

pect changed in a remarkable manner to the depressed look of fever. She complained of pain over the cæcum, and the bowels were relaxed. Wine, and starch and opium injections were ordered. This depressed and listless appearance increased, and profuse perspiration with vomiting and abdominal tenderness came on. On the 23rd, she had several rigors, and complained of acute pain in the right hip and thigh. The urine was scanty and albuminous. Every day up to the 3rd of May, the shiverings returned at uncertain intervals. The succeeding perspirations were most profuse, and gradually wore her down to extreme emaciation before death took place.

At the examination, which was made twenty-two hours afterwards, the lower part of the left lung was found hepatized. A considerable quantity of yellow fluid existed in the peritoneal cavity. The right lobe of the liver contained a very large abscess, and the right kidney was adherent to its under surface, so that on separating the two organs the abscess broke. Another abscess existed in the same kidney, which probably bore some relation to that in the liver, but the connexion could not be clearly traced. The left kidney was large and rather softened. All the other organs were natural.

**REMARKS.** The most interesting feature in these cases is the total absence of ulceration or other unhealthy appearance in the intestines; unless, indeed, we look upon Case II as one of abscess of the liver consequent upon ulceration of the duodenum. I am, however, of opinion that the correct view is that taken in the account of the autopsy, which attributes the lesion of the intestine to arrested gall-stones. This explanation is to some extent strengthened by the statement of Mr. Curling (*Med.-Chir. Trans.*, 1842) founded upon careful examination after deaths from burning. He considers that abscess of the liver never occurs as the result of ulceration of the duodenum in these cases.

Annesley looked upon pain in the right shoulder as a tolerably sure indication of suppurative inflammation of the right lobe of the liver. In Cases II and III this symptom was absent; at all events, the patients made no complaint of it; whereas in Case I, where the suppuration involved the lobulus Spigelii, and the right lobe was unaffected, constant pain was experienced between the shoulders.

In Case II, the malignant disease had probably been making progress for some time before the man's admission. His sallow and cachectic look led us to diagnose *this*. On the 14th April, exactly a month after he had been under treatment, the pain was more localised, and accompanied by vomiting. It is not unlikely that some peritoneal inflammation was caused at this period by the cancerous growths, and that the inflammation and suppuration of the adjacent hepatic tissue were secondary to this peritonitis.

## Original Communications.

### LECTURE ON THE SOUNDS OF THE HEART, DELIVERED IN THE LIVERPOOL ROYAL INFIRMARY SCHOOL OF MEDICINE.

By A. T. H. WATERS, Esq., Lecturer on Anatomy and Physiology, etc.

GENTLEMEN,—In the present lecture, I propose to consider the causes of the sounds of the heart. The subject is one on which the minds of physiologists seem to be by no means settled; and a careful and candid inquiry into the different causes which have been, or still are, considered to be equal to the production of the phenomena, will, I venture to hope, not be unattended with benefit.

If you place your ear over the præcordial region, you will hear two sounds: one heard most distinctly towards the apex of the heart, dull and somewhat prolonged; the other heard best at the base of the heart, less prolonged and sharper in its character. These sounds are called the *first* and *second* sounds of the heart. The first is synchronous with the contraction of the ventricles and the propulsion of the blood into the aorta and pulmonary artery, and slightly precedes the pulse at the wrist. The second immediately succeeds the first, and is synchronous with the relaxation of the ventricles, and the attempt of the blood to regurgitate from the aorta and pulmonary artery.

Experimental investigation into the causes of the sounds of the heart is of recent date; and at this we shall not be sur-

prised, when we learn that, up to the close of the last century, so little was known even of diseases of the organ, that the only two recognised were polypus and palpitation—the latter being considered a nervous affection.

The first object which I propose to myself is, to trace down from the earliest periods to the present time the history of the different theories which relate to the subject of the lecture.

The theory that muscular contraction, or, in other words, the successive shortening of the muscular fibres, produced the sounds, appears to be the oldest, and is the one advocated by Galen, Senac, Haller, Bichat, and Corvisart. The immortal Laennec embraced the same opinion as his predecessors. He attributed the sounds to the alternate contraction of the auricles and ventricles. "One of the sounds," he says, "is clear and rapid, and corresponds with the systole of the auricles; the other is more dull and prolonged, and corresponds to the systole of the ventricles."

