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A

PROBATIONARY ESSAY

ON

THE POWERS WHICH MOVE THE BLOOD:

SUBMITTED,

BY THE AUTHORITY OF THE PRESIDENT AND HIS COUNCIL,

TO THE

EXAMINATION OF THE

Royal College of Surgeons of Edinburgh,

WHEN CANDIDATE

FOR

ADMISSION INTO THEIR BODY,

IN CONFORMITY TO THEIR REGULATIONS

RESPECTING

The Admission of Ordinary Fellows.

BY

WILLIAM BROWN, JUN.

MEMBER OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH.

SEPTEMBER 1817.

EDINBURGH:

PRINTED BY J. & C. MUIRHEAD.

1817.

TO

WILLIAM BROWN, M.D.

FELLOW OF THE ROYAL COLLEGE OF SURGEONS

OF EDINBURGH,

THIS ESSAY IS INSCRIBED,

AS A TOKEN

OF RESPECT AND LOVE

FROM HIS AFFECTIONATE SON.

James Bryce Esqr
with respectful compliments

AN

ESSAY

ON THE

POWERS WHICH MOVE THE BLOOD.

THE Sanguiferous system demands a large share of the Physiologist's attention. It holds a distinguished place in the animal economy, it meets him in every point of the body that is accessible to his senses, and the deductions of reason lead him to conclude that its distribution is universal. It, accordingly, has not been neglected; but in all ages has, in some degree or other, been subjected to his strict observation and scrutiny. Since the discovery of the Circulation, which has immortalized the name of William Harvey, this subject has been even more minutely inquired into than before, and men of the brightest talents and most patient industry have applied all their energies to its

investigation. Their endeavours have not been fruitless, and they have added many new and important facts to those which were known before; but much still remains to be discovered, and until they can inform us of the essence of matter and the nature of life, or until our faculties be elevated and refined to a degree far above what they at present are, we must remain ignorant of even the first principles of the science before us.

Unacquaintance with these has given rise to a great many differences in opinion among inquirers, and has exercised much the talents and ingenuity of men, anxiously desirous of discovering truth, and yet, it is to be feared, leaning in no inconsiderable measure, towards a preconceived and favourite theory. One of the most important and widely-extended of these controversies, is that respecting the share which the arteries have in the circulation of the blood. Some physiologists assert that the heart alone is the moving power, while others, of equal eminence, consider the arteries as co-operating, by their muscular energy, in the performance of this function. It is proposed to devote the greater part of this Essay to the discussion of this subject, and afterwards to

consider shortly another question of considerable importance, viz. How the blood is circulated in the veins. In examining the former of these subjects, I shall first consider the arguments brought to support the muscularity of the arteries, and then mention such as may seem to favour the opposite hypothesis.

The arguments adduced in support of the muscularity of the arteries, are these:—1. Gangrene of the foot succeeding to ossification of the femoral artery. 2. The effects of palsy on arteries. 3. The existence of muscular fibres in their coats. 4. Their irritability. 5. Their contraction stopping hæmorrhage. 6. Fœtuses without hearts. 7. Disproportion between the heart and its alleged office. 8. Disproportion between the size of the heart and that of the animal to which it belongs. 9. Hæmorrhage would put a stop to the circulation. 10. State of the vessels after death.

I. Gangrene of the foot succeeds to ossification of the femoral artery.*

The existence of this fact does not appear to be properly established. Gangrene of the foot

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* Monro's Outlines of Anatomy, vol. ii. p. 354.

has occasionally occurred after ligature of the femoral artery for the cure of aneurism; but this arises merely from a mechanical obstruction to the flow of blood in the limb, the anastomosing branches not having become sufficiently enlarged to carry on the circulation. That this affection has occurred at the same time with ossification of the artery, cannot be doubted, as Dr. Monro has founded an argument on it; but it is hardly necessary to say, that the coexistence of two events by no means proves that the one causes the other. And if it be shown that ossification does occur without the supervention of gangrene, or without any material derangement of the circulation, this argument must fall to the ground, and, indeed, must rather appear a fact in favour of the other theory.

