From this case, and those which I have enumerated, it appears to me that there is little danger to apprehend, in laying open the abdominal cavity; and that in diseased ovarium, extra-uterine conceptions, *fœtus in utero* with deformity of the pelvis preventing embryulcia, aneurism of the common iliac arteries or of the aorta, volvulus, internal hernia, cancer of the uterus, and foreign bodies in the stomach threatening death, we should have recourse early to gastrotomy. The delay in such cases is more dangerous than the operation. To show what freedom may be used in diseased ovarium, I have received, since writing the above, the following history of a case from my friend Mr Edward Scudamore, surgeon at Wye in Kent.

In 1821, A. C. 36 years of age, had been repeatedly the subject of *paracentesis abdominis* for ovarian or encysted dropsy, when fluid in increased quantities, and varying in quality in each operation, was drawn off. Her health declining, and her constitution resisting each effort to cure the disease, any proposition holding out the most remote hope was eagerly listened to. The trocar and canula were again introduced, the fluid drawn off, and the canula left with a plug inserted into its mouth. In a few days the plug was removed, and the accumulated fluid discharged, which operation was repeated for several successive times, after eight days interval between each. These attempts proving fruitless, and no irritation being produced by the canula, diluted port-wine was injected in one instance, and a solution of sulphate of zinc in the other; both of which merely produced a sensation of heat while they remained in the cavity. Many weeks elapsed after these operations, when the constitution gradually sinking, she expired.

III.

On the Muscularity of Arteries. By Robert Hunter, Lecturer on Anatomy and Surgery, Glasgow.

That the arteries are endowed with a contractile power, and consequently can assist in the circulation of the blood, is a prevailing opinion, I believe, among physiologists. A few, however, of considerable reputation have questioned the truth of the opinion, and denied, *in toto*, the muscularity of arteries.

As the subject is of considerable physiological importance, I shall first examine the arguments that have been brought forward against the muscularity of arteries, and, *secondly*, men-

tion a few facts and experiments which seem to lead to an opposite conclusion.

The arguments adduced against the muscularity of arteries may be arranged in the following order:

1*st*, That muscular fibres have never been detected in arteries.

2*dly*, That no kind of stimuli can make arteries evince a contractile power.

3*dly*, That what is commonly called the muscular tunic of arteries, is not a continuation of the ventricles of the heart.

4*thly*, That fibrine, the essential chemical constituent of muscle, does not exist in arteries.

5*thly*, That the arterial system has been ossified without deranging the circulation of the blood.

The first of these allegations, if well-founded, may seem to set the matter at rest. Are no muscular fibres discoverable, then, in arteries? If we appeal to authority, the answer to this interrogatory will be sufficiently unsatisfactory, for authorities are conflicting. While Bichat, Scarpa and others, have been unable to discover them, Munro, J. Hunter and Bell maintain that they have found them in man, as well as in many of the inferior animals. But relinquishing authority altogether, and admitting, for the sake of argument, that they have not been discovered in man, still this would be no proof of their non-existence. Muscular fibres differ so much in their appearance in different animals, and even in different parts of the same animal, that even though existing, they may not easily be recognised. The substance of the aorta probably differs less in appearance from common muscle than the texture of the snails or the oyster; yet this last is muscular, and acknowledged by all to possess that character, not indeed from the muscular fibres that can be easily seen, for some may be so blind as not to discover even here any thing like muscular fibre; but the motions with which these animals are endowed, are overpowering arguments, and as indisputably prove the existence of muscular fibres as the most palpable ocular demonstration.

This allegation, then, is not so formidable as might, at first view, be imagined; for even admitting that Bichat and others have not been able to detect them, still if we can prove that arteries are possessed of a contractile power, either the conclusion that muscular fibres exist, or the absurd alternative, that an effect may exist without a cause, would be unavoidable.

2*dly*, Bichat, however, maintains, that arteries possess no contractile power. Many phenomena, both in the diseased and healthy state of the body, tend to disprove this opinion. At all periods of life, the arteries can accommodate themselves to

the quantity of the circulating fluids. When surcharged with blood, we find them dilated, and evincing, from an excess of stimulus, a strong full and frequent pulsation. When the quantity of blood, on the other hand, is diminished, we find the arteries still in contact with the sanguineous fluid; but, from the subduction of stimulus, exhibiting a small, soft, and weak pulsation.* This accommodating nature of the arteries to the quantity of the contained fluid, is rather a curious property of inert tubes.

