inflammation from the simplest and mildest phase up to an abscess, so the vaccine virus, in the majority of cases, only occasions a mild response in the surrounding tissues and related glands, but occasionally much more exaggerated local and constitutional phenomena. Hence we may meet with all grades of inflammation from a simple blush to formidable cellulitis. These conditions must not be confounded with true erysipelas and other septic poisonings. So also the confluence of too crowded vaccine vesicles or a too extensively vaccinated surface may lead to sloughing. I would place in this group also the auto-inoculations of the vaccine virus, which it is more convenient to discuss with the next group.

Group 2.—That more or less generalised eruptions occasionally accompany vaccination is universally admitted. They may be scarlatiniform, morbilliform, urticarial, vesicular, pustular, bullous, purpuric, or may take on the erythema multiforme type. Those who believe that vaccinia is an exanthematic fever, and that these eruptions are the direct outcome of the virus in the economy have called them vaccinides, and referred to them as *vaccine généralisée*. The morbilliform phase (the so-called vaccinal erythema) is by far the more common, and appears, as a rule, about the time of maturation of the vaccine pustules. It is a benign eruption, and disappears in a few days. Urticaria is not at all uncommon. The other forms are of rarer occurrence, and are recorded only in special papers and treatises. The small vesicular, the bullous and pustular eruptions may be scratched and give rise to some trouble. The rare purpura is of ill omen. Very exceptionally the latter eruptions may become ulcerous or gangrenous, as with varicella, either from constitutional peculiarities or scratching, and the introduction of micro-organisms. We should remember that similar eruptions may arise in pyæmia, ulcerative endocarditis, and other infective maladies; but eruptions of this class fall into another group. I do not believe that any of these rashes can be considered specific, though, of course, they arise in consequence of something absorbed into the system. Similar eruptions are constantly seen after the ingestion of drugs, sometimes in Bright's disease, and other conditions where a foreign body is introduced. But there is another very interesting eruption, quite distinct from the other vesicular and bullous eruption, which has occasioned much dispute, and some who believe it to be the specific vaccine eruption have called it "vaccinola." I refer to the cases in which a more or less widespread evolution of true vaccine vesicles occurs. After a careful study of the records, I strongly incline to the opinion that they are all, certainly most are, cases of auto-inoculation. I find this the simplest explanation, though the matter in a few instances is a difficult one to decide. There may be only a few supernumerary lesions on the vaccinated arm or elsewhere, or a copious eruption on an eczematous surface, or a widespread eruption of many vesicles, which has been mistaken for small-pox. That they are vaccine vesicles is proved by inoculation; but the doubtful cases are very rare, and the eruption does not involve the mouth. It is said that the contents of the primary vaccine eruption are capable of successful inoculation on the same person only from the sixth to the tenth day. All these exanthematic rashes may arise with either calf or human lymph.

Group 3.—The diseases which occur to one in this group are syphilis, leprosy, yaws, tuberculosis, and the acute specifics. The spread of leprosy has been attributed to vaccination; but, however suspicious some few facts may be, we know nothing about it. It might be supposed that if tuberculosis were so transmitted a local development would be found; but only two or three cases are on record of lupus occurring in a vaccination scar, and no conclusion of value can be drawn. It will be in the recollection of some that Meyer and Acker vaccinated 105 undoubtedly phthisical persons under antiseptic precautions, and in no single instance could tubercle bacilli be found in the lymph. That syphilis has been thus inoculated we know, but I have never seen a case either in my own or others' practice. To the question, Can the constitutional existence of infectious diseases specifically infect the contents without modifying the characteristic development of a true Jennerian vesicle? Sir John Simon answered No; but this opinion he has now probably modified. Zeissl said that clear vaccine lymph taken from syphilitic children does not inoculate syphilis. The latter was only introduced if the lymph was contaminated with syphilitic tissue or syphilitic blood or by an unclean instrument. Unfortunately this dictum cannot now be accepted. Whether syphilis can be inoculated by lymph taken from a syphilitic infant in whom the symptoms are not yet

declared on the skin is more doubtful. Certainly Mr. Hutchinson's statement is a very grave one. At any rate, if it is possible it is excessively rare, and it speaks volumes for the care with which vaccination is performed that, with the multitudes of syphilitic infants about, no cases come under observation in London, in which the disease is so transmitted.