This theory of Laennec remained unquestioned for some years, until it was shown—which would have been at once apparent, if the rhythm of the heart had been better understood—that the contraction of the auricles precedes that of the ventricles; whereas the sound that Laennec attributed to their action—viz., what we call the second sound—immediately succeeds it.

The physiologist who first disproved Laennec's view was Mr. Turner; and he endeavoured to account for the second sound, by supposing that it was produced by the heart falling back on the pericardium, after the ventricular systole. This is disproved by the fact, that the sounds are heard when the pericardium is removed; and further, that the pericardium, in a state of health, is always in close contact with the heart, so that the latter cannot fall back upon it.

Another theory, for which we are indebted to Dr. Corrigan, was, that the impulse and first sound were produced by the rush of blood into the ventricles during the auricular contraction; and the second, by the ventricular systole, which he considered instantaneous. These conclusions are incorrect, because the first sound takes place not synchronously with, but subsequently to, the auricular contraction; and with regard to the second sound, the conclusions are untrue, because the ventricular systole, and the pulsation in the arteries near the heart, do not coincide with, but precede, the second sound.

M. Pigeaux expressed an opinion similar to that of Dr. Corrigan, with regard to the first sound; and of the second, he says: "it is caused by the collision of the blood against the walls of the aorta and pulmonary artery." This is disproved by the fact, that the pulsation in these vessels precedes the sound.

The celebrated physiologist Magendie has given us the following view of these sounds. "The first sound," he says, "is produced by the apex of the heart coming in contact with the ribs during systole; and the second by its anterior surface coming in contact with the sternum during diastole." These conclusions are evidently incorrect, inasmuch as both sounds are distinctly heard after the sternum and ribs are removed.

M. Marc d'Espine says: "The first sound is due to the pure and simple effect of the ventricular systole." This is a recognition of *bruit musculaire*, to the exclusion of all other sources of sound. "The second sound," he says, "is the pure and simple effect of the ventricular diastole." This recognises the power of the muscle to produce sound in relaxation, and excludes the valvular sound.

We are indebted to Mr. H. Carlile for another theory, viz.: that the first sound is produced by the rush of blood into the arteries during the ventricular systole. This is a view held at the present day, and to which I shall have again to refer. He says that the second sound is due to the closure of the semilunar valves.

In the year 1832, Dr. Billing published a view of the causes of these sounds, which he had previously taught to his class, and which has been lately proved to be correct by actual demonstration; viz., that they are both produced by the forcible closure of the two sets of valves; the first, by the closure of the mitral and tricuspid valves; the second, by that of the semilunar valves.

This view was afterwards adopted by Rouanet and Mr. Bryan, and with some slight modification by Bouillaud.

Dr. C. J. B. Williams, who performed many experiments with reference to these sounds, has adopted the muscular theory to account chiefly for the first sound. To this, I shall again have to refer.

In the year 1830, Dr. Hope performed a number of very interesting experiments, with the view of proving what were

the causes of these sounds. The conclusions he came to were, "that the first sound was caused by the systole of the ventricles; and the second by their diastole." From subsequent experiments, he had reason to alter his opinion with reference to the cause of the second sound; and in 1835, he published the following view. "That the first sound was compound; viz., consisting—1st, of valvular sound; 2nd, of the sound of extension, 'a loud smart sound, produced by the abstract act of sudden jerking extension of the braced muscular walls'; 3rd, a prolongation and possibly augmentation by *bruit musculaire*. That the second sound was produced by the sudden expansion of the semilunar valves, resulting from the recoil upon them of the column of blood in the aorta and pulmonary artery."

If you consult the different works on physiology and medicine, where reference is made to these sounds, you will find that the views of authors of the present day are by no means settled on the subject. The difficulty seems to be with the first sound; for the experiments of Hope proved that which Billing propounded with reference to the second sound—viz., that it was due to the closure of the two sets of semilunar valves.

Let us next examine into the events which are taking place in the heart at the time when these sounds are produced, and then ascertain the opinions most generally entertained with reference to their causes at the present day.

The events which correspond with the first sound are—first stage of dilatation of auricles; contraction of ventricles; closure of auriculo-ventricular valves; opening of ventriculo-arterial valves; propulsion of blood against auriculo-ventricular valves, and through the orifices of aorta and pulmonary artery; impulse of heart.

The events corresponding with the second sound are—relaxation of ventricles; dilatation of auricles; backward flow of blood in aorta and pulmonary artery towards ventricles; closure of semilunar valves.