I quote the following passage from Richerand, in the words of Dr. De Lys's translation: "The action of the main arterial trunks, near the heart, has so little influence on the motion of the blood sent into them by that organ, that the aorta is frequently ossified, without affecting the circulation. The aorta is naturally bony in the sturgeon. J. L. Petit, in the case of a bookseller whose leg he had

taken off, found all the arteries of a certain calibre, in a state of ossification; they were indurated, and, of course, incapable of acting in the slightest degree on the column of blood which flowed along them."* Besides this case, which appears to be satisfactory, it is generally known that ossifications to a large extent are found, upon dissection, in the arteries of many old persons who were never attacked with gangrene, and never showed any symptom of deranged circulation. As the deposition of calcareous matter acts on the arterial coats by rendering them incapable of contraction, any other disease disorganizing their structure, and thus producing the same effect, ought also to obstruct the circulation. But arteries are frequently dilated or become affected with aneurism, and yet the circulation remains unaltered, or very slightly injured.

II. Arteries are affected by palsy.†

I do not know of any dissections which prove a change of structure in their coats by palsy; but the amount of the argument appears rather to

* Elements of Physiology, p. 173.

† Monro's Outlines, vol. ii. p. 354.

be as follows: The function ascribed to the arteries is improperly performed in a palsied limb; the artery in the affected limb is supposed to be palsied; but palsy can attack only a muscle; hence the arteries are muscles.

A limb under the influence of paralysis, manifests, it is true, an apparently imperfect circulation; it is cold, pale, and œdematous, and, instead of increasing with the rest of the body in size and plumpness, remains emaciated or continues to diminish. This is caused, in a great measure, by the absorption of the fat which exists in such abundance under the skin, and between the muscles; but the muscles themselves are, in the course of time, partly absorbed, and in some instances have been diminished to a wonderful extent. These facts show that the circulation is in a very peculiar state. It seems to be weakened, and to have but little of that vigour which the other parts of the body exhibit; but the pulse in many cases is equally strong as in the sound side. Absorption of the solid parts goes on, while serum is effused and is not taken up. But these circumstances may be explained with equal readiness by supposing the arteries not muscular, as by the opposite theory; for the circulation in the extremities is

so much assisted by the action of the voluntary muscles, that when they become paralyzed, we need not wonder that the blood moves with greater languor. This appears rather an argument in favour of the non-muscularity of the arteries; because were they muscular, surrounded as they are on all sides by voluntary muscles, and receiving nerves (if they have any) from the same sources, they ought necessarily to be affected in the same way; but, while the voluntary motions are totally destroyed, the circulation of the blood is merely rendered weaker. Besides, in the proposition mentioned, there are two clauses which are taken for granted, but which remain to be proved, and are indeed the questions under consideration, viz. Is the circulation carried on by the arteries? Can the arteries be affected by palsy?

III. The existence of muscular fibres in the arterial coats.*

That the middle coat consists of fibres, has been often asserted by anatomists, but they contradict each other so much in the evidence they give, that it seems reasonable either to deny

* Hunter, Monro, &c.

their existence, or to suppose that the difficulty of discovering them is very great.

Thus, Mr. Hunter says, " Every part of the vascular system is not equally furnished with muscular fibres; some parts being almost wholly composed of the elastic substance, such as the larger vessels, especially the arteries, in which, were they equally muscular with the smaller vessels, the existence of muscular fibres might be more easily proved. Neither does the elastic substance equally prevail in every part, for many, especially the smaller arteries, or what have been called the capillary vessels, appear to be almost entirely muscular; at least I am led to think so by my observations and experiments on that subject. From these I have discovered that the larger arteries possess little muscular powers, but that as they recede from the heart towards the extremities, the muscular power is gradually increased and the elastic diminished." *

In direct opposition to this extract is the account of Haller. When describing the " tunica musculosa," he says, " In minoribus arteriis sensim malignius adparet, in capillaribus

* A Treatise on the Blood, vol. i. p. 206.