In the dead subject, the arteries are admitted to be of greater diameter than when the blood is circulating through them. (Richerand.) This is just what might have been expected *a priori*, if the arteries were admitted to possess a contractile power, but is totally irreconcileable with their non-muscularity. The same phenomenon occurs in other tubes, respecting the muscularity of which no doubt exists. After death, the diameter of the intestinal canal is much increased; the tonic and contractile power of its muscular fibres are gone; and the tube hangs relaxed and dilated. The same phenomenon takes place in the arteries: is it, therefore, unreasonable to ascribe it to the same cause? While the blood circulates through them, it can only arise from the stimulus of the blood and the tonic action of their muscular fibre, that a certain degree of pressure is maintained upon the circulating fluid. When the vital spark becomes extinct, the contractile power is lost, and an increased diameter is the consequence.

If such a contractile power exist, it is but natural to suppose that we can increase it at pleasure; and this is absolutely in our power. All are acquainted with the numerous applications used by the ancients for arresting the progress of hemorrhage; Though we treat their practice now with derision and contempt, it would be absurd to deny the utility, to a certain extent, of these applications. I do not speak of cauterization and compression, but of those numerous cold and astringent lotions which were held in such repute, and the partial efficacy of which no man can doubt. If the bleeding arise from small arteries, these applications will instantly stop it; and the question then occurs, how is this effect produced? It must obviously arise from one of two causes:—either by inducing a coagulation of blood in the mouths of the vessels, or by exciting a contraction of their tunics. It cannot depend entirely upon the first of these causes, for the effect will follow, though the cold lotions be applied, not to the mouths of the vessels

* Hunter on the Blood, &c. p. 200.

themselves, but at a considerable distance from them. Thus, the secondary hemorrhage that occurs after amputation, will frequently be stopped by applying cold water to the *skin* over the seat of the wound: and cold applications to the *scrotum* will relieve bleedings at the *nose*.

The contractile power of the arteries may still farther be deduced, from a fact that is well known to every practical surgeon. An artery that can be discovered through the integuments pulsating with considerable force, when exposed to the cold air, loses in part, and sometimes altogether, its pulsation. A fact of a similar kind has been mentioned by Mr J. Hunter, respecting the arteries of inferior animals. After exposing the tibial artery of a dog, Mr Hunter found that the pulsations became weaker and weaker, till they ceased altogether. * These facts, I presume, can only be satisfactorily explained on the supposition that arteries are muscular, for it would be absurd to suppose that cold air contracts them as it would a piece of inanimate matter; no such contraction follows the application of cold to the dead arteries. If we admit, however, the existence of muscular fibre, the phenomenon can easily be explained. The cold air, as a stimulus to the muscular fibre, induces at first a slight contraction; but by the continued action of the stimulus, the contractions are urged on till the diameter of the artery is nearly obliterated.

I will freely admit, that many chemical and mechanical stimuli will produce no appreciable contraction of their tunics; but though we were more certain than we are that many stimuli would produce no effect whatever, this, I contend, would be no proof of their non-muscularity, for some muscles contract by a specific stimulus only. The iris, we have reason to believe, is muscular, for in many animals its movements are under the influence of the will, like the other muscles of voluntary motion: † still the iris can be made to evince no contractile power by any artificial stimuli that can be employed. ‡ But are we certain that the arteries are insensible to every kind of stimuli? The facts stated above, and still more the experiments of Dr Hales, militate against such an opinion.

Dr Hales fixed a pipe in the descending aorta of a living animal, and raised a column of water till it produced a power equal, as he supposed, to the force of the heart. He then cut a few small vessels on the intestine, as far from the mesentery

* Treatise on the Blood, p. 200.
† Bell's Anatomy, vol. iii. p. 45.
‡ Bell's Anatomy, vol. iii. p. 48.

as possible. A certain quantity of water flowed from the cut vessels in 46 seconds, while the same quantity of brandy, with an equal pressure, took double that time. By a similar set of experiments, he discovered that warm water passed more readily than cold water.

These experiments lead directly to the conclusion, that arteries are stimulated, in various degrees, according to the nature of the stimulus;—the brandy being more irritating than water, and cold water more stimulating than warm, a contraction had been induced, the calibre of the arteries diminished, and, of course, less fluid could pass through in a given time.

From these facts I think we are warranted to conclude, that arteries are possessed of a contractile power, and, consequently, of muscular fibres, by which these contractions are induced.