From the time of Laennec up to the present day, as many as twenty-nine theories have been proposed to account for these sounds. To some of them I have alluded; and I now proceed to give you the principal ones held at the present day.

We have, as alleged causes, 1. Impulse; 2. *Bruit musculaire*; 3. Tension of the auriculo-ventricular valves; 4. The rush of blood through the narrowed orifices of the great arterial trunks; 5. The collision of the particles of blood with one another, and with the parietes of the heart; and, lastly, the pressing back of the semilunar valves. Such are the theories with regard to the first sound. With regard to the second, scarcely any difference of opinion exists; it is acknowledged to be due to the closure of the semilunar valves.

Let us now examine the various causes above alluded to. That impulse has nothing whatever to do with the first sound, is most satisfactorily proved by the following experiment; viz., that on removal of the anterior part of the thorax, and on placing the stethoscope on the surface of the heart itself, the sound is in no way whatever diminished in intensity, but if anything more distinctly heard than before. This observation was made by Dr. Hope, and has been confirmed by all subsequent experimenters. Independently of this demonstrative proof, it is difficult to imagine how the impulse could produce a sound, inasmuch as the heart does not strike the chest, always being in contact (in a state of health) with its anterior wall; but simply makes it bulge, as the ventricles during contraction assume a globular form.

I pass over for the present the second and third causes assigned, and come to the fourth, viz., the rush of blood through the narrowed orifices of the great arterial trunks. This view is assumed from the fact that sound is produced by pumping fluid through tubes out of the body, and further that whenever the calibre of a vessel in the body through which blood is circulating is diminished, a sound is produced. That such an analogy as is here assumed, exists between the forcible and sudden pumping of fluid through an inert tube out of the body, and the action of the heart in impelling its blood into the elastic and yielding vessels situated at its base, I cannot admit; nor is the condition of these vessels at their origin at all similar to that of a compressed artery. There is no natural impediment to the onward flow of the blood, the parts leading to the vessels are perfectly smooth, and the vessels themselves are of so elastic and yielding a material, that they dilate in every direction when they receive the blood, as can be readily felt if they are grasped during systolic action of the ventricles. If the arterial orifices were compressed during the time the blood was passing through them, so that their shape would be altered and their calibre diminished, then the same result would take place as in the arteries elsewhere; but no such

compression occurs, nor is there any augmentation of sound, over the base of the heart as there ought to be if this view were correct; on the contrary, when the heart is exposed, and a stethoscope applied to it, the sound is found to be most intense over the situation of the auriculo-ventricular valves.

That the fifth cause assigned, viz., the collision of the particles of blood amongst themselves and with the parietes of the heart, a view advocated by some physiologists, can be an element in the production of the first sound, I can scarcely imagine. It appears to me that if such were the case, it would produce an almost continuous murmur, for there must be collision equally during diastole of the ventricles, and especially when the contents of the auricles are propelled into them, as during systole, and sound ought to be produced during the one act, as well as during the other; but such is not the case; no audible sound is produced by the propulsion of the blood into the ventricles, and it is but fair to infer, that none is produced by its propulsion from them.

I pass to the consideration of the muscular sound, the *bruit musculaire*. It has always been the opinion of physiologists that the contraction of the muscular walls of the ventricles, was either the sole cause, or an element in the production, of the first sound. Dr. Hope adopted the latter view, and it is generally entertained at the present day.

If you place your ear or a stethoscope over a muscle during its contraction, you will hear a sound, but in its character it differs entirely from the first sound of the heart. Perhaps you cannot have a better example of the sound produced by muscular contraction, than that afforded by the action of the masseter muscle. If, when your head is resting on a pillow, you forcibly contract your masseter muscle, you will hear a rumbling noise—a noise which continues during the contracted condition of the muscle, and is also heard during its relaxation. The exact cause of this sound I am unable to tell you. It may be due to the change in shape of the muscular fibres—it may be due to the alteration of the circulation—or to some other cause; but the more you examine it, and all other muscular sounds, the more will you be convinced, that they bear no resemblance to the first sound of the heart.