denique ne lente quidem vittrea adhibita, qua rerum species augetur."* To the same purpose Bichat observes, in his account of "membrane propre des arteres," "Cette membrane est dense, serree, tres-apparente sur les grosses arteres, est moins sensible sur les dernieres divisions on elle se perd insensiblement."†

This diversity of opinion among anatomists of accuracy and judgment, tends to render doubtful the existence of fibres at all. But let it be granted that the middle coat is fibrous, it remains to be proved that these fibres are muscular. The experiments of Berzelius prove the contrary, if it is allowed to be just reasoning that parts having a similar function, ought to have a similar, or at least, not a very dissimilar chemical composition. His words, as translated by Dr. Brunnmark, are these: "In consequence of the experiments thus made, it is beyond all doubt, that the fibrous membrane of the arteries cannot be a muscle; for while the latter is soft and flaccid, and contains more than three-fourths of its weight of water, the artery is dry and very elastic; the muscular

* Elementa Physiologiæ, tom. i. p. 63.

† Anatomie Generale, tom. i. p. 278.

fibre possesses the same chemical properties as the fibrin of the blood; for instance, that of being soluble in acetic acid, and of forming scarcely soluble compounds with sulphuric, nitric, and muriatic acids; but the arterial fibre has altogether opposite qualities, viz. that of not being soluble in acetic acid, but pretty easily soluble in mineral acids, diluted with water to a certain degree, from which solution it cannot be precipitated by means of alkali, or alkaline prussiats, which are the tests for the acid solution of fibrin, &c. Consequently as the arterial fibre neither has the structure of a muscle, nor its chemical properties and composition, it cannot be a muscle, nor perform the functions of a muscle, which is, besides, sufficiently evident from its elasticity." *

Berzelius is a chemist of the first celebrity, and although, doubtless, all nice chemical examinations are liable to error, yet until the facts related in the above extract be controverted, they remain an unsurmountable obstacle to the doctrine of the muscularity of the arteries.

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* A View of the Progress and Present State of Animal Chemistry, p. 25.

IV. The arteries possess irritability.*

This argument is one of the most important in the controversy, and if substantiated, seems directly to prove that the arteries are muscles.† Most physiologists have made experiments upon it, and it has been investigated with circumspection by men eminent for talent and industry. It was to have been expected that so much ability and caution would have led to a uniformity of result; but, on the contrary, their conclusions have differed very widely. Some have been induced from their experiments to view the arteries as inert tubes, or as possessing merely elasticity, while the greater number consider them as endowed with complete muscularity, contracting upon the application of the most delicate stimuli corporeal and mental. Much of the difference appears to arise from an inattention to the kind of vessels which were the subjects of experiment. Some inquirers having discovered that the capillaries are irritable, conclude that the arteries in gene-

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* Hunter, Monro, Blumenbach, &c.

† However, this proof, if established, is not decisive, as irritability does not constitute the peculiarity of a muscle, and can be conceived as belonging to another body.

ral are so; while others finding the larger arteries destitute of this power, infer that all are in the same situation. But there is a very great difference between these two kinds of vessels.

The capillaries evidently possess a very peculiar constitution, and are governed, in some parts at least, by laws totally distinct from any at present known. They are the immediate instruments of all the hidden operations of nature; it is by them that nutrition is applied, and the various secretions carried on. And although these functions are not explicable on any of the known laws of matter, or of the animal economy, yet it would seem probable that the capillaries perform their part of the circulation by muscular action. Many experiments have been made on them, which, particularly those of Dr. Thomson,* seem to prove that *they* are irritable.

The larger arteries, however, appear to be in a very different situation. They have repeatedly been made the subject of experiment, but there has not yet been given any satisfactory evidence of their irritability. The apparent contraction which has been noticed in

* Lectures on Inflammation, p. 82.

the observations of some inquirers, may be explained on the ground of a chemical action having taken place between the stimulant employed and the arterial coats. When a concentrated acid is applied to an artery, it contracts; but this is not an animal motion, it is merely a shrivelling or tanning of the coats, which Bichat calls "un racornissement,"* which takes place equally well on the dead body, is not a contraction followed by relaxation, and is not produced by alkalies, although substances equally stimulating. The application of mechanical stimuli is not liable to this objection, and, accordingly, it has been used by some observers.

