REMARKS

ON

THE MECHANISM OF THE SOUNDS OF THE HEART.

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In the JOURNAL for August 5th, appeared a letter from Dr. Leared, taking exception to a statement in my communication of July 22nd, entitled "A few Personal Facts concerning Experiments on Animals, etc." In this I say that in animals in a state of insensibility I made the series of observations on the sounds of the heart and on the means of stopping or altering them, which gave the first complete knowledge of those sounds. On this, Dr. Leared remarks: "Dr. Williams intended by this to convey that the mechanism of the sounds of the heart was there and then definitely settled. I must beg to differ from him. Pathological changes in the sounds make it certain that his explanation of the first sound, at one time almost universally received, is incorrect. With some diffidence, I venture to assert that the true explanation was given by myself many years ago."

I refrained from noticing Dr. Leared's letter at the time, because it was not expedient then to enter into a controversy, which might be quoted by the persecutors of vivisection as disparaging the value of its results; but now, as the matter has been settled by the legislature (in a manner calculated, as I conceive, to limit the usefulness, quite as much as to prevent the abuse, of experiments on animals), I need no longer delay my reply to Dr. Leared, which is to the effect that I reassert that those observations made in February 1835 supplied the first complete knowledge of the sounds of the heart, which has given to the profession the means of distinguishing the physical signs in health and in disease. I do not maintain that every point on the subject was "then and there settled". Further observations have enlarged our knowledge of details, and have extended, as well as confirmed, the inferences drawn from those experiments; but, according to my judgment, no facts or arguments subsequently brought forward have controverted or materially modified those inferences, as laid down and explained in the fourth edition of my work on the *Pathology and Diagnosis of Diseases of the Chest*, which appeared in 1840.*

Before I proceed to recapitulate the leading facts and inferences by which my experiments established the view that the first sound of the heart is produced by the tightening of the ventricular walls and valves in the systole, and the second by the tightening of the arterial valves by the recoil on them of the column of blood in the arteries at the diastole, I will discuss the hypothesis of Dr. Leared and others, that the sounds of the heart are produced by the motions of the blood. In the first place, I would object that, of all bodies, liquids are the least adapted to produce sound. In contact with air, they become very sonorous; and all sorts of noises, rushing, splashing, etc., result, from the trickling of a brook to the roar of the sea or of a cataract; and, in collision with solids, they cause the noises of wave-dash and rainfall, also musical and other murmurs in the circulation, and the loud notes of the syren and other water instruments. But, without air, or without a solid, it is difficult to make a sonorous vibration in a liquid. It may receive waves of sound from solids or from air, and reciprocate them; but its molecules are too yielding, too little elastic, to originate the vibrations of sound. I have made many experiments on this point, and have constantly found the motions of liquids almost noiseless, except when mixed with air, or in collision with a solid. On forcibly squeezing an India-rubber bottle under water with the end of a stethoscope (avoiding the use of the hand, because that makes its own muscular sound), I failed to get any noise, except that of a slight rushing from the orifice. But, if the orifice were brought near a solid, or near the side of the vessel, then a noise of the solid resistance to the motion became obvious, caused, not by collision of fluid with fluid, but of fluid with solid. So, also, the blowing or bellows sound which Dr. Leared produced by forcing water from an India-

rubber bottle into a reservoir was caused, not by the collision of liquids, but by the vibrating resistance or friction of the edge of the discharging orifice; if this edge, instead of being abrupt, be sloped outward, as in a conical or trumpet orifice, there will be less resistance, and little or no sound, although the so-called collision of the liquids remains the same. In short, the whole idea of noisy collision of liquids, without air or solid, is a mistake. The molecules of liquids are too yielding, mobile, and inelastic in their motions to produce sound; and, although under favouring conditions they can receive and transmit sounds from solids and from air, not only are their own motions in most cases silent, but they act as dampers or neutralisers to other sounding bodies. The common impression of rushing and splashing liquids is derived from their behaviour in contact and combination with air and solids, and does not apply to liquids alone or to the circulating blood. Dr. Elliotson's explanation of cardiac murmurs (quoted by Sir T. Watson) by comparing them to the noises of the rushing of a river through the arches of a bridge, although plausible and striking, is not exact, because the respective conditions are imperfectly analogous in the two cases.