Before alluding to the experiment which some of you have witnessed, and which proves beyond all doubt that the contraction of the ventricles produces no sound, I think it right to bring under your notice, other arguments which tend to prove the point. If the ventricles during contraction produced sound, it is but fair to infer that the auricles would do so. We find, however, that such is not the case. If it be said that the muscular substance of which the walls of these cavities are composed, is so slight, that no audible sound could be expected from its contraction, I maintain that such an argument is unsound, and I would ask, what is the exact amount of muscular fibre necessary to produce an audible sound? Are not the auricles of a full-grown man as large, and do they not contain as much muscular fibre, as the ventricles of a *fœtus in utero*?—and shall we admit that the contraction of the latter will produce the clear audible click, with which many of you are familiar, and deny that the former can have any such effect at all? Again, presuming that the auricular fibres in man are not sufficient in number to produce a sound, are there not animals which possess auricles larger than the ventricles of some adult men?—but yet, if you listen to the sounds produced by their hearts (as, for instance, the ox or the horse), you find that they are the same in number and character as those of the human being—during the auricular contraction no sound is heard. These facts seem to me conclusive against the muscular theory, and to render it perfectly untenable.

We are not, however, without positive evidence of the truth of the assertion, that the ventricular contraction produces no sound. To Dr. Halford, who has lately brought the subject under the notice of the profession in London, and who has performed his experiments in this theatre, we are indebted for the beautiful experiment which some of you have witnessed.

The experiment consists in depriving an animal (dog or donkey) of sensation by means of chloroform; and, whilst artificial respiration is kept up, the anterior part of the thorax and the pericardium are removed and the heart exposed; on listening to the heart, the two sounds are heard. For the remaining part of the experiment, I quote Dr. Halford's own words.

"The superior and inferior venæ cavae, and the pulmonary veins, were now compressed between the fingers, and the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time, the veins were allowed to pour their contents into



both sides of the heart, and both sounds were instantly reproduced. The veins were again compressed, and all sound extinguished, notwithstanding that the heart acted vigorously. Blood was again let in, and both sounds restored. All that is claimed for the above experiment, is its exemption from any rude interference with the mechanism of the heart's action. The cavities of the heart are untouched; there is no finger thrust into the auricle or ventricle; no hooking back of valves; in fact, not one source of sound substituted for another. Both sounds are destroyed and reproduced by the same means; the strongest argument for their both depending on the same cause, which is simply the backened current of blood, first against the auriculo-ventricular, and second against the ventriculo-arterial valves."

Now what is the value of this experiment? It proves unquestionably that there is no such thing as *bruit musculaire* in connection with the systole of the ventricles, and it disproves all the theories founded on a contrary supposition; it proves, moreover, that whenever the blood is allowed to play upon the valves, the sounds are produced.

Before Dr. Halford performed his experiments, in the original of which I assisted him, a very ingenious experiment had been performed by Mr. Brakyn, which consisted in propelling, by means of bladders and tubes connected with the left side of the heart, air through the cavities, so as to represent the flow of the blood. His experiment proves that when the auriculo-ventricular and the ventriculo-arterial valves are thrown into a state of tension, by air acting on them in the same way as the blood, sound is produced. This experiment I have frequently repeated—the original apparatus used by Mr. Brakyn being still in my possession—and the sounds resemble in every respect, considering the nature of the fluid in which they are produced, those of the living heart.

These two experiments seem to me to settle entirely the question which has so long agitated the minds of physiologists. Mr. Brakyn's proves that the tension of the valves is equal to the production of the sounds, and Dr. Halford's that muscular contraction has nothing to do with them.

The pressure of the semilunar valves against the sides of the great vessels is said to assist in the production of the first sound. This requires but slight consideration. Against what are they pressed? Against the *yielding* walls of the vessels, and can there be any element of sound in such an occurrence? I think not.

I think I have now proved that all the phenomena synchronous with the first sound, except the closure of the valves, are unequal to its production, or even to assist in it; and also, that such closure is equal to produce the effect. It is then to the *tension of the mitral and tricuspid valves, produced by the blood being forcibly propelled against them*, that this sound is due.

With regard to the second sound, there is but little difference of opinion; the experiments of Hope proved that it was solely due to the semilunar valves. When the arteries recoil after being distended by the ventricular systole, the blood in them is forced back towards the ventricles, the semilunar valves then come into play, they are stretched across the vessels, and suddenly made tense, and then sound is elicited.

If you listen to the sounds carefully, you will find that they do not differ in *kind*, but only in *degree*. The auriculo-ventricular valves are large, thick, and strong; consequently their vibrations are slow, and the sound they produce prolonged. On the other hand, the ventriculo-arterial valves are small, thin, and comparatively weak, and their vibrations are rapid, and the sound they produce short. Both sounds may be illustrated by making tense two pieces of membrane of different size and thickness.