The experiments of Verschuir, detailed in his Thesis, printed at Groningen in 1766, have been appealed to as evidence on this subject, and Dr. Thomson considers them so decisive, as to "leave us no room to doubt of the existence of this property in the larger arteries of warm-blooded animals."† I feel diffident in hazarding an opinion at variance with that of my respected teacher, but I think that when

* Anat. Gen. tom. i. p. 315.

† Lectures on Inflammation, p. 81.

the subject is considered, I shall appear warranted in withholding my assent from his conclusion just quoted. I have not had an opportunity of seeing Verschuir's book, but I find an abstract of it in an Inaugural Dissertation by Richard Dennison, printed at Edinburgh in 1775. This work is professedly meant to prove the irritability of arteries and veins, and he adduces the observations of Verschuir as strongly corroborative of his opinions; he makes, however, some acknowledgments which deprive them in a great measure of weight, and lead to a conclusion, opposite to that which he wishes to establish.

The experiments are 22 in number; in 5 of these, no sign of irritability could be perceived; in 6 it was doubtful; in 11, (to use Dr. Dennison's words,) "contractiones haud paulo manifestiores, quam in sex proxime relatis, erant."* So that only one-half of the experiments succeeded. But Dr. Dennison informs us, that afterwards, when Verschuir tried to repeat them in the presence of the Professors of Edinburgh, his attempts failed; showing not his want of veracity, but the uncertainty of the

* Disput. Physiolog. Inaugural. p. 32.

experiments, and consequently, the little dependence which is to be placed upon them. Indeed, Dr. Dennison himself confesses, that they fail much more frequently than they succeed. His words are—"hæc de musculari arteriarum potestate longe quidem sæpius spem nostram fallunt, quam ad sententiam succedunt."*

V. Contraction of an artery stops hæmorrhage.†

This appears rather a fact directly subversive of the hypothesis. Every definition of a muscle supposes that it first contracts, and then relaxes, and continues in this alternate state, as often as the stimulus appropriate to it is applied. Now, the arteries are always full of blood, and upon this idea, always under the influence of their appropriate stimulus; hence they are always in a state of contraction, which is a condition of a muscle quite unknown in the animal system. Besides, the fact above mentioned proves that an artery wants the distinctive character of a muscle. Were an artery a muscle, instead of

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* Dennison's Disput. p. 25.

† Monro's Outlines, vol. ii. p. 354.

contracting to a point when cut, and thus putting a stop to hæmorrhage, as it ordinarily does when the vessel is small, it would alternately contract and relax, and thus promote the hæmorrhage until death took place. By Dr. Parry's experiments,* it is proved, that during circulation, there is no sensible dilatation or contraction in an artery, and hence, we cannot conceive of what use muscularity can be to the arterial system. It is said, that the uterus gives an example of a muscle, which merely contracts without relaxation. But the uterus can hardly be allowed to be a muscle. It is obviously an organ *sui generis*, possessed of great contractile power, but deprived of it and also of its thickened and enlarged coats, when its office is done.

VI. There have been instances of fœtuses without a heart.†

At first sight this appears a decisive argument, but upon consideration it ought not to have much weight. It is hardly fair to reason from a monster, a thing bearing in its front the acknowledgment of nature that it is some-

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* Observations on the Pulse, &c.

† Monro's Outlines, vol. il. p. 354.

thing irregular, and that we ought not to draw from it general conclusions, upon the structure and functions of a being perfect in his organs and in their uses. In a similar way it may be proved that the brain is a useless lump, because there have been acephalous monsters. The argument should be repelled altogether as irrelevant; but we are allowed to make suppositions which may, in some degree, explain the circulation in these cases. Perhaps there may have been an uncommon conformation of the placenta, which allowed the impulse of the mother's heart to affect the blood in the fœtus;—or, it may be granted that the arteries were muscular in these cases, though it by no means follows that they are so in all instances.