3dly, The fact, "that what is commonly called the muscular tunic of arteries is not a continuation of the ventricles of the heart," proves nothing. As an argument against the muscularity of arteries, it is perfectly ridiculous. Let us place the syllogism in its proper form, and this will be apparent. The ventricle of the heart is muscular; the aorta is not a continuation of the ventricle, *ergo*, the aorta is not muscular. By a similar kind of reasoning we could prove that the ventricle itself is not muscular, for it is not a continuation of the auricle. In the fœtus, the auricle and ventricle are distinct bags, which soon become united by condensed cellular substance; but the muscular fibres are not continued into each other. * It is inconsistent with the economy of the circulation that it should be so. They are two separate muscles, not acting simultaneously, but in succession. If the reason is obvious why the ventricle should not be a continuation of the muscular fibres of the auricle, the reason is equally clear, why the ventricle and aorta should be likewise separated. When the auricle contracts, the ventricle must be in a state of relaxation to receive the blood; and when the ventricle contracts, must not the aorta be relaxed for a similar purpose? Were we warranted to draw any conclusion, then, from the fact that the aorta is not a continuation of the left ventricle, it would be one, I presume, rather in favour of the muscularity of arteries than against it.

4thly, Whether fibrine form an ingredient in the chemical composition of the arterial tissue, I will not take upon me to determine. Animal chemistry is still in its infancy; and although Berzelius and others who have extended the boundaries of this branch of knowledge, have not been able to discover it, still

* Carson on the Motion of the Blood, p. 101.—Bell's Anatomy, vol. i. p. 460.

this is no decisive proof of its non-existence. Till very lately, fibrine was supposed to exist exclusively in animal substances. Vauquelin, however, has detected it in vegetables. * Fibrine, it is obvious, may exist in such tissues, but so obscured by other animal products, that the science of the profoundest chemist upon earth may be baffled to detect it. If, indeed, the chemical analysis of the arterial tissue had been proved to be absolutely correct, and no fibrine found, the argument would carry with it some little weight; but so long as the analysis is confessedly imperfect, it would be illogical in the extreme to draw from such premises any conclusion either for or against the muscularity of arteries.

5thly, That the arteries have been partially ossified, without deranging, to any great extent, the circulation of the blood, is a fact which is well authenticated. We are not at liberty, however, to draw the conclusion from this fact, that the arteries in the healthy state are not muscular, or cannot assist in the circulation of the blood. We know how nicely the system can accommodate itself to the exigencies that arise both in health and disease. If an external sense is destroyed, the deficiency is partially compensated for by an increased acuteness of all the others. If an arm or a leg is lost, the organs of motion that remain generally increase in size and muscular power. So it is with the heart and arteries. When the heart is preternaturally small, changed in texture, or, as in some monsters, wanting altogether, the arteries always display an increase of strength by the thickness of their parietes, and their stronger contractile power. When the texture of the arteries, on the other hand, is so changed by disease as to render them incapable of assisting the circulation, the heart evidently acquires a stronger propulsive power than natural, as its increased magnitude in such cases demonstrates. Admitting, as we cordially do, that the arteries, to a certain extent, have been ossified without a great derangement of the circulation, the concession would be too broad and unguarded did we admit that the circulation of the blood would remain unaffected by the complete ossification of the whole arterial system. Ossification to such an extent, I believe, never has existed. The arterial *trunks* have indeed been found in this state, but the smaller branches, and the capillary extremities, could not become ossified without the immediate destruction of life. The work of nutrition, in such a state of the vessels, would be annihilated, and those numerous secretions which conduce to the necessities of the system, would be

* Ure's Chemical Dictionary, Art. Fibrine,

changed, or permanently arrested. Those who have supposed that the circulation is not affected by the ossification of the arterial system, have fallen into an egregious error;—the pulsation of the arteries is irregular and almost lost,—the whole system evinces powerful effects from so much derangement,—the patient is unhealthy, and of a remarkably sallow complexion, * and, from the imperfect distribution of the blood, mortification frequently supervenes. †

Secondly, The *action*, and, consequently, the muscularity of arteries, may be deduced from the following facts and observations.

1*st*, The pulse is not uniformly synchronous with the contraction of the left ventricle of the heart. Thousands of cases are on record to substantiate this fact. Such inaccordancies between the action of the heart and arteries, have been found to arise from various causes,—from deranged and organic affections of the heart,—from diseases of the arteries,—and even from diseases of organs which have only an obscure and inexplicable sympathy with the arterial system. Thus the phenomenon of a double pulse has been discovered to attend a morbid contraction of the left auriculo-ventricular opening of the heart; ‡ from ossification of the aortic valves, the heart has been known to contract twice for each arterial pulsation; § in whitloe, the arteries which lead to the disease sometimes pulsate more frequently than the arteries in any healthy part of the body; ‖ and the pulse has become extinct at the wrist and other parts of the body, from diseases of the alimentary canal and pulmonary organs. ¶ These facts are totally incompatible with the

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* Burns on Diseases of the Heart.

