Dr. Hughes Bennett F.R.S.
with the author's compliments

(15)

ON THE

SOUNDS CAUSED BY THE CIRCULATION OF THE BLOOD.

BEING

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ON THE

SOUNDS CAUSED BY THE CIRCULATION OF THE BLOOD.

IT is a matter not merely of interest, but of great importance, to determine the causes of the sounds produced by the circulation of the blood. But while the labour bestowed on it has been proportionably great, the want of general principles is amply proved by the very contradictory results. Even in the explanation of the different sounds by the same authorities there is much discrepancy. Thus we find the cause of the first sound of the heart affirmed to be totally different from that of the second sound, which is *primâ facie* improbable. The explanation I am about to give is consonant with general laws, and is certainly more comprehensive than any other that has been proposed.*

Two essential circumstances relative to the formation of sound have been overlooked by all investigators—the effect of pressure on the blood in the heart and vessels, and the effect of the consistency of the blood.

All sounds formed in connexion with the circulation are produced by and in the blood itself, and their mechanism is virtually the same.† If this statement be true, it would be found in practice, that a sound of one species would be liable to

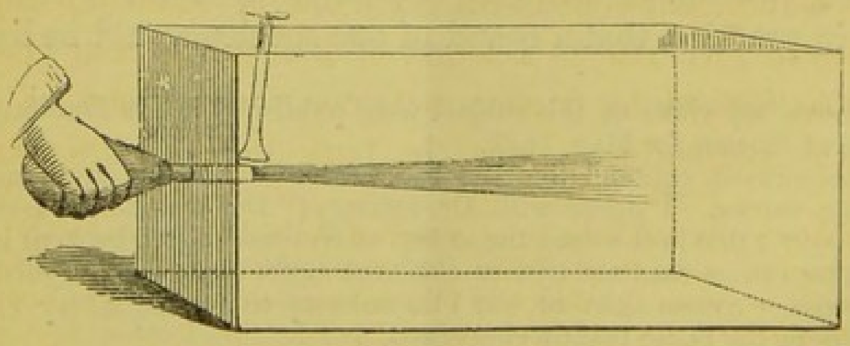
* Some of my views on this subject were published in the *Dublin Journal of Medical Science* for May, 1852.

† Some authors contend that the heart's sounds are due to a number of coincident causes. I agree with Dr. Billing, ("Diseases of the Lungs and Heart," p. 27,) that in this case the axiom of Newton should be kept in view, that "more causes than are true or sufficient ought not to be assigned." But whether minor causes exist or not I do not stop to inquire, as my explanation refers to the cause mainly concerned.

be changed into a sound of another species. And this is the case; for the normal first sound of the heart is under certain pathological conditions converted into a murmur; and on the other hand, a murmur thus produced may again give place to the natural sound. In short, all the sounds may be regarded as modifications of a typical sound, because under varying circumstances they are convertible into each other.

The first thing to be established is, that rapid movements of a fluid produce sound, independently of any action, as friction or vibration, upon the vessel in which the fluid is contained. This was proved by the following experiment:—A horizontal reservoir, one foot and a half long, eight inches wide, and as many deep, the end of which was perforated at its centre by a brass tube, was provided. This tube had an internal diameter of half an inch; one of its extremities projected two inches into the reservoir, while to the shorter external part of the tube an india-rubber bottle was adapted. When the reservoir was filled with water, a column of it could be forcibly projected in an intermittent manner into a body of water in a state of rest, by alternately compressing and relaxing the india-rubber bottle grasped in the hand. If during these actions the end of a solid stethoscope was introduced beneath the water close to the point where the jet issued from the tube, but so as to avoid the current, a blowing sound was heard through the stethoscope, loud or weak in proportion to the force employed in compressing the bottle.* The blowing

* The nature of the apparatus and position of the stethoscope will be better understood by a glance at the woodcut.



I varied the experiment by employing a perpendicular reservoir—the sound, *bruit de soufflet*, might also be heard in all parts of the reservoir. If the stethoscope was so held that the jet of water impinged upon it, a louder sound, but still a distinct *bruit de soufflet* was produced.

In this experiment the stationary water was forcibly penetrated by the moving column, while, owing to the mobility of the fluid, sufficient friction occurred between its particles to give rise to low-pitched sounds—murmurs. The size of the reservoir precluded the possibility of friction of the water against its sides being concerned in causing sound.

