpipe, or any of its branches; but it is erroneous. The water enters freely into the windpipe, and is imbibed, not only by the bronchia, but passes often from them into the pulmonary veins, and proceeds even to the left auricle of the heart. The lungs of animals which have been deprived of life by drowning, do not collapse after the chest is opened, in consequence of the water with which they are distended not being an elastic fluid, and in consequence of its greater gravity. The lungs therefore are in this case deprived of the suction-influence which belong to them after death and before the chest has been opened, when the air vessels are filled with air only. The cause by which the arteries are emptied after death is intercepted; and the arteries of animals, which have been drowned, are placed in the same situation with those of animals which have been killed by the collapsing of the lungs; they remain full of blood after death. If water, in the process of drowning, enters freely into the lungs, how happens it, it may be asked, that no water is found in the windpipe of an animal that has been drowned? While the animal is still in the water, the windpipe is filled with that fluid; but, when the body is taken out of the water, it is relieved from a share of the pressure which it bore in the water. The pressure from which it is relieved is equal to the difference between the weight of a column of water, whose height is equal to the depth to which the body was immersed, and of a column of air of the same height. The body sustains less pressure when taken out of the water than it did when immersed in it. The elasticity of the ribs will have a greater reaction, and the chest will be more expanded, and the lungs will be capable of imbibing a greater quantity of matter which can only be supplied by the windpipe and its branches. Hence arises the absence of the fluid, in which an animal has been drowned, from the windpipe and its branches, upon an examination after death.

Dr. Harvey asserts, that the same appearance is made by the arteries of animals killed by immersion in mephytic air; by which I suppose he means carbonic acid gas. As this gas is heavier than common air, the collapse of the lungs would be, to a certain extent, prevented after the chest was opened, and the collapsing effort lessened after death, before the chest was opened. The suction of the lungs would be less powerful, and the accumulation of blood in the chest, would not take place to the same extent after death. The causes therefore by which the arteries of animals are emptied after death, would not, in this case be so strong, allowing some blood still to remain in these veesels. I have not had an opportunity of ascertaining this by experiment.

The lacteals and lymphatics of animals killed by drowning, or by the admission of air into the cavity of the chest, will be found to be full of chyle and lymph after death.

It must appear to every one, who has considered the circulation of the blood with any share of attention, extremely unaccountable, that the blood should flow with continued impetuosity from a divided artery, while the stream, from the accompanying vein in the same situation, is gentle and easily subdued; for, considering that the quantity of blood, which, in the sound state of these vessels passes through the vein, must be equal to that which in the same time passes through the artery, and that the fountain from which each is supplied is the same, the venous stream ought to be equally strong and lasting with the arterial. Hence arose the unphilosophical conclusion, that the laws of hydrostatics were not applicable to the motion of the blood. But the surprising difference between the momentum and permanency of the stream from a divided artery, and of that from a divided vein, admits, upon the principles we have attempted to establish, of a solution, ot once simple, satisfactory, and perfectly conformable to the laws of hydrostatics. It has been maintained that an important share of the motion of the venous blood is derived from the diminution of resistance in the direction of the heart, occasioned by the removal of a part of the atmospheric pressure, from the convex surface of that organ, by the elastic effort of the lungs. But when a vein is divided, the blood contained in the part of the vein beyond the point of section, is, on all sides, exposed to the undiminished weight of the atmosphere, and a powerful cause of its motion is, in consequence withdrawn. The blood beyond the division is now placed in circumstances very different from those in which it flowed in the sound vessel. If it be intended therefore to estimate, from the force with which the blood flows from a divided vein, the actual force with which it flowed before the division of the vessel, it will be necessary to add, to the discharging stream, a quantity of motion equal to that which would be generated by the share of atmospheric weight supportable by the elasticity of the lungs, at the full dilatation of these organs.

If a puncture be made in the side of an artery, the blood will continue to flow from the orifice, even to the extinction of life; but if a similar puncture be made in a vein, no blood will be spilt or at least the discharge will soon cease.—According to the supposition, that the venous blood is propelled by a *vis a tergo*, and by lateral pressure, there appears no reason to infer, why the blood should not flow from the orifice made in the vein as permanently as from that in the artery. It would be pressed out of the orifice of the vein by a force measurable by the weight of a column of blood, of which the height is equal to the distance of this orifice from the heart, and the base to the area of the transverse section of the vein. The resistance which, in those circumstances, the blood would have to overcome in proceeding along the channel of the vein, would unquestionably, be more powerful than that which would be opposed to it in flowing out of the orifice. The punctured vein ought therefore to bleed as freely, and as long, as a punctured artery.

We are now enabled to assign a satisfactory reason for a punctured vein refusing to bleed.—In taking its course out of the orifice, the blood would have to surmount the resistance which the whole weight of the atmosphere is capable of opposing; but, in flowing along the channel of the vein, it is relieved from a share of that resistance. It therefore necessarily takes the less resisted passage, and is as securely confined within the coats of the punctured, as within those of the uninjured vein.

If a vein be surrounded by a ligature, the part of the vein between the ligature and the heart becomes empty and collapsed, while the part on the other side of the ligature swells and becomes tense. From the operation of the causes usually ascribed to the motion of the blood, the swelling of the vein, behind the ligature, will easily be deduced ; but its depletion on the side of the ligature nearest the heart does not admit of so ready an explanation. The blood which had just cleared the section of the vein about to be rendered impervious, would not, after the power, by which it is supposed to have been moved, had beeu intercepted by the ligature, continue impressed with such a degree of momentum as would be sufficient to convey it all to the heart, and evacuate the vein. According to the laws of fluids moving in tubes, part of the blood would, from the reaction of the particles, become retrograde and be driven back against the ligature. This would more certainly take place in tubes formed in the manner in which the veins are ; for other branches unite with the stem from which the bound vein has sprung ; the blood in those branches is not, as in this, intercepted from the influence of the powers by which it is supposed to have been propelled. The superior force of the streams, in the unbound branches, would necessarily prevent the blood; placed between their confluence with the bound branch, and the ligature, and cut off from the influence of the propelling powers; from advancing into the common stem. But the part of the vein before the ligature, supposing it to have been by some means depleted, would soon be filled with a fresh supply, by a reflux from the other branches; and the coats of the vein, on the side of the ligature nearest the heart, would continue to be distended by a force equal to that of the circulation in the veins. The retrograde motion of the blood could not in these circumstances be always at least prevented by valves.

The depletion of the vein before the ligature follows, necessarily, from the operation of those causes to which the motion of the venous blood has been attributed in the preceeding pages.—Yielding to the balance of pressure, which ponderates against it from behind, and which the application of the ligature could have no effect in lessening, the blood will continue to advance towards the heart after the permeability of the vein behind it has been destroyed; and the same causes, by which the part of the vein before the ligature was emptied, would effectually prevent it from becoming, in the same circumstances, the receptacle of a fresh supply.

If a ligature be put round all the branches or veins which return the blood from a particular part, as from the arm, so as to render them impervious, without at the same time compressing the accompanying arteries, the parts of the veins between the ligature and the extremity of the member will swell and become very tense, and will continue so until the bandage shall have been removed, or until the death of the part. If a puncture, in these circumstances, be made in the swelled vein, the blood, without the relaxation of the bandage, will continue to flow even to the extinction of life. This appearance, with which mankind have been familiar from the most remote ages, was justly held by Dr. Harvey, to supply one of the strongest arguments in favour of the circulation of the blood; and, when we consider the inferences which it so plainly indicates, we are struck with surprise that it did not earlier direct the notions of mankind to that great discovery.

If, however, one of the veins only of the arm be compressed by a ligature, the part of the vein between the ligature and the extremity of the member will swell, but not to the same extent, nor with the same continuance, as it would have done had all the veins of the arm been compressed at the same time. Nor will much blood ingeneral be discharged from an orifice made in the vein behind the ligature. The uncertainty of a vein always swelling behind the ligature with which it is surrounded, and the speedy disappearance in many instances of the swelling after it had taken place, are circumstances which gave great perplexity to the first supporters of the circulation of the blood, which supplied their keen adversaries with powerful weapons, and which it mnst be acknowledged do not admit of a satisfactory explication from the views that have been afforded of the causes of the Harvean circulation. The uncommon interest which the circulation of the blood created at its first promulgation, in consequence of the violence with which it was assailed by its ardent opponents as well as of the vast importance of the subject, and the plausibility of the objection afforded by the absence or speedy cessation of tumor behind the ligature by which a vein had been tied, caused these experiments to be multiplied exceedingly, and to be repeated on all kinds of animals, in every variety of form.

The illustrious Baron Haller, unquestionably one of the ablest and most zealous followers of Harvey, with that candour, for which that great physiologist was eminently distinguished, admits indeed the difficulty, but at the same time attempts to elude it by devices that are more dexterous than convincing. He advises the experimenter in these cases to be quick in making his observations ; as otherwise the swelling behind the ligature may have in the mean time subsided, in consequence of the failure of propelling force accompanying a cessation of the powers of life. He also recommends as the subject of these experiments warm blooded animals rather than those which are less perfect; observing, that in the latter the blood soon retires from behind the ligature. A remarkable circumstance this; for if the decrease of the swelling proceeds from the sinking of the powers of life, the swelling behind the ligature ought in such experiments to continue longest in the cold blooded animals, as they are more tenacious of life than those which are termed the more perfect. The opponents of the Harvean theory maintained, that the appearances observed upon tying a single venous ramification, tended most powerfully to confirm the opinions which prevailed respecting the motion of the blood, before the doctrine of the circulation had been proposed; and which were, that the blood flowed to and from the heart alternately in the same vessels, after the manner of the flowiug and ebbing of the sea. The retiring of the blood from behind the ligatnre was supposed to occur during the refluent movement of the fluid. Microscopic observations made upon the blood flowing in the transparent parts of animals, added to the perplexity into which the supporters of the Harvean theory were thrown. Globules of blood were observed, after advancing through a vein, and trying unsuccessfully, as it were, to find a passage onwards, to retrace their steps, and to retreat, for a considerable space, along the same channels in which they had before advanced.

All these appearances will derive a plain and satisfactory explication from the supposed operation of the causes to which the motion of the venous blood has been attributed, and the objections which these appearances were supposed to oppose against the doctrine of the circulation, will be completely removed.

When all the veins, belonging to the arm, are compressed by ligatures or by a bandage, every avenue for the return of the blood from the part beyond the ligatures to the heart is closed; but, as the arteries remain pervious, and continue to transmit blood into the extreme veins of the member, the force derived from the heart and arteries will continue to throw blood into these vessels, until it shall be balanced by the resistance opposed by the distension of the coats of the veins beyond the point of compression; and the permanent operation of the same cause will maintain the same state of distension so long as the bandage remains.

But when one of the veins only of the arm is tied, the case is extremely different. Other passages are still open between the blood in the part of the vein behind the ligature and the heart.—The more direct road is indeed intercepted ; the suction influence of the heart will not reach it through the usual channel but will still be extended to it through another but a more circuitous route. The blood, contained in the part of the vein beyond the ligature, is intercepted by the compression of the vein from the exhausting influence of the heart ; it sustains, on the side of the ligature, the full weight of the atmosphere ; but the particles further removed and situate at the entrance of the nearest anastomosing vessel are less resisted by the blood in this vessel than by that between them and the ligature ; they therefore give way and take the least resisted course ; other particles succeed to their place ; and, in turn, yield to the same influence, till at last the whole part of the vein, between the ligature and the nearest anastomosing branch, is completely emptied, and must remain so, as long as the ligature compresses the vein.

The course of the globules of blood, observed to become retrograde in the pellucid parts of animals, admits of a similar, and no less satisfactory, explanation. Should a small vein be for a time obstructed, which may reasonably be supposed often to happen, in cousequence of pressure, or even by the magnitude of some of the globules themselves, the globules behind the obstruction will be effected in the same manner that the blood has been observed to be, in the part of a vein behind the ligature by which it has been bound. It may easily be conceived that the blood, in the small vessels, particularly in those which form a communication between the branches of one stem and those of another, will flow in different directions, without the supposition of any obstruction. The supplies of the vessels belonging to one branch may not be sufficient for their demands, while the supplies of another may at the same time be superabundant. The resistance oppossed to the entrance of the blood into these different sets of vessels will vary according to circumstances. The blood in the connecting vessels will uniformly pursue the less resisted course, and will therefore move through these vessels, sometimes in one direction and sometimes in another. Hence we may account for that oscillatory motion which is observed in the capiliaries and which was supposed, by that excellent physologist, Dr. Robert Whytt, to be the cause of the passage of the blood through the small vessels.

It is impossible at this part to withhold our admiration of the simple, yet most effectual, manner in which provision is made against the stagnation of blood in case of any obstruction in the veins, and its various consequent mischiefs. If it had been propelled by causes which must always drive it in the same direction, the blood must have often been accumulated and become stagnant, and in a short time putrescent; especially when it is considered that the veins are very dilatable, and have no power in themselves of reacting against the distending force. But so long as there is open a communication between the blood, in any part of a vein, and the heart, however circuitous that communication may be, the stagnation of blood, either before or behind the obstructed part, is not to be apprehended.

In the arteries the same mischiefs are prevented by the contractile energy of these vessels displacing the accumulated blood, or correcting the error by the more troublesome and often dangerous remedy of exciting inflammation in the part.

It was remarked in the numerous experiments that were made by surrounding the veins of living animals with ligatures, that, in some cases, the swelling behind the ligature was considerable and of long duration; that, at other times, it was slight and soon subsided. These appearences may all be explained from the difference with respect to the proximity, number and capacity of the anastomosing vessels connected with the part of the vein behind the ligature. For if these anastomosing vessels be at a short distance, numerous and large, no tumefaction will take place behind the ligature, or it will soon subside; but if they are remote, rare, and of a minute calibre, the tumefaction will be greater and more durable.

It was observed that the extremities of the arteries and veins were supposed to communicate in general directly, but that it was acknowledged, that in some parts of the fabric, they form communications, through the intervention of cells. These parts are chiefly, the corpora cavernosa penis, the clitoris, the uterus and the mammæ By what agency the cells of these parts are emptied, and the blood which they contained is returned again into the circulation, after the causes by which they were filled have been withdrawn, is a question that has been much agitated by physiologists. The *vis a tergo*, proposed by Dr. Harvey, is not adapted to the end, and supposes a continuance of the causes by which the cells had been filled. Nor have Dr. Brown Langrish, and the advocates of the venous absorption, been more fortunate in their explanation of the phenomenon, as they have not been able to shew by what means the veins are rendered capable of acting as absorbents. The elasticity of the cells cannot be supposed singly to produce the effect; for, as was justly observed by Dr. George Fordyce, the cells are not tense at the time of their depletion. After examining maturely every hypothesis that has been advanced, this last mentioned author concludes that the phenomenon is inexplicable on the principles of hydrostatics, "For supposing," says he, "an opening made in a vein, there is a pressure equal to the circulation in the vein, to force the blood out of this opening, it would therefore flow out and remain out, unless there was a force superior to this pressure to throw it in again. But we know of no such force in the veins of the common structure." But the force for which the learned author searched in vain, and the discovery of which he considered unattainable, has, I trust, at last been found. In consequence of the causes by which the cavities of the heart are expanded and of which the explanation need not here be repeated, less resistance is opposed to the blood at the opening into the vein than at any other part of the spherical cavity. The elasticity of the cell and of the substance surrounding it, and even the specific gravity of these parts will co-operate with the abstraction of resistance in the direction of the venous orifice, and effectually produce the evacuation of the cell upon the removal, or adequate diminution of arterial force, by which the cell had been filled and distended.

How certain emotions of the mind should circulate through the arteries terminating in these cells, an increased quantity of blood, or should for a time obstruct the ordinary return of it from them by the veins, is a subject into which we shall not presume to enter.

Upon the same principles, the rapid contraction of the uterus, after the delivery of the child, may be plainly, and it is hoped, satisfactorily explained.

When the sides of a recent wound are united, and kept in that situation for some time, a communication will be formed between the vessels on one side of the divided portion and those of the other. The mouths of the wounded vessels become united by vessels of the same kind, newly formed for the purpose, and the wound is healed. If a tooth be taken out of the jaw of one person, and instantly inserted into the place from which the tooth of another person has been extracted, the newly planted tooth will take root, and will live in the head into which it has been transplanted. The spur of a young cock may be transferred to his comb, where, in consequence of the superior richness of the new soil, it will flourish with fresh vigour and far outstrip its fellow, which had been left on the native but more barren soil of the other leg.

A piece of the flesh which has been cut out of the body of one animal and instantly applied to the fresh wounded surface of that of another, so that the two divided surfaces remain in contact, will unite with that other; and any injury afterwards done to this newly ingrafted portion, will excite pain as sensibly in the animal to which it now belongs, as it would have done to its first possessor, had it been inflicted before its separation. The practice and recommendation of this art have conferred a ludicrous immortality upon the name of Taliacotius.

These curious phenomena depend upon principles which, though sometimes fanciful in their application, are of the greatest benefit to animal existence. Without them, the list of incurable diseases, already alas! too long, would have been greatly extended, and the loathsome affections of life multiplied beyond calculation. The process, which nature pursues for the union of divided parts, has always excited the eager attention of the curious observer. The late ingenious and indefatigable Mr. John Hunter bestowed particular attention upon this curious and important speculation. This celebrated physiologist, in the course of his labours, was led to the conclusion that the blood not only possessed that species of life by which it resists the chemical attraction subsisting between its constituent parts, but also, that in certain circumstances it had the power of forming itself by its own action into a vascular structure. Mr. Hunter supposed that when a wound is made and the lips of it brought together, the blood oozing from the mouths of the divided vessels, exerts its creative powers and is converted into new vessels, which form a channel of communication between the vessels which had been separated. The communication between the vessels being thus established, and the plastic nature of the gelatine of the blood uniting the other parts, the circulation is restored and the wound healed.

But it would not, upon this reasoning, be sufficient to allow the mass of blood the wonderful and improbable properties here ascribed to it. It must be supposed to possess others still more singular. It must have judgment to fix the proper direction of the vessels, which it has formed from itself. For it has been often remarked that a vessel, finding upon the surface immediately opposed to it no vessel with which it can form a communication, goes in quest of one; and it is at a distance frequently that it finds the object of its search. In fact the new vessels which form the communication between the mouths of vessels opening on divided surfaces are observed to pursue an oblique, crooked, or even a circular course. Before the time of Mr. Hunter, it was supposed that the arteries in these cases became elongated, and that it was by the stretching of the old, not by the formation of new vessels, that the vascular communication between divided surfaces was restored. But this is contrary to observation; the vessel which unites the mouths of two opposite vessels, Mr. Hunter ascertained, from an examination of the process in its various stages, to be a new formation. Besides, the same difficulty occurs upon this supposition respecting the direction of the elongated vessel, and the faculty of being able to judge upon the course that is to be taken, would be as necessary to the elongating vessel, as, upon Mr. Hunter's supposition, it is to the blood itself.

Let us attentively consider the means by which the blood is circulated, and examine whether a more simple and satisfactory explication of these phenomena may not be found.

When a wound is made in the body, and the parts brought together and kept in that state, the plastic nature of the blood that had oozed out of the arteries, drying near the surface, unites the edges of the wound. But the blood oozing out of the arteries below the surface remains liquid. The veins in the mean time have become emptied in consequence of the blood which they contained having taken the less resisted course and returned to the heart. The blood then, which had flowed from the mouths of the arteries into the interstices between the divided surfaces, being less resisted towards the mouths of the veins than in any other direction, necessarily enters the veins and continues its course to the heart. Other blood follows, and thus a communication is established between the artery and the opposite vein.

The blood forming this slow current, coagulates upon the external surface, and in due time a vessel is thus formed between an artery on one side and a vein on the other side of the wound.

Thus the formation of new vessels communicating between the arteries on one side and the veins on the other side of a wound, with a direction conformable to the relative position of the mouths of these vessels, is easily explained upon this simple principle, the tendency of a fluid to restore the equilibrium of pressure between the particles which compose it; and without being under the necessity of ascribing to the blood properties and even faculties, which it is impossible to conceive that it possesses.

But it may be urged that this explanation will not apply to all the cases. In this instance there are on each side arteries and veins, the communication between which and the heart has not been interrupted. But in the instances of the transplanted tooth, of the ingrafted spur, and of the transposed flesh, the communication with the heart on one side is completely cut off.

In reply to this objection, it is contended that in the case of the tooth, the cavities of the blood vessels with which it is supplied, from the incompressible nature of the substance of the tooth, are always of the same dimensions. If, therefore, a diminution of pressure, which was previously equal, be made at one end of a small incompressible tube, and the other end of the tube be inserted in a liquid, a current will necessarily be produced from one end of the tube to the other. This canal may be supposed to perform the function of a vein, and its demands, as they are perpetually renewed, will be supplied by an artery on the other side of the tooth ; the same will take place in all the vessels of the tooth ; so that a communication will be established between the tooth and the parts which surround it ; the circulation will be restored and the life of the part preserved. The same reasoning may be transferred to the transplanted spur of the cock.

But it may still be urged that this reasoning is not applicable to a piece of flesh which has been inserted, as the vessels belong to it are compressible, and their tubular form not so easily preserved. It is to be observed in this case, that though the larger veins collapse upon abstraction from the body, yet the capillary veins, though possessed of the same structure, may, in the same situation, be supposed to preserve their tubularity in a considerable degree, in consequence of the minuteness of the circles formed by their composing fibres and the great curvature and proximity of the tubular arches. As the transverse section of the larger arteries forms a complete circle, the minute arteries may be admitted to preserve their tubularity in a still more perfect degree. The blood which is poured from the arteries, the connexion of which with the heart has not been interrupted, into the interstices between the aproximated and adherent surfaces, will in time accumulate and be compressed by the resistance opposed by the two surfaces.

The transplanting of glandular bodies, as of the testes of animals, may be still more certainly effected, as the vessels of these parts preserve their tubularity more perfect, and continue more permeable, after their separation from the body.

Hence from the conjoined operation of the pressure affecting the extravasated blood, and of the exhausting iufluence extending to it from the heart through the veins, in connexion with the capacity which the small vessels possess of preserving their tubularity and of the adherence of the principle of life to the newly applied part, the circulation between that part and the adjoining substance will be gradnally established, their union effected, and the life of the former preserved.

Thus, from the principles which have been attempted to be established in thep receding pages, there is derived an easy, and, I hope, satisfactory explanation of the manner in which the parts of a body that have been divided are brought to unite ; and a step seems to have been made towards ascertaining the process which nature pursues in the greatest of all her works, the formation of animals.