† Mr White of Manchester, when speaking of gangrene, makes the following observation. "There is a species of mortification from an ossified artery, which has ever been, and ever will remain, the *opprobrium medicorum*. The whole art of medicine is here in vain exhausted, and the complaint remains uninterrupted in its fatal progress, till it arrives at the extremity of the ossification."

‡ Hodgson on Diseases of the Arteries and Veins.

§ Burns on Diseases of the Heart.

‖ Good's Study of Medicine, vol. ii. p. 6.

¶ Parry on the Nature, Cause, and Varieties of the Arterial Pulse, p. 140.—The following facts from Dr Parry may prove interesting:

"Twenty cases have occurred to me of the total loss of pulse in the radial arteries, from maladies of the alimentary canal, whether a considerable time before death, or under circumstances that admitted of a recovery, while at the same time the pulse in the carotids was full and strong. In one instance of general dropsy, the patient had no pulse at the wrist for 17 days, yet was restored to perfect health. Sometimes the pulse has been wanting in the radial artery only. In another case, a young man labouring under pulmonary hectic was found to have lost the pulse at the wrist immediately after coming out of the warm bath. Several months after-

supposition that the arteries are passive instruments in the circulation. They are, indeed, anomalies of pulsation, arising from some morbid condition of the system; but they are not the less valuable on that account, for it is only from attending to the condition of parts in disease, that we are often enabled to arrive at a correct knowledge of their healthy function. Were the arteries naturally passive instruments in the circulation, it is inconceivable how any disease could confer upon them a motive power; but if such a power primarily existed, even in the most obscure degree, it would require no great stretch of fancy to imagine how such a power could either be increased or annihilated. *

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wards, it had returned, although almost in an imperceptible degree. Another patient, a female of middle age, the mother of several children, affected with severe cough, was apparently convalescent and walking about the house, when it was discovered that the pulse in one arm was wholly wanting; a few days afterwards she died suddenly, when the whole course of the aorta was carefully examined, but no deviation from the healthy state could be perceived."

* Respecting the cause of the pulse in the healthy state of the body, much difference of opinion exists. Galen among the ancients, and Dumas among the moderns, supposed that the arteries had an inherent pulsation, independent of the action of the heart. Richerand, Portal, Sœmmering, and others, adopt the opinion of Haller, who maintained, that the phenomenon was attributable to the impulse of the blood from the left ventricle distending the arteries. "D'abord je me suis assuré, que le sang, poussé par le coeur, dilate les arteres et forme ce battement qu'on appelle le pouls." (*Memoir sur le Mouvement du Sang*, *p.* 33). Bichat conceives, that no dilatation of the arteries during the systolé of the heart takes place, but that the pulse is dependent upon a certain degree of locomotion in the whole artery, by which it springs against the finger during the contraction of the heart.

Dr Parry, by a variety of ingenious experiments, has attempted to prove, that the arteries suffer no dilatation nor contraction during the systolé and diastolé of the heart, nor locomotion of any description: that the pulse is dependent, therefore, solely upon the action of the heart.

"In a ram," says Dr Parry, "both carotids were laid bare by Mr George Norman, in the presence of Mr Coombs, two others, and myself. Notwithstanding the animal was greatly agitated, there was not in either carotids the least appearance of dilatation during the systolé of the ventricle, nor contraction during its diastolé: nor, except when the animal breathed, was there any degree of locomotion perceived in the arteries, which remained completely quiet and at rest. Nevertheless, when either artery was compressed between the finger and thumb, the pulse was very strong and distinct."

None of the above-mentioned explanations appear to be correct. The pulse, I conceive, is dependent on the action both of the heart and arteries.