In order to test the effect of viscidity of the fluid upon sound so produced, the experiment was modified as follows:—The apparatus having been filled with glycerine instead of water, the same motions were performed. The first thing obvious on listening with the stethoscope was the comparative difficulty of producing sound. But when the india-rubber bottle was compressed very strongly, a faint sound, resembling the first sound of the heart, was heard near the mouth of the tube. When, however, the moving column was allowed to strike the stethoscope, a loud sound, exactly resembling the heart's first sound—and therefore presenting a striking contrast with the sound produced by water under similar circumstances—was heard.

In this experiment, the divisibility as well as the mobility of the fluid was greatly diminished by its increased viscidity and specific gravity. The impact between the fluid forcibly expelled from the tube and the fluid in the reservoir was therefore more complete than in the case when water was employed. Hence a sufficient concussion occurred between the opposing particles to give rise to a sound of a very different character from that formed by friction between the particles of water.

It was perfectly obvious, in both experiments, that the

india-rubber bottle and tube being attached at the bottom, and a stethoscope being arranged so as to perforate the reservoir; but the pressure of the fluid greatly increases the difficulty of producing sound. I have been always careful to eliminate any extrinsic sound, such as that from compressing the bottle, from consideration.

sounds were formed in the fluid external to the tube; in the case of the water, because the murmur, although heard from transmission when the stethoscope was applied to the tube, was much louder beyond it, and because it will be afterwards shown that water, moving under pressure in a tube in which there is no obstruction, does not give rise to murmur; in the case of the glycerine, because no sound whatever could be heard in the tube, although a sound was heard external to it.

Even when the jet was allowed to impinge against the stethoscope, the results clearly showed the very different action of water and glycerine. In both cases there was increased loudness, but the characteristic sound was in each case preserved.

Two important principles were established by these experiments. First, that sounds may be formed by the motions of fluids only. Second, that the quality of a sound thus formed is materially influenced by the nature of the fluid.

The sounds formed by the circulation of the blood are produced on principles similar to those detailed in these experiments. I shall, therefore, for convenience, and as conveying their true nature, designate them as blood sounds.

Blood sounds are divisible into two classes—

Sounds which give the impression of a shock.

Sounds which give the impression of a current.

Shock sounds (*Bruit de choc* of French authors) comprise the normal sounds of the heart, and certain abnormal sounds formed in aneurismal sacs.*

Current sounds are formed in the heart, in aneurismal sacs, and in the larger arteries and veins. These sounds are

* The shock sounds of aneurism have been regarded by some authorities as being merely the heart's sounds transmitted, but the best authorities (*e.g.* Skoda, Stokes, Gendrin, &c.) hold that they are independent sounds; nor do I think that any careful observer will arrive at a different conclusion. I cannot, however, with Skoda, regard as independent formations shock sounds heard in rare cases in apparently healthy arteries distant from the heart, even the crural and bronchial. I agree with Dr. Stokes, ("Diseases of the Heart and Arteries," p. 252,) that the sounds in question are more probably transmitted.

all abnormal; they are extremely diversified, some closely resemble shock sounds, others are aptly compared to the blowing of bellows, to rasping, sawing, cooing, &c.

Although well-marked examples of the different kinds of sound cannot possibly be confounded, it is sometimes difficult to determine to which class a particular sound belongs; the first sound of the heart is frequently replaced by a murmur only to be detected by great attention, and this indicates a close alliance in the mechanism of the two kinds of sound.

Pressure is a condition of material influence, whether as regards the production of sound in rigid tubes or in living vessels.

Current sounds are never produced in a tube if the fluid moves through it under sufficient pressure. If the pressure is insufficient, sound is generated by friction between portions of the fluid moving with more rapidity than others. It seems probable, that in the case of a fluid moving under low pressure the outside portions of it are detained by friction with the interior of the tube, while the central portions in consequence move with increased rapidity.

The above may be easily demonstrated by the following experiment:—Let a tube some feet in length be inserted in a perpendicular manner in the bottom of a large cistern, and let the other extremity of the tube be immersed in water. If the cistern be filled with water, and considerable pressure is exercised on it as it flows through the tube, little, if any, sound is produced; as, however, the water in the cistern diminishes, a low continuous current sound is developed.