It has been remarked, that valves are stationed at the passages between the auricles and ventricles, and at those between the ventricles and the roots of the arteries, but that no valves are discoverable at the entrnaces of the large veins into the auricles. The use of these valves are now considered sufficiently evident. But no satisfactory reason can be drawn from the explanation of their offices given by the illustrious Harvey, or by any subsequent physiologist, why valves should not be as necessary at the roots of the veins, to prevent the regurgitation of blood during the coutraction of the auricles, as at the passages in which they are found, to prevent the reflux of blood from the ventricles and arteries during their contraction. Some authors, impressed with the necessity of their existence, have maintained, that they have seen a species of valve at the root of the vena cava. But their assertions have not been confirmed by general observation. And the valve which could serve the purposes which are assigned to it, must upon all occasions have been too evident to escape the most careless examination. But, if the causes, to which the motion of the venous blood has been attributed in the preceding pages, are real, it will be found, that valves, stationed at the roots of the large veins would be altogether useless, and their existence inconsistent with the usual œconomy of nature. Synchronous with the contraction of the auricle is the dilatation of the ventricle. Consider the causes by which this last chamber is dilated.—They are not the forces, which are supposed to proceed from the contracting auricle, and which singly would, unquestionably, drive the blood as forcibly back into the cava as forwards into the ventricle; they are derived, in part, from the simple relaxation of the fibres, which is necessarily accompanied with an expansile effort; but chiefly from the suspension of a share of that pressure, to which all the objects on the face of this globe are subjected, from the convex surface of the heart. The blood, therefore, pressed upon by the contracting auricle, necessarily takes the less resisted direction, and is all drawn into the ventricle. The difference of resistance which the blood has to encounter in the two passages serves all the purposes of the obstructing valve, which under different circumstances, would be required at the root of the vena cava. But admitting even that the valve, after which anatomists have so long searched with scrutinizing eyes, were to be found, it would not serve the purposes which they have had in prospect. Supposing a valve to be stationed at the opening between the cava and the right auricle, and so constructed as to oppose, in similar circumstances, the passage of a fluid from the auricle into the vein; and suppose that the auricle, which has just been filled from the vein, contracts, what in this case is to shut the valve? There is no diminution of resistance, or suction-influence, which is necessary for the favourable play of valves, on the side facing the vein; on the contrary, the blood is supposed to advance against it with considerable momentum; and the pressure which is made upon the valve by the contracting auricle, is equal on both surfaces. The valve therefore would as likely be continued open as closed.

A slight degree of reflexion will be sufficient to convince the intelligent observer how much more important the offices of the valves which are found in the heart will become, and how much more certainly the regularity and efficacy of their action will appear to be secured, upon the supposition that the chambers of the heart are dilated by the causes which, in this treatise, have been assigned, to that purpose.

The two trunks of the ascending and descending cava meet at the heart in such a manner as to form a straight line, which caused them at first to be considered as one vessel and to be designated by the common appellation of vena cava. The streams of blood, which are conveyed by these vessels to the heart, are placed at that point in direct opposition. Upon the supposition that the blood is returned to the heart by a *vis a tergo*, by the action of the coats of the veins, or of those of the concomitant arteries, or by the compression of the muscles, this position of these vessels is the most unfavourable that can be conceived, for the office that is assigned to them. The momentum of the blood in one vessel would be destroyed by that of the other ; or, if the current in the descending was stronger than that in the ascending cava, the blood in the weaker stream would be prevented from ever reaching the heart. The difficulties, opposed to the return of the blood from the extreme vessels to the heart, already upon these hypotheses, insurmountable, would be accumulated beyond calculation, by this location of the venous trunks.

All these objections vanish from the view that has been presented of the causes of the venous circulation ; and, in their stead, advantages become strikingly displayed. As a share of the ordinary pressure is removed from the blood in the roots of the vena cava by the reiterated actions of the heart, this organ becomes the centre to which all the blood in these vessels is pressed ; and the two apparently opposing streams, at the moment in which they are about to make a collision, are each directed, by the exhaustion of the heart, without sustaining any resistance from the other, into a common reservoir.

In certain circumstances, great advantages must result from this position of the venous trunks. As the impetuosity of the currents, by the conflux of which the streams of the cava are composed, is liable to be greatly augmented by the actions of the superincumbent muscles during violent exertion, a greater quantity of blood may be returned than the heart is fitted to receive, and the organ would be in danger of sustaining violence from the augmented impetuosity of the current. In such a circumstance, the momentum of the blood in one trunk will be expended against that of the blood in the other; and the heart in a great degree defended from violence.

As the blood in the system of the pulmonary veins is not exposed to such transient accelerations, the left side of the heart does not require to be protected from violence in the same manner.

The heart impinges against the left side a stroke, which is not only very perceptible to the touch, but at times excites a sound which is sufficiently audible. The cause of the beating of the heart against the chest has frequently attracted the attention of the curious, and has been the source of considerable controversy.—The opinion, which most naturally arose upon a slight consideration of this phenomenon, was, that the stroke was produced by the blood propelled forcibly against the chest by the contraction of the left ventricle. An examination of the course of the aorta, which does not approach with any of its divisions, the part of the chest generally impinged, presented an undeniable refutation of this hypothesis. It was next urged that the walls of the ventricles were pressed forcibly, during their expansion, against the sides, by the impulses of the contracting auricles, and that thus the stroke in question was produced.—The observation, however, that the stroke was not made during the dilatation, but during the contraction, of the ventricles, became fatal to this supposition. An explanation was attempted to be derived from the momentum of the blood in the veins; but it was not easy to describe how a power acting constantly and uniformly like that of the force of the venous blood, could produce an intermitting impulse. The late renowned Dr. William Hunter proposed, in his lectures on anatomy, a new explanation of this phenomenon; which is certainly ingenious, which received the ready assent of the admirers of that great man, and which is at present generally acknowledged to be satisfactory. This explanation, as it is given in a note contained in Mr. John Hunter's treatise on the blood, which was edited by

Sir Everard Home, is as follows: "By its (the heart's) throwing the blood into a curved tube, viz. the aorta, that artery at its curve endeavours to throw itself into a straight line to increase its capacity, but the aorta being the fixed point against the back, and the heart in some degree loose or pendulous, the influence of its own action is thrown upon itself, and is tilted forward against the inside of the chest." But this explanation, ingenious and plausible as it may appear, will not, it is contended, be found satisfactory upon minute examination. The position of the heart it is admitted could not be effected by the projection of blood singly out of its cavities. It is not supposed in this explanation that the heart was affected by the reaction of the blood, as in that case the blood must be considered to have moved at the time in a retrograde course, and for that purpose the straight would have been equally favourable with the curved line of the artery. With respect to the effects ascribed to the curvature of the aorta, it may be observed that no proof appears to be adduced of the supposed change taking place at the time; that, from an examination of the connexions of the aorta with the parts with which it comes in contact in its course, it is not probable that such a change could be easily produced; and that, allowing the vessel to be quite free and unattached, it would not be brought into a straight line by the impulse of the blood flowing through it; as it is the law of fluids moving in tubes to press equally on all sides. A satisfactory illustration of this law is daily exhibited in the conducting of water from one place to another in pliant flexible tubes. These pipes, though they may easily be extended into a straight line, and though a fluid may be impelled through them with great impetuosity, retain the position, however curved it may be, in which they have been accidently placed. But admitting the force of the blood to have a tendency to restore the curved aorta into a straight line, it is not conceivable that such a degree of rigidity could, by that means, have been given to that vessel, as to enable it not only to elevate, by an unfavourable lever, so massy a substance as the heart, but to drive it forcibly against the chest.

Let us inquire, whether a more easy and less exceptionable explanation of this remarkable and interesting phenomenon, may not be deduced from the principles which we have in these pages been endeavouring to establish.

It has been stated that the stroke which the heart impinges against the side is synchronous with the systole of the ventricles. The apex of the heart is brought nearer the base, and the dimensions of the body of the heart are decreased by the contraction of the ventricles. Within a space of time too short to admit of measurement, the auricles are agumented in an inverse proportion. A small portion of the pericardium covering the apex of the heart is, in the ordinary position of the body, in contact with the side. During the contraction of the ventricles, the apex of the heart is drawn vividly and powerfully from the part of the side to which it is opposed, and from which it is separated only by the pericardium; and, in consequence, the portion of the sides is drawn inwards, so that a slight depression is occasioned in the intercostal muscles; but, on account of the greater facility with which the blood in the veins can be made to change its place, and of the impulse which it sustains either from the residue of the *vis a tergo*, from gravity, or from the contracting roots of the cava, the auricles are dilated, and the apex of the heart is not only rapidly restored to the place from which it had, in a slight degree, withdrawn itself, and which had been occupied by the yielding substance of the side; but an impulse from within is now to be sustained by the parts which, the instant before, had been drawn in an opposite direction. The range of movement which the yielding parts of the chest have to traverse in an outward direction, is lengthened, and the stroke which, in consequence of that movement, will be impinged against a body placed upon the external surface of these parts, must be proportionably more perceptible.

Some circumstances, connected with the beatings of the heart against the chest, require a more particular notice.

The stroke of the heart is not always felt at the same part, and, in some situations of the body, is not felt at any part of the side.

In consequence of the weight of the heart, of its being in a great degree loose and pendulous, and of the range which the pericardium will give it, in the various directions in which it may be urged by gravity; the heart will slightly vary its place and position, with the varying positions of the body. When we place ourselves upon the right side, the apex of the heart will recede to a certain extent from the part of the side to which it points, and the left lobe of the lungs will be extended downwards, between it and the side.—In that case, the impulse to be sustained by the movement of the heart, will not, in ordinary circumstances, extend to the side, and other positions of the body will give the apex of the heart a direction more elevated or more depressed than the common.

Nor will the place of the chest against which the stroke is made be varied by changes in the position of the body alone; it will be altered by variations in the force and in the balance of the circulatiou. The impulse, which the body of the heart sustains and which is supposed to impel it against the side, proceeds from different points. The direction of the diagonal of the parallelogram formed by the currents from the cava and pulmonary veins, will be varied by the direction and force of those currents; but the body of the heart will be impelled against the chest in the direction of this diagonal.

When we consider the vehemence of the stroke which the heart impinges against the chest, aud at the same time reflect upon the injury which the more solid parts, and even bones, iu a short period sustains from the beating of an aneurismal artery, we are struck with surprise that the left side and the heart itself do not exhibit marks of extensive destruction, from the long continuance and frequent repetition of the collisions that take place between them.

The mode in which the heart and the parts impinged by it are protected, during these movements, from that injury which, from analogy of effects, was to have been inferred, may be collected from the preceding statement. Before the yielding part of the side has been pressed outwards, it had been drawn to a certain extent from its natural situation in the direction of the heart. The whole range of its outward movement is composed of a resilient and an impelled action. No injurious impression can be sustained by the side during the share of the movement by which it recovers its natural position; and it is only after it has passed the line of that position, that any violent impaction can take place between it and the heart The positive stroke, made against the side by the heart, is not so great in reality as in appearance, and as the extent of the impulse sustained by the hand would indicate.

Another abundant source of protection, during these movements, is derived from the frequent change of the part of the heart by which the impaction is made, and of the part of the side which sustains it. The bone, against which an aneurismal artery beats, is never for a moment relieved from the pressure of its action. According to the hypothesis of Dr. Hunter, the part of the back which served as a buttress to the aorta would always, and the part of the side against which the heart was tilted would generally, continue the same. But, according to the explanation of the phenomenon which we have ventured to propose, both the instrument by which the blow is inflicted, and the objects sustaining it are changed by every variation in the position of the body, and in the relative forces of the greater and lesser circulation.

It will readily be acknowledged by those whose occupations, or accident, have kept the body long in the same position, that, on such occasions, they have frequently felt, in the left side and in the region of the heart, an uneasiness which was soon removed by exercise, or change of position.

As the weight of the atmosphere has evidently an important influence upon the motion of the blood, it is reasonable to expect, that this influence will be modified by the amount of that weight ; and that, in general, the living body must be materially affected by any great diversity in the weight to which it is subjected by the atmosphere. The slighter changes of atmospheric pressure, arising from changes in the general amount of that pressure, and usually indicated by the barometer, though capable of influencing the movements of the frame, in a degree which renders itself sufficiently perceptible to the feelings, and which is manifested by the flow of spirits and the bodily agility, are not calculated to produce effects sufficiently palpable for our examination. For that purpose, we must have recourse to situations where the difference between the pressure and that to which our bodies have been accustomed is more extensive and permanent. Such a diminution of the weight of the atmosphere, as is fitted to produce effects sufficiently observable to attract the notice of the philosophic traveller, is to be found on the summits of the most elevated mountains. The effects, which are produced upon the body by great elevation into the regions of the atmosphere, were experienced by Saussure and his companions in their journey to the top of Mount Blanc, and have been described with elegance and perspicuity by that celebrated traveller.

It appears that, when Saussure and his fellow travellers had advanced to a considerable height in the mountain, they were seized with excessive fatigue which obliged them frequently to rest. Many of them were seized with faintings from which they completely recovered after lying on the ground a short time. The pulse was quick and small; the tongue parched; the thirst great; the respiration laborious. Though the fatigue was excessive, and obliged them instantly to stop and rest, it in many respects differed from that fatigue which is experienced after labour in lower regions. It was suddenly induced to the highest pitch, and as suddenly removed. After they had rested a few minutes they arose as fresh and as alert as they had been at the commencement of their journey. But before they had proceeded many yards they were obliged to rest again. It was remarked that this difficulty in ascending happened at a lower part of the mountain to some people than to others. Saussure does not describe the persons who were first affected in this manner, but says, that some men, who appeared very strong and capable of great labour without fatigue, could easily reach a certain height, but could ascend no further; that, when they attempted to go beyond this limit, they were seized with faintings. Some could advance higher than others without suffering any inconvenience. He remarks further, that they were frequently seized with slight hæmorrhages from the lungs; that the whole body was unusually turgid and red; and that the blood veesels were uncommonly full.

From an account of a journey to the Peak of Teneriffe, contained in the European Magazine, for December, 1813, the following is an extract:

"In ascending" says the traveller, "the highest part of the mountain, called the Sugar Loaf, which is very steep, our hearts panted and beat vehemently so that, as I observed before, we were obliged to stop above thirty times to take rest. But whether it was owing to the thinness of the air causing a difficulty of respiration, or to the uncommon fatigue which we suffered in climbing the hill, I cannot determine but believe that it was partly owing to one, and partly owing to the other. Our guide, a slim agile old man, was not affected in the same manner with us; but climbed up with ease, like a goat; for he was one of those poor men, who earn their living by gathering brimstone in the cauldron and other volcanoes."

M. Saussure has attempted to give an explanation of these singular and interesting phenomena; and is of opinion that they arise from the rarefication of the atmosphere in these high regions. He argues, that, on account of the lightness of the atmosphere, the volume of the air is greatly augmented; that therefore a given bulk of this fluid contains less oxygen at the summit, than at any lower stage in the mountain; that a sufficient quantity of this substance, which is well known to be necessary for the continuance of exertion and of life, is not in a given time taken into the system by the action of respiration, for the purpose of the body under exercise; and that the fatigue arises from the want of the due oxygenation of the blood.

But many strong objections may be urged against this explanation. It is to be observed that the expansile effects of levity upon the air in these high regions are in a great degree counteracted by the excessive cold, by which the elasticity of the fluid is diminished; that, in consequence, the difference of oxygen, contained in equal bulks of air at the top and bottom of a high mountain, is not so great as was to have been expected by attending alone to the weight of the atmosphere in these different situations; that the symptons which have been described are not usually exhibited by persons breathing an air heated to such a degree as to render it rarer than it exists at the top of the highest mountains, and are not such as are usually produced by breathing air containing a deficiency of oxygen, which occasions stupors and other apoplectic symptoms; that a greater quantity of oxygen is required by the system during labour than rest, is an hypothetical opinion; that the great want of oxygen here supposed should be suddenly removed from the system by the deficiency of expenditure, arising from a state of rest alone, without any increase in the supply of the substance, is altogether unreasonable; and that the slight hæmorrhages from the lungs, the redness of the skin, and the swellings of the blood vessels, cannot be explained from a deficiency of oxygen in the system.

It will be found, I trust, that the inconveniences which travellers experience, in ascending to the summits of high mountains, are to be traced rather to mechanical than to chemical causes, and that they arise not from the rarity, but directly from the levity of the atmosphere.

It is well known that the pressure of the atmosphere is less upon the summits of very high mountains than on the plains that are nearly level with the sea. If the weight of the atmosphere has upon the motion of the blood really that influence which has been ascribed to it in the preceding pages, it was to be expected that the motion of this fluid, and of course the actions of the animal machine generally, must be materially affected by any considerable difference in the amount of that weight. The conclusions, which are fairly deducible by reason alone, are, in this instance, fully confirmed by observation. The singular affections, experienced by Saussure and other travellers, who have ascended into very elevated regions, are supposed to have been produced in the following manner.

As the weight of the atmosphere is supposed to have an important share in producing the diastole of the heart, it is reasonable to infer that the motions of this organ will be affected by any considerable alteration in the amount of this weight. To produce the effect required, it is necessary that the pressure should be sufficient to balance, not only a column of blood of several feet in height, in circumstances in which the resistance arising from gravity is augmented by the tenacity of the fluid and other causes; but also to overcome the inertness of the strong substantial muscle inclosing the chambers of the heart. It may be admitted, that on the summits of the highest mountains which are placed on the face of this globe, there would still be found a sufficient residue of pressure for accomplishing these purposes; but it may be conceived to be so far diminished, as to become incapable of restoring the diastole of the heart with the ordinary rapidity, and with that energy which is consistent with health. In such circumstances the functions of the heart would be more feebly discharged, and the circulation become more languid.

It was remarked in a former part of this Treatise, when the effects of muscular action upon the motion of the venous blood were examined, that the circulation in the veins would be retarded by the pressure of the muscles upon the subjacent veins, in consequence of the aggregate of the bores of the vessels being greatest on the side of that pressure most remote from the heart. If further, a considerable diminution in the energy of those powers, by which the blood is returned to the heart, be accompanied by the obstructions which violent exercise is calculated to oppose to the circulation at this part, the blood will not reach the heart in sufficient quantity, but will be accumulated in the extreme vessels. Hence evidently would proceed faintings, suddenly overwhelming fatigue, redness of the skin, great distension of the superficial vessels, and slight hæmorrhages from the lungs.

The laborious breathing, ranked in the train of affections which the travellers suffered, may be traced to similar causes,

A very remarkable characteristic of the lassitude with which the travellers to the top of Mount Blanc were affected, in so overwhelming a degree, was, that it was as suddenly and as completely removed by rest. After resting a little, they found themselves as alert and vigorous as they had been at the commencement of the ascent.

Two causes seem, at that time, to have conspired in restoring the activity of the body. The first was the horizontal position which the extreme lassitude and faintings forced them, more or less, to assume at short intervals. In this position the column of fluid, required to be balanced by the atmosphere, was greatly reduced, and the blood, of course, more vigorously transmitted to the heart. Its determination to the centre of the system would also be favoured, in a greater degree, by gravity. The other cause was the cessation of muscular action; as then the *vis a tergo* ceased to be counteracted in the veins by the pressure of the muscles.

As the lassitude was not induced by muscular action repeated with vehemence and for a long time; which would have exhausted the irritability of the muscles, and required a considerable period of repose for the recovery of that irritability; but by a sudden abstraction of the ordinary stimuli; the moment at which the stimuli were re-applied in the usual manner, the excitement of the system was renewed, and the vigour and activity restored.

It is worthy of remark that the lassitude came upon some of the travellers sooner than upon others ; and that, in some instances, there was a defined state of elevation above which they could not ascend. Every attempt to conquer the higher ground proved abortive. The regions above a certain point seem to have been unfit for their existence. Saussure has not particularly described the persons who were first affected with unconquerable lassitude ; but observes, that men, who were very robust, and who in other circumstances could undergo great labour without inconvenience, were soon subdued by this species of exertion. The guide who conducted the travellers to the Peak of Teneriffe, a little slim old man, was not affected in the least. All other circumstances being equal, it is presumed that the tallest and most robust would soonest reach the limits of the region fitted for their existence, as in them the column of blood to be supported and moved is higher and more resisted.

It follows, from what has been stated, that this earth is not habitable by animals above a certain magnitude ; and that, as the weight of the column of fluid contained by the animal machine approaches nearer to a balance with the weight of the atmosphere, the circulation through that machine must be more languid. Little animals would appear to be best adapted to the atmosphere of high mountains. Accordingly we find that animals which live upon very elevated mountains are less than those of the same species which dwell upon the plains. Other causes may co-operate in producing this difference. Birds which ascend into the higher regions of the air should be less than the animals which reside continually on the surface of the earth. Fishes, again, which in addition to the pressure of the atmosphere support that of the water in which they swim, and which approaches in its specific gravity to that of the blood, may exist almost of any size, provided that there is sufficient depth in the water.

The benefit of sea bathing may, perhaps in a certain measure, be ascribed to the additional energy that is given, for the time, to the circulation of the blood, by the weight of the water.

The operation of the causes, to which the blood has in these pages been ascribed, is admirably calculated to adapt with precision the demands of the circulation at all parts to its expenditure ; and particularly to provide a sufficient supply of blood to the heart against the time that it may be required. Had the heart affected the blood by its projectile power alone, that fluid must, in its progress through the arterial system, have in time become independent of the power by which its motion was originated ; and a thousand causes might intervene, over which the heart, so limited in its influence, could have no controul, to retard or divert its course ; and which by occasioning one short fatal delay might prevent its retnrn for ever. But in reality the heart extends its empire over the whole circulating system ; it maintains a regulating influence over the blood, through the whole of its course, and directs its return to itself in the veins with the same efficacy that had insured its departure in the arteries.

In the mode in which the blood is circulated, we have to admire the provision which is made against a deficiency being sustained, under almost any circumstances, at the heart. For this organ is made the centre of pressure and of gravity, and designates the stage in the circulation in which a deficiency of supply would be the last in being felt. Hence it happens, that the functions of the heart are performed, and life preserved, notwithstanding long and copious discharges of blood ; which, upon every other hypothesis, must have proved fatal. For according to these hypotheses, the heart, or at least the auricles, are placed at the end of the projection, they mark the highest advance of the tide, and would first be abandoned by the retiring fluid. They would be drained by every profuse hæmorrhage; and the heart would expend its energy in fruitless efforts, to circulate a fluid that came not within its reach.