Group 4.—If anyone wishes to fully realise the disastrous consequences of vaccinating with septic lymph or crusts, let him read the account, given by Dr. Joseph Jones, of vaccination during the American Civil War. I need not refer further to the hospital gangrene, tetanus, pyæmia, etc., so brought about. Happily such effects are very rare with us, and we have not even ignorant or careless operators using impure lymph or lancets obtaining a supply at the wrong time. I would earnestly support Mr. Morris in his appeal to the authorities to formulate further regulations for the antiseptic treatment after operation. What other wounds are so carelessly tended and left to be rubbed and scratched in unhealthy homes and inoculated with organisms? If the various protective appliances are unsuitable, surely some more prolonged supervision and effective treatment can be carried out. Then we should not hear of those regrettable cases of erysipelas and ulceration and gangrene. Certain very exceptional cases of gangrene which occur in connection with the general eruptions of purpura or pustules or bullæ are perhaps unavoidable, because in all cases there is unfortunately a personal factor to be taken into account. Ulceration, whether supervening on the local lesions or auto-inoculated vesicles about the genitals or elsewhere, frequently simulate chancres and give rise to much misunderstanding and great mischief. There is also an excessively common condition met with of a simple character, which, however, causes great alarm to parents and gives rise to much prejudice. I allude to the auto-inoculations of pus from the vaccination crusts, which sets up a vesico-pustular eruption called "impetigo" or "porrigo contagiosa." It is of little consequence and easily cured; but how easily it might be prevented!

Group 5.—Lastly, I come to eczema, psoriasis, the so-called "lichen urticatus," and miliaria. They are in no way the specific outcome of vaccination; but in those predisposed to these conditions they are excited by the disturbance caused by the operation, just as they are by an acute specific disease. Psoriasis is very rarely so excited; miliaria now and again; lichen urticatus occasionally; eczema not so infrequently. I have analysed 600 cases of eczema which have come under my care at the Paddington Green Children's Hospital, and I find that 249, or 41.5 per cent., were under my treatment before 1 year of age. In 40 of these it was noted definitely that the eczema began before vaccination. Of 161 cases in which the date of origin was definitely noted, the eczema began in the first month in 33, second month 22, third 25, fourth 39, fifth 23, sixth 7, seventh 10, eighth 4, ninth 23, tenth 1, eleventh 1, twelfth 3. The conclusion is that eczema is of great frequency in earliest infancy, and vaccination has no specific influence in producing it.

ON THE DIRECT INFLUENCE OF THE HEART OVER THE SOUNDS AND ACTION OF THE LUNGS.¹

By J. S. BRISTOWE, M.D., F.R.S.,
Senior Physician St. Thomas's Hospital.

GENTLEMEN,—I fear that the title I have chosen for my short paper is not calculated to be attractive to medical men in active practice; and yet it is the best title I could think of as embracing several matters of practical interest and importance which I wish to bring under your consideration this afternoon.

The simplest way of dealing with my subject, I think, and the one I shall adopt, is to briefly discuss seriatim the several phenomena to which I desire to call your attention, and to conclude with some general remarks.

1. Some few years ago Dr. David Drummond, of Newcastle, showed that not infrequently in cases of aneurysm of the aortic arch, the aneurysm, by its rhythmical movements, imparts a rhythmical character to the escape of air from the lungs, the existence of which may be recognised by auscultation, and is a matter of diagnostic value. The influence of an aneurysm over expiration can, even under the most favourable circumstances, only be slight, and may easily fail of recognition; and the best method

¹ Read before the South-Eastern Branch of the British Medical Association.

of determining the presence of the phenomenon is to follow the practice laid down by Dr. Drummond—namely, to make the patient take a deep inspiration; and then, while your stethoscope is applied over the manubrium of the sternum or the trachea, to get him to expire very slowly, and in order to promote slowness to make him close his mouth, and, with the aid of his finger one nostril, thus leaving one nostril only for the escape of the breath.

2. What is called indifferently wavy, jerky, or interrupted respiration is not infrequently observed, mostly at the apex of the lungs, and is often regarded as one of the early signs of pulmonary phthisis. Various explanations of it have been given, and not improbably the term is applied indifferently to several auscultatory phenomena which closely resemble one another in character, but are due to different causes. I shall not venture on a general discussion of the subject; but I am quite satisfied from long observation that the common and main cause is the pressure exerted by the heart during its rhythmical movements on the neighbouring lung tissue; and to this I shall limit my remarks.

Wavy respiration is, as is generally stated, mainly and best heard during inspiration, and is not infrequently heard only during inspiration; and although it is sometimes audible over the greater part, or the whole, of both lungs, it is more frequently limited to the apices and to the immediate neighbourhood of the heart. I believe it is much more common on the left than on the right side.