When the heart contracts, a column of blood is sent into the aorta, which, from the incompressible nature of that fluid, must communicate an impulse at the same moment to all the blood in the arterial system, as far, at least, as the capillaries. This, to a certain extent, is the cause of the pulse; but unless the arteries react upon the blood impelled into them by the heart, the pulsation is obscure and indistinct, as we find where ossification attacks the vessels. In the experiments of Dr Parry, indeed, no perceptible dilatation or contraction could be discovered. But we ought to remember that the arteries, in all these experiments, were in an unnatural state;—they were necessarily exposed to the atmospheric air; and from the stimulus of that fluid, the muscular fibres, if such existed, would be called strongly, if not spasmodically, into action. It is to this circumstance, I presume, that an artery

2dly, The arteries after death are always found empty.

In the living subject, the arteries are full of blood. If the blood, therefore, be propelled by the contractile power of the heart alone, the moment the heart ceases to contract, the blood would remain motionless, and the arteries full. To what, then, can the vacuity of the arteries after death be attributed?

This state of the arteries after death, was one of the most powerful arguments which the antagonists of Harvey adduced against his transcendant discovery; and, anxious as Harvey was to explain the phenomenon, his explication is quite unsatisfactory. He supposed the vacuity dependent on a collapse of the lungs, which prevented the blood from passing through them to the left side of the heart, the left side still continuing to contract after it had ceased to receive blood. *

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whose pulsations were obvious to the sight, loses, when exposed, all apparent movement.

Dr Parry would almost lead us to conclude that the arteries cannot contract, as he maintains that they are always in a state of distension, as well under the systolé as the diastolé of the left ventricle. Some of his experiments would seem to lead directly to such a conclusion; but that arises from the imperfect state of the experiment. If the experiments had been followed out, as in a few instances they have been, they would lead to a conclusion in direct hostility to the one which has been deduced. For example, we are told that two ligatures were applied to the carotid of a living sheep, about an inch and a half distant from each other. Midway between the ligatures, the artery measured half an inch in circumference. After the blood was evacuated from the artery, it only measured $\frac{1}{4}\frac{6}{0}\frac{0}{0}$ of an inch; and its circumference, of course, reduced $\frac{4}{4}\frac{0}{0}\frac{0}{0}$ of an inch by the mere evacuation. The diminution of diameter from the evacuation of the blood, may seem a proof that the artery was previously in a state of unnatural distension; but the arteries, as I have formerly observed, have a capability of accommodating themselves to the quantity of contained fluid. If you diminish that fluid, the arteries will contract; and if the fluid be removed altogether from any one of them, the contractions will slowly and imperceptibly go on, till it is converted into an impervious cord. The decreased diameter of the arteries, under such circumstances, is no proof that they were naturally over-distended: it is only a proof that, in the unnatural circumstances in which they were placed, they were called violently, and, if we might so speak, unnaturally, into action. If the experiment had been followed out, by killing the animal, and sometime afterwards examining the arteries, the experimentalist would have found this contractile effort gone, and its diameter not only as great, but greater, than before. For the truth of this observation, I have only to appeal to one of Dr Parry's own experiments. In another experiment, he informs us that the circumference of the right carotid of a horse was $\frac{3}{4}\frac{1}{0}\frac{6}{0}$ of an inch. A portion of it was included between two ligatures, as in the above-mentioned experiment. An hour afterwards it was punctured; but, from the blood being coagulated, serum only flowed out. The vessel was then measured, and found to be only $\frac{2}{4}\frac{5}{0}\frac{2}{0}\frac{8}{0}$ of an inch in circumference. The experiment, in this instance, did not stop here. The animal was bled to death; and in 16 hours afterwards, the artery, at the same point as before, measured $\frac{3}{4}\frac{7}{0}\frac{2}{0}$ of an inch.

* Vacuitas arteriarum in corporibus mortuis inde evenit, quod quando subsident (meatibus occlusis) pulmones, non ulterius respirant; ita per ipsos sanguis libere non potest transire: perseverat tamen per temporis spatium cor in expellendo; unde et sinistra cordis auricula contractior et ventriculus; pariterque arteriæ inanitæ et (non sanguinis successione repletæ) vacuæ apparent.—(*Exercitatio altera ad Riolanum, p.*115. *Edit. of the London College*, 1766.

The recent experiments of Dr Williams prove, indeed, that the blood, after respiration has ceased, will not permeate the lungs, but it is not from any mechanical obstruction which they oppose to its progress, but from the impure state of the circulating fluid, *—impure blood acting upon the pulmonary veins as a torpifying poison. To suppose that the left ventricle, after it has ceased to receive blood, can propel the blood contained in the arteries into the veins, is the most unfounded of all opinions. It may, and indeed it does, contract for some time after the animal ceases to respire, but unless it at the same time propel a column of blood against a preceding column, how, in the nature of things, can its mere contraction affect any fluid that may be contained in the arteries?