Although ample provision is made for a sufficient quantity of blood in due time to the heart, this organ is by a beautiful contrivance protected from all harm, and is relieved from the injurious impulse which otherwise it must have sustained, in resisting this impetuous stream. We have seen that it is principally by the elevation of a share of the atmospheric pressure from the convex surface of the pericardium, and of consequence from the blood in the venous trunks, that the momentum of the venous circulation is generated; that it is only during their dilatation that this momentum could affect the chambers of the heart; that during this period, yielding to a superior force, the walls of the heart recede and become dilated in all directions; but that, as soon as they have reached that state of dilatation at which their fibres might be overstretched and injured by the momentum of the blood, the pericardium has become dilated to its utmost capacity, and the pressure is equalized, on both the concave and convex surfaces of the heart.—The power which is required to resist the impulse of the blood is at this moment transferred from the heart to the strong tendinous pericardium ; and the walls of the heart, at the only state in which they could be injured by the impetus of blood, sustain little more force than the hand does from the pressure of the air with which it is surrounded.

It is by a contrivance no less beautiful and efficacious, that the coats of the veins are relieved from that lateral pressure which, in ordinary circumstances, they must have sustained, by supporting a column of blood often of several feet in height ; and which must have stretched them to such a degree as would have been incompatible with the easy movements of the body, and with the safety of the vessels themselves.—The coats of the veins evidently sustain little lateral pressure. For the purpose of perceiving the cause of this, it is necessary to recollect, that the forces by which the blood is returned from the extreme vessels to the heart, are of two kinds ; the one centrifugal, derived from the heart and arteries ; and the other centripetal, proceeding from the diminution of resistance, in consequence of a removal of a share of atmospheric pressure, in the direction of the heart. By the former of these forces, the coats of the veins are dilated ; by the latter they are pressed inwards ; and it is by the proper balancing of these two forces, that the veins are placed in that stste of easy distension, which they are observed to possess.

ON ANIMAL HEAT.

It is well known, that the bodies of animals are endowed with the faculty of preserving the same heat in every variety of climate. The blood of the bear, prowling over mountains of ice in high northern latitudes, is as warm, as it is, when the same animal has besn transported to the south of Europe, or even into the Torrid Zone. That a fluid, separated only by a thin membrane, from a cold which would instantly convert that fluid, if extravasated, into a solid rock, should retain its liquidity and warmth, is a matter so wonderful, as to resemble the direct and constant operation of miraculous power, rather than any of those processes, by which the physical government of the world is, in other parts, conducted. It is not surprising, therefore, that this subject should, in all ages, have engaged the attention of the curious. But it is not only as a curious speculation that it is interesting. Animal heat concerns us in a more intimate way. We carry it about with us every day ; we are reminded of its importance by every wind that blows ; and we are satisfied, by early experience, that without it, life itself would soon be lost, or retained only in misery. Since the important discoveries of what was called latent heat by Dr. Black, and of the composition of the atmosphere by Scheele, Lavoisier, and Priestley, the causes of animal heat have been investigated with increased industry. By those great discoveries, they seemed to be brought within the limits of that domain, over which the intellect of man is permitted to range. Since that memorable period, it may be said that the furnace has not ceased to burn, nor the recording page to be moist, in any region on which the rays of science have fallen. Notwithstanding animal heat has been long a favorite subject of investigation among philosophers ; and though modern Chemistry loudly boasts that she has supplied the means of unveiling this great secret of nature, it must be acknowledged that the subject is still involved in obscurity ; and the student, after having made himself acquainted with all the views, that have been taken of the question, and after having long and patiently attempted to reconcile discordant statements, and to select the true conclusion from many, all apparently deduced from faithful experiment, rises from the inquiry with a mind stored, it may be, with much important knowledge, but full of perplexity ; and, with respect to the object of research, as little satisfied as he was when he commenced the inquiry.

The following observations will, I trust, be found to throw some new light on the subject.

As in almost all the theories which have been constructed, and particularly in the view which I am about to submit to the public, the lungs, in the Mammalia at least, are supposed to be the organs in which the heat is evolved and regulated, and from which it is transmitted to the rest of the body; it seems requisite to commence the inquiry, by a short description of the substance of the lungs, and with an account of their mechanism, so far at least, as it may seem to be concerned in the present inquiry.

The lungs are familiar to every one; they are that large pale-red spongy mass, which in the shambles is daily to be seen suspended by the windpipe. In this situation, they are rough, corrugated, angular, with some deep indentations, and greatly reduced in volume. They are then in what is termed a state of collapse. In the living body, they fill nearly the whole of the chest, and are, in that state, expanded into dimensions far more extensive, than those which they occupy in a state of collapse, or that to which, when freed from other connections, they naturally recede. As the dimensions of the chest, assigned for the occupation of the lungs, vary considerably in the different states of inspiration and expiration, the lungs, accompanying the variations of the chest, with the parieties of which they are at all times in contact, become considerably more expanded in all directions during inspiration, than during the lowest stage of expiration ; but even in this last stage, they are always of a much greater volume, than that which belongs to them in a state of collapse. The lungs are a double organ consisting of two lobes, which are separated from each other by an intervening membrane placed vertically in man, and passing from the breastbone to the spine in the middle of the chest. These lobes communicate with each other only by the windpipe, which is a passage common to both. The lungs are suspended from the inside of the chest at the top, but at no other place do they, in a state of perfect health, adhere either to the parietes of the chest or to the mediastinum ; but perform, in the process of respiration, a sliding motion along the surface of each.

The lungs may be expanded into dimensions far more extensive than those to which they naturally recede in a detached state, and may be retained in various degrees of that expansion, during the whole course of a long life, without any injury to their structure. This property they enjoy principally in consequence of the elasticity of their structure. Differeut substances seem to be possessed of different kinds of elasticity. Some substances, after being compressed into smaller dimensions, resiliate when the compressing power is withdrawn, into their former volume, without including any internal voids or interstices which require to be filled by a foreign body. To this class of substances belong the gases. But the elasticity which appertains to the lungs, to gum elastic, to sponge, and other substances of that description, is regulated by different laws. If a piece of lung be stretched in length beyond its natural limits, it becomes proportionally thinner; and if pressed within those limits it becomes thicker. In both situations it fills up the same space. If a substance of this nature be extended into a larger volume, having all its external surfaces increased, it can only assume this volume, by the formation of internal cavities, the solid dimensions of which will be together equal to the solid dimensions of the difference between the two volumes. These cavities must necessarily be occupied by some extraneous substance.

The lungs of the unborn fœtus are in the most complete state of collapse; they contain in that situation no air, and their specific gravity is greater than that of water. Though all the vessels may be supposed to be formed, which by their dilatation after birth, permit the lungs to be expanded into a much greater volume, these vessels have then a very minute caliber, filled with blood and mucus. After birth, air finds access into the vessels of the lungs, and assists in filling the internal cavities, now greatly enlarged.

The power with which an elastic substance resiliates will be, in some measure, proportioned to the degree of stretching, to which it is at that time subjected. If it be supposed, that all parts of the lungs are equally elastic, and that, at every stage of dilatation, the power by which they are stretched is equally distributed throughout the whole substance, it follows, that all the fibres composing the lungs will be equally stretched. For, if any part of the lungs were more stretched than another, a power wonld be generated in that part, which would act against the weaker power in the adjoining substance, until a balance was restored, which would be, when the stretching of the different portions was equal.

It results from these suppositions, that when the lungs are expanded to any given degree, the aggregate of the internal cavities, or of the caliber of vessels which belong to any portion of lung, is equal to the same aggregate in any other portion of the same extent.

It also necessarily follows that at any given stage of the dilatation of the lungs, the aggregate amount of the vasculor cavities is fixed, and that this amount is equally distributed throughout the organ. The vessels which enter into the structure of the lungs, and which by their dilatation permits the expansion of this organ, may, at any stage of that expansion, be compared to rigid tubes the caliber of which is fixed and independent. But if another and a higher stage of expansion be taken, the caliber will still be fixed, and will resemble rigid tubes of a greater size.

We are, in the next place, to examine the vessels of the lungs, or those varying cavities which are placed in the interior, and which are necessary to the movements of these organs. The chief of these are the bronchi or air vessels, and the pulmonary arteries and veins, with their ramifications. Of these the description need only to be short and general; and first with respect to the air vessels.

As soon as the windpipe enters the chest from the neck, it divides into two branches, one of which goes to the right, and the other to the left lobe of the lungs. Each of those branches, ramifies into a number of smaller, and these again into others smaller still; and thus the process is continued, till the ultimate ramifications become extremely small and infinitely numerous. The structure of these vessels is at first like that of the trachea cartilaginous, but the cartilaginous structure soon disappears, and is succeeeded by that of simply elastic membrane. The ramifications of the bronchi exist in every and the most minute portion of the lungs. Indeed with some interstitial matter, they seem to compose the chief part of the pulmonary structure. They freely communicate with each other by means of anastomosing branches. With respect to their terminations and communications, we shall afterwards have to speak.

The blood vessels are the pulmonary arteries and veins. The pulmonary artery arises from the right ventricle of the heart and soon divides into two branches, of which one goes to the right, and the other to the left lobe of the lungs. The extreme and most minute pulmonary veins, uniting in parcels to form branches, and these again uniting to form larger, at last end in four trunks, which pour their contents into the left auricle of the heart.

The extreme, or, as they are called, capillary terminations both of the arteries and veins have two communications which require to be noticed. The arterial capillaries have a connection with the venous. Through this connection the blood passes, in the course of its circulation, from the arteries into the veins. But besides these, the pulmonary arteries, as well as those belonging to the larger circulation, have terminations through which red blood is not transmitted. These open into the internal surface of the bronchi, and from their office are called exhalants. It is now well established, that the veins are also well furnished at their extremities with openings, which do not admit, in ordinary circumstances, the red part of the blood. That the pulmonary veins are furnished with such openings, sufficient proof will be supplied in the sequel. These openings perform an office the reverse of that of the arterial exhalants. They take up substances from the surface of the bronchi, and on that account have been termed imbibers. It does not seem necessary for our purpose, to give any account of the nerves, or of the lymphatic vessels of the lungs.

The vessels now enumerated form the internal cavities, the dimensions of which vary, according to the varying volume of the lungs. Without such cavities it is evident that the lungs could not be expanded, nor the chest dilated by any moderate force.

But to enable the vessels to accommodate themselves to the various sizes required for the office assigned to them, there must exist a supply of materials *ab extra*, from which these cavities, increasing with the volume of the lungs on inspiration, may be readily filled; and also convenient passages, through which the superabundant substance may be discharged, when this capacity decreases, with the decreasing volume of the lungs in expiration. As the air vessels communicate freely with the windpipe, and that with the external air, there exists a ready channel, both for the reception and discharge of matter, according as they are made to assume a greater or a less capacity. But it is not quite so evident from what sources the materials are drawn, to allow the blood vessels to assume their share of expansion, which the structure of the lungs would appear to render necessary during inspiration; or through what channel, the superabundant contents of these vessels are discharged out of the chest during inspiration. Neither can the matter supposed to be introduced into the increasing cavity of the blood vessels during inspiration, nor that discharged from the same, as it diminishes during expiration, consist of blood: for no blood can enter the pulmonary arteries, or be discharged out of the pulmonary veins, except through the portals of the heart. But these portals do not open and close in correspondence with the required periods of supply and discharge. The movements of the heart are not timed by the movements of the lungs. Four pulsations of the heart may generally be counted during each complete respiration. It may indeed accidentally happen, that a discharge of blood may be made into the pulmonary arteries, at the moment in which inspiration commences, but, at the same moment, an equal quantity is abstracted from the pulmonary veins to fill the enlarging auricle of the left side of the heart. Whenever then, a quantity of blood is thrown into the chest, an equal quantity is, in the same period, discharged out of it; and also every discharge of blood from the thorax is accompanied by the entrance of an equal quantity through another channel. The quantity of blood therefore at any time contained by the lungs does not appear to be at all modified by respiration.

We have been led therefore to look out for other channels, through which materials may flow into and be discharged from the blood vessels of the lungs, in the different stages of inspiration and expiration, and in our search after these, we have been conducted to that view of the origin of animal heat, which is now to be submitted to the candid consideration of the public.

I shall then, in the first place, exhibit the view which has at least the merit of simplicity. I shall then examine those opinions which may be supposed to constitute the present creed of philosophers on this subject, and, in conclusion, adduce those additional proofs in support of my own hypothesis, which there may have been no opportunity of introducing in the preceding examination of the opinions of others. This process will, I trust, altogether be found satisfactory.

As, during inspiration, the chest is at once both widened and deepened, and the volumn of the lungs becomes at that time greatly enlarged, the cavities in the interior of these organs are proportionally augmented. While these cavities are enlarging, the contents of them are subjected to a diminished pressure. To restore to the substance within the luugs an equality of pressure with that of the substances without them, the air rushes down the windpipe, and fills the enlarging air-vessels. Bnt the inequality of pressure does not end here. The air-vessels are powerfnlly elastic and resist further distention. One part of the substance of the lungs becomes more stretched than another. In consequence, the contents of the blood vessels of the lungs are subjected to a less degree of pressure than the rest of the system. Our observations on the effects of this diminution of pressure shall be confined, at present, to the contents of the pulmonary veins. In these circumstances, the air received into the windpipe will not terminate its progress with the bronchi. It will necessarily pnrsue the less resisted course, and passing through the openings between the bronchi and the pulmonary veins—openings now greatly enlarged—will enter into the cavity of the veins, and enable those vessels to assume that proportion of dilatation, which, during the increased volume of the lungs belongs to them. The air thus introduced by a thousand minute passages into the blood becomes intimately mingled with it. Partly by mechanical, and partly by chemical agency, a portion of this air, while the blood with which it is commixed is still in the lungs, is converted from the aerial into the fluid state. The consequence of this conversion is well known to be an evolution of heat. But all the inspired air is not converted into liquid in the pulmonary veins. After the passage of the blood out of the lungs, a portion of it still retains the gaseous condition; it is mingled with the blood in the form of small globules, and in this state is transmitted by the heart into the aortic system. In its passage through this system to all parts of the body, it is gradually converted into a liquid form; as this conversion takes place, giving out heat. As, therefore, the stream of blood proceeds on its course from the heart to the extremities, it receives from itself a new supply of heat, and is thus enabled to preserve the same temperature, throughout a course in which its heat is rapidly conducted from it, into the surrounding substances. When the blood has reached the capillary arteries, the conversion of air into liquid may be supposed to have been completed. The blood thus passes from the arteries into the venous system. But, it is not the blood alone which at this part enters the veins. The animal fabric is in a constant state of renovation—of waste, and repair. The substances of which the living body is composed exist in that body, in a state different from that to which their natural affinities would bring them. The waste of the system may be said to arise from the tendency, which the different parts of the body have, to yield to their natural affinities. Those substances which have yielded to their natural affinities, so as to become impnre or offensive, require to be discharged. The important office of removing from the body the impurities which are the result of decay, is in a great degree performed by the veins. Some parts of the blood itself will, in every circuit, have undergone, from the causes which have been stated, the deleterious change which unfits it for longer residence in the system. Some parts of the solids will, in the same period, have undergone the same change. These being separated, are taken up by the imbibing ramifications of the capillary veins, and, along with the blood from the arterial capillaries, enter the red veins. The introduction of these results of decay into the venous blood is manifested by an alteration of its color. It is converted from a vermilion, into a purple hue. Loaded with what may be called excrementitious matter, it is returned to the right side of the heart, from whence it is thrown by jets into the pulmonary arteries.

When in the pulmonary arteries, in the period of inspiration, the blood is subjected, for reasons which have been assigned, to a diminished pressure. It is placed, as it were, in the exhausted receiver of an air pump. Under this diminished pressure, and at the temperature it posssesses, gases are evolved from it, and a part of the blood is converted into air. In consequence of the great reduction of temperature accompanying the conversion of a liquid or solid into a gas, this evolution of gaseous matter wonld soon terminate ; but, at the instant, in which a part of the blood in the arteries is converted into air, air in the veins is converted into fluid ; and, by this means, the heat of the blood in the arteries, during the period that it supports a diminished pressure, is kept up at the temperature, at which the ebullition is coutinued, by the conversion of the requisite parts of it into gas. Those parts of the blood which are yielding to their natural affinities, which are already in a state approaching to fermentation, those impurities with which the venous system is loaded, most readily, in these circumstances assume the gaseous form. By the formation of an elastic fluid, the pulmonary arteries are enabled to assume that augmented caliber, to which they are urged in consequence of the expansion of the lungs. They are thus qnalified to take their share of the dilatation. In the succeeding expiration, the lungs, resiliating upon the pulmonary arteries, press these vessels into their former caliber ; which, however, in the changed circnmstances they can only resume, by the expulsion of some of their contents. In this state, the attenuated aerial matter finds a ready exit, through the capillary vessels of arteries which open into the bronchi, and which, in ordinary circumstances, do not carry red blood; and then, makes its exit through the windpipe. The blood now shows its liberation from the adulterations with which it was charged, by resuming the vermilion hue; by being converted from venous into arterial blood, and is again fitted to be the vehicle of heat and of nutriment to the whole system.

In this view of the subject, the lungs serve the double purpose of a furnace and a refrigeratory. Such an accumulation of heat, arising from the rapid conversion of a quantity of air into liquid, as might be destructive to the organ, is prevented, by the stimultaneous conversion of a fluid into air in the adjoining vessels; and, unless an excess of heat was generated in the pulmonary veins, to be supplied to the blood rapidly cooling in the pulmonary arteries, that ebulliton, supposed to be so necessary for freeing the venous blood from its impurities, would not take place.

According to the preceding view, the greater part of the air which we inspire, is received into the blood vessels of the lungs, and mingled with their contents; and gradually changing its form is circulated, with those contents, through the whole system. The part of the air which has been inspired is discharged from the windpipe in an undecomposed state. On the contrary, the greater part of the air expired has proceeded from the venous blood returned to the lungs, and consists of the usual gaseous products of the vegetable and animal fermentation. These are carbonic acid gas, nitrogen gas, hydrogen gas, to which may be added a watery vapour, of which the source is more uncertain.

Having given an outline of our hypothesis, we are now to adduce the proofs by which we hope to establish the truth of the different parts of it. This attempt it seems advisable to commence with an examination of the chief hypotheses which have been maintained respecting the sources of animal heat. It will be sufficient to confine this examination to some of those of a recent date, and having still a great share in the formation of the general opinion. For this purpose, we shall select the opinions of Lavoisier and of Crawford, Theories, which superceded all the preceding, and of which all the subsequent are only modifications.

Having proved that combustion was effected by the union of oxygen with an inflammable body, and surveying, from the splendid edifice which he had raised on the ruins of the doctrine of Phlogiston (long deemed an impregnable fortress in the philosophical world) the extent of the sovereignty he had acquired in the natural kingdom, Lavoisier threw a penetrating eye upon the phenomena of respiration ; and pursuing the analogies which appeared between it and the process of combustion, was led to the conclusion, that respiration was a slow combustion, by which animal heat was supplied, and conseqnently claimed it as a province of his own.

The atmospheric air had been ascertained to be composed of nitrogen and oxygen gases, in the proportion nearly of four of the former to one of the latter. The air emitted in expiration, was found to be composed of nitrogen and carbonic acid gases. From these facts, it was inferred that the air inspired had undergone a decomposition, or a separation of its constituent parts, during its residence in the lungs ; that the part consisting of nitrogen gas was returned unchanged, while the oxygen uniting with the carbon, supposed to float loosely on the venous blood, formed with it carbonic acid gas, which, along with the nitrogenous gas constituted the expired air. What seemed to give great force to this supposition was, that the first experiments on the subject made the consumption of oxygen just equal to that which was required to form the quantity of carbonic acid gas expired, Hatzenfratz found that the quantity of oxygen, which disappeared in the process of respiration, was greater than what was necessary to form the carbonic acid expelled. But a watery vapour was expelled also. The excess of oxygen was supposed to unite with the hydrogen (also according to supposition, floating loosely in the blood with carbon) forming water, which was discharged in the shape of vapour. The heat evolved by the conversion of oxygen gas into a liquid, minus that quantity expended in converting the carbon with oxygen into carbonic acid gas, and the hydrogen with oxygen into vapour, was supposed to constitute that heat, which being carried along with the blood in the course of its circulation, gave to every part of the body an equality of temperature, in all circumstances. The separation of the hydrocarburet from the blood was supposed to convert it from venous to arterial blood, and to change it from the purple to the vermilion hue.

Dr. Crawford, of Glasgow, about the same time, or perhaps before Lavoisier, was led to the same views; but he carried his theory further than Lavoisier had done, and certainly gave it a more perfect form. He did not limit the production of heat to excess evolved in the reduction of one gas, over that consumed by the formation of others, but he contended that by the removal of hydrogen and carbon from the blood, its capacity for heat was changed; that in passing from the venous into arterial blood, that capacity was increased, and that it therefore employed a greater quantity of heat, without increasing its temperature. But he contended, that the capacity of the arterial blood thus formed, began to diminish as it advanced in its progress ; that it became, as it were, more venous ; and that process of diminishing capacity continued, till it returned to the heart again ; and that in this way the temperature of the blood was preserved, notwithstanding its necessary communication with the cooler substances, by which, in its course, it might be surrounded.

The foundation upon which the doctrine of Crawford was raised—the diversity between the capacities of venous and arterial blood for heat—has not stood the test of the rigid examination to which it has subjected. Indeed, if there is any difference in those capacities, that difference, I suspect, will not be found in favour of the hypothesis of Crawford. We have to return therefore to the narrower view exhibited by Lavoisier, and which, with some variations, is still the doctrine of the day.

The fundamental parts of this doctrine are, that two substances in the form of gas constituting atmospherical air are separated in the lungs ; that the oxygenous part, after uniting with the carbon or hydrocarburet floating in the blood, is discharged in the form of carbonic acid gas and vapour, and that the nitrogen gas, the other constituents of atmospherical air (for I pass over the small quantity of carbonic acid found in it) is returned with the newly formed carbonic acid gas and vapour, through the windpipe.