If the theories I have mentioned require any further confirmation, they receive it from the sounds which result when there is disease of the heart. If you hear a murmur, you know there is something wrong with the valves, you do not think of the muscular walls; you know that there is some deposit in connexion with the valves, which either from its roughness causes a sound as the blood passes over it, or else prevents the valves properly closing, and thus allows of regurgitation.

Moreover, consider the alteration in the sounds, which is the result of a change in the muscular walls. When the ventricles are hypertrophied, the first sound is less distinct, and of a muffled character; it has to pass through the thickened muscle, and necessarily comes less sharply to the ear. Again, when there is dilatation of the ventricles and thinning of their walls, the sound is clear and sharp; it has to pass through a smaller space and is less altered in its character. If the sound were due to muscular contraction, surely, in obedience to the

law of physics, that if you increase the cause you increase also the effect, the sound of a hypertrophied heart would be louder and more distinct than that of one in which the muscular fibres are diminished; but the contrary obtains, and this fact affords an additional proof of the valvular theory.

## REMARKS ON DR. HALFORD'S EXPERIMENTS CONCERNING THE SOUNDS OF THE HEART.

By W. O. MARKHAM, M.D., Assistant Physician to St. Mary's Hospital, London.

DR. J. B. HALFORD has lately performed a series of experiments at different medical schools in London, for the purpose of demonstrating, amongst other things, that the opinions long ago laid down by Dr. Billing, respecting the nature and causes of the sounds of the heart, are correct; viz., that both the sounds are entirely and alone produced by the valves of the heart.

It would appear from remarks, which have been made in some of the medical periodicals, upon those experiments, that they have been by many persons received as positively demonstrative of the opinions above mentioned. As I happened to be a witness of the experiments which were made at St. Mary's Hospital, and as I could not convince myself of the correctness of the above conclusion from what I then and there observed, and as I think them quite untenable on other grounds, perhaps I may be permitted to state, in a few words, my reasons for being so unfortunate as to differ from many others, who have assisted at these vivisections.

The position assumed by Dr. Halford is this:—When no fluid—liquid or gaseous—passes through the cavities of the heart, the valves are not called into action, and no sounds are produced. Hence, the sounds of the heart depend upon the action of the valves. In order to demonstrate this position, Dr. Halford rapidly opens the thorax of a dog under the influence of chloroform, and by the aid of a bellows inserted into the trachea keeps the animal alive by sustaining the respiratory functions. He then skilfully cuts off all the sources through which blood, venous or arterial, can find its way into the heart's cavities. When this has been effectually accomplished, Dr. Halford finds that the sounds of the heart are no longer audible.

In the two cases in which I saw Dr. Halford operate, I could not admit the correctness of his views; and for the reason, that I still heard two sounds associated with the movements of the heart, although he assured me that all sounds were at the moment inaudible.

Certainly, the sounds I heard differed vastly from the healthy and natural sounds of the heart; they were weak, dull, and muffled, resembling rather the obscure flutterings of a heart rapidly and feebly beating in the last agony; but, nevertheless, of such a character, there they were to be heard, at least by the evidence of my sense.

I am well convinced, from the very nature of the experiment, and from the difficulty of rightly manipulating with the stethoscope under such circumstances, that the sounds might be readily overlooked; and I can quite understand that many persons might not be lucky enough to catch them. However this may be, it is clear that the negative evidence of numbers cannot destroy the positive evidence of one's own senses; and therefore I cannot admit, on this ground, that Dr. Halford's experiments prove the correctness of the position which he takes up.

Admitting, however, the incorrectness of my own observation, and assuming the correctness of Dr. Halford's—viz., that in the cases before us no heart's sounds were audible—I really think some objection may be very fairly taken to the sweeping views thence deduced by him. Is it right, one might ask, to assume that in an animal thus mutilated, and subjected to sudden and violent shocks of the nervous, arterial, and respiratory systems, *all* the elements which may possibly conduce to the formation of the heart's sounds can be left in undisturbed action? Surely, all the other possible causes which may form, or assist in forming, the heart's sounds, as they are heard during healthy life and under a natural condition of existence, must be eliminated from the calculation, before Dr. Halford can assume, as he does, that to the motion of the valves, and that to it alone must be ascribed the cause of the sounds in question. What becomes of the impulse of the heart against the thoracic walls? How is the muscular bruit to be got rid of? How is the rush of blood through the heart's orifices and