But, let us consider more narrowly Dr. Leared's notion as to the cause of the systolic sound of the heart. "Blood, driven from the ventricles into the aorta and pulmonary artery, comes into forcible contact with the 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." I have already shown that no sound results from the impact of one portion of fluid against another when no air or space intervenes. The whole movement is one of quiet displacement. But do the supporting semilunar valves alter the case by offering resistance? Dr. Leared ascribes no share to them in producing the sound otherwise than as supporting the stationary column of blood. Neither do I believe them capable of producing sound in this direction, because the solid resistance which they offer to the current is so soft and yielding that the elements of sound or sonorous vibration are wanting.* The case is one not of striking through a space at a tight barrier, but of a soft, rather sluggish, fluid in close contact, being pushed against a thin membrane, which yields and loosens in proportion to the pressure. Thus is the onward movement of the blood rendered as easy and quiet as possible. The tightening and sounding parts are those concerned in the active propulsion of the blood, and in the barriers to prevent its reflux.

Certain instances have been cited to prove that liquids can produce sounds like those of the heart: they are those of the water-hammer, the forcing pump, and the stop-cock suddenly closed in a pipe with running water. But, in all these cases, the noises produced are obviously referable to the collision of a fluid with solids. In the water-hammer, a body of water falls through a vacuum with unresisted force, till suddenly stopped by the solid bottom of the tube: forcible motion meeting with sudden resistance. The noises of the forcing pump are evidently also due to the motion and resistance of the solid piston and valves, forcing and directing the motions of the liquid.

The case of the water-pipe and stop-cock deserves a little further consideration, because its true nature has not been fully explained; and it was supposed even by Dr. Arnott to bear on how the heart-sounds are produced. If a water-cistern have a stop-cock at its bottom, or connected by only a short tube, no particular sound attends the closing of the cock; but if there be a long descending-pipe of several feet in length, the sudden closing of the tap by turning the cock is accompanied by a shock and noise, more marked in proportion to the length and descent of the tube. The cause is obviously, as explained by Dr. Arnott, the momentum of the fluid in the long descending tube being suddenly stopped by the solid of the closed cock. This noise is another instance resulting from fluid motion resisted by a solid, and so far has a resemblance to the second sound of the heart; but it supplies no proof of the production of sound by a liquid alone, and is quite different from any of the processes of the circulation.†

* In these and similar discussions on the production of sound, it is useful to have a more definite idea of what sound is than any that is contained in most works on acoustics. In the year 1834, I proposed to the Physical Section of the British Association this definition of sound: *Resisted motion, or motion of a certain force, resisted with a certain force.* The moving force and the resisting force, impelling matter alternately to and fro in opposite directions, constitute the vibrations of sound. These vibrations are transmitted through various media—solid, liquid, and aerial—in waves, and produce in the ear the sensation of sound. In order to the production of sound, there must, therefore, be enough force of motion and enough of resistance: if either be wanting, there is motion, but no sound.

† In my garden at Cannes, there was a water-pipe exhibiting remarkably this phenomenon. The tap was many feet distant from the cistern, and connected by a long tube descending with several curves. The noise on stopping the tap was not single, but was followed by two or three diminishing repetitions, which I ascribe to the rebound and successive vibrations of the column of water. This implies a slight tendency to a vacuum and momentary separation of the surfaces by gas or vapour, which could take place only in a rigid tube, and never in blood-vessels, which are everywhere exposed to atmospheric pressure.

* It has been much to my regret, as well as to my disadvantage in the permanent establishment of my views, that this work has been long out of print, and that my health and the engrossing engagements of consulting practice have prevented me from bringing out new editions. It is less surprising, therefore, that few of the present generation have seen the book; and some of those who do refer to it, by their misquotations, render it probable that they have not read it. See Dr. Shapter's *Notes and Observations on Diseases of the Heart*, 1874, p. 8.