Such objections to this hypothesis, as appear to me to be insurmountable, present themselves at the very outset. The combination must take place with carbon, supposed to exist in the blood in a disengaged state, not by coming freely in contact with the air, but through an exceedingly thin, and it may be porous membrane, and must take place in a few seconds. The slight transient change of colour, which is observed on the surface of venous blood contained in a bladder, exposed to a stream of oxygen gas, is the only fact by which this most improbable supposition is supported. But why does not venous blood assume the vermilion colour, when exposed to an atmosphere of oxygen gas without any intervening membrane? Though it may redden a little on the very surface at first, it becomes in a short time, instead of arterial, more decidedly venous; instead of vermilion, its colour becomes that of a deeper purple. What proof is to be found that carbon exists in a disengaged state in the blood? It is well known that carbonic acid is contained in the venous blood in great abundance. The experiments of Mr Brande, Professor of Chemistry to the London Royal Institution, make the quantity to be four cubic inches of this substance, when converted into gas, belonging to every ounce of blood. Though I conceive the amount here stated to be greatly over-rated, there can be no doubt that carbonic acid exists in the blood, in considerable quantity. If I understand aright the doctrine of Berthollet, carbon could not exist in this state in an entirely disengaged form, it must already have combined with some oxygen, which would diminish the force of its attraction for the oxygen of the atmospheric air. But it is only the carbon in contact with the fine membrane that could be affected: what existed in the interior of the stream, how small soever, would not be combined. Such a rapid formation of so great a quantity of carbonic acid would be accompanied with other phenomena. There is no other known chemical operation, which, in all its circumstances, is similar to this now supposed. The only argument in support of it is, that oxygen has disappeared, which can in no other way be accounted for. But this is a deceptive mode of reasoning, and can never be relied upon excepting in the science of pure mathematics. When Dr. Hales found that the venous blood, though flowing with the same velocity as an equal column of arterial, had not the same momentum, it was this species of reasoning that induced that philosopher to conclude, that the motion of the blood was exempt from those laws, by which the motion of all other substances is regulated. But it is now known that the motion of this fluid, notwithstanding the correctness of Dr. Hales' experiments, is made in strict comformity to the laws of hydraulics. But proofs are supplied I think, by Lavoisier's own experiments, that carbonic acid is expelled in breathing, in circumstances, in which the oxygenous part of the composition could not be supplied from the inspired air. In his remarkable experiments on guinea pigs, he found that air containing only one part of oxygen in fifteen, the rest being nitrogen, was sufficient for the support of these animals for any length of time, and with their usual vivacity, provided the carbonic acid that was formed, was instantly separated from the expired air, and prevented from being again inspired; and, in this case, the carbonic acid continued to be expired, in the same manner as when these animals breathed an atmosphere containing nearly one fifth of oxygen. No one will surely contend, that the oxygen necessary to form the acid, was in this case taken from the inspired air. When the proportion of oxygen is so small, its further separation from the nitrogen becomes an operation of great difficulty, requiring the most powerful chemical agents. Supposing, contrary to all experience, that the whole of this oxygen were separated, it would still be insufficient to form the quantity of carbonic acid, which seems to have been expired in as great a quantity as when the animal breathed an air containing many times the amount of oxygen.

We have here then a decided proof, that the carbonic acid gas discharged in expiration does not always require the inhaling of oxygen for its formation. But it may be urged, that the inspiration of oxygen must take place in the lungs, as the nitrogenous gas is expired in a separate state, or in a state containing still less oxygen. But what proof is to be found that this nitrogenous gas is wholly the identical nitrogen that had been inhaled? The experiments of Jurine of Geneva afford, in my opinion, complete proof to the contrary. When that gentleman inspired an air consisting of pure oxygen, he found that the air expelled in the succeeding expiration, still contained nitrogen, and nearly in as great proportion as when atmospherical air had been inhaled; and further, when atmospherical air was breathed, a greater quantity of nitrogen gas was discharged in expiration than had been admitted into the lungs in inspiration. In the first case, no nitrogen nor any thing from which it could be formed was inspired. The nitrogen gas expired must have been derived from other sources than the air; and, in the other instance, when atmospherical air was inspired, a portion, at least, of the nitrogen gas expired must have been derived from some other fountain than the air inhaled in the preceding inspiration. Nitrogen gas, therefore, may be discharged without the necessity of supposing a decomposition of the air in the lungs. All the constituents therefore of the air expired, are expired, in certain cases at least, in which it is impossible they could, in any part, be derived from the air previously inhaled. There is no occasion therefore for the highly improbable supposition of a decomposition of the atmosphere in the lungs, to account for the phenomena of expiration.

Supposing still the theory of Lavoisier, or any modification of it which has hitherto appeared, to be true, the phenomena of animal heat are not explained by it. All the heat which respiration supplies is supposed to be evolved in the lungs. The heat of the blood in the lungs must be greater, than that of the same blood when it has arrived at the extremities, especially if the surrounding atmosphere has been cold, or considerable evaporation has taken place from the surface of the body. But this is not sanctioned by the observation which faithful experiment has enabled us to make.

The hypothesis then which has for its foundation the decomposition of the atmospheric air in the lungs must necessarily be altogether rejected. In the subversion of this hypothesis, we shall be found, I trust, to have made considerable progress towards the establishment of that which has been proposed. We have not only obtained the negative proof in favour of our hypothesis, that no other has been found to be true, but some part of it has even been established. It has been ascertained, that nitrogen and carbonic acid gases can find access into the bronchi without entering by the windpipe ; and that there is no other conceivable source from whence these gases could have sprung, excepting the blood in the pulmonary vessels. It may I think be safely inferred from the preceding examination, that the air inspired may find its way from the bronchi, without being transmitted back through the windpipe, or without being decomposed. But the proofs we have hitherto had an opportunity of adducing of this important part of our inquiry are not clear and decisive. We are next then to adduce those proofs, by which the existence of the supposed passages for the air from the bronchi into the pulmonary veins is believed to be established. We have first to observe that the existence of those passages is rendered probable by the analogy of structure. But in opposition to this supposition, it will no doubt be urged that if the lungs be removed from the body, and fully inflated by blowing through the windpipe, air ought to pass through the supposed channels into the pulmonary veins, but that it does not in these circumstances do so. In answer to this objection it may be observed that when the lungs are dilated in this manner, only one class of vessels belonging to the lungs is distended. No force is applied to dilate the blood vessels. On the contrary the pressure upon their external surface is by this operation increased. When the lungs are taken out of the body they are in a state of collapse. The vessels of every description contained by them are reduced to the smallest caliber, that their contents will permit them to take. The venous capillaries supposed to pass from the bronchi, being empty, will be nearly closed. Pressure, in that case applied to their mouths, will not open them, but close them still more securely, by making the sides of those mouths act as valves; especially if, as most certainly will be the case, these vessels enter the bronchi with some degree of obliquity. Hence it happens that when part of the gut of an animal is inflated with air, it is found to be air-tight; although it is well knowu, that between the internal and external surfaces of the gut, there existed passages, through which during the life of the animal the nourishment was transmitted, in its course from the alimentary canal to the blood. Hence, it appears to me, that there was a great defect in an experiment lately made by Majendie to ascertain the vascular structure of the lungs. That physiologist inflated the lungs with air to their utmost extent, and when they had been kept in that state, till they had become dry, and had lost all elastic power, he sliced them in expectation of finding all the vessels, in the condition in which they exist during life. But one class of vessels, the air vessels, were alone dilated on this experiment. The course of these vessels so far as it extended, might have been seen to advantage, but by no means the further communications which they might have had with the veins. Had he dilated the lungs, not by blowing into the windpipe, but by removing a part of the asmospherical pressure from their external surface, he would have imitated the process of nature in expanding those organs, and all the internal vessels, both sanguineous and aerial would have been proportionally dilated. The slicing them when dry might then have been expected to have exhibited a true picture of the pulmonary structure.

The existence of the communications contended for is supported, by the analogies supplied by comparative anatomy. The air vessels in many insects, as in the locust and silk-worm, are observed to communicate freely with the blood vessels, from which the return of air or any liquid is prevented by well adapted valves.

The opinion, that air passed directly into the pulmonary veins in breathing, was maintained long ago, by many physicians. In the year 1739, Berthier of Bourdeaux published a work, on the passage of the atmosherical air into the pulmonary veins, which, on its appearance, was crowned by the faculty of medicine. I have not had an opportunity of examining the work itself, but, from the short abridgement of it, contained in

Haller's faithful record of all preceding physiological research, it appears that substances capable of being inspired, and at the same time of indicating their presence by sensible qualities, when mixed with blood, were received into the lungs with the air; these substances were found upon examining the animal after death, to have passed into the blood, In this way finely levigated powder of stone and of metal, minute particles of lead, and numerous other substances were found to have obtained a passage from the lungs into the pulmonary veins, and the left chambers of the heart. Haller appears to have agreed on this subject with Berthier, and the same opinion has been advanced in our time by the late Mr. Abernethy, of London.

It has generally been supposed that, when an animal is drowned, none of the fluid in which the animal has been immersed enters the windpipe. This opinion has arisen from the circumstance, that no water is found in the windpipe, or any of its branches of an animal recently drowned. The opinion, that water does not enter the windpipe of an animal in the act of drowning, is erroneous; it does so freely; in the various experiments which I have made, to ascertain this point, I invariably found the lungs greatly distended; that they never collapsed when the chest was opened, and that the usual collapsing of these organs was prevented by their being filled with water, an incompressible fluid, instead of air. I drowned rabbits in milk ; I found the lungs distended without collapsing in the least. The whole substance of the lungs was as white as the milk in which they had been immersed. The milk I found had reached the left auricle of the heart. The reason why water is not found in the windpipe of an animal that has been drowned, or in any of its ramifications, appears to be this ; while in the water, the body of the animal is subjected to the weight of a column of water, in addition to that of the the pressure of the atmosphere. This additional pressure acts agaiust the diaphragm and ribs, and lessens the dimensious of the chest. The dimensions of the lungs are lessened to the same degree. But when the drowned animal is taken out of the water, the pressure made by the column of water, which the body supported when immersed, is removed ; the chest expands to a certain exteut, and the lungs, necessarily following this expansion, become enlarged, and imbibe, from the trachea and its branches, the water, with which they were filled during the immersion of the body. This conclusion has been, in the most satisfartory manner, confirmed by the experiments of M. Piolet, of Paris. M. Piolet submersed various animals in the prussiate of potash, and in all cases ascertained the presence of that substance, in the pulmonary veins ; in the left auricle and ventricle of the heart ; in the aortic system ; and all the secretions which derive their origin from that system. M. Piolet ascertained, by numerous experiments, that pulmonary absorption applies to gases, vapours, intoxicating fumes, and to putrid miasmata.

The experiments of Dr. Meyer, of Germany, made prior to those of M. Piolet, also prove that liquids poured into the windpipe, readily find a passage into the pulmonary veins, and into the left auricle and ventricle of the heart, with the blood of which they mix.

The next question is, what becomes of the air which is admitted into the pulmonary veins during inspiration. Some eminent Chemists of the day, particularly Dr. Thomson of Glasgow, admit the absorption of atmospherical air by the lungs, but contend that it is decomposed, and that the nitrogenous part of the air is afterwards discharged. The only circumstance in support of this opinion is the existence of nitrogen gas in the air expired, separated as it is supposed from the oxygen ; but all the arguments which have already been advanced against the decomposition of the atmospheric air in the lungs, bear with equal force against this supposition. It would appear further, that the gas which had entered the pulmonary veins could not be returned, in consequence of the valves with which there is every reason to suppose these vessels to be furnished ; and a strong argument in favour of the existence of such valves is, that air, when blown into the pulmonary veins, is not readily made to pass into the bronchi. The air taken into the blood vessels in the way supposed must be received in infinitely divided portions, and must be intimately mingled with the blood. The particles of air may be said to be brought in contact with the particles of blood, particle with particle. It may be absorbed by the blood, it may be chemically combined with some parts of it, a combination which would be greatly favoured by the manner in which the substances are brought together ; or, it may be mixed with it, still retaining its gaseous form. There is every reason to suppose that each of these processes takes place in part. By the operation of the two first, a portion of the air will be changed from the gaseous to the liquid form while it is still in the lungs, and heat will necessarily be evolved ; but the temperature of the blood in the lungs is prevented from rising beyond a certain degree by a process which has already been alluded to, and which will be more particularly noticed in the sequel. The remaining portion of the air drawn into the pulmonary veins, and existing still in the form of gas, is transmitted, with the blood in which it floats, through the heart into the arterial system. As it proceeds in the system, it is gradually changed by the two processes already noticed, from the aerial into the liquid form; and, as this is effected, heat is necessarily disengaged. This change may not be supposed to be completed, before the blood has finished its journey in the arteries; and thus a fresh supply of heat is, with every pulsation of the heart, transmitted to the ends of the system, to supply the place of that, which in the interval has necessarily been abstracted.

When the blood has passed from the arteries into the veins of the general circulation, its colour is converted from vermilion into purple. Various opinions have been advanced respecting the cause of this change. It is generally I think believed, that the vermilion or bright red colonr is given to the blood by the addition of oxygen to it in the lungs, and that on the arrival of the blood at the ends of the arteries, the oxygen has been so far abstracted as to be deprived of the power of maintaining this colour in the blood. This is supposed to be strongly supported by the fact, that the colour of the venous blood is rendered brighter by being exposed to a stream of oxygen gas. But the change of colour is effected so suddenly, as to render this supposition I think inadmissible. Besides, it does not appear that the colour of the blood in the pulmonary veins is brighter than that at the end of the arteries. The ingenious Dr. Crawford already mentioned contended that the purple colour was given to the blood by the decays of the system, which, at this part of the circulation, found admission into the blood vessels. It was objected to this supposition, that, at the passage of the blood from the arteries into the veins, such results of decay as are supposed could find no access to the blood; as, said his opponents, all such impurities must pass through the absorbent or lymphatic system, which only becomes connected with the blood vessels near the root of the veins. But this objection will now, I trust, be admitted to have no force. The absorbent property of the veins, for the purposes supposed by Dr. Crawford, will be found, it is expected, to be fully established, in the subsequent article on absorption. With this part of the doctrine of Crawford, I fully acquiesce. Loaded, in the way supposed by Dr. Crawford; with the products of decay, the blood returns to the lungs where these products are discharged from it into the bronchi, and thence out of the system through the windpipe, in the form of impure air. Thus purified, it is transmitted into the pulmonary veins, resumes its vermilion hue, and becomes again the vehicle of heat, and of nourishment, to the system.

An important service seems to be performed, by the impurities to be discharged from the blood assuming in the pulmonary arteries the gaseous form, and one more intimately connected with our present inquiry. By the conversion of a great portion of the inspired air into the liguid form in the lungs, it is easy to suppose that too great a quantity of heat might be at once disengaged. But the synchronous formation of gas in the pulmonary arteries, is calculated to employ the excess of heat which might thus be generated in the lungs. The lungs therefore act in a double capacity, as a furnace to supply heat when it is deficient, and as a refrigeratory to remove it when it is excessive. The heat generated by the conversion of atmospherical air into a liquid form is necessary for the separation of the impurities with which the venous blood is loaded, for it maintains the blood, notwithstanding the cooling process of the formation of gas from it, at that temperature at which the ebullition, though favoured by a diminished pressure, could alone take place. These processes have reciprocally a necessary action upon each other. It is only when the heat of the blood has reached above a certain degree, which may be called its maximum, that the cooling process commences; and when it is below this maximum, the heating process goes on unchecked, until that maximum is exceeded.

Such are the advantages which animal existence receives from the wonderful process of respiration.

In one important point, one in which it essentially differs from all former inquiries into the sources of animal heat, the preceding view is supported, and indeed confirmed, by the labours of Dr. Edwards ; I mean that in which it is maintained, that the air inspired passes directly into the mass of blood, and, that the air expired, consisting in none of its constituents of the air which had been inspired immediately before, proceeds *from* the mass of blood.

The following is the statement of the conclusions to which Dr. Edwards has been led, extracted from the recent translation of his work, On the Influence of Physical Agents on Life, by Hodgkins and Fisher.

" 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 the 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 alsorbs 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."

The work of Dr. Edwards consists of a detail of experimental investigations. The peculiar and transcendent merit of this work, is, the wonderful skill with which he has contrived to disengage every agent in its turn from having any influence on the phenomena under review; and has by this means been enabled to ascertain, with unusual exactness, the share which each agent has in producing a result depending in the whole, on a multitude of agents. Dr. Edwards has, successfully, applied the analytical method of reasoning, to physiological subjects. I had not read Dr. Edwards' work, till after the preceding view of the sources of animal heat had been read before the literary and philosophical society of Liverpool; and, indeed, not till after it had been published in the North of England Medical and Surgical Journal. It was not without much satisfaction, that I found Dr. Edwards' statement of the derivation and course of the air employed in respiration, to agree so completely with that which I had made. And it is a matter not a little confirmatory of the truth of the conclusion, that we had been led to it by pursuing courses of inquiry, altogether different.

A source of animal heat still remains to be examined. I allude to that intercourse which takes place between the atmosphere and the vascular system, and of which the skin is the medium. In some animals, particularly in Batrachian tribes, such a vivifying agency is derived from the functions of the skin, as would appear to be more necessary to their existence, than that supplied by respiration itself. For these animals, according to the experiments of Dr. Edwards, live longer when deprived of respiration, than they do when the access of the air to their external surface is intercepted. This agency is highly important to those animafs whose respiration is the most perfect. There are, however, numerous species of animals, to which the skin, for the purposes in question, seems to be of no use. Of this description are fishes; such animals as are inclosed by a bony incrustation or shell, or are incased in scales; and, perhaps, birds. At present, however, we are to attend to the functons of the skin, as they are possessed by the mammalia.

The most striking phenomenon respecting the skin is, the constant exhalation in the form of vapour, which is generally imperceptible; but which under exercise, or in cases of disease, becomes evident in the form of sweat. Experiments have proved that air is imbibed by the skin, and that the quantity of matter imbibed frequently exceeds that which is exhaled. Respecting the fountain from which the imbibement takes place, there can be no doubt. that it is the atmosphere; and reasoning from the analogy of what takes place in the lungs, we conclude, that the air thus imbibed, passes through the skin unchanged. According to the division proposed of the absorbents, and which will be explained at length, in a succeeding article; the air must be imbibed by the lymphatics, and is conveyed by them, after undergoing changes, more or less, by which heat is evolved, through the thoracic duct to the pulmonary vessels, there to join with the air taken in by respiration, and to aid in supplying heat and nourishment to the system.

The matter exhaled consists of carbonic acid gas, of nitrogen gas, and vapour, and is nearly the same as the air expired. This exhalation has generally been supposed to proceed from the terminations of the arteries, those terminating tubes of which the caliber is too small to admit of the red globules, but it appears to be more probable, that it proceeds from intermediate spaces, supposed to exist between the arteries and veins, which are the receptacles not only of the blood which flows from the arteries, but of those parts of the solid system which having undergone new affinities, have been let loose from their adhesion, and so find their way to the skin, either through organic pores, or, as some suppose, by an exudation or exhalation, through simple interstices. The functions of the skin seem to serve the same purpose with respiration. They are calculated to supply and abstract heat, to nourish and purify the system.

In the course of these physiological inquiries, there has been repeated occasion to remark, that no important office is confided to one set of organs alone; that, in case the most important set be impared, by accident or disease, their place may be supplied, in some degree, by another.

As the organs of animals have been formed, and the functions of these organs discharged, in connexion with a heat that is uniform, it is reasonable to suppose, that any considerable deviation from this standard of temperature, will affect these organs, and modify the functions discharged by them. In this case, the heat is said to be in a morbid condition. The foundation of the differences of temperature which exist in a diseased state of the frame, may be ascertained by a reference to the preceeding theory of animal heat. Every arrangement between the proportions in which materials are converted from the gaseous into the liquid, or from the liquid into the gaseous form, in the process of respiration, or in the course of the circulation of the fluids, will be accompanied with a temperature of the body that may be termed more or less morbid: when the conversion of gas into liquid is more extensive in a given time than usual, while that of liquid into gas is the same, or in a diminished extent; or when the conversion of liquid into gas is less copious than usual, while that of gas into liquid is the same, or to a greater degree. In these cases, the heat will be morbidly increased. The opposite condition will arise, or the heat will be morbidly diminished, when the conversion of gas into liquid is more extensive than usual, while that of liquid into gas is the same, or in a greater degree; or when the conversion of liquid into gas is greater, while that of gas into liquid is the same, or to a less extent. In some one of the conditions enumerated in the preceeding statement, the animal heat when morbid, will be found to be expressed.

The causes which effect the conversion of liquid into air, or air into liquid, in the process of respiration, may be derived either from the conditions of the fluid engaged, or the organs employed. The quantity of air which may be converted into liquid in the lungs, or in the course of the circulation of the blood, will depend upon the condition and qualities of the blood, with which the air is made to mingle, modified by the state of atmospherical pressure. The quantity of liquid, which may be converted into air in the same process, will depend upon the condition of that fluid, or its temperature, and state of atmospherical pressure. If the venous blood be in a state more disposed than ordinary to undergo the process of fermentation ; then, the quantity of gas evolved from it, will be augmented, and the process will commence, before the usual temperature has been reached, and will prevent that temperature from being attained. This will more readily take place, when the pressure of the atmosphere is light. If in these circumstances the quantity of air converted into liquid be the same, or less, than usual, the heat of the system will be morbidly low. If on the other hand the venous blood upon reaching the lungs is in a less fermentable state, and if the pressure of the atmosphere is high, the conversion of liquid into air will commence at a higher temperature, and will not be sufficient to reduce that temperature, if the conversion of air into liquid should be the same, and still less, if it should be to a greater extent. In this state the heat will be morbidly increased, or a state of fever will ensue.

If the condition of the blood in the pulmonary veins be such as to admit of a less consumption of air, or a more limited conversion of the air absorbed by it into liquid ; a smaller quantity of heat would be generated, and, in that case, the heat may be morbidly deficient. But if the condition of the arterial blood be such as to occasion, by it, a rapid absorption of air, and a ready conversion of it into liquid, then the quantity of heat generated may be greater than the refrigeratory process can reduce to the healthy standard. In this case a state of fever will be induced.