That the alternate contractions and dilatations of the heart must in some degree directly affect the lungs in the sense of causing perturbations in the otherwise uniform movements of respiration is too obvious to need discussion. It is also obvious that, other things being equal, such perturbations must be greater near the heart than at a distance from it; must be greater and more widely diffused when the heart is acting violently, especially if it be dilated, than when the heart is healthy and acts healthily; and must be greater in any comparatively thin portion of lung confined between the heart on the one side and any solid or unyielding expanse on the other, than in lung tissue not thus circumstanced.

This influence of the heart over the movements of the healthy lung is in most cases insufficient to cause any modification in the respiratory movements or sounds detectable by palpation or by auscultation of the chest. But I know that occasionally, when the heart is in all respects healthy, where there is no aneurysm, where the lungs are sound, and where no waviness of respiration is audible over the lungs, one may, by adopting Dr. Drummond's manœuvre, recognise, through the stethoscope applied over the trachea, a feebly rhythmical escape of air during expiration. And, in discussing the subject some year or two ago in the wards, my house-physician, Dr. Charles Evans, now of Shaftesbury, told me that while smoking he had often observed that on allowing inhaled smoke to escape leisurely from his lungs it came away in rhythmical pulses, synchronous with the beats of the heart; and he performed the experiment successfully in my presence.

3. It is well known, or at any rate largely believed, that some murmurs, generally termed cardiac, are of no cardiac significance. So-called hæmic murmurs are common at the base of the heart. Of these I do not propose to speak. But at the apex it is not unusual to hear systolic murmurs, which there is also just reason to believe are not organic. The murmurs to which I here refer correspond to ordinary mitral systolic murmurs in the fact that they are usually best heard at the extreme apex of the heart; but they differ from them in the fact that they occupy a much-restricted area, that they are not conducted into the axilla or to the back, that they are considerably modified by the acts of respiration, and that there is no necessary evidence of enlargement or dilatation of the heart or accentuation of the second sound in the pulmonary area. I believe them to be caused in the same way that the *bruit de pot fêlé* is caused, only the percussion of the lung to which they are due is effected by the impulse of the heart's apex and not by the fingers of the physician. Such murmurs are largely influenced by respiration and not uncommonly by the pressure of the stethoscope. As to their relation to the respiratory acts, I believe they are generally intensified during expiration and inspiration, and especially during the latter, and that they often disappear absolutely at the end of an expiration; and, other things being equal, they are usually increased by firm pressure of the stethoscope. I may add, in confirmation of the view that these murmurs are due to the cause above specified, that I have occasionally

noticed in cases in which the breath is wavy over the upper part of the left lung, that during inspiration the wavy character along the edge of the heart has been replaced by an obvious but very slight murmur resembling the murmur more frequently and better heard at the apex.

Within the last year I have had two cases under my care in St. Thomas's Hospital which seem to me to have a special interest in connection with the subject under discussion.

The first was that of a young girl of delicate aspect, who was stated to have had, some few months previously, scarlet fever, which was followed by loss of power and wasting of the extensors of both forearms, for which mainly she sought admission. From this affection she slowly improved under treatment, and is now, I believe, quite well. But, in the course of our investigation of her case, we detected a soft, but perfectly obvious systolic murmur at the apex. This murmur was audible over a very small area, and was not conducted into the axilla or to the back. It occupied the greater part of the systolic period, but, instead of running off from the first sound, began a little later than it, and ran into the second sound, being in fact prædiastolic. Some of those who listened to it made the remark, without intending to imply any hypothesis as to its production, that, although its rhythm corresponded to that of the apex beat, it seemed independent of it. The intensity of the murmur was distinctly modified by respiration, and was loudest, I believe, during inspiration, and it occasionally disappeared, according to my ear, at the end of an expiration. It was increased also in some degree by external pressure. There was no evidence of hypertrophy or dilatation, or of undue action of the heart, and the second sound at the left base was not accentuated. The murmur and attendant conditions correspond exactly with those which I am in the habit of regarding as characteristic of the murmur in question, with one exception, which tends, I think, to confirm the correctness of my interpretation of it, namely, the relation of the murmur to the healthy sounds. This relationship is hard to understand if the murmur was endocardial, but seems easy of explanation if the murmur was caused by the pressure of the heart's apex against the overlying portion of lung. On the other hand, it must not be forgotten that the girl had had scarlet fever, which often leaves a legacy of actual valvular disease.