A partial obstruction at any part of a tube produces diminished pressure beyond the obstruction.

If during the flow of water in the preceding experiment the caliber of the tube is contracted at a given spot, a murmur is formed immediately below the point of contraction. It is plain that in this case the murmur is caused by the rush of water from the contracted part of the tube into a part in which the area is undiminished. Owing to the immersion of its lower extremity, the tube remains full, and the water rushing from above the obstruction, in diminished volume but with increased rapidity, penetrates the more slowly moving water below it. A murmur (in this case continuous) is thus produced on the same principles as in the experiment, in which a jet of water was projected into the full reservoir.

If these experiments with the cistern and tube are repeated, using fluids of various degrees of viscidity instead of water, it will be found that the difficulty of producing sound bears a direct relation to the viscidity of the fluid. If we apply these facts to the living body, we find that in a state of health the pressure on the blood bears such a relation to its viscidity, that, except at the heart, no sound is produced by its circulation. When, however, the blood's viscidity is notably diminished, although the quantity of blood is normal—and consequently its pressure unimpaired, as happens in anæmia—murmurs are produced.*

On the other hand, if while the blood retains its proper viscidity its pressure is much diminished, the same results follow. This is proved by the murmurs heard in the heart and larger arteries in cases of excessive hæmorrhage.

Varieties in murmurs are due to special circumstances, but the general conditions under which venous humming and the blowing sound of an aneurism are formed are precisely the same.

Continuous murmurs are commonly heard in the jugular veins of the anæmic, and the same kind of murmur can easily be produced in the healthy by pressure, as of the stethoscope

* It may be objected, that in some well-marked cases of anæmia no murmurs are formed. But the objection can be answered in at least two ways: 1. An increase of pressure may compensate for decrease of viscidity. Venous humming in the neck disappears when the head lies below the level of the thorax; and when the person is standing, a deep inspiration produces the same effect, which is obviously due in both instances to increased pressure. A congested state of the *vena cava* would, on the same principle, prevent *bruit de diable* from being at all developed. 2. Although a thin condition of the blood is a common result of anæmia, it is possible that in some cases the lesion may mainly consist in simple diminution of the red particles, with little or no loss of fibrin.

during examination. The caliber of the vein is by this means diminished locally, and a murmur is formed, as in the experiment with the contracted tube. In these instances the *proximate causes* of the sounds—diminished viscidity in the one case and diminished pressure in the other—are very dissimilar; but the *immediate cause*, increased mobility of the blood, is in both identical.

We are now in a position to comprehend the formation of the sounds of the heart. If the motion of the blood through the heart and arteries were equable, no sounds whatever would be produced in a state of health. But the blood is by the action of the heart thrown into impetuous and interrupted motions, and these give rise to normal sounds, known as the first and second sounds of the heart; both are formed in the outlets of the two great vessels,* and the mechanism of both is essentially the same.

THE FIRST SOUND coincides with ventricular systole, and is caused as follows:—Blood having been forcibly driven from the ventricles into the aorta and pulmonary artery, comes into forcible contact with blood in these vessels, which, supported by the semilunar valves, had attained a state of momentary repose.† The impact between the fluid in motion and that in a state of rest gives rise to the sound.

The well-known thumping sound heard during the action of the common forcing pump is chiefly due to an analogous cause, that is, to the impact between water thrown into violent motion and stationary water in the discharge pipe.‡ But the

* On this point, the assertion of so competent an authority as Cruveilhier is worth quoting. In his examination of the remarkable case of an infant born with the heart exposed, he found that the maximum intensity of both sounds was at the same place—the base; and he observes, "hence it is at the base of the organ we are to look for the cause of these sounds."—Cruveilhier, *Gaz. Medicale de Paris*, 1841.

† I shall afterwards show that there can be little doubt that a secondary propulsion of blood occurs from the elastic contraction of the arteries constituting their systole. But this does not affect the general truth of the above, since the blood resting immediately upon the valves must remain almost, if not absolutely, at rest.