We are not, perhaps, prepared to particularize the various conditions of the blood, from which the different states supposed may be deduced. If a portion of the venous blood should approach more nearly than usual, to a state of fermentation, a gaseous ebullition will take place from the blood in the pulmonary arteries, before the temperature has been raised to the height usually required for that purpose. This will occur in typhus fever; in cholera; in confluent small-pox; in erysipelass; in the plague; in the collapse that follows intemperance in wine. In these morbid states, the body, particularly the extremities and the breath, are cold. Hence may be accounted for, the unexpected inefficacy of the attempts that have been made to restore the heat and action of the circulation, by stimulants in the collapsed stage of cholera. By vinous stimulants, a temporary excitement may be produced, but it is soon followed by a contrary condition, to a greater extent than before their application, as by their introduction into the blood, a fermentation of air will take place under a temperature still more reduced. Hence it happens, that persons exposed to severe cold, as in cases of shipwreck, have always been found to have perished sooner, if they have attempted to defend themselves, as it is called, from the effects of cold, by the use of ardent spirits. Hence may be explained the almost miraculous, and in most cases only temporary, resuscitation that has attended the introduction of water impregnated with salt into the veins, in the lowest stages of cholera. The blood is rendered by it less readily convertible into gas at a low temperature; but recovery did not in most cases take place, as the qualities of the infused substances were not, in all probability, compatible with the continuance of life. This practice, though not so successful as the first trials of it gave reason to hope, is only in its infancy, and is worthy of further application. A new road has been opened by it for the introduction of remedies, which, under the failure of every other, may be found to be safe and effective. Hence, also, may be explained the sanative effects which are represented, by some practitioners, to result from the copious drinking of water, a fluid not convertible into gas at a low temperature. Hence, also, may be explained the beneficial effects of copious bleeding in circumstances, in which, according to our experience in other diseases, it would be extremely hazardous. That part of the blood, which is characterized by the dark colour, and which is loaded with fermentable ingredients, is abstracted before it reaches the refrigeratory of the lungs. Thus the sources, from which the heat generated in respiration is cooled, are seasonably cut off.

The venous blood may be in a state that is the converse of what has been supposed. It may be too little disposed to assume the gaseous form, either in consequence of the body at the time being freer from particles in a fermentable state, in which case the blood will be less loaded with the products of decay, or those products may be from certain causes less evaporant ; or from the fabric of the blood at the time. Hence the ebullition from the blood in the lungs will not take place to a sufficient extent, under the healthy temperature. The heat will rise higher before the refrigeratory process prevails, and a state of fever will be the unavoidable effect. The supposed constitution of the blood is indicated by the blood, when abstracted, assuming, what is termed the buffy coat, and is generally shewn by blood taken from patients affected with fever and inflammatory complaints. In those cases, stimulants, though possessing the quality of being more readily evaporable, are inadmissible; as their immediate effects upon organs, already too much excited, might be deleterious, before their more distant cooling properties came into action. Nevertheless, our predecessors gave wine in enormous quantities, according to their evidence, with marked advantage, in fevers with great increase of heat. The benefit derived is, in part at least, to be accounted for, from the fermentable and cooling qualities of the wine. The sanative effects of the wine, it is fair to admit, may in the cases in which it was so profusely administered, in the last century, be ascribed to its diluent properties, as wine was the only liquid from which the patient was not restrained. The safer practice in such cases is the reduction of the heat, by the use of cold applied internally, by cooling drinks, and externally by the cold effusion so elegantly and forcibly recommended by the late Dr. Currie of this place. By the cold effusion, the skin is made to perform the part in which the lungs are found to be deficient.

We perceive, from the preceding statement, how remedies, seemingly the most opposite, and incompatible, may be admitted, to produce the same effects, after a better knowledge of the manner in which they produce those effects, had been unfolded.

The constituents of the venous blood, which require to be withdrawn from the system, before another circuit is peformed by it, are not all susceptible at the common temperature of the body, at least of assuming the gaseous or vapoury form, and therefore cannot be discharged in the process of respiration. For these substances the constitution supplies other outlets. The chief of these are the kidneys, the liver and the skin. The saline fixed substances no longer required, or not retainable without injury, are carried off by these emunctories in the form of urine, bile, or sweat. The urine and matter of perspiration, proceed from the arterial blood. It is not agreed among writers, whether the bile is secreted from the blood of the hepatic artery, or that of the vena portarium. If we are to be influenced in our opinion by analogy, we would be inclined to the belief, that the bile is formed from the hepatic artery. By these various channels, the products of decay are carried out of the system. The frugality of nature in employing matters which are useless and would become deleterious to beneficial purposes, is exhibited here also. The bile serves to stimulate the intestinal canal, and in that way, and perhaps in others, to promote the purposes of digestion. The sweat lubricates and cools the surface. The secondary purposes, as they may be called, which the urine serves, are not so manifest.

I have stated that the animal heat will be affected, not only by the state of the blood in connection with the degree of atmospherical pressure, but also, by the condition of the organs employed. By the organs employed, must be meant chiefly the lungs. Whatever affects the elasticity of the lungs will neccessarily have an influence upon the extent to which air is introduced in inspiration, into the mass of blood; and upon the extent in which air is abstracted from the blood in expiration, In consequence, the heat of the system must be much influenced by the condition of the lungs. The formation and progress of hectic fever, may, I think, be explained from the impaired mechanical structure of the lungs themselves.

We have hitherto been considering the heat of man, and of the animals which are usually ranked under the class of mammalia; but, for our present purpose, at least, a more correct designation of that class would be, that of animals with elastic lungs. Our attention is now to be directed to another class of animals; and, to examine briefly the sources of the heat of birds. The usual and healthy heat of birds, is much higher, as is well known, than that of the animals we have been considering. The heat of man varies from 96 to 98 degrees of Farenheit's scale; that of birds ranges from 105 to 107 of the same scale, making a difference of nearly ten degrees of heat.

The chief cause of the difference of heat in birds and in man, is, that the lungs of birds are not elastic. There exists in the lungs of birds no machinery, such as has been shewn to be possessed by the respiratory organs of the mammalia, for removing any part of the pressure of the atmosphere from the venous blood, during any period of their respiration. Before then, an ebullition of air from the blood, or the conversion of any part of the venous blood into gas, can take place, a higher temperature is required. In other words, before the refrigeratory process commences, a higher temperature must be attained, than is required for this process in animals, whose lungs are elastic.

Though I consider, what has just been stated, as the chief cause of the higher temperature of birds, there are other peculiarities in their structure, which are calculated to give this cause its full effect. They are covered with feathers, which are bad conductors of heat. The structure of the skin is such as to preclude any considerable exhalation. That heat which in animals with elastic lungs, when it rises too high, would be conveyed off, finds in birds no exit, by the skin.

The peculiar structure of birds is thus well calculated to prevent the expenditure of heat. We are now to consider, the means by which the heat of these animals is generated, and distributed through the system. The air which is inspired by birds, does not, as in animals with elastic lungs, enter into the sanguineous circulation; for the mechanism by which it is introduced into that circulation, is not to be found in them; but is conveyed by separate channels, into every part of the body, as completely as it would have been had it been mingled with an arterial current. These channels, termed air cells, are extended into the cavities of the bones, which are filled by that means chiefly with air, instead of marrow; into the interior of the hardest bone; of the bills, and claws; and are continued out of the body into the quills; and, in a word, pervade the whole structure, in such a way, that every minute part of it is penetrated by them. The air which is inspired by birds forms a distinct circulation. The office which in animals with elastic lungs, is performed by the arterial circulation alone, is in birds divided between the arteries and air cells. It is the conversion of the substance, circulating in the air cells from the gaseous form, that generates the heat of birds. This would appear to take place chiefly in the minuter cells. This air, like the arterial blood of the mammalia, contributes to the nutriment of the system; and, in becoming either a fixed part, or forming moveable juices, it is necessarily divested of its gaseous form.

What may be the chemical affinities, or mechanical agencies, by which the supposed conversion is made, I know not; but this air, while passing into pores so minute as scercely to admit a single particle, is in the situation most favourable for the influence of those affinities, and of that agency. Whether the office performed by veins, is like that of the arteries also divided in birds, I will not pretend to say. The anatomy of these animals, has not been prosecuted with this view.

Without entering fully into the causes by which the fluids are circulated through the frame of birds, it may be proper to observe, that gravity is calculated to have a greater effect in circulating air through the structure of birds, than it can have upon the motion of the blood in the mammalia. According to the hypothesis that has been made, the air in the extreme cells or bony passages is constantly undergoing a change into the solid or the liquid form; the air thus changed occupies a smaller space, the space left by the conversion of air must be occupied by some other substance; the walls of the extreme air cells are constructed chiefly of unyielding materials; air from behind readily, in obedience to the laws of gravity, fills the space left by the conversion or removal of the preceeding occupant. The evolution of heat ensures a fresh supply of the fluid by which that evolution is fed.

If we consider for a moment the form of birds, the length of their limbs and claws, and of the necksfrequently subtended by bills of enormous size, it is impossible to suppose, that heat could be maintained in these remote parts, by the formation of it at the centre. It must be generated in these remote parts themselves. It is from this cause that the long-necked, and equally long-limbed, heron stands for whole days on the brink of a half-frozen stream, watching the precarious approach of its prey, with a patience and attention that are incompatible with any great annoyance from cold. The limbs and talons of these animals are not covered with feathers, and are formed of materials which are not bad conductors of heat; nevertheless they are not benumbed by the half-frozen current, in which they are so long immersed. It is the formation of heat in this manner, that enables the eagle to dwell with impunity in regions of eternal snow, and to ascend into heights in the atmosphere, where the intense cold is incalculably increased in its sensible effects on the living body, by the rapidity of the aerial current against which even it often delights him to fly. He carries with him into these regions a furnace which sets at defiance the utmost rigour of the elements.

ON ABSORPTION.

Offices, various in their nature, arising from different causes, and accomplishing different purposes in the animal frame, have received the name of Absorption. Absorption, however, in general, may be defined, to be the function by which matters are taken up by extreme vessels from one part of the body, and conveyed to another; either for the purpose of being discharged out of the system, or for further employment in it; either in a moveable or fixed condition. The existence of this function has been admitted by physiologists, from the earliest times. Previous to the middle of the last century, absorption was universally supposed to be performed by the veins; although other vessels, by which a considerable share of it is now admitted to be performed, were discovered long before that period. By the veins, the nutriment was supposed to be taken up from the alimentary canal, and conveyed to every part of the body for its support. These vessels were also supposed to be the channels through which the waste of the body was discharged. Indeed, until the period that has been mentioned, the veins containing blood; and, before the discovery of the circulation, the arteries, believed to contain air; and, if they may be called vessels, the nerves, were the only vessels by which the system was supposed to be generally pervaded.

Aselli, an Italian anatomist, when dissecting a living dog, in 1622, observed vessels containing a milky fluid, running on the surface of the intestines, and mysentery. Surprised and delighted with this appearance, he immediately abandoned the pursuit for which the dissection of the living animal was instituted, and killed the dog, that he might have a better opportunity of observing and tracing these vessels. But, to his astonishment and disappointment, these vessels entirely disappeared with the life of the animal.

By what he saw, however, at this transient glance, he was convinced of the existence of new and unknown vessels. He eagerly engaged in the search after them; and discovered those vessels, which from the colour of the fluid they contain, he denominated lacteals, a name which they still retain. Though ignorant in a great measure, of the course and termination of these vessels, he believed that they acted as absorbents; and that a part at least of the nutriment from the alimentary canal was conveyed by them. The lacteals were soon afterwards found to unite as they proceeded, and to form larger vessels after the manner of the veins, and at last to constitute a single trunk, terminating in the left subclavian vein. Vessels similar in fabric to the lacteals, and differing from them only in appearance, arising from the colour of the fluid they contained, were discovered in other parts of the animal frame, by authors belonging to different countries, and at different periods.

These newly discovered vessels contained a pellucid fluid, and from that circumstance were called lymph-ducts, or lymphatics, a general term under which what were then termed lymph-ducts, or lymphatics, were then designated. These vessels were found, by their junction, to form larger vessels, after the manner of the lacteals, and at last to unite with the lacteals in constituting the thoracic duct.

It is not my intention to give a history of the progressive steps by which a knowledge of the lymphatic system was obtained, although every name that had any share in making this great discovery, second, only as a physiological discovery, to that of the circulation of the blood, ought, and, I trust, will be held in immortal remembrance. In the state now described, it came to the hands of Dr. William Hunter, of London, and of Dr. Monro, of Edinburgh, by one, or by both, of those writers independently, it was conceived that the lymphatics, of which the lacteals were a part, pervaded the whole frame, and that the office of absorption was in a great degree, if not wholly, performed by them. The claim to priority in the advancement of this doctrine, gave rise to a controversy that was conducted with uncommon acrimony. Mr. John Hunter, who adopted not only the opinions, but supported the claims of his brother, subjected those opinions to the test of experiment. By a series of experiments at once the most simple, the most ingeniously contrived, and the most satisfactory, that are perhaps on record in any branch of science, Mr. Hunter proved, that the lacteals and lymphatics, at their radical extremities, diverge upon surfaces from which they imbibe whatever is presented to them, provided it be of sufficient tenuity to enter their mouths, and that no part of the substance which they absorbed, and which was placed upon the same surfaces, was imbibed by the veins. Hence he inferred, that the whole office of absorption was the exclusive work of the lymphatics, and that the veins to which this work had been previously ascribed, had no share in it.

The opinions of Mr. Hunter were almost universally received by physiologists, and the only matter connected with the lymphatic system, that seemed to require further investigation, was the structure, course, and situation of the vessels composing it. This part of the labour, a most important and difficult one, was discharged with singular industry and success, by Hewson, Cruikshanks, and other pupils of the Hunterian School, and Dr. Monro, of Edinburgh, already mentioned. Some distinguished exceptions still remained to the doctrine of absorption being exclusively performed by the lymphatics. Haller, though not unacquainted with the labours of Mr. Hunter on this point, still adhered to the opinion of his Master, Boerhaave, that absorption was the work of the red veins. As these, however, died away, they had no successors; and for a period, I suppose, of nearly fifty years, the doctrine, that the office of absorption, was exclusively discharged by the lymphatics, was admitted, without the sound of a single voice being raised, to disturb the general harmony. Within the last few years the conclusions of the British physiolgists have been questioned; particularly by Majendie, Delisle, Dyupetren, and the physiologists of France. The French physiologists have certainly proved, that the veins act as absorbents, in certain circumstances.

In a work which I published in the year, 1815, a view of the causes of the motion of the blood in the veins, was presented to the public, different from that which had been received. This view, as it ascribed to the veins the power of acting as absorbents, was favourable to the doctrine, that they were employed as such. And in that part of the work, in which the process of nature for uniting divided surfaces, and for the union of recently separated parts, is attempted to be explained; the absorbent property of divided veins is clearly recognized. Since that period, the experiments of the Philosophers of France, already mentioned, have decidedly proved that, in certain circumstances, the veins act as absorbents; but they have proceeded farther than this, they contend that the principle part of the office of absorption everywhere, and in all conditions, is accomplished by veins. That the nutriment from the intestines is taken up by the veins, as well as the lacteals, and that even matters deposited in the cavities of the bowels, have been taken up by the veins, while the lacteals seemed to have no share in the operation.

In carefully examining the experiments made, and arguments advanced, by the abettors on each side of the controversy, with a view to draw a true conclusion from them; taking for my guidance the peculiar physiological views which I have adopted, which indeed have originated in a great measure with myself, and on which as a safe conduct I can confidently rely, I have, I think, been able to reconcile the conflicting testimony seemingly arising from these experiments, and to propose an hypothesis which they all harmoniously tend to support.

The hypothesis, or, if it may be so called, theory, which I have to propose, is simply this ; the office of absorption is twofold ; it consists, in the first place, in conveying from surfaces, both internal and external, whatever is fitted for the nourishment and repair of the system ; and, in the second place, in taking up and carrying off such constituents of the arterial blood, as may be unfit for the former purpose, or such matters as having for a season constituted a part of the system, and discharged a useful purpose, have, by the changes they have undergone, or by solution of their adhesion, become useless and noxious, and cannot be longer retained with benefit or safety. I allude solely at present to the matters which are or may become contents of the sanguiferous and lymphatic systems. The first office, that of supplying the system with nourishment, is, I contend, performed solely by the lymphatics, considering the lacteals and imbibers of the lungs, a part of them ; the latter, or that of depurgating the system, by the red veins.

The supposition is reasonable, if not quite necessary, that offices so distinct and incompatible, should be performed by different vessels. What was intended for the refitting of the frame could not conveniently be convey-veyed in the same vessels, with what may be in a manner termed excrementitious. For the contact and commixture of the latter, for any length of time, would infallibly disqualify the former for its intended purpose. Taking this view of the case, the result of the experiments of Majendie and other French physiologists, and of those of Hunter and Monro, apparently so irreconcilable, may, I think, be accounted for. In all the instances in which, according to the French physiologists, the red veins were found to absorb, the structure of the veins had been violated. In the experiment made by Majendie and Delisle, an experiment much relied upon, in which a piece of the gut of a living animal was insulated, and all the vessels connected with that part divided, excepting a single artery and vein, liquids injected into the insulated portion were found to pass into the abdominal veins, but were not at all discoverable in the lacteals. The venous structure of the insulated portion of gut was violated. The passage into the veins was opened, while the influence to which the lacteals owe their absorbent property was destroyed, according to the conditions of the experiment. In those cases in which substances taken into the stomach, were found to have obtained a passage into the veins, without passing through the lymphatics, the communication between the stomach and the intestines was intercepted by firmly applied ligatures. It is contended, that in these cases the fabric of some of the veins of the stomach, or of the intestine above the ligature was violated, by the application of the ligature, and that it was through these violated veins, that the matters found access into the venous circulation. In the celebrated experiment of Majendie, the *experimentum crucis*, in the opinion of that philosopher, in which poison embedded in the foot of a dog, while every access to this foot had been previously cut off, except by a simple artery and vein, was found destructive of the life of the animal; the poison was applied to a divided vein. In the hundred experiments and upwards that were made, by Dyupetren in support of the doctrine of venous absorption, the matters absorbed by the veins were introduced under the skin. It is very easy to perceive, how in these experiments, the matters found a passage into the veins. Camphor, assafœtida, and other fœtid substances, that had previously been introduced into the alimentary canal, are said to have been detected in the veins, by the organs of smell and taste, but not in the lacteals;—such a detection, would, I think, require very delicate organs. Besides, before the animal was opened, all the snbstances that had entered the lacteals may have passed into the veins. In some of these experiments, some of these substances were detected in the urine, and not in the blood. As well might it be argued that in these cases, the subtances had reached the urinary organs without passing through the circulation, as that in the other cases they had reached the veins without passing through the lacteals. In an experiment made by Tiedman and Gmelin, the celebrated physiologists of Germany, who seem to have adopted the opinion of the French physiologists respecting venous absorption, quicksilver injected into the lacteals, after passing the mesenteric glands, was found to have mingled with the blood in the vena portarum. From these experiments, these physiologists infer, that the veins arising from the mesenteric glands act as absorbents of the chyle. These experimenters admit, that by the column of injected mercury, the lacteals composing the gland, might be ruptured, but that to afford the supposed access to the vein, a corresponding vein must be ruptured, a supposition they contend so improbable as to be altogether untenable. But, I would ask these gentlemen, how was it possible to rupture a lacteal vessel without rupturing a vein?

It is stated, that Flandrin, the Professor of Veterinary Surgery at Paris, had repeated the experiments of Mr. Hunter, and that results very different from those stated by that great physiologist had been obtained, and such as were not at all favourable to the doctrine of lymphatic absorption. The experiments of Flandrin are not detailed, but I will venture to assert, that the statement is either altogether fallacious, or that the process of experimenting by Flandrin had deviated, in some particulars from that of Mr. Hunter, and that it is to this deviation that the difference of result is to be ascribed.

It may be safely concluded, I think, that the veins, in the sound state of the system, do not absorb or immediately take into their cavities any substances that are placed on the external surfaces of the alimentary canal, upon the surface of the skin, or on that of any of the internal membranes. From all these sources, substances only which contribute to the growth and repair of the system, are taken. This is solely the office of the lymphatic system, considering, as I have before stated, the imbibers of the lungs as a part of that system ; but with respect to the office of these, the imbibers of the lungs, we shall afterwards have a better opportunity of speaking. But there is another very important part of the office of absorption, that of taking up and carrying off the waste of the system, which I shall endeavour to shew, belongs chiefly, if not entirely to the red veins. With this view it will be necessary to ascertain the terminations of the venous systems ; and, first, to enquire in what manner the veins by their radical extremities, communicate with those of the arteries. In some parts of the body, it is admitted that the arteries communicate with the veins through the intervention of cells. This is known to be the case in the corpora cavernosa penis, in the clytoris, in the mammæ, &c. The force of analogy would lead to the conclusion, that this was the way universally in which the union was made. Nothing to contradict the analogical argument is drawn from experience. The finest injections, and all the devices of the most skilful anatomist, supply no evidence on the subject. The argument in favour of a direct communication of the arteries with the veins in all other parts of the body, excepting in those stated, and which is the generally received opinion at present, is solely derived from the supposed necessity of that structure, for enabling the heart and arteries to propel the blood through the veins. How is the blood propelled through the corpora cavernosa penis, and other organs in which the communication between the arteries and veins is admitted to be made through the medium of cells? So important did the answer to this question appear to Dr. George Fordyce, that this eminent physiologist maintained, that the powers by which the blood was circulated in the veins, were in his time totally unknown. Bnt the blood in the veins is now known to be moved by other causes, than those supplied by the projectile force of the heart and arteries.

The argument drawn from the supposed necessity falls to the ground, and leaves the argument supplied by analogy in full force. My belief is, that the same structure prevails in every part, and that the arteries transmitting their contents to veins, terminate in cells, from which the veins arise. The capillary veins drink up whatever comes into those cells, and to that office their absorbent power in the sound state of the system is limited. The part of the blood required for the repair of the system, in those cases when the bulk of the body is stationary, when it is neither in a state of increase nor decay, must be equal to that which, having submitted to new affinities, has become detached from its adhesion, falls into the spaces over which the venous absorption has influence, and is made to mingle with the blood. And in the cases in which the system is in a state of increase or decay, it must be more or less than what the veins absorb. It was an important discovery of the late Mr. John Hunter, and in the opinion of the learned Dr. Bostock, in which I concur, the most important which that eminent physiologist has made, that any part of the body, even the hardest bone, when it increases in bulk, does not receive that increase by an accession of matter to its outward surface, but that every, the most minute portion is augmented comparatively with the whole, by the accession of particles to that portion. Nature observes the same process inverted, when any part is diminished in bulk.

The increase or renewal of structure, can only be drawn from the arterial blood, into which the lymphatics, the lacteals, and, as I contend, the imbibers of the lungs, ultimately pour their contents. It is evident, that a part of the arterial blood becomes at every circuit at the extreme terminations of the capillaries, a fixed part of the system, added to or taking place of those particles already fixed, or in the act of separation from their adhesion, and about to be absorbed by the veins.