The second case is that of a little boy, aged 8, now under my care. He was admitted for angular curvature of the upper dorsal vertebrae of some months' standing, now attended with almost complete motor paralysis of the lower extremities, and complete paralysis of the intercostal muscles. It is curious that the boy has no pain or tenderness in the spine, that he has no anæsthesia or loss of control over rectum or bladder, and that in all other respects he looks and feels in good health. But he presents an apex murmur almost exactly like that observed in the former case. It is audible only in a small area, it occupies the systolic period, running up (though not so strikingly as in the other case) to the second sound; it is similarly affected by respiration and pressure; and there are no other indications of heart disease. It is right, however, to say that there is a history of a slight rheumatic attack some months ago. Nevertheless in this case, as in the last, and for the same reason, I regard the murmur as being exocardial. It is probably aided by the falling in of the ribs which during inspiration accompanies the contraction of the diaphragm.

My aim—as doubtless you will have already gathered—has been to link together by a common explanation several phenomena which at first sight might well be, and generally are, regarded as having no connection with one another. That the heart and large vessels must, in virtue of their rhythmical movements, cause some rhythmical perturbation in the movements of respiration and in the ingress and egress of air is an obvious physiological fact; and, as I have already argued, such perturbations must certainly tend to be aggravated whenever the heart acts with undue violence, whenever the heart is dilated or hypertrophied, whenever an aneurysm is so placed and so large as to press on the lungs or larger bronchi; and especially in such parts of the lungs as from any cause lie between the heart and some more unyielding tissue, whether this be the thoracic walls themselves or lung tissue consolidated by inflammatory or tubercular infiltration, or lung compressed by pleural effusion. It seems probable, also, that the pulse in the branches of the pulmonary artery distributed throughout the lungs may, under certain circumstances, act in the same direction. The most delicate evidence of this effect of the pulsation of the heart and vessels over respiration is afforded by the rhythmical escape of the breath from the lungs, as demon-

strated by Dr. Drummond's method of auscultating the trachea during slow expiration, and by the rhythmical discharge of inhaled smoke, as in Dr. Evans's experiments; in both of which cases the rhythmical effects are usually so feeble as to be inaudible by the ear applied to the chest walls.

As I have already stated, the mere action of the heart leads in some cases to rhythmical perturbation of the expiratory sounds in the trachea, just as an aneurysm sometimes does, and for the same reason.

The next phenomenon is that of wavy respiration. This is generally best recognised during inspiration—probably in part because interference with the entrance of air into the lungs is more likely to reveal itself to auscultation of the chest than interference with the escape of air from the lungs, and in part probably because the influence of the movements of the heart is more likely to be felt by a full than by an empty lung.

The last phenomenon is the production of a definite murmur by the actual squeezing of the air out of a limited portion of lung by the impulse of the heart against it. This, as I have remarked, may occasionally be feebly heard along the edge of the præcordial area of dulness, especially in cases in which wavy respiration is present, replacing mainly during inspiration the simple rhythmical impulses of the wavy respiratory action. But it is always best heard at or near the apex of the heart, simulating a cardiac systolic murmur, but presenting the characters which I have assigned to it. I need scarcely again draw attention to the similarity in the mode of production of the *bruit de pot fêlé* and of the apex murmur in question. The main factor in both cases, and that which gives each sound its distinctive character, is the sudden compression of the lung tissue by a blow suddenly inflicted upon it, and the production thereby of a murmur due to the escape of air from the tissue into and along the bronchial tubes.

If the views I have advocated be correct, all the various phenomena above considered are intimately related to one another; and though each one may still have a diagnostic value of its own, their chief clinical interest lies in the consideration that undue importance may often be attached to their presence, and in their ready confoundability—if I may coin the word—with phenomena indicating serious organic disease.

ON LIGATURE OF THE POPLITEAL ARTERY FOR THE CURE OF ANEURYSM IN THE HAM.

By ANTHONY A. BOWLBY, F.R.C.S.

Assistant-Surgeon to the Metropolitan Free Hospital; and Surgical Registrar and Demonstrator of Practical Surgery to St. Bartholomew's Hospital.

In the very interesting paper by Mr. Holmes on the "Surgery of the Large Arterial Trunks," published in the *BRITISH MEDICAL JOURNAL* for November 15th, the question of the advisability of employing Anel's method of ligature is discussed, and the conclusion drawn by Mr. Holmes is "that no reason has been shown for the performance of Anel's operation in non-recurrent popliteal aneurysms."

I would venture to submit that there are two good reasons for preferring the ligature of the popliteal artery to that of the superficial femoral, namely, first, diminished risk of gangrene; secondly, greater certainty of cure; and, as Mr. Holmes has already alluded to the cases treated at St. Bartholomew's Hospital, I shall venture to quote some figures in support of my contentions.