‡ In this instance the great force employed, and the great pressure upon the water resting upon the valve, make amends for want of viscidity.

analogy between the conditions causing the heart sound and those causing sound in the experiment with glycerine is more complete than might be at first supposed. The column of glycerine forcibly driven through the glycerine at rest does not, owing to its viscidity, diffuse itself so as to cause friction between its molecules and those of the surrounding fluid sufficient to produce murmur, and an imperfect shock sound results. But enclosure in a tube of both the moving and stationary blood, and their separation by valves, greatly favours the development of sound on the same principles.

Four conditions are necessary for the production of the perfect first sound:—

1. Sufficient viscidity of the blood.
2. Sufficient force in the circulation.
3. Sufficient pressure upon the blood.
4. The absence of obstruction at the outlets of the heart.

When all these conditions are fulfilled, the result is a true shock sound. If, however, the first condition, viscidity, is wanting, we find in place of it the blowing sound of anæmia, as already mentioned.

The force of the adult left ventricle has been variously stated, but the estimate of Valentin, that it is sufficient to overcome a resistance of four pounds, seems probable. It is, at all events, certain, that when the force of the heart is much diminished, as occurs in certain fevers and in syncope, the first sound becomes impaired, or is altogether lost.

The fact that pressure upon the blood prevents murmur by preventing friction need not be here repeated. A common cause of diminished pressure is incompetency of the intra-cardiac valves, since regurgitation reduces the volume of blood sent into the outlet vessels.

The fourth condition, absence of abrupt interference with the current, is resolvable into the third. For any obstruction at the mouth of the aorta, or pulmonary artery, whether it be contraction of its caliber or the projection of an atheromatous deposit, must cause diminished pressure in the vessel beyond the obstruction. The principles upon which *bruit de soufflet* is then formed have been already detailed.

It is highly probable that a deviation in any one of these conditions, although too slight to destroy the character of the sound, causes a corresponding modification of it. The water and glycerine used in separate experiments respectively represented exaggerated conditions of thinness and viscidity of the blood, and I have obtained intermediate results by employing fluids of medium viscidity. But the delicate relation between pressure and viscidity that exists in the living body was wanting in the experiments.* A close study of this relation will alone throw light on the modifications of all the sounds produced by the circulation. Every one accustomed to study the heart's sounds must have observed wide variations in these sounds, even regarded as within the limits of health. For instance, in the robust subject we frequently hear sounds of a dull and muffled character compared with the loud and resonant sounds in the delicate. These facts cannot be explained on any prevailing theory of the heart's sounds, but are entirely in accordance with my views—that unusual pressure and viscidity, by which are implied an abundance of blood rich in fibrin, as would occur in a vigorous subject, cause the sounds to be relatively low. Probably impairment or loss of the first sound in certain fevers in which the fibrin of the blood is known to be in excess, is partly due to consequent increased viscidity. The first sound is always affected in fever before the second, and this circumstance, as well as the fact that the second is much less subject to alterations than the first, will be afterwards seen also to depend on the mechanism of the sounds at present advocated.†

* I have laboured hard to produce an artificial circulation which should fulfil all the requisite conditions for the formation of sounds like those of the heart, by means of an india-rubber apparatus, but hitherto without success as regards the first sound. The delicacy and nicety of adjustment necessary in the valves form the great difficulties.

† It was my intention to have considered the bearing of my views upon the various pathological alterations of the heart's sounds, but I found the

The Second Sound occurs during diastole, and in its mechanism closely resembles the first. The blood having been driven with much force into the aorta and pulmonary artery, a portion of it recoils, but is checked in its rapid descent towards the heart by the semilunar valves. The sound is caused by the concussion thus induced, the force of which is, however, by no means sustained by the valves alone, for they are thoroughly supported by the ventricles and their contents. This is obvious since there can be no approach to a vacuum in the heart. The valves are to be regarded as separating media which do not themselves sustain the force of the descending blood. A valve thus supported is known in the arts as an equilibrium valve.

An experiment at hand in most houses demonstrates the principle on which the second sound is formed. When a cock, attached to the lower end of a perpendicular pipe of some length, through which water is flowing from a cistern, is suddenly turned, a loud jarring sound is heard. It is caused by a concussion * in the water from the sudden arrest of its onward flow. The semilunar valves are here represented by the plug of the cock, and, allowing for the difference between rigid and flexible materials, the conditions are very similar, since the elastic reaction of the vessels effects a pressure on the blood which is effected in case of the water by length of the pipe. If, then, the pipe and cistern are capable of yielding a sound

subject altogether too extensive for the present essay, and mean to return to it on a future occasion. Meantime, I venture to assert that I can offer rational explanations of many phenomena hitherto difficult of comprehension. Certain physiological points also require notice; in reference to the relative loudness of the second sound of the fœtal heart compared with its first sound, I would refer the reader to my observations in the *Medical Times and Gazette*, November 3, 1860.