It is evident, that every the most minute portion of the system is reached by an artery, and lies within the imbibing influence of a vein. It is contained within, or has access to, the channel continuous to this artery and vein. What applies to each individual portion, must equally apply to the whole in its aggregate state. The fixed parts of the body, then, are all inclosed by the vascular system. They are contained within it; or, what appears to be the same thing, have access to it. During every circuit which the blood makes, a portion of the fixed or solid parts of the system, has become detached from its adhesion, by the influence of new affinities, and falls within the reach of venous absorption. The matters so detached approaching to a putrescent state, amalgamate with the blood conveyed from the arteries, and convert it from a vermilion to a purple hue. That this change of colour takes place at the moment the blood passes from the arterial into the venous system, is evident from this ; that blood taken from a small artery, at the greatest possible distance from the heart, bears as bright a red as that taken from the aorta itself; and the blood taken from the smallest vein, at a point equally distant from the heart, is of as deep a purple as that abstracted from the vena cava. A further proof that the colour of the blood is changed in the extreme vessels, is supplied, I think, by the simple operation of bleeding with leeches. Though the greater part of the blood abstracted in this way, is of a deep purple, it is often streaked with blood of a bright red. In the first instance, the blood is supplied by the capillary veins, in the last a small artery has been wounded. The venous blood loaded with the products of decay is conveyed to the pulmonary arteries. These products are in a state ready to assume, under favourable citcumstances, the gaseous form. They are converted into air in the process of respiration, while still in the pulmonary arteries, in a manner that has been fully explained in the preceding article on animal heat, and are expelled in this form through the windpipe, in the succeeding expiratory process. Such is the origin of the air that is expired, and such is the way in which the body is relieved, in a great measure, from its useless and noxious ingredients.

A property in my opinion, appertains to capillary vessels, which, so far as I know, has not been noticed by physiologists, and on which their absorbing faculty in a great degree depends; I mean the extent to which these vessels are calculated to resist compression, without the rupture of their coats.

The circle formed by the transverse section of a cylindrical tube, may be supposed to be composed of a great, though defined, number of arches, which, when the circle is of considerable size will approach nearly to a straight line or will be arches with a small curvature. If the substances composing the circle be very pliant, as that formed by the section of a large vein or lymphatic trunk, the resistance which the arches will make to any pressure directed from without, will be inconsiderable. The vessels will in such circumstances sustain a collapse and be easily rendered imperviable. But as the circle diminishes in diameter, the curvature of the arches of which it is supposed to be constructed, will increase. The resistance which the circle will make to external pressure, will be directly as the curvature of the arches, and inversely as the pliability of the substance composing the circumference of the circle. If we suppose the pliability of the vessel to continue the same, whatever be its diameter, or to increase, though not at the same ratio with the curvature of the arches, comprising the circle formed by the transverse section, we shall be unavoidably brought to a point, as the diameter of the tube diminishes, at which the compression of the vessel will be impracticable, without the rupture of the vessel. From this cause it arises, that the permeability of these vessels is the more effectually secured, the smaller they become.

It may be contended that the coats of these vessels; however minute, are stretchable, and that the pressure upon any particular part may cause the coats of that part to be stretched to such a degree without rupturing them, as to admit of the internal surfaces being brought into contact, by which the permeability of the vessel would be destroyed. This does not appear probable; but admitting it to be true, it can only take place when the pressure is partially applied, not to pressure applied like that of the atmosphere to all parts of the external periphery of the tube equally. The atmospherical pressure can only diminish the bore of a vessel of this description, not close it; as the structure is required and calculated for defending the vessel against the collapsing effects of atmospherical pressure, and to make that pressure available to the securing of the entrance of fluids into the tube, instead of compressing it; no contrivance can be conceived better adapted for the purpose.

It was a knowledge of this property that was alone wanted to render complete and unassailable, the view of the causes of the motion of the venous blood, founded upon a supposition of a diminished resistance in the front of the column, occasioned by the elasticity of the lungs and the automatic expansion of the heart, that has been so long before the public.

It is owing to this property of the capilliary absorbents, that all the animals who dwell on the surface of this globe, are not squeezed to the form and consistence of a mummy, by the pressure of the atmosphere.—It is in consequence of this, that the whale is enabled to support the weight of many hundred fathoms of water, in addition to that of the atmosphere; as the pressure upon the internal and external surface of every vessel, and on the periphery of every particle of matter composing its frame is rendered equal.—It is owing to this property that fluids are found to be imbibed by the absorbents and circulated without aberration through the smallest organs of the infusiora, a class of animals whose whole fabric is too minute to be discernable, excepting through the medium of extensively magnifying powers. —It is in consequence of this property perhaps that the pressure of the atmosphere reaches and influences the motion of the fluids in the centre of the largest and hardest oak of the forest.

It seems to be a necessary property of vessels, continually decreasing in diameter, as they ramify, that their termination should be by open points. Though they were to diminish according to this law *ad infinitum*, they could never be completely shut. Their terminations may be upon surfaces; into the cavities of other vessels; or upon the inclosures of cells. According as these are the terminations of absorbents, as veins and lymphatics, or of propelling vessels, as arteries, they imbibe or exhale, whatever is of sufficient tenuity to pass these open points. Whether the imbibers have the property of selecting materials, according to their sensible qualities, is, I think, doubtful.

It has been generally supposed that, what is called vitality, or the principle of life, preserves the body when alive, from that state of putrefaction, which it soon reaches after life has been extinguished. It has for some time been the fashion among physiologists, whenever they found themselves at a loss to account for anything, to solve the difficulty by ascribing the effect to vitality, or as the French call it, the *organique vitale*. In this course, they are perfectly safe; for as no one knows anything of vitality, there exist no means of refuting its supposed operations. A shadow carries no marks of blows however severely inflicted; and if you cut off its head, as happens to some newly discribed animals, another will come in its place. Thus the stomach is supposed to resist the influence of which digests its contents, in consequence of its being pervaded by the principle of life. The blood is moved in its course by causes which could move no other fluid, in the same circumstances, in consequence of its being alive. The living body is supposed to be prevented from passing into a state of putrefaction, in consequence of its vitality.

Vitality is to the physiologists of the present day, what *faculty* was to the philosophers of the days of Queen Anne. This resource of the philosophers was not taken from them by argument, but by the sarcastic wit of the Dean of St. Patrick, who in one of his treatises, I think, the Tale of a Tub, maintained, that his spit had the faculty of roasting meat. The protection which the system enjoys against putrefaction, may, I think, be accounted for, without having recourse to vitality, in the sense in which it is now used. During life there exists a machinery, by which the products of putrefaction are removed out of the system, before they become offensive, although those products may at that time be more abundant, in consequence of the higher temperature; but of this machinery, the body after death is deprived. These products are in a great measure breathed out of the system.

Such parts only of the waste of the system, as are readily capable of assuming the gaseous state, are removable by respiration. The other parts of that waste, which are not convertible into gas at the usual temperature of the body, and a reduced atmospherical pressure, find an exit by other emunctories; the chief of which are the urinary and biliary passages. If we take into consideration, that the matters forming every expiratory process, proceed from the blood, and are, with the urine, the bile, and, to a certain extent, the insensible perspiration, the products of waste; the rapidity with which the animal fabric is renovated, will appear far greater, than what is usually supposed.

As the lungs are the channel through which, in the process of renovation, a great part of the expenditure of the system is removed; so are they the conductors, through which a considerable share of the materials, by which that expenditure is replaced, are supplied. It was contended in that part of this work, which treats on animal heat, that the air which is inspired passes directly into the pulmonary veins, and thence through the left side of the heart into the general circulation, undergoing changes both chemical and mechanical in its course. This view to which I had been conducted by a process of reasoning, founded upon known and admitted data, has been confirmed by the experiments of Dr. Edwards, of Paris. The inspired air mingling in the course of the circulation with the chyle introduced from the alimentary canal, contributes to the replacement of those particles, which, at the ends of the vascular system, had lost their adhesion, and fallen into the vortex of venous absorption. It would appear that the body may be nourished for a considerable time, by the inspired air alone. Animals have been found to live for weeks, after the thoracic duct has been ruptured, otherwise rendered impervious, or removed. Hence an argument was drawn in favour of other channels, through which chyle might be conveyed into the blood vessels, besides the lacteals ; a supposition which must now appear unnecessary. Indeed in some very old people, it would appear to be the only channel through which nutriment can be supplied. It has been found by dissection, that persons in extreme old age, must have lived for years in the enjoyment of health, without great emaciation, and in the full possession of their faculties, after the thoracic duct, and the lacteal system generally had been converted into impervious bone.

The life of birds is sustainable for a longer period without food than that of the mammalia. Birds, though under the incessant exertion required for their suspence in, and transit through, the air, have been met with in some parts of the ocean, in which they must wandered for weeks without perhaps an insect to devour. They have been fonnd alive in receptacles in which they had been contained for weeks, without any possible access to food. Nourishment and heat is conveyed to these animals more abundantly by respiration, by means of the air cells which are extended to every part of the body. This is the cause of their not being exhausted by the long continuance of their exertions on the wing.

Some parts of the body are not supplied with lympatics. This is known to be the case with the brain. As the brain has no access to the external air, or to any surface from which nourishment can be directly drawn, lymphatics, according to our view of their use, would have been useless and unnecessary. The brain is not the organ through which any fresh supply can be conveyed to the system. The want of lymphatics in the brain affords an argument of no inconsiderable force, in favour of the truth of the use to which these vessels have been destined in the preceding pages. That absorption takes place in the brain there can be no doubt, but it is that kind of absorption by which the products of decay are removed. The water in the ventricles is in a perpetual state of renovation. Substances, a short time before taken with the food, are discoverable by their sensible qualities in the water of the ventricles of the brain. The ventricles must in this case be placed with that cellular structure, which, in other parts of the body, intervenes between the arteries and veins. They are cavities to which the venous absorption has access.

From the preceding examination, it may, I think, be concluded, that no proof has been given, by the advocates of venous absorption; that the veins imbibe from sound surfaces any substance placed upon those surfaces, whether internal or external; or that the veins receive from the lymphatic system any portion of its contents, excepting through the medium of the thoracic duct; that the doctrine, implying that the lacteals and lymphatics reach with open mouths all the surfaces of the body, whether external or internal, from which they imbibe whatever is placed upon those surfaces, and of sufficient tenuity to enter their mouths, remains unimpeached; that the inspired air passes through the imbibers of the lungs, into the current of the circulation, where, undergoing changes, both chemical and mechanical, it amalgamates with the chyle introduced from the lymphatic system, improves, in all probability, its assimilation, and contributes, in an important manner, to the nourishment of the body; and that the veins absorb, by their radical extremities, not only the portion of arterial blood, which has not been expended in the repair of the system, or in exhalations, or in the formation of secretions; but also the products of decay, which the arterial blood had been employed in replacing; and that the venous blood, charged with these products, and converted by them from the vermillion to the purple hue, returns to the lungs; in which it is relieved from the products of decay, in the form of air to be expelled through the windpipe, at the succeding expiratory process; and in consequence of that discharge, and of the introduction of fresh air from the atmosphere, resumes, upon its passage into the pulmonary veins, its lost vermillion hue.

ON MUSCULAR MOTION.

Of the cause which puts the universe in motion, and of the manner in which it connects itself with, and influences its agents, we, physically speaking, know nothing. It, however, always subjects its agents to certain fixed laws, from which no deviation is suffered ; insomuch so, that if any deviation from them should be observed, it receives the name of miraculous. The discovery of any of these laws has always been considered as the highest and most felicitous effort of the human mind. Of the cause of gravitation, and of the manner in which that cause acts upon the falling stone, we are ignorant ; but the law which that stone in falling observes has been accurately ascertained, and the discovery of it fills one of the most important and brilliant pages in the history of our species.

With respect to the cause which produces motion in the limb at the will of the possessor, or of the chain by which it connects itself to the muscle producing the motion, we are totally ignorant; but, if nature pursues the same course with regard to muscular motion that she has been found to do in all the instances in which her laws have been detected, the motion of the muscles must be subjected to some law from which no deviation is admissable, and from which all the phenomena arising out of that motion may be explained.

A muscle in a state of contraction, or in that situation in which it produces motion in substances attached to it, shortens itself. The shortening which a contracting muscle undergoes can only be occasioned by the particles composing it making a nearer approach to each other. It is well known that the particles of which the hardest bodies are composed are never actually in contact; that between these particles there exists a repellant power, which, after they have passed a certain limit in their approach, increases at a great ratio; that the particles of homogeneous bodies, after they have passed a certain limit in their approach to each other, are attracted, at a greatly increasing ratio; and that, when the two powers of attraction and repulsion have been balanced, the substance assumes its permanent condition, from which it is disturbed only by the interference of some extraneous cause. When a muscle contracts, the affinity of aggregation between the particles placed in the direction of its action is augmented, the straight line lying between the centre of any two contiguous particles, and in the same direction, is shortened, or, in other words, the axis of a chain of particles, extending the whole length of a muscle, is shortened. The affinity of aggregation may be augmented in two ways, either by increasing the power of attraction, while that of repulsion remains the same, or by a diminution in the power of repulsion, while that of attraction remains the same or is increased. An increase in the affinity of aggregation is accompanied with an increase in the power of cohesion, or, in other words, by an increase in the strength of the muscle. It is well known that a slender muscle, which, when taken out of the body, will be ruptured by the weight of a few ounces, will, in the living subject, support, without injury, the weight of as many stones. Indeed no limit can be assigned to the strength of muscles; for the affinity of aggregation increases with the effort which the stimulus of distension excites; and when muscles have been found to be ruptured, by efforts to support or move inordinate loads, the tendinous portion which, in the dead body, is by far the strongest, has always been found to be the part that has given way.

The agent which nature employs for increasing the affinity of aggregation in a contracting muscle is evidently of an electrical nature; for an electrical application made to the muscles of a body lately dead, and before these muscles have parted with their irritability, produces violent muscular action in that body.

Upon the supposition that the preceding statement of the properties of muscles, or at least of the manner in which they act, will not be disputed, we venture to assume it as the foundation of the following argument.

Suppose a muscular fibre to have a fixed attachment at one end, and a moveable attachment at the other, and that this fibre contracts itself. The contraction through its whole extent is simultaneous, and not successive, or at least not sensibly so. The centre of each particle, through the whole chain of particles, approaches nearer to that of the particle immediately behind; at the same instant of time, the particle nearest to the fixed attachment, be it tendon or bone, approaches nearer to that attachment; at the same instant, the second particle approaches as much nearer to the first as that has done to the fixed attachment; but the second particle has not only to move through a space that is measurable by the difference of distance between the first and second particle in a state of contraction, compared with that distance in a state of relaxation, but has, in addition, to move through a space equal to that through which the first particle had moved in approaching to the fixed attachment. The third particle in succession has, in the same instant of time, to move through a space which is equal to the difference of distance between it and the second particle in the state supposed, and also, in addition, through a space equal to the whole space through which the second particle moves in the same time, so that the movement of each succeeding particle increases according to an arithmetical progression. The movement of the last particle, or that farthest from the fixed attachment, is composed of the movement equal to that of the first particle, with the sum of the difference of distance between all the particles in the muscular chain in the contracted, compared with that distance in the relaxed state. As the more extensive movement of the last particle is made at the same instant of time with the shorter movement of the first particle, the velocity of the last particle must exceed that of the first by the sum of the differences of the velocities of all the preceding particles. But the momentum or force with which each particle in the chain moves will, in these circumstances, be as its velocity.

If we suppose, then, instead of a single fibre, there is a bundle of fibres of the same length, having all a fixed attachment at one end; and suppose that a number of bodies of equal magnitude be fixed, at equal distances from each other, upon this bundle or muscle, placed horizontally, and that one of these magnitudes is placed at the end of the muscle most distant from the fixed attachment; and suppose that in this situation, the muscle contracts, these bodies will move with a momentum that will increase according to their distance from the fixed attachment in the ratio of an arithmetical progression, and the momentum of the last body, or that at the top, will be composed of the momentum of the first, together with the sum of the difference of the momenta of all the intervening bodies. Let it next be supposed, that the bundle of muscular fibres just mentioned have a fixed attachment at one end, that at the other it is free, but that longitudinally it adheres to a substance which will not admit of being shortened, but that this substance is jointed and of course may be bent.

When the instrument to be moved is long and jointed, it generally does not happen that the same muscular fibres pervade the whole length, but have fresh attachments to the successive bones. This serves the purpose of producing separate movements of these bones; but when a general movement of the whole chain of jointed bones is required, this separate attachment does not impair the effect, which is equally powerful with that which would be produced by the same muscles, connected by moveable ligaments. As an illustration of what has been stated, take the human arm ; and suppose an effort to be made to strike a violent blow, or to send to the greatest distance some missile weapon. The arm, in the first place, is bent backwards as far as possible. By the contraction of the muscles, which have their fixed attachment in the bones of the breast, the shoulder bone is drawn inward by the muscle contracting in the way that has been stated ; at the same instant the muscles which have their fixed attachment in the shoulder, and their moveable attachment in the fore arm contracts ; but this contraction does not proceed from the first position of the arm, but produces its movement, beginning at the point in which the muscles which moved the shoulder bone had terminated, those muscles which have their insertion in the fore arm and terminate in the ligaments which move the hand and fingers, commence their motion at the point at which the preceding had ended. As these successive movements are performed at the same time that the contraction of any one set of muscles is moved, the velocity and momentum increase with the succession of distinct muscles at the same ratio with which they would in the case of a continuous muscle. Each muscle in the chain has its own independent action, and it takes up the velocity and momentum where the preceding had left off, but it does not take it up in succession with respect to time, but at the same instant, and it is by the accumulation of velocities and momenta that the whole effect is produced. Hence can be explained the force with which a missile weapon issues from the hand. For giving the utmost effect to an effort of this nature, great advantage is obtained by bending the arm previously backwards. There will occur a better opportunity for explaining the cause of this afterwards, when the action of fishes is considered.

In those cases in which the stem to which the muscles are longitudinally attached, is flexible, the view that has been given of the law of muscular motion may be still more clearly demonstrated. In fishes, for instance the salmon, the muscles, subservient to the movement of the animal have their fixed attachment to the spine, near the head, and their moveable attachment to the same bone at its other extremity, the tail. They, longitudinally, adhere to the same bone, which does not admit of being shortened by their contraction, but may be easily bent. According to the law of muscular motion already stated, this figure must increase in curvature, the nearer it approaches the moveable termination. The restoration of this curvature to the straight line is the great engine by which the animal is transmitted with rapidity through the water, and often even to great heights in the air. This restoration is effected by the relaxation of those muscles which had produced the curvature by their contraction, and by the contraction of their antagonists, the muscles on the other side of the fish. It is highly worthy of remark, that a great increase of velocity, and of consequence, power, is gained by the spine being bent in a contrary direction at the instant at which the antagonist muscles begin to contract; for, while the same curve is formed, by the spine being bent the other way, the tail has not only to describe the space through which it has to pass, by the body of the fish being restored to a straight line, but has also to describe an equal space in addition, to form the curve on the other side. As the time required for producing this movement will not, with the same effort, be greater than that which was required to produce the curve from the straight line: the velocity and momentum will be increased at a double ratio. The velocity and force with which the body of the salmon is restored from a state of curvature to the straight line, increases with the approach to the tail in a ratio that is the duplicate of that of an arithmetical progression, and, as it may be supposed that the tail or fan of the tail is expanded to the utmost, some idea may be formed of the force and rapidity by which a fish is transmitted through the water, by the contraction of the muscles, which are the antagonists of those which had formed, by their contraction, the curvature on the opposite side. It is evidently in the period of recovery from the curved to the straight line that the powerful impulse is given. In completing the movement made by the contraction of these antagonist muscles, as a curvature similar to the first is made on the other side, a retarding power would appear to operate; but this may readily be lessened by this part of the movement being made more slowly, or by the fan of the tail being closed, or by it being placed obliquely or horizontally. This movement is necessary for directing the course of the fish. The power which the whale possesses, by the movement of its tail, so tremendous to mariners, may be conceived by the rapidity with which such a mass of matter is transmitted through the water, and with which it is often depressed to immeasurable depths in the ocean, in opposition to resistances derived from the *vis inertia* of matter and the weight and tenacity of the fluid. An example of the increase of power which is gained by the action of the muscles, by a body which, previously to that action, is bent in an opposite direction by the antagonist muscles, is supplied by the movements of the human hand. By the extension of the hand and fingers, the hand is made to form a curve outwards with the fore arm. When the muscles, which bend the hand inwards, contract, they, in this position of the parts, produce a motion, which not only increases with the distance from the fixed attachment of the muscle according to an arithmetical progression, but, in consequence of the causes which have been explained as operating in the movement of the tail of the fish, in a much greater ratio.

This rapid motion, added to the motion derived from the bending of the shoulder and arm bones, gives to the hand a momentum which is well known to pugilists, and which is less painfully ascertained by the velocity with which a stone or other missile is discharged from it, for gain or amusement.

But it is in the reptile tribes, particularly in the Ophian race, that the most complete examples of the muscular curve are to be found. By the contraction of the muscles of one side, the snake coils itself in an instant into a succession of circular foldings ; with the tail the termination of the moveable attachment of the muscles, in the centre ; and the head and shoulders the fixed attachment of the muscles approaching to a straight line, terminating the external circumference. In this condition the animal is prepared for flight or attack. By the contraction of the muscles, which are the antagonists of those by which it was brought into the coiled form, and with its tail pressed against the ground as a buttress, it rapidly transmits its whole body to a distance through the air, to inflict, perhaps, upon its unwary or deluded prey, a mortal wound. A more pleasing, and to us, fortunately, a more familiar, but, perhaps, not so perfect, an example of the same curve may be observed in the tail of the colly-dog when he approaches an antagonist in an attitude of defiance, or when he runs up to his master in the confidence of receiving the wonted caress.

From the law that has been proposed of muscular action, it will appear that the power of muscles, *ceteris paribus*, will be as their length. But the same effect which a muscle of given length would produce, may be produced by a succession of muscles acting upon the same moveable or jointed stem. Hence may be explained the immense strength possessed by animals of great length, as by the great boa constrictor, which, by making the convolutions to produce a fixed attachment at both ends and contracting the muscles, the length of which are equal to that of the animal, or, which produce the same effect, by a succession of insertions and attachments, can break the spine and the limbs of the strongest animals of the forest. Whether any of the muscles of this animal extend with uninterrupted fibres from one end of the animal to the other, I am not sufficiently acquainted with the anatomy of these animals to determine. Perhaps the carcase has not been examined with a view to that determination.

