or vapours may be introduced into the lungs either by simple inhalation or by artificial respiration. For the inhalation of a gas, a conical bag of oilskin, India-rubber, or bladder, must be made to fit the snout of the

stopcock, pure air may be passed direct to the lungs through c', F, and C, or it may be loaded with vapour by passing it through D' into the bottle, and then up through D and C to the lungs.

animal, and connected with a bag, bladder, or gas-holder containing the gas. Or a tube may be put into the trachea and connected with the gas-holder.

For the inhalation of a vapour, a cone of strong paper or cardboard may be used, the wide end being put over the muzzle, and the liquid, the vapour of which is to be inhaled, dropped on a piece of blotting-paper and put on the small end. Or the whole cone may be made of blotting-paper.

Many different kinds of apparatus have been used for the artificial respiration of gases, among which may be mentioned the ingenious instrument of Thiry and the beautiful respiration-pump of Ludwig. The simplest method probably is to have the gas in a bag, connected by means of the bottle or Müller's valve with the tracheal cannula. The gas may then be forced into the lung at intervals, by alternately compressing and relaxing the bag.

Air may be loaded with vapour of any kind of fluid before it is sent into the lungs, by either mixing the fluid with the water in the bottle-valve, or by emptying out the water and putting a little of the fluid alone on the bottom of the bottle. Pure air or air loaded with vapour may be sent into the lungs alternately by the arrangement shown in fig. 5. A stream of air is sent from the bellows through the India-rubber tube S, and divided into two by the T-tube T'. When the clip X is removed, and X' put on, the air passes straight through to the tracheal cannula Z. If X be now put on, and X' removed, the air passes through V, and becomes loaded with the vapour of any fluid placed in the bottle.

The alternation may be effected still more rapidly and conveniently by a stopcock which I have had made for this purpose. Two tubes, C and C', pass from its sides, and two others, D and D', from its bottom. The interior is perforated with three holes. Two of these, E and E', are L-shaped, and one (F) passes straight through from side to side. When the handle B is in a line with C and C', their lumen corresponds with that of the hole F, and air passes straight through. When B is transverse, the hole E corresponds with C and D, and E' with C or D', so that air passing in through C' passes down through D', and may pass up through D and out at C. When B is neither in a line with C nor yet transverse, but half-way between, the holes in the interior do not correspond with those on the exterior of the stopcock, and no air can pass at all, and it may thus be used for experiments on asphyxia. When such experiments are made, the hole in the tracheal cannula must be carefully stopped with white wax. By means of the screw G the stopcock may be fastened to the rod E of the rabbit-hold in fig. 1. The tubes D and D' may either be attached by pieces of India-rubber tubing to tubes of a bottle such as V, or they may be themselves passed through the cork and a small piece of glass-tubing long enough to reach the bottom of the bottle attached to D'. By then simply turning the

THE PHYSIOLOGY AND PATHOLOGY OF THE CIRCULATION.

By GEORGE JOHNSON, M.D., F.R.C.P.,
Physician to King's College Hospital, etc.

IN an able and interesting leading article on Blood-letting in the BRITISH MEDICAL JOURNAL of March 18th, with the greater part of which I entirely agree, reference is made to some recent observations of Messrs. Legros and Onimus on the influence of the arteries upon the circulation. The writer says: "The part of the circulation the most important to the life of the individual is probably the peristaltic wave, which carries on the blood in an even stream through the muscular arterioles, and which Messrs. Legros and Onimus describe as closely resembling the motions of the alimentary canal." So highly does the writer estimate the doctrines of these physiologists, that, after referring to the observations of Waller, Claude Bernard, and Brown-Séquard, he adds, "but, above all, those of Messrs. Legros and Onimus." The doctrines of Legros and Onimus are also expounded and accepted in an article on the Pathology of the Microscopic Arteries, in the last (April) number of the *British and Foreign Medico-Chirurgical Review*. The papers of the French physiologists are published in Robin's *Journal de l'Anatomie et de la Physiologie*, 1868. I have read them with great care; and not only do I find there no proof that the arteries, by a peristaltic contraction, exert a propulsive influence upon the blood, but I find it impossible to reconcile this hypothesis with the facts regarding the physiology of the circulation which have been established by the observations of Waller, Bernard, Brown-Séquard, Marey, and others.

Messrs. Legros and Onimus are not sufficiently careful to distinguish between the functions of the large elastic arteries and those of the microscopic muscular arterioles. The pulsating wave in the former is obvious and palpable, and it is explained by the action and reaction of the ventricular contraction and the arterial resiliency by which the intermitting rush of blood from the heart is gradually converted into a continuous stream in the small arteries and capillaries. This is the pulsation which Messrs. Legros and Onimus have seen without a lens in branches of the retinal artery and in the ear of the rabbit. The arteries that are large enough to be visible by the unaided eye are mainly elastic, and contain very little muscular tissue. If with a high power we watch the circulation in the web of the frog's foot, we see that the blood-current is equable, and continuous both in the capillaries and in the muscular arterioles. It may sometimes be seen that the impulse from the heart slightly distends the larger microscopic arteries. There is a slight dilatation of the arteries at each contraction of the heart. More commonly, however, the minute arteries remain of uniform size; there is no appearance of dilatation or contraction, and certainly there is no indication of a peristaltic contraction.