VII. The size of the heart is disproportioned to its alleged office.*

The heart, it is argued, is unable to move such a large quantity of blood, expended as its force must be, on the sides of so many thousand vessels, the current of the blood being changed and diverted by so many ramifications and anastomoses, and constantly pressed

* Dennison's Disput. p. 5.

upon, as the whole mass is, by every surrounding part. Although Borelli may have gone too far in estimating the heart's power as high as 180,000 pounds, yet we need not, with Keil, run to the other extreme, and suppose it equal to only five ounces. The heart's force is very great, and this is now allowed by all authors. But it does not appear that an impulse more than the heart can give, is necessary to carry on the circulation; for Hales found that a weight so moderate as the pressure of a column of warm water, only four feet and a half high, was sufficient to impel the water from the aorta of a dog, even through the exhalant vessels. Anatomists do not find a very great force necessary for the impulsion of their injections, and it must be a much easier matter for blood to flow in the living body, than for a foreign fluid to be impelled through the rigid vessels of a cold carcase.

We are accustomed, when thinking of the circulation, to consider the heart as putting in motion the blood for the first time; but, as Dr. Barclay observes, "In the living body, the circulation, already begun, is only continued; all the arteries are already full; the blood is flowing in its customary channels, in channels purposely adapted for its use, and for which it has been

purposely adapted; nothing is omitted, nothing unforeseen, to promote circulation; nothing is wanting in point of fluidity; and nothing whatever expected from the vessels that they had not performed a thousand times before. In these circumstances, the heart meets with only comparatively small resistance, and hence the impulse which it gives to the blood is instantly felt through all the larger ramifications of the aorta."*

VIII. Disproportion between the heart and the animal to which it belongs.

"Had the blood," says Dr. Carson, "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 the ox does not bear nearly the same proportion to the bulk of his body that the heart of a dog bears to his."†

Upon a little reflection, this argument does not appear stronger than the preceding ones. Is it established that there is not a proportion

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* The Muscular Motions of the Human Body, p. 569.

† An Inquiry into the Causes of the Motion of the Blood, page 37.

between the hearts of animals and their sizes? This ought first to be proved, and it is well known how much difficulty there is in accurate measurement and weight, or in the estimating of proportions. But let it be granted, the argument would hold only upon the supposition that the circulation has to meet with equal difficulties in each animal, and that it needs to be carried on with equal vigour and rapidity in each. Now, this is far from being the case. It is well known that in the dog, the circulation is in a very peculiar state, from the great rapidity of his pulse, from its frequent intermissions, and from his incapability of perspiring. The dog is a quick animal, acute in all his senses, and rapid in all his motions; on the other hand, the ox is well known as a slow, phlegmatic animal, dull in his apprehensions, and having a heavy, slow pulse. Similar differences may be discovered in the characters and habits of other animals.

IX. Hæmorrhage would put a stop to the circulation.

Had the blood been moved by the heart alone, "life," says Dr. Carson, "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."*

This argument would be conclusive, were it believed that the arteries are merely hollow tubes without life or motion, destitute even of elasticity. But this idea is not entertained by physiologists of the present day, so that Dr. Carson is combating an imaginary enemy. It is, I believe, universally agreed that the arteries are endowed with great elasticity at least, which gives them, as they are always on the stretch, a constant tendency to recoil and contract into a less calibre, and thus to compress the contained blood.

Many authors consider them as possessing another power, similar to elasticity, but differing from it as being an animal power, while elasticity is merely a property of matter. This property is called by Bichat "contractilité organique insensible," and by Dr. Parry, who has largely illustrated it "tonicity." It accounts

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* Inquiry, &c. p. 37.

for all the facts as well as muscularity does, while it is subject to none of its objections. If an animal has plethora, the vessels are distended; if it suffers a profuse hæmorrhage, they are found contracted in their diameter,* and thus they accommodate themselves to all the variations in the mass of blood.