Various opinions are entertained respecting the actual change which a muscle undergoes while it is contracting. Some are of opinion, that the particles approach in all directions without displacing any substance between them. In this case the specific gravity of the muscle would be augmented. It does not appear, however, from the experiments, which have been made to ascertain this point, that the specific gravity is altered during the action of the muscle. It would appear then, that a muscle increases as much in diameter, as it is shortened in axis, during contraction. Microscopic observations have detected, during contraction, and in the direction of the axis, the approach of points, between which angular bulgings in a zigzag form occur, which increase the thickness of the muscle. It is sufficient for my argument, that the muscle is shortened during contraction. By the approach of these points, which would seem to constitute the essential muscular structure, the adhesion and strength of the muscle are increased, and at the same time the muscle is shortened, and power given to it, in the manner that has been explained.

By the view that has been given of the laws of muscular action, a facility and an increase of interest will be given to the researches of the anatomist. By simplifying the arrangement and giving a more natural purpose to the objects of research, the knowledge which is at present irksome in the attainment and easily forgot, will be more pleasant in acquisition and maintain a larger and more accurate hold on the memory.

I conclude the present very defective account of muscular power by a brief statement of the doctrine which at present prevails among physiologists with respect to it. Dr. Bostock, in a recent publication, has in this, as indeed in almost every other department of physiology, presented a perspicuous and comprehensive view of this doctrine. Since the days of Borelli, an eminent mathematician, who flourished about the middle of the seventeenth century, physiologists have considered muscular action to be accomplished according to the laws of mechanical powers, particularly the lever. To illustrate how the powers, by which the animal machine is moved, act as levers, Dr. Bostock selects the movement of the forearm. The bones of the arm are bent by the contraction of muscles which have their fixed attachment to the shoulder bone, and their moveable attachment to the bones of the arm a little in front of the elbow joint. They reach their moveable attachment by passing through the elbow joint in form of a ligament inclosed in a sheath. The lever thus constituted consists of the weight which is at the hand, the fulcrum which is the elbow joint, and the power which is placed at the insertion of the muscles of the shoulder in the arm bone, between the weight and the fulcrum, and near the latter. To have a favourable lever, it is well known that the weight should be near the fulcrum, and the power at a distance from it. But here matters are reversed. Dr. Bostock acknowledges the unfavourable construction of the lever, but he apologizes for nature by saying, that this expenditure of power, which can be well afforded, is made for the purpose of giving animals a more convenient and seemly form. So unfavourable, indeed, is the lever that he has selected as a specimen, that if the power be reckoned 20, one part of this only reaches the weight, the remaining 19 being expended in overcoming the obstacles from position. What becomes of the other nineteen parts of the power? The sheath through which the ligaments pass must sustain a great part of them. This sheath would certainly require to be made of iron. How comfortable would such an expenditure of power be to gouty and rheumatic joints! This view of muscular motion is certainly little conformable to the usual economy of nature, which, out of simple means, surprises us with the production of astonishing effects.

Dr. Bostock contends, that though the weight is placed at a great distance from the fulcrum, velocity and, of course, power are gained by it, as in that situation it describes in the same time the segment of a greater circle than it would have done, had it been situate nearer the fulcrum. But with what an expenditure of power is the increase of velocity obtained? Dr. Bostock, in some parts of his statement, seems to approach the boundaries of a better hypothesis. He admits, that the power of aggregation is increased during the contraction of the muscle; that the increase of the force of aggregation is effected through the whole muscle at the same instant of time, and that electricity is the agent by which it is produced. Another step only seems to be wanted, to arrive at the view which I have given.

The following is a reprint of the Thesis, entitled, "De Viribus Quibus Sanguis Circumvehitur," which was published at the Inauguration of Graduates in Medicine at Edinburgh, in the Autumn of 1799, and of which mention has been repeatedly made in the preceeding Work.

DISPUTATIO PHYSIOLOGICA
INAUGURALIS DE VIRIBUS
QUIBUS SANGUIS CIRCUMVEHITUR;

QUAM,

ANNUENTE SUMMO NUMINE,

EX AUCTORITATE REVERENDI ABMODUM VIRI,

D. GEORGII BAIRD, SS. T. P.

ACADEMIÆ EDINBURGENÆ PRÆFECTI;

NECNON

AMPLISSIMI SENATUS ACADEMICI CONSENSU, ET NOBILISSIMÆ FACULTATIS MEDICÆ DECRETO;

PRO

GRADU DOCTORIS,

SUMMISQUE IN MEDICINA HONORIBUS AC PRIVILEGIIS RITE ET LEGITIME CONSEQUENDIS;

ERUDITORUM EXAMINI SUBJICIT

JACOBUS CARSON,

SCOTUS:

Ad diem 12. Septembris, horâ locoque folitis.

EDINBURGI:

EXCUDEBANT ADAMUS NEILL ET SOCII.

MDCCXCIX.

VIRO NOBILLISSIMO,

THOMÆ, COMITI DE SELKIRK;

QUI,

NATALIBUS SPLENDIDISSIMIS, SCIENTIAM,

ANNIMUM BENIGNUM ATQUE

VITÆ INTEGRITATEM ÆQUAT;

HANC DISSERTATIONEM,

EO QUO DECET CULTU, SACRATAM VULT

JACOBUS CARSON.

DISPUTATIO PHYSIOLOGICA

INAUGURALIS

DE VIRIBUS

QUIBUS SANGUIS CIRCUMVEHITUR.

Sanguis est fluidum ultimi momenti in corpore animali. Organa vitalia gratè stimulando, totum corpus in vitam motumque ciet; et fons atque origo est unde omnes partes nutriuntur, atqus omnia fluida, ad munera vitæ accommodata, secernunter. Quidcunque itaque fluidum respiciat, naturæ animatæ tam necessarium, summam attentionem ab omnibus qui rerum naturæ indagatione delectantur, optimo jure sibi vindicat. Nunc apud omnes una sententia tantûm habetur, de cursu quem hoc fluidum in corporibus animantium tenet. Harveius, nomen in omne ævum in deliciis humani generis mansurum, sanguinem in arteriis ad omnes corporis partes a corde portari, et ad idem, circulo descripto, rursus reddi, orbes perpetuos volventem, olim comprobavit. Plurima autem hoc fluidum spectantia, multis tenebris adhuc obruuntur. Vires etiam ipsæ, quibus in hoc cursu bene cognito vehitur, non aduc fortasse optimo successu indagatæ fuerunt.

In sequente disputatione, ad leges hujus Academiæ celeberrimæ exequendas, omnibus alumnis summos honores medicos ambiuntibus præscriptas, susceptâ, vires quibus sanguis plerumque circulari habetur, ad tale opus minus valentes, ostendere conari; et de aliis quæ has satis adjuvare possint, investigare, proponitur.

PARS I.

Alii, quamvis perpauci credo, sanguinem per totum cursum vi cordis propellente solâ portari contendunt; dum alii, et quidem major pars, cùm plurimùm corde ipso effici credunt, haud paululùm etiam operæ sanguinis circulandi vasis quibus continetur ascribunt. Et inter hos, aliis hoc esse munus arteriarum solùm, motu vibrante planè præditarum, placet; alii vero vase capillaria, æquè ac arterias majores motum vibrantem habere, quamvis, propter vasorum exiguitatem, vix aut ne vix quidem conspiciendum, et ita multum ad transitum sanguinis per se conferre, putant. Idem munus venis ipsis multi etiam largiuntur. His omnibus viribus operi imparibus visis, alii varias alias in auxilium protulerunt. Pressura musculorum venis incubentium ; attractio illa quâ vasa minutissima, fine altero in fluidum immisso, sese implent ; motus vibrans arteriarum venas sodales comprimens ; pondus aëris circumfluentis ; singulæ harum diversis auctoribus plus minusve potentiæ ad sanguinis motum conjicere creditæ sunt.

Quò certius compertum habeamus quantâ potentiâ singulæ harum virium in sanguinem circumagendum polleant, primùm haud abs re videtur, quid sit momentum quo sanguis in ulla cursûs ejus parte moveat, cum isto collatum, quo in quacunque alia ejusdem cirumferatur, computare.

Ne autem conspectu primo a consequentiis omninò abhorreatur quæ ab illis quas experimenta Stephani Hales aliorumque confirmâsse videantur, haud parùm discrepent ; imprimis idoneum videtur explanare quid de *momento* sanguinis intelligimus. Momentum sanguinis hìc habetur illa potentia quâ sanguis in corpore vivo polleat ad superanda obstacula, iis quibus inter circulandum plerumque subjictur, superaddita. Quò itaque verum momentum sanguinis in ulla corporis parte computetur, necessarium est animnm ad res diversas in quibus hoc fluidnm in diversis partibus circuitûs versetur, advertere. Nunc autem contenditur, et posteà demonstrari tentabitur, quòd, venâ transversè sectâ, id sanguinis inter partem sectam at arterias sodales, cujus momentum tali experimento æstimandum est, in rebus longe diversis ab his versari in quibus ante sectionem fuit. Si hoc verum sit, sententiæ quæ ex talibus experimentis collectæ fuerint, funditus ruerent oportet. Quandocunque itaque mentio fit de *momento* vel *vi* sanguinis, hæc verba semper accipienda sunt in sensu usitatissimo, jam definito; et diversæ res in quibus sanguis in diversis corporis partibus versetur, spectari supponuntur.

Quantùm sanguinis, tempore dato, in auriculam dextram a vena cava, tantùm a ventriculo sinistro in aortam funditur. Sanguinem enim a ventriculo dextro propulsum, circuitu per pulmones perfecto, vix ullum vel incrementi vel jacturæ fecisse oportet; et si pulmones in cor minus redderent quàm in eos influxisset, brevi sanguine prorsus farcirentur; si autem plus ex sese funderent, quàm recepissent, mox desiccarentur. Dehinc satis constat, tantùm sanguinis, eodem tempore per truncos venæ cavæ cor versus, quantùm per truncum aortæ fines arteriarum versus, fluere.

Quoniam autem argumentis non infirmandis certiores facti sumus, ut vires quibus fluida in tubis movent, habent inter se rationem, rationum luminis tuborum quibus continentur et velocitatis quâ ruunt, compositam;

et quoniam quantitates fluidi, quod, tempore dato, per tubos transit, eandem inter se rationem compositam habent ; planè sequitur, ut quâ ratione sint inter se quantitates per tubos transeuntes, eâdem esse oportet inter se vires quibus progrediuntur.

Dehinc momentum quo sanguis in truncis duobus venæ cavæ fluit, par esse illi quo in aorta, apparet.

Eodem modo probari potest, sanguinem in omnibus ramis aortæ vel venæ cavæ conjunctis, parte ullâ utcunque longè a corde remotâ, pari momento ac in utroque eorundem vasorum trunco, ferme pollere : nam necessarium est ut, dato tempore, eadem copia sanguinis ex omnibus ramis aortæ unà sectis, ac ex trunco ejusdem vassi solo secto, mitteretur. Idem posset de vena cava ramisque ejus, si tam multùm sanguinis ex venis transversé sectis effluxisset, quàm ante sectionem per cava eorum transibat.

Minima pars sanguinis, fatendum est, inter utrumque circuitum exhalatur, vel mutata secernitur ; et quamvis aliquid fluidi, massæ sanguinis redeuntis, loco illius amissi plerumque substituatur, antequam singulæ undæ, unde pars sejuncta fuerit cor iterum petant ; probabile tamen est, quod additamentum non semper fit eâdem parte quâ jactura ; quoniam plurimùm additamenti massæ sanguinis tribuitur propè circuitus finem, nempe quâ ductus thoracicus in venam subclaviam sese aperit. Sanguini itaque in ramulis aortæ conjunctis vel venæ cavæ, paululùm minus momenti erit quàm eidem in eorundem vasorum truncis. Sed quantitas sanguinis quæ inter unum totius circuitum exhalatur, vel in alios liquores excernendos mutatur, pro ratione totius sanguinis circulantis tam parva est, ut imminutio momenti quæ eandem toti momento rationem gerit, in hoc calculo sine damno omninò negligi potest.

Hinc nobis concludere licet, momentum sanguinis seu in trunco simplice sive in plurimis ramulis vel arteriosis vel venosis in quos hic truncus dividitur fluentis, per totum corpus idem valere. Velocitas autem quâcum momentum nequaquam confundi oportet, semper, in his rebus, rationem rationi areæ orum omnium vasorum transverse sectorum inversam gerit, et itaque pro amplitudinis hujus areæ decrescere debet.

Plurima verò repagula sanguinem inter circulandum oppugnant. Si reputemus quod ramuli minores ab ramis vel truncis, ad angulos circiter semirectos subinde separantur; quod particulæ sanguinis et vasa, præsertim capillaria, mutuò se attrahunt atteruntque; quod vasa minima sæpe in se invicem redeunt ad angulos non valde acutos; quo fit ut flumina sanguinis quo replentur in se plus minusve recte ruant, et partis virium quibus gaudent, sibi ipsis oppugnandis, jacturam faciant; quod impedimenta sanguini a corde fluenti tam magna et plurima occurrunt, ut, dimidio itineris tantummodo superato, retro arcetur in cursum illi quo primum progressus fuit prorsùs contrarium; quod denique obstaculis fere omnibus renovatis, demum post longum spatium ad cor fertur vi illi equali quâ primum ab eodem movere cæpit; absurdum apparebit putare sanguinem circumvehi vi projectili cordis solius.

Non certè dubitandum est quin arteriæ, saltem, ad sanguinem quo replentur transmittendum, haud paululum conferunt; nequaquam autem plus momenti quàm id quo antea pollebat, ei largiuntur; sed tantummodo illud ei restaurant cujus in impedimentis quibus in his vasis occurrit, superandis, jactura facta fuit. Hoc ex eo quod supra demonstratum est apparet; nempe, quod momentum sanguinis in omnibus ramis aortæ non augetur quo longiùs a corde progreditur, sed idem, quod in trunco simplice valuit, immutatum manet.

Cor itaque; adjuvatum vi resiliendi arteriarum, quâ vim illam quæ in iis dilatandis expensa fuerat fere omnem reddunt, et sic vim a corde sanguni impressam, usque ad suos fines, quàm plurimùm imminutam servant; atque vi earum musculari, quâ sanguini largiuntur novum robur, par illi quæ, ob vim suam resiliendi necessariè imperfectam et plurima obstacula opposita, impenditur ; cor, dico, his viribus ab arteriis impertitis, juvatum, ad sanguinem circumferendum momento omni quo gaudeat, quoúsque motus vibrans in his vasis conspicitur, par esse facilè conceditur.

Arteriæ quidem ad angustiam capillorum coarctatæ, nullum motum oculo, optimis microscopiis armato, exhibent. Attamen admodum verisimile est, quòd motu vibrante, quo ad sanguinem transmittendum conferunt, æqè ac arteriæ majores, præditæ sunt. Ramificatio eorum a vasis quæ ipsa motu vibrante satis evidenter gaudent, eos eâdem structurâ et dotibns cum his potiri arguit. Inflammatio autem quæ ex acribus parti cuivis adhibitis oritur ; fluxus salivæ, lacrymarum, aliorumque fluidorum, stimulis ad propria organa quæ ea secernunt applicatis auctus ; et alia multa, quæ hìc enumerare non opus est ; clarè probant, hæc vasa parvula, vi musculari gaudere ; at gradum probabilitatis, ad demonstrationem quàm proximè appropinquantem, præbent, ut stimulo fluidi intûs communi se in motum ciente, ad hoc fluidum transmittendum copiâ communi et sanâ satis valeant-

Num surculi venosi æquâ vi cum arteriosis in sanguinem per se transeuntem agant, dubium est. Rami ficatio ex vasis quæ vix saltem vibrant, hoc redarguere videtur. Inflammatio ipsa, in parte ista ubi quodvis acre applicatur, oriens, surculos venosos quamvis æque stimulatos, sanguinem non reducere in eâdem ratione quâ transit per arteriosos comitantes, ostendit, quod tamen efficere debent si æquè cum his irritabiles essent; sed si sanguinem in eadem copiâ reducerent, nulla inflammatio fieret oportet. Inflammatio itaque validissimum fortasse argumentorum, quibus surculi arteriosi musculares esse probantur, eandem dotem venosis, saltem in gradu eodem, denegare apparet.

Non certè angustiæ ascribi potest quod venæ non vibrare conspiciuntur; quoniam ampliores arteriis comitantibus plerumque sunt. Nunc si illud momenti, semper æquè valens, quo sanguis in venis pollet, multa obstacula adversus, vi a vasis ipsis ei impressâ, servandum esset; ictus venarum saltem æquè validi, atque itaque æquè evidentes, cum illis arteriarum esse debent. Quamvis enim sanguis fluit per venas tardiùs quàm per arterias quæ illas comitantur; tamen venæ, secundum proportionem majoris velocitatis sanguinis arteriosi, ampliores sunt quàm arteriæ: majorem copiam sanguinis continent; ad quam movendam velocitate quâ gaudet, potentiâ æque validâ opus est, ac ad movendam minorem copiam sanguinis in arteriis, velocitate majore quâ in his fluit.

Præter hæc, arteriæ, ut jam explanatum, vi cordis inter sanguinem per se propellendum multùm adjuvantur; dum sanguini a surculis venosis in arteriosos fluenti, minimum tantum velocitatis, propter latum spatium in quod diffunditur, restat. Secundum enim calculum KEILLII, sanguis in capillariis tam tardus fit, ut momentum cujuscunque sanguinis particulæ non pluris suo pondere bis æstimato ibi valet * ; quare necesse est ut brevi prorsus moraretur, nisi juvatus aliquâ novâ vi, vel a vasis in quibus fluit, vel ab aliquo alio agente, ei concessâ.

Quamvis itaque illud momenti quo sanguis in omnibus capillariis conjunctis præditus est, par sit illi quo in aortâ fluit; tamen id quo unaquæque particula sanguinis, ab arteriis in fines venarum transiens, pollet, ad provehendam hanc particulam, contra vires frictionis, attractionis et fortasse gravitatis, quibus ibi strenuè oppugnatur, vel in punctum spatii longius, vix par est. Multò minùs ad flumen altum sanguinis ante se propellendum, aliquid vis conjicere potest.

Cùm obstacula, sanguini per venas redeunti objecta, illis quæ in arteriis oppugnat, ferè æqualia habere licet; si sanguinem circulari per vim cordis propellentem,

* KEILLII Tentamen Med. Phys. 2.

viribus quas arteriæ atque venæ adjicere possunt, adjuvatum supponamus; pars hujus operis quæ venis solis perfici restat, æqua erit illi quæ corde et arteriis viribus conjunctis perficitur. Quomodo autem tali labore fungerentur, nullum omninò conatum exhibentes, præsertim si respiciamus nisus strenuos quos cor et arteriæ, inter suum dimidium operæ fungendum, ostendunt, non animo comprehendendum est.

A quibusdam verò contenditur, ut una unda sanguinis nunc tardior evadens, novam vim ab undâ sequente et fortiore acquirat. Hæc in sermone scholarum *vis a tergo* nuncupatur. Motus autem sanguinis nullum accelerationis assequitur ab ipsâ collisione quam duæ undæ sanguinis inter se faciant; cùm vis a corde sanguini impressa, non sit *vis percussionis*, uti vocatur, sed *vis pressuræ* *; et cum sanguini sit nulla potentia sibi propria, nec ei liceat aliæ undæ impertiri plus momenti quàm antea sibi ab externis agentibus concessum fuerit. Tali collisione itaque tanti vis unæ undæ jactura fit quantum aliæ acquiritur.

Cor autem, contenditur, vi resiliendi atque contractili arteriarum adjuvatum, in venas jam antea sanguine

* Whytt's Enquiry into the Motion of the Fluids in the small Vessels.

plenas, aliquid sanguinis omni ictu impellere ; et venas propter plenitudinem tantum sanguinis ex uno fine tradere, quantum altero receperint, oportere.

Si nobiscum volvamus, quàm ingentes vires requirantur ad superandum uno impetu omnia obstacula quibus sanguis per totum corpus subjicitur ; quod omne hoc virium contra latera aortæ prope cor et valvulas semilunares ageret oportet ; quod membranæ ex quibus aorta conflatur non robustiores sunt membranis ramorum, pro ratione majoris roboris cujus in his rebus illis opus esset ; quod hominibus qui immersi fuerant in vitam redeuntibus, totum sanguinis in motum propellitur tam debili impulsu cordis et arteriarum, quàm omninò impar esse ad promovendum omnem sanguinem in venis stagnantem, evidenter apparet ; quod, demùm omnem fluxum sanguinis copiosiorem lethalem esse oporteret ; quia venis ferè nulla vi resiliendi vel musculari præditis, sat sanguinis non restaret, iis implendis idoneum, et his non plenis nullum sanguinis ex altero fine in auriculas hoc modo propelli potuit ; sanguinem viribus cordis et arteriarum hâc viâ nequaquam per venas circulari, satis superque comprobatum habebimus.

Aliquid muneris sanguinem circumvehendi musculis venis incumbentibus ascriptum fuit. Sed quoniam musculi non nisi inter contrahendum sanguinem provehere possunt, et quoninm, musculis ferè omnibus otiosis atque relaxatis, puta inter dormiendum, sanguis circumfluere perstat; hi inter vires circulationi necessarias vix habendi sunt. Verum quidem est quod nisus musculorum validos, arteriarum pulsus frequentior atque validior plerumque comitatur. Tamen utique disputari potest, num musculi, nisibus validis factis, sanguinem accelerent quâ vasa sanguifera directè afficiant vel quâ respirationem festinantes, vim acriorem et stimulantiorem sanguini largiantur. Motus vero musculorum violentos, sanguine nequaquam accelerato, sæpe videre est. Nec bene quidem constat quomodo circuitus sanguinis, viribus respirationi subservientibus, in conatus validiores atque celeriores non actis, nisu musculorum quamvis valido festinaretur. Musculus quivis super venam sanguine repletam premens, ut id sanguinis, inter locum pressum et cor, velociùs cor versus fluat, efficit. Id autem sanguinis ultra partem quæ premitur, eodem tempore retrò urgetur; ne longum in hoc cursu fluat valvulâ obstatur; sed valvulum firmiter claudit; et itaque sanguinem ultra valvulum omninò moratur; eumque momento omni quo jam anteà movebat, privat. Musculi itaque inter contrahendum, sanguinem tam multum in unâ parte morantur, quàm in alterâ festinant.

A priore itaque ratiocinantes, colligimus, ut sanguis, musculis validè nitentibus, non velociùs sed inæqualiter flueret: et hoc esse verum ab experientiâ ipsâ docemur; nam in morbis convulsivis sine febre, ubi musculi relaxatione atque contractione alternâ maximè laborant, ictus arteriarum, quibus de motu sanguinis optimè judicare possumus, abnormes potiùs quàm veloces vel æqualiter volidi fiunt.