Dr Carson has attempted to account for the phenomenon, from the natural resiliency of the lungs, which, after death, allows the blood to accumulate there as in a vacuum. †

Plausible, and even correct to a certain extent, as this view of the matter is, it is liable to objections. In birds, whose lungs are bound down to their back-bones, no resilience can take place. In fishes, too, no tendency to a vacuum can be formed, for they are destitute of lungs. How, then, can the vacuity in the arterial system of such animals be accounted for? From certain experiments which I some time ago performed, I was induced to believe that the vacuity was attributable to the contractile power of the arteries alone. A more extensive and correct set of experiments obliged me, however, to abandon that opinion. These experiments, notwithstanding, prove, what is sufficient for our present purpose, that the arteries are concerned, and assist in the process.

To ascertain the cause of the vacuity in the arteries, the following experiments were instituted.

Experiment 1.—The thorax of a living cat was opened on the 22d of January last, in presence of a number of my pupils, by cutting through the cartilages of the ribs on each side of the sternum. The lungs instantly collapsed, with a considerable noise. The heart was then seen beating irregularly, but pretty strongly, through the thin pericardium. The pericardium was slit open, and a ligature applied around the roots of the systemic and pulmonic vessels, just where they issue from the heart.

Upon examining the animal 12 hours afterwards, the veins were found remarkably turgid, and the blood in many of the

* Edinb. Med. and Surg. Jour. Vol. xix. p. 526.
† Medico. Chirurg. Trans. Vol. ii. Part 1st.

larger ones coagulated. The aorta was next examined throughout its whole extent, and some of its branches likewise, to their minute ramifications. It was flatter and more flaccid than when the animal was alive, or than it appeared for half an hour after the death of the animal. Having opened the aorta, it was found about half filled with thin uncoagulated blood; and as we traced the branches which proceed from the aorta, we found them still partially filled with the same kind of blood.

Exp. 2.—Next day the thorax of another living cat was opened, and a ligature applied round the aorta and pulmonary artery; the *venæ cavæ* were not surrounded with a ligature. Twelve hours afterwards, the vascular system was minutely examined. The right auricle and ventricle were preternaturally loaded with blood, and the veins turgid. From the heart nearly to the cœliac artery, the aorta was empty; but beyond that point the arteries contained a small quantity of uncoagulated blood.

Exp. 3.—On the 25th of January, the thorax of a living cat was opened, when the lungs collapsed, and the heart continued to pulsate irregularly for some time, as in the foregoing experiment. A ligature was then applied around the root of the aorta, no other vessels being included. Six hours afterwards, the arterial system was found nearly in the same state as in the second experiment.

Exp. 4.—On the 1st February the thorax of a living cat was opened, and a ligature applied round the aorta, as in the third Experiment. An incision was next made into the trachea, and the lungs forcibly inflated and the trachea tied. After the animal had become cold, the arteries were examined and found perfectly empty.

The object of the first experiment was to insulate the vascular system from the heart and lungs, and to observe the effect. Had all the blood been found in the venous system after this experiment, the vacuity of the arteries would have been attributable to their own contractile power; but as we found a considerable quantity of blood in these tubes, we are forced to the conclusion, that the arteries of themselves are inadequate to the effect. That they assist the process, however, is indisputable from the result of the experiment; and the question then occurs, what other power or powers cooperate with the arteries in producing the effect? In the second experiment, another power, viz. the right side of the heart was added, and the result was considerable; still the arteries were only partially emptied. In the third experiment the lungs were added, but no farther effect was produced. Upon considering the matter, I re-

membered that the lungs from the entrance of the atmospheric air into the chest, had been, during the whole experiment, in a state of collapse. In the fourth experiment, this objection was obviated by inflating the lungs, and the result was such as has been mentioned.

The vacuity of the arteries after death, then, is attributable to three causes; 1*st*, To the contraction of the arteries; 2*dly*, To the suction-power of the right side of the heart; and 3*dly*, To the natural resilience of the lungs, producing a tendency to a vacuum. These three causes appear indispensable to produce the effect. If any one fail, the arteries remain comparatively full. When the arteries are ossified, so that they cannot contract, or when the irritability of the heart is suddenly destroyed by the electric fluid, or by any other agent, the arteries remain partially filled with blood.

3*dly*, The contractile power of the heart has been lost by organic disease, without affecting to any great extent the circulation of the blood.