X. The arteries are found empty after death.

"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 that of the heart."†

This is easily explicable on the principle of tonicity, and is no objection to the theory of the arteries not being muscles. The arteries, it is presumed, do not die quite so soon as the heart does, so that as they have less blood in them, and receive no more, they contract upon it, and force it forward through the capillaries into the veins, where it stagnates. When the arteries die too, their tonicity being gone, they

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* Bichat's Anat. Gen. tom. I. p. 308.

† Carson's Inquiry, &c. p. 38.

relax, and are found nearly empty at death, while the veins are gorged with blood.

The arguments to show that the heart circulates the blood independently of the arteries, are these:—1. Circulation in a palsied limb. 2. Circulation although the arteries are ossified. 3. Pulsation of veins. 4. Injection of fluids. 5. Ligature of arteries. 6. Pulsation simultaneous through the system. 7. The pulse affected by diseases of the heart.

I. The circulation continues in a palsied limb. It is impaired indeed, but were the arteries muscles assisting in circulating the blood, they would be rendered incapable of motion as well as the voluntary muscles, and thus the lood would not move in the limb at all.

II. The arteries are frequently ossified without the circulation being injured. Ossification indurates the coats, and thus were the arteries muscular, they could not, when so diseased, circulate the blood.—But it moves independent of them.

III. Pulsation is sometimes observed in veins. It is well known that pulsation in the natural state of the body is confined to the arteries, and to those only of a certain size. Occasionally, however, in diseases of the heart, it may be perceived in the jugular and other veins. There have been many instances of this, but I can refer to only one, recorded by M. Homberg in the Memoirs of the French Academy for 1704. The patient was a Lady who had been subject to breast complaints for many years. Whenever she had a fit of palpitation, which was a frequent occurrence, there was an evident pulsation of the veins of the neck and arms, corresponding nearly to that of the arteries.

The fact is explained in this way. When the auricle contracts, owing to the stiffness of the valves, which in these cases are generally ossified, and open and shut only imperfectly, the blood is forced back into the vena cava and other veins, producing a pulsation. This appears to prove distinctly that the pulse, and consequently the circulation, is caused by the heart alone. The veins possess a coat, weaker indeed, but similar in structure to that called fibrous or muscular in the arteries. Should we not naturally imagine that these vessels would

be under similar laws, and that their actions would be the same? But there is pulsation in the one, while the other is destitute of it. Is it not a legitimate conclusion that the cause of the difference is, that the one is impressed by the contraction of the heart, while this impulse is almost or wholly expended before it reaches the other? Hence the circulation is produced by the heart.

IV. Transfusion of blood and other fluids.

When water is impelled from a syringe, or when blood is transfused from a living animal into the artery of a dead body, pulsation may be immediately felt. It is weaker indeed than in the living body, but it is sufficiently strong to be observed, even through the integuments, cold and stiff as they are. The experiment may be varied, by throwing the fluid into the vein of a dead body, with the same event. In the living body, when a vein is wounded, the blood flows from it slowly and in a continued stream, because the impulse communicated to it is small, and it passes from the capillaries without any alternate interruption; when, on the other hand, an artery is punctured, the blood flows by jets, caused by the impetus com-

municated by the heart's systole. This view is proved by experiments similar to those mentioned above. If blood from an artery be transfused into a vein, the vein when wounded sends forth blood by jets; and if the experiment be reversed, by wounding an artery into which blood has been transfused from a vein, the blood flows slowly and uninterruptedly. These interesting experiments, contrived with so much genius, and varied with such spirit by the celebrated Bichat,* are quite decisive, and certainly lead to the conclusion, that the heart is the cause of the circulation.

V. The ligature of arteries.

As soon as a ligature is put upon an artery, intercepting the influence of the heart upon the blood, the pulse instantly ceases below the ligature. Whenever this does not take place, the pulse is produced by the heart's impulse, communicated through a collateral branch. When part of an artery is included within two ligatures, unless there is a collateral branch, the insulated portion contracts and becomes obliterated.

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* Anat. Gen. tom. i. p. 322.