* As the closest contact must exist between the water and the barrier instantaneously presented to its egress from the pipe, the sound cannot be caused by an actual stroke of the water against the barrier, although it is much exaggerated by the metallic resonance of the pipe and cock. I explain the sound by a relaxation of molecular cohesion in the fluid owing to rapid motion, and that, from its almost incompressible nature, a concussion occurs between its particles when suddenly brought to a state of rest, the sound being naturally most intense near the obstruction. Whatever explanation is adopted will not affect the analogy between the mechanism of this remarkable sound and the second sound of the heart.

which may be heard at a considerable distance, it cannot be wondered at if the heart and its vessels, on the same principle, give rise to sounds audible through a stethoscope or by direct contact with the body.

If the cock is only turned so as to allow even a small portion of water to pass through, a rushing sound (in this case continuous) results. The change of the normal second sound into a murmur from incompetency of the valves is thus demonstrated.

The second is a sharper and shorter sound than the first, because the separation between the opposing blood is definite and complete. We have seen that in case of the first sound several conditions are necessary to ensure sufficient molecular cohesion in the separate bodies of blood at the moment of contact, which must also be accompanied by considerable force. But the intervention of valves in the present case causes the conditions to undergo considerable modifications.

Less force is requisite. It is for this reason that loss of power in the heart affects the first sound so much sooner than the second. This has been already dwelt upon.

Less pressure on the blood is necessary; and hence, while the first sound is changed by excessive hæmorrhage into a murmur, the second sound merely becomes fainter.

Viscidity is not a necessary condition; hence in anæmia the second sound never degenerates, like the first, into a murmur. On the other hand, a condition peculiar to the second sound is, that the valves shall be competent. If the closure of the aortic valves is imperfectly performed, the well-known *bruit* from aortic regurgitation occurs, and this may completely supplant the normal second sound.*

* As each sound of the heart is formed in duplicate, it might be supposed that when a morbid change only occurred in the portion of sound emanating from one side, the compound sound would always resolve itself into two parts —a normal and an abnormal—heard coincidently, which, however, is not the case. But structure and position allow of little doubt that the left side of the heart is mainly concerned in forming the sounds, and it is certain that, owing to greater liability to organic changes, the same side is far more productive of murmurs than the right. Well-marked changes occurring in the sounds

The absence of any considerable obstacle in the great vessels is necessary; and hence the change of the second sound into murmur from encroachment upon the aorta by a morbid growth or deposit, the semilunar valves remaining healthy, is possible.*

I succeeded in producing a sound closely resembling the natural second sound by the circulation of water through the left side of the heart and portion of the aorta removed from a calf. The aorta was connected by means of a short glass tube with another of india-rubber, while connexion with the ventricle was established by means of a screw flange adapted to a brass tube, to which an india-rubber tube was also attached. The circle was completed by attaching the free ends of the india-rubber tubes to an apparatus provided with valves in imitation of the heart, by the alternate compression and relaxation of which circulation was maintained.

M. Bouillaud also succeeded by the following arrangement—"he attached one extremity of a short glass tube of an inch bore to the aorta, immediately *below* the semilunar valves, and to its other end a bladder full of water. Another tube, four feet long, was connected with the aorta *above* the semilunar valves. The bladder was suddenly compressed at intervals, so as to jerk up the fluid, and each time that the pressure on the bladder ceased, and the column of liquid was allowed to fall back upon the valves, a sound very analogous to the second sound was heard."

A somewhat similar experiment was made by Dr. Corrigan, but with a different result:—He removed the heart and ascending aorta of an ass, and then "tied it on the end of a leaden tube of corresponding diameter, about five feet long; about two or three inches of the aorta then being free from the lower extremity of the tube. In this state, holding the sides of the aorta together below, he filled the tube with

of the left side therefore absorb, as it were, the faint normal sounds proceeding from the right side.