Mr John Hunter, has justly remarked, that the heart cannot be an essential organ of the circulation, since it is wanting in so many animals. By a parity of reasoning, I think we may conclude, that even in those animals in which a heart naturally exists, it cannot be the sole circulating agent, as its contractile power has been destroyed without a cessation of the circulation. That the heart should be liable to many diseases, both functional and organic, is by no means surprising. It stands associated to so many organs by situation and sympathy,—is so easily affected by every mental and corporeal excitation, that its action is incessantly liable to derangement; and functional diseases are known to terminate frequently in textural or organic. Many curious cases of this kind are on record; and as they form the basis upon which our present argument is founded, I shall briefly glance at one or two of them.

In Mr Allan Burns's work on Diseases of the Heart, we are informed that one of his patients was for many years afflicted with a disease of the heart, which frequently became so urgent, that he dropped down apparently dead; his pulse ceasing to beat, and his face becoming livid and swollen. On the afternoon preceding his death, he had for a little time been engaged in some laborious employment, which completely exhausted him. He went to bed earlier than usual, and was found dead about 11 o'clock the same evening. On inspecting the body, the ventricles of the heart were found completely changed in structure, being now composed of a substance "intermediate between fat and cartilage."

The heart has been found disorganized in a still more wonderful degree. It has been found converted into solid bone. Several cases of this kind will be found by referring to Burnet, Baillie, Good, Bordenave, Simmons, and Burns. An interesting case of this kind is related by the last mentioned eminent author, of a woman, whose disease at first seemed to consist principally in a cough and difficulty of breathing; her pulse being regular and appetite unimpaired. For two months previous to her death, she was afflicted with nausea, great constriction in the chest, and diffused pain over the abdomen. She died; and on examining the body "the whole extent of the pericardium which covered the ventricles, and the ventricles themselves, except about a cubic inch at the apex of the heart, were ossified and firm as the skull."

In the case related by Dr Simmons, the ossification extended from the base to the apex of the heart, and consequently involved both auricles and ventricles.

Here, then, is matter for serious consideration. In such cases, the heart must have been useless for years; yet the circulation was not only maintained, but maintained with such regularity, that the disease was frequently overlooked or mistaken. Either these cases are unworthy of credit, or the heart cannot be the sole agent in the circulation. Were such cases adduced by men of no reputation in their profession; were they adduced by men eager in the support of some favourite hypothesis, we might be inclined to receive them with caution. But, brought forward as they are by men of the first respectability; warped by no theory, and influenced by no hypothesis; who state, in the calm language of reason, facts which have fallen within the sphere of their sober observation—such facts are entitled to our implicit confidence. We are forced then to adopt the only alternative, that the circulation must be carried on under such circumstances by other agents than the heart; and where can these agents be found, but in some of the remaining apparatus of circulation? When the arteries are ossified, we readily admit that the blood is moved through the arterial system by the percussive action of the heart; and when the heart is ossified, and the arteries sound, have we not, at least, a strong presumptive proof that the sanguineous movement is dependent, under such circumstances, upon the contractile power of the arteries?

4*thly*, The heart is wanting in many classes of animated beings in which a circulation exists.

We are apt to consider these animated beings as of a less perfect organization than ourselves, but with how much reason, is difficult to determine. They may be less complex in struc-

ture, but simplicity of organization is surely no proof of inferiority. Upon the same principle, the organ of smell may be denominated less perfect than the eye; but, though the one is less complex, or, if you please, less noble and useful than the other, yet, as both are equally fitted for their several functions, both are equally perfect. So it is with "the lower animals." The relationships which they bear to the external world are different from ours. We are placed, by a peculiar organization, on a mighty vantage ground; but, so long as these animals can perform the parts assigned them in the general arrangement of the universe, they are as perfect as we. Be this as it may, by investigating the structure of these animals, we obtain a knowledge of many important facts in physiology. In framing many of the human organs, nature has displayed all her exuberance of power. In other animals, however, we find her unravelling, as it were, her own intricacies;—removing first one part, and then another, till the essentials only of the organ remain. By thus tracing Nature through her "sportive movements," we receive much both of pleasing and profitable instruction. While we are charmed with varied and exhaustless resources, we are no less edified by the correct views of the animal economy, which such exercises are so calculated to impart. How are we, for example, certain that the auditory nerve is the only essential part of the organ of hearing, and that all its other tissues, exquisitely delicate and useful though they be, are comparatively unimportant appendages? Chiefly because comparative anatomy has proved it.