VI. Pulsation simultaneous through the system.

" Comment la pulsation," says Bichat,* " de toutes les arteres est elle simultanée, si une centre unique ne preside pas a cette pulsation?" How would it be possible to have the pulse uniform and synchronous, if every inch of artery, throughout the whole system, had the motion of the blood in its own power, and was capable of contracting or not, (as it were) at its own will? There are thousands of arteries, of various sizes, situated in different positions, constantly intersected by anastomosing branches, pressed upon by various weights, and agitated by muscles. From such a state we should expect irregularity and disorder; the circulation would be impeded, and perhaps stopt altogether. But the vascular system, in the healthy body, presents a state of things in the greatest order. One moving power directs the whole, and all the vessels obey its mandates. Count the pulse in the hand, or the neck, or the foot, or any where, you will find it invariably correspond with the pulsation of the heart. Bend one knee upon the other, a situation in which we

* Anat. Gen. tom. I. p. 325.

might naturally expect an oppression of the circulation, were it carried on by the arteries; it will be found that the force of the circulation will raise the incumbent knee, and that, at each pulsation of the heart.

There have been cases recorded, it is true, in which the pulse was more frequent in one part of the body than another, but such cases are very rare, and perhaps may have been inaccurately observed; at any rate, they prove nothing in opposition to such an overwhelming collection of facts as may be adduced to show that the pulse is simultaneous throughout the system.

VII. The pulse is affected by diseases of the heart.

Had the arteries assisted in propelling the blood, we should find every little local affection make the pulse irregular, while diseases of the heart would but little affect the remote vessels. But the very reverse is the case. Local affections do not alter the pulse; even below an aneurism, it is synchronous with that above it; large pieces of the arterial canal become diseased, and yet it remains the same. But the slightest affection of the heart alters the circulation.

Mental emotion increases or diminishes the flow of blood, and organic diseases render the pulse irregular or intermitting. There are indeed instances in which the pulse was not affected by disease of the heart; these are, however, very rare, and we can easily suppose the existence of a disease which should not materially affect the circulation.

The natural conclusion from all these arguments appears to be, that the heart alone circulates the blood, and that the arteries merely possess the power of accommodating themselves to the quantity of their contents, without giving any assistance in their propulsion.

I SHALL now consider shortly, the method by which the venous blood is returned to the heart, and how the cardiac cavities are enabled to act upon the blood which flows towards them.

Every contraction of the left ventricle forces into the aorta a certain quantity of blood. This pushes forward the blood already in the artery, and the impulse is communicated to all the arterial system. The blood in the capillary system is in a great measure beyond the influence of the heart, at least the impulse com-

municated to it must be very small, and it is moved forward, chiefly, perhaps, by muscular action: For it can hardly be supposed that the minute vessels propel their contents by capillary attraction, as this would move the blood only an inconsiderable way, and, from the frequent anastomoses, the motion would sometimes be in a retrograde direction.

How the blood gets from the capillary vessels along the veins, and thence into the heart, is a question of considerable importance, and of much interest. It has been generally supposed that the influence of the heart is indirectly communicated to the venous blood, through the *vis a tergo* of the arterial blood; that thus a considerable rapidity of motion is given to it, and that it rushes forward into the right auricle, forcibly dilating it, and thus causing it to contract. But it is by no means proved that the venous blood is urged on by the *vis a tergo;* it is more probable that there is no impetus, at least none to such a degree as to force the heart in the manner supposed.

Dr. Carson has lately proposed a very ingenious theory upon this subject. He supposes that the pressure of the atmosphere is communicated to the venous blood at the lungs, and

that it is this pressure which dilates the cavities of the heart.* This gives, doubtless, a new view of the subject, and discovers a principle which must have some power in forwarding the circulation; but it cannot be considered as the great moving power; and when we reflect that the blood cannot be influenced by the air in the fœtus, this opinion loses in a great degree its appearance of importance. Dr. Carson's assumption, that the diaphragm in the fœtus performs the same office as to the circulation that the lungs do in the adult, is quite gratuitous.