* See case by Dr. Cockle, *Brit. Med. Journal*, April 25, 1857.

water, and then placing the thumb on the upper end, so as to close it, the fingers were withdrawn from the lower end, and, the upper end still remaining closed, the external pressure of atmospheric air kept the two sides of the aorta below together, and no fluid escaped. The ear was then applied to the lower end of the tube, close to the aorta, and the thumb being suddenly withdrawn from above, the whole column of fluid came suddenly down and distended the aorta and valves, and yet there was no sound whatever similar to the second sound produced." He then "attached a piece of sounding-board to assist the ear, and the result was the same as before."

I give these extracts from Dr. Bellingham's work on Diseases of the Heart, for the purpose of explaining why M. Bouillaud's and my experiment succeeded, while Dr. Corrigan's failed, because the failure is of itself very instructive.

In my experiment a short glass tube formed the connexion between the aorta and the india-rubber tube, I had, therefore, an opportunity of seeing that the presence of even a few air bubbles completely prevented the sound. The sound was only properly developed when all air was excluded from the apparatus, and the mode of M. Bouillaud's experiment was such as to ensure this exclusion. Now Dr. Corrigan states that he held the sides of the aorta together until he had fixed them in that position by removing atmospheric pressure from the water in the tube. But it is quite certain that below the point where the sides were in contact a small quantity of air was lodged between the valve and the aorta, and this air would as certainly prevent the formation of sound, on the principles on which it is formed from turning a cock; indeed, an invention to prevent noise from the latter cause consists in attaching a short perpendicular tube, closed at the top and containing air, to the discharge pipe, above, but near the cock. This air acts as an elastic cushion, and breaks the force of the concussion, which would produce sound. Now, supposing the second sound of the heart to be the result of tension of the valves by the blood, the air in contact with them, in the experiments with water, would only increase the sound; whereas its effect in small quantity is to destroy it. Nothing can show more clearly than this, that, although in accordance with received opinions, the second sound depends on closure of the semilunar valves, it is not formed by vibration of the valves, but by a concussion in the blood itself. Moreover, the possibility of the latter in case of the second sound proves the same possibility in case of the first sound, and should be a sufficient answer to those who allege that no concussion can occur between blood issuing from the ventricles and that in the vessels.* It has been previously pointed out, that one condition—force—is here in excess as compared with that which occurs in the formation of the second sound, in which the more complete resistance presented by the closed valves makes amends.

The first sound is usually loudest over the apex of the heart, and the second over the commencement of the aorta, but it is also heard very clearly in the track of the aortic arch. This accords with the present explanation; for since the first sound is formed by the meeting of blood expelled from the ventricles with that in their outlet vessels, it is easily conducted downwards by blood still in the ventricles, and is at the same time intensified by their solid walls; while contact of the apex of the heart with the interior of the chest causes the corresponding external point to be that at which the sound is most distinctly heard. The second sound, on the other hand, is formed at a time when no connexion exists between the blood in the ventricles and that in the vessels; its conduction downwards is therefore interfered with, but it is heard with much distinctness for some distance in the track of that vessel at whose base it is mainly formed. The relation of inferior and superior—names proposed for the sounds of the heart by M. Pigeaux—is thus accounted for.

* This objection has been made to my explanation of the first sound by a reviewer in the *British and Foreign Medico-Chirurgical Review*, April, 1860.

It is well known, however, that the point of greatest intensity of the sounds occasionally varies in a remarkable manner: the second sound may be loudest over the apex, or the first over the base, &c., and these anomalies have been relied on as proving the complicated nature of the sounds—that, for instance, the second sound of the apex is due to a distinct cause from the second sound of the base.* But I can see no reason for such a supposition in these exceptional cases, which appear to depend on accidental circumstances connected with conduction of sound.

The occurrence of aneurismal sounds identical with those of the heart affords the clearest proof that shock sounds are not dependent on vibration of valves or muscular contraction, because neither of these agencies exist in the aneurismal sac.

It is impossible to understand how shock sounds could be formed by friction; but as friction between the blood and lining membrane of the orifice and parietes of the sac has been affirmed to be the cause of sound, I tried the following experiment, in order to ascertain the kind of motion which exists in the sac.