Si autem pressura musculorum nil sanguinem provehat, quid opus est valvularum quibus plurimæ venæ suppeditantur? Huic, locus respondendo magis idoneus infrà erit.

Arteriæ, quatenùs, suo motu vibrante, venas sodales afficiant, continuò agunt; sed effectus earum in sanguinem venas implentem, perexiguos esse, et nequaquam diversos, his jam explanatis pressurâ musculorum productis, oportet.

Vim illam, quâ vasa diametri minimi fluidum cui alter eorum finis inseritur, bibunt; et eò sese, gravitate etiam adversâ, implent, haud paululùm ad sanguinem transmittendum conferre multis persuasum fuit. Sed hæc vasa parvula tantum humoris solummodò bibunt, quantum seipsa ad certam altitudinem implere sufficiat; et fluidum jam absorptum ex se evacuare, quo iterum altero fluidi sese impleant, parùm idonea sunt. Præterea cùm in corporibus animantium uterque finis horum vasorum in fluidum inseritur, atractio capillaris sanguinem morari debet, æquo modo ac eum promovet. Hæc vis itaque sanguinis circulationi nullo omninò auxilio est.

Pondus denique aëris atmosphærici, viribus quibus sanguis circumagitur interdum accensetur. Nunquam autem, quoad ego cognovi, explanatum fuit, quâ ratione tali munere fungi potest. Quoniam verò hanc pro vi maximè potente earum quibus sanguis ad cor per venas reducitur, habemus, nunc ad modum aperiendum hujusce vis in sanguinem reducendum agendi, progredimur.

PARS II.

Primum idoneum videtur, pauca de situ et motu cordis dicere.

Thorax in duas partes mediastino dividitur. Hoc septum ex duabus membranis, ab utrâque pleurâ pulmones succingentibus, continuatis, conflatur. Ponè connectitur spinæ dorsi; anteriore parte sterno cartilaginibusve costarum. Inferiore et anteriore parte, duæ laminæ non conjunguntur nec communi insertione in sternum gaudent. Lamina dextra parti sterni dextræ plerumque inseritur; dum sinistra cartilaginibus costarum sinistri lateris, parte a sterno paululùm remotâ affigitur*. Hæ laminæ spinam retrò petentes, paulatim propius ad se accedunt, et tandem in unum ferè coëunt. Inferiore parte in tendinem septi transversi tali modo inseruntur, ut illam tensam tenent dum ipsæ tensæ tenuntur ab illa ; et ita fit, ut pars septi transversi cui cor innititur inter respirationem nec descendat nec sursum tollatur †. His parietibus receptaculum, in quo cor securè habitet, formatur. Est præcipue obliquitate sinistræ laminæ quâ hoc receptaculum fit ; nam dum dextra, rectâ deorsum descendit, sinistra a socio sejungens, iter obliquum latus sinistrum versus tenet. " Comme le cœur s'avance dans le coté gauche de la poitrine, la membrane gauche du mediastin s'etend vers cette cavité. Dans cette loge les ailes du poulmon embrassent le cœur, mais l'aile gauche le couvre seulment ; elle est plus courte que l'aile droit : il y a dans l'extremité de cette aile une echancrure qui derobe le poulmon au mouvement du cœur, lors qu'il frappe les cotés ‡." Cavum quod hoc modo formatur, non potest sine vi violentâ coarctari §. Cor et appendices, omni latere bene munita, securè in illo habitant. Motus hujus visceris nobilissimi non obnoxii sunt, qui coërcerentur seu impedirentur, vel ab injuriis externis, vel visceribus aliis, secundum varias corporis mutationes variè illud prementibus.

* MONRO Prælect. et SENAC de la Structure du Cœur, liv. 1. chap. 1.

† SENAC de la Structure du Cœur.

‡ SENAC de la Structure du Coeur, liv. 1. Chap. 1.

§ Idem.

Cor humanum, sic permunitum, in duas partes dividitur; quarum utraque duo cava, ventriculum et auriculam nuncupata, continet. Parietes horum cavorum, ex fibris muscularibus mirum in modum intertextis conflatæ, stimulo sanguinis contenti in contractionem cientur; quà cava ad nihilum ferè arctantur*. Contractiones cavorum cordis in certo ordine fiunt. Auriculæ motus concordant; et cum illis ventriculorum qui etiam simul agunt, alternant. Auriculis contrahentibus, sanguis in ventriculos conterminos ruit; contractione autem ventriculorum, viâ retro, in auriculas valvulis firmiter clausâ, in arteriam pulmonalem et aortam projicitur †.

Contractionem utriusque partis cordis, stimulo sanguinis contenti fieri, minimè dubitandum est. Quibus autem viribus iterum dilatatur nunc dicendum est.

Lex quædam, quâ omnes musculi astringuntur, est; ut hi, post contractionem quæ spasmum non induxerit, ad statum relaxationis naturalis unde detracti fuissent, protinùs resiliant ‡. Cor ab hac lege nequaquam liberum esse, satis comprobatum habemus. Cadaveribus enim insectis semper dilatatum detegitur §. Prætereà corda renarum, et aliorum animalium vitæ maximè

* MONRO. Prælect.
† MONRO. Prælect.
‡ CULLEN's First Lines
§ HALLERI Physiol.

tenacium, ex corpore omnino secta, contrahere et dilatare alternis vicibus per aliquod tempus perstant; quando dilatatio non certè fit vel impulsu sanguinis redeuntis vel aliâ vi a corde ipso alienâ *,

Præter autem vim resiliendi muscularium fibrarum cordis ipsius, animantia, aliis viribus, quibus dilatatio singularum partium hujus visceris certiùs, pleniùs et vividiùs perficitur, potiuntur. Cor, in systole, et brevius et arctatius fit; quoqueversum imminuitur †. Ulla pars cordis itaque, quum contraxerit, in angustiores limites redacta, in pectore relinquit spatium; quod ab alia materia, cum reliquo corpore æquè pressa, occupari oportet. Ad hoc vacuum implendum, vel quasdam partium cor et appendices circumdantium fitum mutare et in cor descendere, vel aliquam aliam cordis ipsius partem, pro ratione spatii contractione alteræ partis relicti, tumere, necesse est.

Ex his de situ cordis suprà dictis, et ex consilio naturæ, ne cor in motibus suis vitæ tam necessariis, coerciretur, vel ullo modo impediretur, caventis, evidenter apparet; ut partes cor appendicesque circumdantes, fixæ, tensæ, rebusque aliis connexæ, non facilè vel subitò

* HALLERI Physiol. et MONRON. Prælect.

† Apud MONRON. Prælect. Anat.

subindè e loco deprimi possunt; dum temporis momento ipso quo ventriculi sanguine pleni atque stimulati contractionem inchoant, auriculæ, contractione finità, minimè impeditæ, et resilire jam conantes, facilè celeriterque resurgunt, spatiumque ventriculis contrahentibus relictum, citiùs dicto occupant.

Inter dilatationem itaque auricularum cordis, sanguis in venâ cavâ venisque pulmonalibus, et ex eo in omnibus earum ramis ramulis que, minùs a parte cordis quàm alibi gravatus, cedit, ut omnes substantias oportet, ponderi majori, et cor versus pleno rivo fluit. Ventriculi deinde suâ vice dilatantur et implentur, eodem modo quo auriculæ; et hæc motuum nobilissimorum series, dum labatur vita, continuò iteratur.

Hoc mechanismo, consilio prorsùs divino fabricato, cor simplice conatu duplice fungitur officio: conatu enim illo quo sanguinem insigni vi ad omnes corporis partes propellit, hoc fluidum ab his partibus, flumine æquè copioso, exhaurit.

PARS III.

Demùm examinare progredimur, annon explanatio jam data de modo quo cor reditui sanguinis consulere supponitur, aliquid roboris recipiat, ab ullis quæ de cursu sanguinis aut structura cordis vasorumve sanguiferorum, vel ex observatu tradita, vel ab experimentis quæ variis temporibus à viris eruditis ad varios fines excogitata fuerunt.

1. Thorace vivi animalis aperto, pericardioque infecto, ventriculi, quod etiam accidere oportet de auriculis, nec coarctantur nec dilatantur eodem tempore; nequaquam ut anteà concordant; sed omnino irregulares fiunt *. Hinc apparet consonam illam auricularum ventriculorumque cordis actionem, non ex ulla fibrarum muscularium vel nervorum consensu pendere; sed omninò ex cordis intra thoracem situ; quo certi motus non nisi in fixo ordine fieri possunt.

2. Quapropter fit, ut valvulæ desint apud orificia venarum; cùm satis robustæ satisque idoneæ inveniuntur inter ventriculos et auriculas illis conterminas; et apud ostia aortæ arteriæque pulmonalis? Altera auricula, putà dextra, sese contrahens, sanguinem quo repletur æquali vi in venam cavam ac in ventriculum dextrum propellat oportet. Unde sanguinem redeuntem magnoperè impediret, si non paulisper omninò moraretur, vel etiam in cursum retrogradum premeret.

Secundum autem modum jam explanatum, ratio cur

* HALLERI Physiol. vol. 1.

valvulæ ad ostia venarum absunt, satis idonea et pulchra dari potest. Auricula jam plena et stimulata, contrahere incipit ; eodem tempore, ventriculus conterminus, contractione absolutâ, et propter vim suam resiliendi et amotionem gravaminis aëris atmosphærici, auriculâ diminuente, productam, turgescere externè incipit , hinc sanguis ex auricula pulsus, minus ponderis ventriculum versus quàm venam cavam versus sustinet ; quare omnis in ventriculum dilatantem ruit ; et sanguis in vena, nulla licèt valvula interponitur, haud minimè repellitur.

3. Venâ quâvis ligatâ, pars venæ inter ligaturam et finem tumet ; pars autem, inter ligaturam et cor, depletur, albescit et comprimitur *. Si sanguinem in venis fluentem per vires cordis atque vasorum, præsertim moveri habeamus ; ut pars venæ, ac in hoc experimento, ultra ligaturam tumeret, non omninò disputabitur ; ut pars autem inter ligaturam et cor depleretur, non tam facilè concedi potest. Vix ullo contenditur, venas in quarum plerisque nullus motus percipitur, sanguine plenas, propriâ potentiâ pollere, ad se ex hoc sanguinis deplendum. Vim autem a tergo, quâ sanguinem per venas propelli supponitur, ligaturâ venæ circumdatâ a sanguine ante ligaturam, intercipi oportet. Quid nunc idoneum restat ad sanguinem ante ligaturam ad cor us-

* Harveii Exercit. de Motu Cordis.

que movendum? Momentum, quod, venâ non adhuc lagatâ ei impressum fuerat, vix satis pollens haberi potest. Quamvis vero sinamus, partem venæ ante ligaturnm, hoc vel quovis alio modo, depletam fuissse; tamen ut statim ferè iterum impleretur contenditur. Licèt enim unus ramus venosus ligetur; sunt tamen alii rami, non ligati; qui etiam in truncum unde ligatus ramus oritur sanguinem infundunt. In his rebus, partem sanguinis, ab ramis non ligatis, in truncum communem infusam ad orificium ligati rami adventam, retrorsum in illum premi et ibi refluere, donec pars evacuata omninò iterum impleretur, oportet. Fortasse autem objiciatur, valvulas esse in venis ne sanguis regurgitaret obstantes. Apparet autem, ab experimentis eruditissimi STEPHANI HALES, sanguinem, in illis etiam venis in quibus valvulæ frequentissimæ occurrunt, nihilominus sæpe regurgitare; nempè, in venis jugularibus viventis equæ *. Et quamvis valvulæ injecta in cadavere impediant, res se aliter habere potest in corpore vivo. Idem autem accidere observatum fuit in venis in quibus nullæ valvulæ inveniuntur; et ubi plurimi alii ramuli ab eodem trunco oriuntur; veluti in ramo quovis venæ portarum, iliacarum internarum, et quidem plerumque in omnibus venis quæ ligaturis arctè circumdatæ fuerant †.

* HALES's Hæmostat. Ex. vii.

† Apud HARV. de Motu Cordis, et HALLERI Phys.

Depletio autem venæ ante ligaturum, ex hypothesi data, facilè explicari potest. Venâ ligitâ, id sanguinis ante ligaturum minori gravaminis cor versus, quam venam cavam versus, subjicitur. Quare cedit a parte quâ plus gravatur, 'et ad cor progreditur. Eadem causa, quæ sanguinem ante ligaturam ad cor vexit, ne ullum aliud sanguinis, in truncum communem quibusvis ab ramis infusi, ad ligaturum reflueret, obstabit.

4. Nunc autem questio de usu valvularum nobis iterum redit.

Valvulæ non in omnibus venis deteguntur ; frequentiùs in venis colli et extremitatum ; et ibi in his venis præsertim quæ sub superficiem curıunt. Si designatæ a natura fuerant, ad sanguinem, musculis contrahentibus, cor versus dirigendum ; verisimile est ut omnes venæ his extruerantur quæ pressuræ musculorum obnoxiæ sunt. Hoc autem nequaquam accidit. Quoniam plerumque in partibus, quæ causis comprimentibus objiciuntur ; uti in collo et extremitatibus ; verisimile est ut designantur obviam ire malis quæ ab his causis oriri possunt. Quò sanguis reducatur ab extremis venis, necessarium est ut venæ ab his partibus venientes in nulla parte comprimantur. Venæ superficiales brachiorum et colli, a figura partium, aptæ sunt quæ ligaturis aliisve arctè iis circumdatis, comprimantur. Possuntne itaque valvulæ seu parvulæ projectiones in cava venarum ita agere in corpore vivo, ut venam quâdam parte compressam citiùs dilatarent; ne, lateribus longiùs compressis, concrescerent obstarent, formamque tubularem venarum certiùs servarent? Si autem hæc explanatio usus ualvularum minus placeat, æquè utiles haberi possunt regurgitationi obstandæ sanguinis pondere aëris atmosphærici ac ullâ aliâ causâ reduci suppositi.

5. Arteriâ sectâ, sanguis usque ad mortem fluit; venâ autem comitante, parte æque a corde remotâ, sectâ, vix ullum sanguinis mittitur. Quare fit ut sanguis fluat ex arteria sed non ex vena? estne quia sanguis moveat celerior in arteria quàm in vena? Si autem velocitas sanguinis in arteria sit major quàm in vena sodali, cavum venæ est in eadem ratione amplius illo arteriæ. Quoniam autem velocitas sanguinis in vena tardior est illo in arteria, solùm pro ratione majoris amplitudinis venæ; momentum, cujus ratio, ex hujus velocitatis atque vasorum diametrorum quadrarum rationibus, componitur, par esse illi sanguinis in arteria necesse est. Sanguis in vena itaque, si motus fuerit vel vi cordis seu vasorum in quibus continetur, vel alia vi, post sectionem venæ agente, tam longe et æquâ copiâ ex vena ac ex arteria flueret.

Si autem sanguinem in vena sublatione aëris ambiuntis moveri habeamus, causa cur sanguis ex vena secta non fluat, evidenter apparebit. Sanguis circumfluens, cum venas petiverit, momentum a corde et artreiis acceptum fere omne dimisit. Venâ itaque transversè sectâ, sanguis apud orificium sectionis toti ponderis subjicitur aëris, ablatione cujus in motum tractus fuerat. Quare nunc fluidum ultra sectionem, tam multùm gravatum a parte secta quàm a tergo, ferè omninò sistere subitò cogitur. Eodem modo ratio dari potest, cur sanguis ex vulnere in venæ latere facto non manat: Sanguis enim ex tali vulnere manans currere obviam toti ponderis aëris debet; sed cor versus manans, a parte saltem hujus ponderis levatur.

6. Nisi hanc dissertationem longiùs extenderet, sanguine, his viribus quibus circulatio ejus plerumque ascribitur circumvehi supposito, fusiùs objici posset: Truncis venæ cavæ ita apud cor sibi obviam euntibus, ut rectam lineam fere referunt, flumina sanguinis contenti, per omne spatium quod ab initio systoles auriculæ dextræ ad dilatationem ejus ineuntem, elabitur, rectè in se omnibus viribus ruere oportere: Propter majorem copiam fluidi, eodem tempore, cavâ inferiore ad cor reductam, et itaque majus hujus momentum, nullum sanguinis superiore cava unquam cor assequi posse: Cùm ad sanam valetudinem copia sanguinis æqua illi a ventriculo sinistro projectæ, per venam cavam ad cor reddi requiritur,

auriculam dextram se contrahere contra vim ad reducendum hunc sanguinem idoneam oportere; sed ad talem laborem auriculam imparem evidenter fabricari: Sanguinem per sinus cerebri, quos omnes nunc confitentur vel ad se dilatandum vel coarctandum impares esse, non vi transmissa a tergo per teneros surculos cerebri, circulari posse: Denique cellulas penis semper sanguine distentas esse oportere; nam quamvis conceditur, eas ligamento vi resiliendi prædito circumdari, tamen attribui huic ligamento dotes quibus nullum aliud in corpore præditum est, necessarium esse; nempè, in viro a gravi metu vel anxietate laborante, potestatem sese contrahendi in diametrum ter vel amplius minorem quàm pene erecto, habuit.

Facillimum est intelligere quàm benè hæc omnia, ex hypothesi datâ explicari possunt.

7. Ante terminandum, autem, a notando unum aliud insigne beneficium ex hac fabricatione et machina cordis oriens, abstinere nequeo. Magna copia sanguinis, ad sanam valetudinem servandam necessaria, rapidè circulanda est. Vis verò quâ tantum sanguinis ad cor redit, insignis sit oportet. Si cordis solius esset hanc omnem vim resistere, hoc viscus non solum interrumpi et impediri in suis motibus obnoxium esset; sed, fabrica auricularum spectata, in discrimen rumpendi duceretur.

Subsidium cordi autem natura potentiam satis validam tulit. Tempore quo auriculæ ad idoneum gradum dilatantur et sanguine implentur, et ipsæ et sanguis quem continent, toti ponderis aëris, anteà partim lævati, subjiciuntur. Cor ipsum itaque nullum vix impulsum a sanguine reduce sustinet; inter contrahendum nullam resistentiam oppugnat; et omnes motus vivide, sine violentia autem, perficit.

FINIS.

F. B. WRIGHT, PRINTER, LIVERPOOL.

Fig. 1.

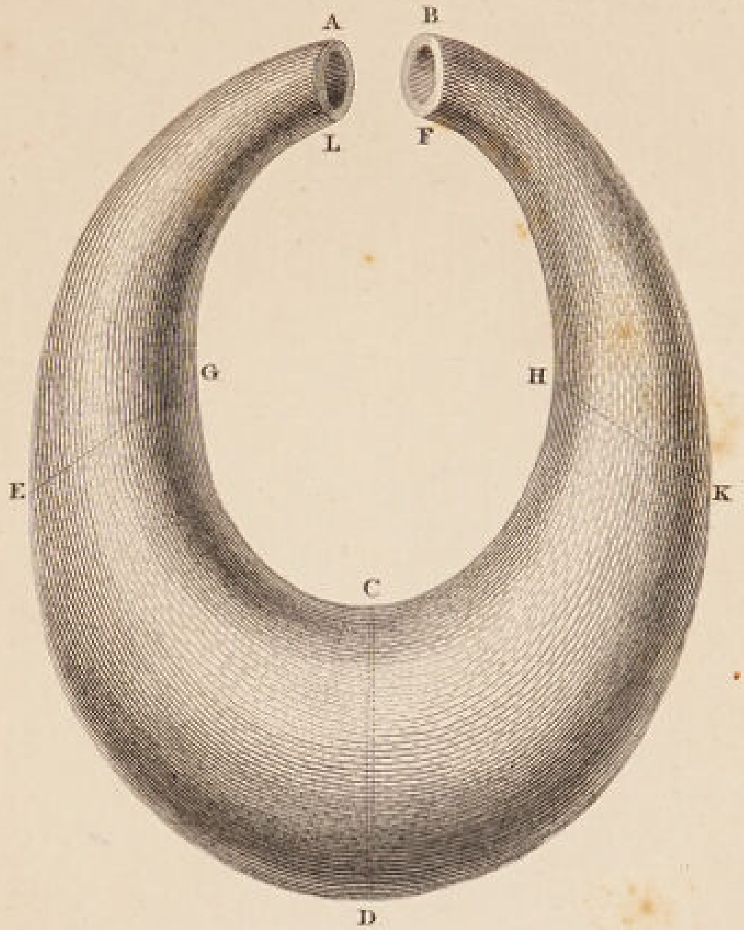

Fig. 2.

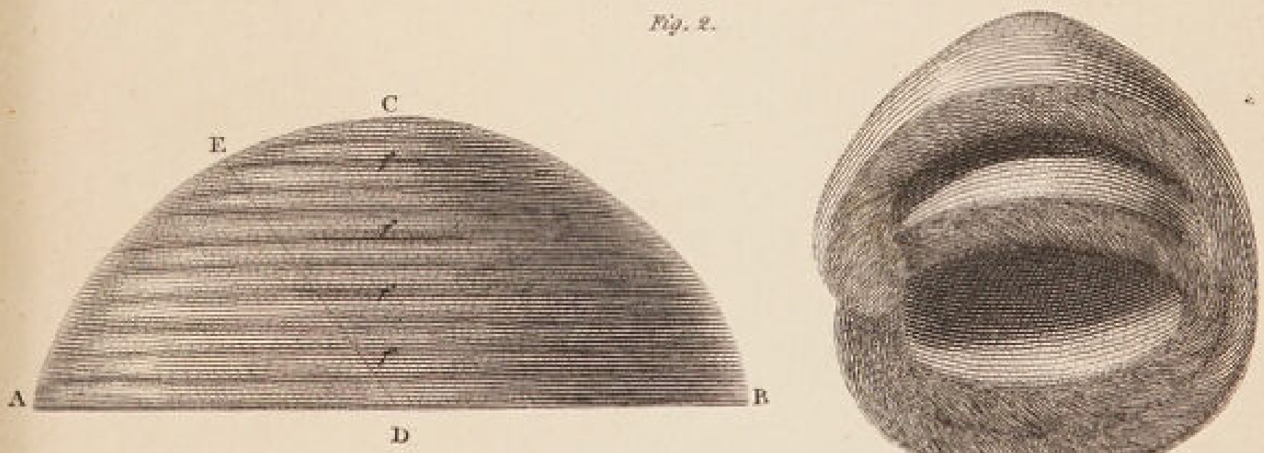

Fig. 3

Thos Smith sculp.

Engraved for Dr. Carson's Inquiry.