It appears most probable that each cardiac cavity upon the cessation of its systole becomes a vacuum, and that the blood without any impulse rushes into it, and fills it. This opinion has accordingly been adopted by several authors. But if we consider the heart simply as a hollow muscle, all the fibres of which tend to contract it, its relaxation could not produce a vacuum. The sides, when left to themselves, would lie close to each other in wrinkles or folds, and no cavity would be left capable of constituting a vacuum of such size as

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* An Inquiry into the Causes of the Motion of the Blood.

to admit a sufficient quantity of blood. It is natural, then, to look to the heart itself as obviating the difficulty, by possessing a property of dilating itself. Such a power was believed by various anatomists, but Dr. Langrish was its most eminent supporter. He supposed that the heart possesses muscular fibres for dilatation, interwoven with those whose office is contraction. With regard to this opinion, it is sufficient to observe, that such dilating fibres have never been discovered, and that all the fibres of a hollow muscle must, when acting, tend to contract that cavity.

Although these attempts to account for this part of the circulation have failed, yet it does not follow that the inquiry is to be given up, or that we are to rest satisfied with ignorance. We must prosecute our researches, and if the least ray of light can be thrown on the subject, it will be some recompense for the labour bestowed upon it. That the heart possesses the property of dilatation by muscular action, is so like truth, and at any rate so like the last resource to which we can repair for an explanation of this function, that it may be worth our while to look towards it.

The following quotation from Richerand corroborates this opinion. "The cavities of the heart, however, are not entirely passive during dilatation, and the action of that organ does not wholly depend on the excitement of the blood on its parietes, since the heart, after it has been torn from the body of a living animal, palpitates, its cavities contract and dilate, though quite emptied of blood, and appear agitated by alternate motions, which become fainter as the part gets cold. If you attempt to check the diastole of the heart, this organ resists the hand which compresses it, and its cavities appear endowed with a power which Galen termed pulsive; in virtue of which they dilate to receive the blood, and not because they receive it."*

The only parts of the heart which seem qualified by their contraction to dilate the heart, are the columnæ carneæ and musculi pectinati.†

When we consider the uses which have generally been attributed to them, we see the little foundation on which they rest, and we are led to inquire if there be any better. The only ones which I have been able to discover, are

* Elements of Physiology, p. 167.

† For the first hint of this use of the Columnæ Carneæ and Musculi Pectinati, I am indebted to my Father.

the following: "These columnæ carneæ render the heart stronger, and also assist in preventing the coagulation of the blood."* It is not easy to conceive how they can operate in producing either of these effects; at least their influence, if any, must be very inconsiderable. Mr. Hunter's view is this: "The tendons which are longest are inserted into columns of muscle—the intention of which is very evident; for if they had gone the whole length in form of a tendon, they would have been too long when the heart contracted, and the valves, in such a case, would have allowed of being pushed into the auricle so far as to admit of the blood escaping back again into the cavity; but the carneæ calumnæ keep the valves within the ventricle, in the contracted state of the ventricles; and the dilatation of the ventricles counteracts them, and places the valves in their proper situation in that state."† To see that this cannot be the real use of these organs, it is only necessary to observe their number and situation. They are seen on almost every part of the inside of the heart, and very few of them terminate in cordæ tendineæ. Besides, the

* Monro's Outlines, vol. II. p. 334.

† A Treatise on the Blood, vol. I. p. 244.

auricle is studded with them, while it has no valves (towards the vein) to be regulated by their tension or contraction.

If they be muscles, and Haller calls them "exquisite irritabiles,"* the certain consequence of their contraction must be to dilate the heart. Being placed longitudinally upon its sides, they will draw their extremities towards each other; and as they exist in such numbers, and in such various positions, they will have the effect, when acting together, of bending it. Thus a vacuum will be produced; and from the known laws of matter, the blood which is in immediate proximity, being pressed on by the weight of the superincumbent atmosphere, will flow in.

This mechanism appears to explain the motion of the blood in the veins, and thus to throw light upon a subject which is otherwise much in the dark.—I may have been too sanguine in fixing the office of these organs; I may have overlooked objections which would have occurred to one of more experience; but I am satisfied that their use has not yet been ascertained, and that which I have assigned to them appears at least probable.

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* Elementa Physiologiæ, tom. I. p. 312.