During the last ten years the superficial femoral artery has been ligatured for the cure of aneurysm on twenty-two occasions, and gangrene has ensued in four cases, and recurrent pulsation, or failure to cure, in two others. Further, another case of recurrent pulsation after ligature of the femoral, performed at another hospital, was admitted for treatment. Thus, in seven cases out of twenty-three either gangrene or failure to cure ensued, and it may be added that in no case was the gangrene the sequence of an injury to the femoral vein at the time of operation.

With regard to the gangrene, I would submit that it is less likely to result from ligature of the popliteal than from a similar operation performed upon the superficial femoral. For, in the latter case, the operation results in the occlusion of the vessel destined for the supply of the leg and foot in two places, namely, at the seat of ligature and in the popliteal space; whereas, if the popliteal be tied, the artery is closed in but one place. Further,

the muscular branches of the femoral and of the first part of the popliteal, as well as the anastomotica magna being above the ligature, the circulation in the leg is necessarily better than after the application of a ligature higher up. It may also be added that in one case in which Mr. Thomas Smith did ligature the popliteal instead of the femoral for the cure of a popliteal aneurysm, pulsation returned in the tibial vessels within forty-eight hours, whilst the aneurysm became firm and ceased to pulsate.

Secondly, the failure of the ligature of the superficial femoral to stop pulsation in the sac in three cases out of the twenty-three mentioned above, indicates that failure is sufficiently frequent to demand attention. In these three cases the aneurysm was finally cured; once by pressure, and in the other two patients by ligature of the popliteal. It is certain that in such cases of failure the pulsation is maintained by the too free passage of blood from the branches of the profunda into those of the superficial femoral, and thence into the aneurysmal vessel, for it is now generally recognised that the vessel between the apex of Scarpa's triangle and the seat of aneurysm remains patent. And, this being the case, it is clear that if the popliteal itself be tied, failure from this cause could not result, a conclusion which is emphasised by the fact that ligature of the popliteal sufficed to cure both the aneurysms mentioned above after failure of the operation in Scarpa's triangle in each case, and of ligature in Hunter's canal, in addition, in one.

These facts, therefore, are apparent: (a) That in a certain number of cases of ligature of the superficial femoral for the cure of popliteal aneurysm the supply of blood to the limb is insufficient and gangrene ensues; (b) that in a few cases the blood stream is insufficiently arrested and the aneurysm is not cured. I would suggest that after ligature of the popliteal for the cure of similar disease neither of these misfortunes is so likely to ensue.

As to the objections that have been raised to the ligature of the vessel near to the sac, it may be said at once that they are purely theoretical. The examination of a number of specimens shows clearly that the upper part of the popliteal is no more likely to be diseased in a case of aneurysm at the bend of the knee than is the superficial femoral itself, and, although opinions and teaching to the contrary are abundant, the statement I make here can be confirmed by reference to any museum, and is supported by Sir W. Savory in his paper in the *Medico-Chirurgical Transactions*.

Next, I would suggest that one assumption of Mr. Holmes is unwarranted, namely, that after ligature of the popliteal no circulation goes on in the sac; for, whatever may be generally understood by the operation of Anel, I venture to assert that it is most probable that some arterial blood does continue to enter the sac (after ligature of the popliteal) through the articular branches, which are generally given off quite close to or at the seat of aneurysm itself, and that some regurgitation is practically certain to result from the communication between the anastomotica magna and the anterior tibial. The objection, therefore, which is founded upon the statement that ligature of the popliteal stops the circulation in the sac, cannot be seriously considered until the accuracy of the assumption is proved.

With regard to the relative difficulty of the operations of ligature of the superficial femoral and of the popliteal, I am of opinion that there is but little choice. I am aware that ligature of the popliteal is generally considered to be a difficult operation; but, after having seen it performed on four occasions in patients and very many times in the dead body, I feel sure that the difficulties are greatly exaggerated. The artery in the upper part of the popliteal space is not nearly so deeply seated as is commonly believed, and, with this warning before them, students of operative surgery make far fewer mistakes in exposing the popliteal than in cutting down on the femoral. The chief risk arises from the proximity of the popliteal vein; but the latter is readily avoided by opening the arterial sheath well to the inner side, as in the similar operation of ligature of the common carotid. In cases of large aneurysms occupying the greater part of the popliteal space the operation should, of course, not be attempted.

Finally, I would say that, although I venture to support what may be called Anel's method, I do so because I fully concur with Mr. Holmes when, in speaking of the operation of ligature for the cure of aneurysm, he says: "But are there no dangers in the operation except hæmorrhage? My own belief is that phlebitis and gangrene are in the present day the chief ill results of the operation." With aseptic wounds and ligatures gangrene is cer