It is a well established fact that, in order that a muscular canal may drive on its contents in a definite direction, it must either be provided with valves, or it must contract in the form of a peristaltic wave, like the intestine. We know that the minute arteries have no valves; and we see in the transparent parts of animals that they do not contract peristaltically. It is obvious, therefore, that while, by their contraction, they regulate the blood-supply, they have no power to forcibly drive the blood onwards. This is the generally accepted doctrine, and it is not likely to be more than momentarily disturbed by the inexact observations and loose reasoning of Messrs. Legros and Onimus. These writers maintain that, if the muscular fibres in the arterial walls were intended merely to regulate the current of blood, they would be found most abundantly in the arteries near the heart; whereas, on the contrary, the most minute and distant arteries are notoriously the most muscular. Exactly so: for the obvious reason that the smallest arterioles alone can regulate the blood-supply to the capillaries of each particular tissue and organ.

As an illustration of their unsatisfactory method of dealing with well known phenomena, I will refer to two points. The characteristic pulse of aortic regurgitation—momentarily full, but quickly collapsing—which they inaccurately call "hard and strong", they explain by saying that "the arterial contractions tend to supplement the insufficiency of the cardiac contraction". This explanation is intended to replace that which is generally received, and which seems so simple and satisfactory—namely, that for a brief period the whole arterial system

is distended by the forcible propulsion of a large volume of blood from a dilated and hypertrophied left ventricle, while the speedy collapse of the pulse is explained by the reflux of blood through the unclosed aortic orifice into the ventricle during its diastole.

The venous murmur of anæmia—the *bruit de diable*—they affirm to be arterial, and not venous. It is difficult, they say, “to believe that the continuous murmur occurs in the veins where the current is so feeble, and the walls of which have too little tension to vibrate.” Dr. Ogier Ward was the first to demonstrate that the *bruit de diable* is a venous murmur. We refer those who are sceptical upon this point to the unanswerable arguments in favour of the venous origin of the murmur set forth in Dr. Walshe’s work on *Diseases of the Heart* (third edition, p. 143.)

The writer of the article in the *British and Foreign Medico-Chirurgical Review*, to which I have already referred, has done me the honour to notice approvingly my own labours in the field of arterial pathology, for which I feel grateful. In return for the criticism with which he favours me, I venture to intimate to him that he has himself fallen into one serious error. In proof of the doctrine that the arteries alone suffice by their contractions to carry on the circulation, he refers to the case of an acardiac foetus published by Sir Benjamin Brodie, of which the reviewer says that it “lived several days after birth”. Startled by this seemingly incredible statement, I referred to the paper in the *Philosophical Transactions* (1809, p. 161), where it is stated that “both foetuses were born dead”. It is a well established fact that an acardiac foetus has never been known to occur except in association with a twin foetus perfectly developed; and Sir Astley Cooper, describing a case of the kind (*Guy’s Hospital Reports*, vol. i), first clearly demonstrated that, by means of the communication between the arteries and veins of the two foetuses at the junction of the umbilical cord with the placenta, the heart of the perfect foetus drives the blood through the vessels of its acardiac companion. In the words of Sir Astley Cooper, “the heart of the developed child impelled the blood into the other which was appended to it in the same way as it caused that fluid to circulate through the vessels of one of its own limbs.” Appended to Sir A. Cooper’s paper is a note from Sir B. Brodie, accepting Sir Astley’s explanation of the facts, and admitting his former error in supposing that the circulation in an acardiac foetus was carried on independently. It is evident that no proof of the independent propulsive influence of the arteries upon the blood is afforded by the case of an acardiac foetus; and it is surprising that a learned writer on physiology in a scientific journal should suppose it possible that such a monstrosity, not only heartless, but headless, could live for several days after its birth and consequent separation from its hearty companion.

With reference to my discovery and interpretation of hypertrophy of the muscular walls of the minute arteries in chronic Bright’s disease, the reviewer, while he accepts the facts, disputes my interpretation of them. He doubts (1) whether hypertrophy of muscle results from over-action, and (2) whether “abnormal increase in bulk implies an increased power of action”. Obviously, then, he doubts, for example, whether the increased action of the heart which is associated with an ossified aorta leads to hypertrophy of the left ventricle, and whether the hypertrophied ventricle propels the blood with greater force than a ventricle of the normal thickness. His scepticism upon this point is as surprising as his credulity with respect to the viability of an acardiac foetus.