If we appeal, then, to the same tribunal in deciding the essential parts of the apparatus of circulation, we will assuredly be led to form a very different opinion from that entertained by the generality of physiologists. Do we say it is the heart? Comparative Anatomy instantly confutes the assertion, by pointing out a numerous class of animals in which a circulation exists, without any such organ. Those beings which possess a heart and circulation like man, are indeed but a handful compared to the countless millions that inhabit the ocean; the bowels of the earth; or people the atmosphere, and flutter in every sunbeam. The great majority of such animals are destitute of a heart; yet can we believe that their circulatory apparatus is not adequate to every useful purpose in their economy? Does not the blood move, and are not the functions of nutrition, conservation and reproduction, executed with as much regularity as when the heart is attached to the

sanguiferous system? Did we find a heart in all animals in which a circulation existed, the conclusion that it is an essential organ of the circulation would be unavoidable; but, varying in structure, as it does in so many animals, and wanting in part, or altogether, as it is in so many more, we are forced to draw a very different, if not an opposite conclusion. That the heart is a most useful organ in those animals in which it exists, is undeniable:—it is a useful reservoir for the blood-vessels; and, by its strength and excessive irritability, is admirably adapted for maintaining in the vessels a constant and equable supply of blood, as well as powerfully assisting the sanguineous movement. That it however is the *only* agent of the circulation, is a gratuitous assertion. There is no *corporeal* heart for the fish, yet the blood circulates with ease through its *body*;—no pulmonic heart for the snail, yet the blood stagnates not in its respiratory organ;—no heart, either corporeal or pulmonic, for worms and certain insects, yet the blood experiences no interruption in its course;* and, even in man and quadrupeds, in whom the heart might seem at first view the only organ of the circulation, we find the blood moving with uniformity in the system of the vena porta, where the action of the heart cannot possibly extend. In some of her playful moments, Nature too has enabled us to prove, that the blood can circulate over the whole human system, independently of that organ; for monsters have been born without a heart, and have lived for some time, the blood circulating with apparent regularity.

In all these instances, then, unless the vessels be admitted to be the principal moving powers in the circulation, how can the sanguineous movement be at all accounted for?

5*thly*, Different parts of the arterial system may be at the same moment in different states of excitement and action.

We cannot contemplate the sudden and deep blush of shame, or the pallid hue of terror, without admitting the truth of this observation.† Do we not likewise find the blood circulating with more impetuosity in an inflamed than in a healthy part; and rushing with great force into the penis during the erectile state of that organ, while the circulation remains unaffected in other parts of the body? These are not the only facts which prove the truth of the above mentioned position.

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* Blumenbach's Comparative Anatomy, p. 247. B. F. Bening de Hirudinibus. Cuvier's Leçons, &c. l. 23. Sect. 2. Art. 4. Mitchel in Aliber's American Annals, Part I. p. 121.

† Abernethy's Physiological Lectures, p. 227.

We cannot observe the growth of any tumour, or indeed of any portion of the body, either in health or disease, or the states of action and repose of secretory organs, without being forcibly impelled to the same result.

The secretions are all formed from the blood; yet we find the secreting organs not only differing from each other in their secreting power, but each exhibiting likewise alternate periods of repose and high activity. The saliva does not always flow into the mouth, yet the idea of savoury food will instantly call it forth in copious profusion. The gastric juice is not always secreted; let food, however, be introduced into the stomach, and the digestive menstruum will soon appear. The testicle, too, has long and indeterminate periods of inaction; yet how easily can it be roused from this state into vigorous excitement!

That the vessels which lead to an active organ are unusually loaded with blood, is a fact which has been long known and universally admitted. We have even ocular demonstration of the fact in the living subject; and experiment has proved the increased magnitude of the vessels in the dead. The question then arises, what is the cause of this greater influx of blood? Is it attributable to the heart? Can the heart instantly direct a stream of blood to any organ that may require it, without transmitting, at the same time, a corresponding increase to all the others? The thing is impossible.

These, and many other phenomena of the animal economy, are quite inexplicable, unless we admit that particular arteries have the power of dilating and contracting, of directing and impelling the blood to particular organs; and if particular arteries have this power, with what reason can we refuse the same power to the whole arterial system.

24th April, 1824,
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IV.

An Inquiry on several doubtful Points connected with the Circulation. *By* James Macfadyen, Surgeon. Lecturer on Natural History, &c. Glasgow.

This is one of those subjects which, after having been for a long time laid aside, has of late come again to occupy a considerable portion of the attention of Physiologists. Not-