A glass globe of moderate dimensions, having a free communication with a short tube of the same material, so as to resemble an aneurism seated upon an artery, was provided. A tube of india-rubber was adapted to each end of the glass tube, while, to complete the circle, the other extremities of the india-rubber tubes were attached to an apparatus of the same material provided with a valve. By this arrangement an intermittent circulation could be maintained through the tubes and globe when filled with water. I observed that during systolic action the fluid in the globe underwent much less displacement than might have been anticipated. A slow eddy or revolution of the fluid in contact with the interior of the globe occurred, in a direction contrary to the motion of that in the tube, while the only active motion was at its neck. As the conditions are essentially the same, it was thus proved

* Skoda. Translation by Dr. Markham, p. 198.

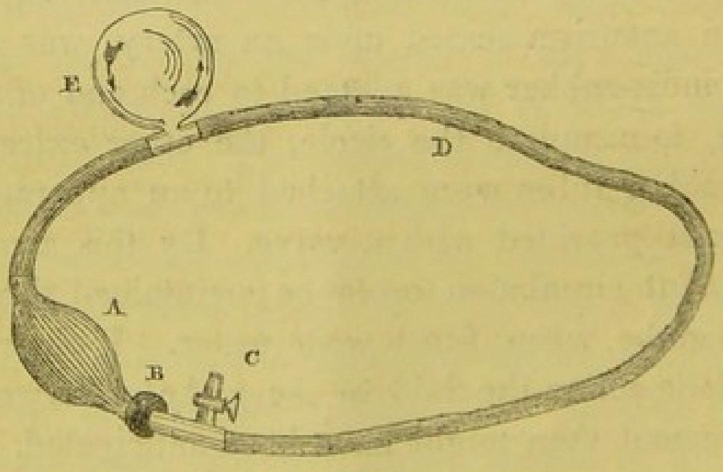
that the motions of blood in an aneurismal sac are restricted, and that no friction occurs sufficient to produce the sounds attributed to it.*

Aneurismal shock sounds appear to be only formed when the sac of the aneurism is well defined,† and the following is their true mechanism. Blood is forcibly driven by systole through a comparatively narrow orifice into a more or less globular reservoir filled with the same fluid in a state of comparative repose, the first sound is formed by concussion, on principles already stated and which require no repetition.

The second aneurismal sound is formed precisely in the same way, but by a weaker influx of blood, which happens during diastole. The cause of this secondary influx has been much debated, but no explanation I have seen appears sufficient, and I offer that which follows.

In case of an aneurism situated on the aortic arch, the increased pressure which existed in the sac during systole is subsequently diminished by the loss of some of its contents, partly from the reaction of the sac itself, but mainly owing to the backward rush of blood towards the heart. But this

* The apparatus is here represented:—



The india-rubber apparatus to represent the heart is shown at A; B, the box containing the valve; C, the pipe provided with a cock for filling the system; D, the india-rubber tubes connected; E, the glass globe. The direction in which the water moves is shown by the bent arrows.

† Dr. Lyons believes that double impulse and sound are peculiar to distinctly sacculated aneurisms.—See *Dub. Journ. of Med. Science*, May, 1850.

blood is suddenly arrested by the semilunar valves, and a portion of it is—by the arterial systole or reaction, which is a consequence of previous distension by the systole of the heart—again forced upwards and into the sac, attended by the formation of a shorter shock sound—the second sound of aneurism. The valve is here an important agent, and sufficiently explains a fact which has been found so puzzling, that double sounds are almost confined to aneurisms of the upper part of the aorta. For it is plain that a force arising out of the elasticity of the vessel, whose point of resistance is the valve, would be operative only within moderate limits. Hence the secondary blood wave is rarely capable of producing sound in aneurisms situated beyond the limits of the aortic arch.

I have already hinted my belief that murmurs alone are produced in aneurisms which consist rather of a dilatation of the vessel than of a distinctly formed sac. This appears to me the explanation: all the blood contained in a mere dilatation is moved onwards with some velocity by the heart's systole, but it is penetrated by a more quickly moving portion, whose axis and dimensions nearly correspond with those of the unaffected part of the vessel; hence, the conditions being favourable for friction instead of concussion, a murmur is formed.