As an antidote to the novel doctrines of this writer, I would suggest a careful study of Mr. Paget’s philosophical account of hypertrophy and its conservative tendency in his *Surgical Pathology*. With reference to a specimen of hypertrophy of the walls of the bladder, Mr. Paget quotes the following characteristic passage from John Hunter. “The bladder in such cases [of obstruction to the passage of urine], having more to do than common, is almost in a constant state of irritation and action, by which, according to a property in all muscles, it becomes stronger and stronger in its muscular coats; and I suspect that this disposition to become stronger from repeated action is greater in the involuntary muscles than the voluntary; and the reason why it should be so is, I think, very evident; for in the involuntary muscles the former should be in all cases capable of overcoming the resistance, as the power is always performing some natural and necessary action; for whenever a disease produces an uncommon resistance in the involuntary parts, if the power is not proportionately increased, the disease becomes very formidable; whereas in the voluntary muscles there is not that necessity, because the will can stop whenever the muscles cannot follow; and, if the will is so diseased as not to stop, the power in voluntary muscles should not increase in proportion.” Upon this extract from Hunter, Mr. Paget remarks: “Nothing, surely, could more appositely or more exactly express the truth concerning hypertrophy of muscle.”

ON THE SELECTION OF CASES FOR THE OPERATION OF SUBCUTANEOUS DIVISION OF THE NECK OF THE THIGH-BONE.*

By WILLIAM ADAMS, F.R.C.S.,
Surgeon to the Royal Orthopædic and Great Northern Hospitals.

SINCE I brought under the notice of this Society, on April 25th, 1870, the operation of the subcutaneous division of the neck of the thigh-bone—an operation which I had performed for the first time in the annals of surgery on December 1st, 1869, and it may be remembered that I exhibited the man in this room, able to stand and walk without any assistance—the operation has excited a great deal of attention, and been successfully performed four times by different provincial hospital surgeons—viz., twice by Mr. T. R. Jessop of Leeds, once by Mr. Furneaux Jordan of Birmingham, and once by Mr. F. W. Jowers of Brighton. The operation has been only once performed in London, by Mr. J. Croft of St. Thomas’s Hospital, on March 4th, 1871, on a boy aged eight years, who had fibrous ankylosis of the left hip-joint, with the limb in a deformed position, the femur being flexed at a right angle and considerably adducted. There had been abscesses in the neighbourhood of the joint, but for five years all suppuration was said to have ceased, and the hip-joint disease was said to have commenced when the child was about two years old. I was present at this operation, and concurred in the advisability of its performance, in consequence of the extreme deformity, which rendered the success of any other method of treatment very doubtful; as well as of the length of time which had elapsed since the closing of the abscesses, although the case belonged to a class altogether different from that for which I had originally proposed the operation. In this case, extensive suppuration followed the operation, and the child died of pyæmia on March 30th. The *post mortem* examination showed that the neck of the bone had been divided. The head of the bone had been, to a great extent, destroyed by caries, and the fibrous ankylosis broken up by the suppurative process, and perhaps partly by some attempts at forcible extension, which had been employed a week previously, when the contracted muscles in the neighbourhood of the joint had been divided.

The general result, therefore, is, up to the present time, that this operation has been performed in six cases: in five cases for bony ankylosis, successfully; and once for fibrous ankylosis in a child, unsuccessfully; and in the first class of cases, either no suppuration, or very little suppuration followed the operation; whilst, in the latter, acute suppuration set in and terminated fatally.

Amongst the various questions which have arisen in connection with this operation, two of the most important refer to the particular class of cases to which the operation is applicable, and to those in which the operation cannot be performed; and the object of the present paper will be to define these cases, for practical purposes, with as much accuracy as possible.

Bony ankylosis of the hip-joint is known to take place as the result of several morbid conditions and diseases, which, in their general pathology and progress, especially in reference to their destructive or non-destructive character, as affecting the bones, are essentially different. For example, when bony ankylosis has taken place as the result of strumous disease of long standing, and accompanied with suppuration, destruction of the head of the bone from caries and necrosis, and sometimes also of the neck, to a greater or less extent, generally occurs, the disease itself being essentially of a destructive character, tending to caries and necrosis of bone. In these cases, displacement and fusion of parts in abnormal positions frequently take place.

On the other hand, when bony ankylosis has been the result of acute rheumatic inflammation, in its more severe form, as we not infrequently see it in the so-called gonorrhœal rheumatism, when it is generally localised in one joint, no disposition whatever exists either to the destruction of bone, or to dislocation: acute rheumatic inflammation not being of a destructive character, but tending to the effusion of plastic lymph, and organisation of adhesions, without any disposition to suppuration, to ulceration, or to caries in the bones. Rheumatic inflammation will, therefore, produce adhesions within the joint, and a kind of fibrous ankylosis, which, after a slow disintegration of the articular cartilage, terminates in true bony ankylosis, but without any loss of bone-structure; so that, in such cases, the head and neck of the thigh-bone invariably remain of their full natural proportions.

It is equally true that the head and neck of the thigh-bone remain unaltered in size in cases of bony ankylosis after pyæmic inflammation, especially in its subacute form, from which patients generally recover,

* Read at the Medical Society of London, April 24th, 1871.