As for murmurs occurring in well-defined sacculated aneurisms, the absence of one or more of the conditions stated as requisite for the formation of the first sound of the heart applies equally to the shock sounds of aneurism. A rough condition of the orifice of the sac is probably a common cause, but the size of the orifice must also be concerned, since, if very small, the impulse of blood sent into the sac would be insufficient to develop sound; while, if very large, the conditions would approximate too closely to those of non-sacculated aneurism. But it must be confessed, that the relation between the different forms of aneurism and their sounds have not been sufficiently studied to enable us to speak with certainty in every case.

It forms no part of my plan to criticise the various explanations of the heart's sounds; one, however, requires passing notice, because by it the sounds are referred to the blood, and another claims attention on account of its general acceptance.

Dr. Bellingham maintained that the first sound was caused by friction between the blood and the parietes of the arterial orifices, and the second sound by friction between the blood and the auriculo-ventricular orifices. But, as I have previously said, it is impossible to conceive *how* such sounds could be produced by friction.

Two distinct views appear to be entertained of the valvular mechanism of the sounds, that they are produced by the valves either in the act of closing, or by their sudden tension when already closed. If a piece of membrane or textile material is held by both hands under water, and being relaxed is forcibly extended, a degree of sound will be elicited. This experiment has been confidently put forward in support of the valvular explanation. But it will be at once perceived that such an effect could only occur in the heart when the valves are passing from a state of relaxation to that of tension, and this must be at the moment of closing.

The mitral and tricuspid valves are closed at the very commencement of systole, and the semilunar valves at the outset of diastole. I forbear to enter into arguments in proof of facts about which no reasonable doubts can be entertained. In neither case, therefore, can the closing of the valves be regarded as the cause of the heart's sounds, which are coincident respectively with the duration of systole and diastole.

It is quite impossible that the action of blood against already tense valves can produce "a tympanic sound" from strong vibration, as happens in air when a tensely extended membrane is struck. The mitral and tricuspid valves are no sooner rendered tense by systole, than the semilunar valves are opened by the same action, and the intra-cardiac valves are thus relieved from undue pressure. But it has been shown that the pressure at either side of any of the valves varies but little, and their delicate structure indicates that they are not adapted for the never-ceasing strain, and consequent wear and tear, implied in noisy vibrations.* The viscidity of the blood and its compression within cavities are of themselves highly unfavourable to vibration.

If the heart's sounds are caused by valvular tension, why is either of them so easily impaired or destroyed by slight imperfection of the valve which produces it? Is it not reasonable to suppose, that in case of *bruit de soufflet,* from slight leakage, the tympanic sound from the valve would always remain intact, or nearly so? Why, therefore, does a bellows' murmur of a low pitch annul the entire natural sound? No satisfactory reply can be given to these questions.

In case of too thin a condition of blood, or of too low pressure upon it, the first sound alone degenerates into *bruit de soufflet.* But in the first instance, as membranes vibrate best in thin and mobile fluids, on the valvular hypothesis, the first sound in anæmia *ought to be* louder and clearer, instead of being totally altered. In the second instance, the change of the first sound into *bruit de soufflet* from excessive hæmorrhage cannot be explained on this hypothesis, since simple diminution of the natural sound *ought to be* the result of diminished pressure upon the mitral and tricuspid valves.

Nothing has tended more to untrue deductions than the liability to confound sounds of totally dissimilar origin with the true sounds of the heart. Thus, in applying a stethoscope to the still acting heart, exposed by vivesection, sound is inevitably produced by friction; and there can be no doubt that this sound has been sometimes attributed to *bruit musculaire,* and other causes.

When by a suitable arrangement air is made to circulate through the dead heart, sounds can undoubtedly be produced

* Dr. Markham asks, ("Diseases of the Heart," p. 154,) "Càn membranes and chords be suddenly and forcibly stretched and remain toneless?" I venture to ask, Is it possible that we constantly suffer so much internal violence and survive?

by its action on the valves. This has been triumphantly regarded as the *experimentum crucis* of the valvular explanation. But no just comparison can be instituted between sounds produced by air and those produced by fluids, and no one has succeeded in imitating the first sound by the action of water on the mitral or tricuspid valves. As for the imitation of the second sound by water, I have already pointed out its real source. In short, vibration of valves or other structures, when in contact with blood, has no more to do with the sounds than the vibration of a door with the sound produced by the passage of wind through its keyhole. The sounds are in the one case due to rapid motions in the air itself, and in the other to rapid motions in the blood itself.

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