Although we are thus led to the conclusion that the sounds of the heart originate, not in the blood, but in the solids, it is nevertheless certain that the presence of that fluid, its quantity, and its quality, have as much share in modifying the sounds as they have in influencing the action of the heart. To this I shall recur; but, to enforce my chief conclusion, I will contrast the difficulty of producing sound in liquids with the readiness with which they are caused by solids under water. The slightest knock, rub, or scratch of one solid against another makes quite a little noise, which the stethoscope transmits from the water to the ear; and the sudden tightening of a tape or of a bit of cloth or membrane under water gives a similar result, the sounds in the latter examples often closely resembling those of the heart. The noise of such motions under water is less than in air, because the liquid somewhat deadens the sound; but they are quite distinct, and the more exactly represent the sounds of the heart, inasmuch as the motions of this organ are enveloped in liquid and wet membrane. The chest-walls, or a stomach distended with gas, sometimes act like a sounding-board to the heart-sounds, and make them as loud as if they were produced in air.

I will conclude with a summary of the most conclusive observations in my experiments on asses in 1835. The animals were poisoned with woorara—dead, in fact, except the heart, which, by aid of artificial respiration, continued to contract vigorously for more than an hour.

EXPERIMENT I.—1. The first sound was equally audible on all parts of the ventricles, simultaneously with the shock of their sudden tightening in the systole. 2. The second sound was most distinct near the roots of the large arteries, being audible there in the weaker pulsations, when it could not be heard elsewhere. 3. Pressure on the arterial roots, by the fingers or the stethoscope, invariably stopped the second sound. Slight pressure caused a bellows-murmur with the first sound. 4. On pushing the auricles by the end of a finger into each auriculo-ventricular opening, the ventricular contractions became weak and irregular; but the first sound, although weak, was still heard alone. 5. The left auricle was cut open, and the mitral valve partially destroyed. The blood issued in jets at each systole; yet the first sound still accompanied the systole, but the second sound ceased. The right auricle was freely laid open; still the first sound continued. 6. I pushed my finger through the mitral orifice into the left ventricle, and pressed on the right so as to prevent the influx of blood into either ventricle. The ventricles continued to contract strongly; and the first sound was still distinct, but weak.

EXPERIMENT II.—Observations 6 and 7. A dissecting-hook was passed into the pulmonary artery, and made to draw back its valves, and thus prevent their closure. The second sound was weakened, and attended by a hissing sound. A shoemaker's curved awl was then passed into the aorta, so as to act in the same way on the aortic valves. The second sound now entirely ceased, and was replaced by a hissing. On withdrawing the hook and awl, the hissing ceased, and the second sound returned. These observations were several times repeated, and with the same results. Observations were also made, that both heart-sounds were distinctly heard when the heart touched no part of the walls of the chest, and, in one instance, when it hung out of the chest and touched nothing; that at the root of the aorta both sounds were heard, but at three inches from this point the second sound was alone heard.

Without detailing the inferences from these observations, they seem to me sufficiently to prove that the essential cause of the first sound is in the walls of the ventricles; for it distinctly accompanied their contractions when the action of the valves was prevented by pressure or division, and even when there was no blood in the ventricles. Of course the sound was much weaker than usual, but it was sufficiently audible and distinctive to convince myself and my assistants, all skilled auscultators. The persistent vigour of the heart's action in this extremity was quite striking and unexpected, and supplied positive evidence of greater weight than all the negative results and doubts that have been urged against it.

Among the opponents of my view of the physical cause of the first sound, the most formidable is Dr. Halford with his striking experiments with the *bull-dog* forceps. By aid of this, he compressed the afferent veins of the heart in a dog so effectually as to stop all supply of blood, with the result, as he states, of entirely suspending the sounds, although the ventricles continued to act. On removing the forceps, the sounds returned with the restored supply. Dr. Halford inferred that the absence of blood suspended the action of the valves, and thence concluded that the valves only are the source of the sound. Dr. Leared with equal reason ascribes the cessation of sound to the absence of the blood, which he holds to be its essential cause. Not having witnessed these experiments, I cannot help entertaining a doubt that the sound was *totally absent* where the muscular contraction was

vigorous; but I should expect it to be much weakened, not only from want of the proper stimulus, but also from the absence of contents to distend the walls into a vibratile condition. A mass of muscle contracting on itself cannot fulfil the transition from slack to vibrating tightness, which is exhibited by a heart propelling a body of blood. The argument that, because the heart without blood gives no sound, the blood or the valves must be the seat of sound, is about as logical as that the bridge of a violin is the seat of its notes because the strings yield no sound without the bridge. In either case, the bridge or the blood is necessary to complete the instrument which yields the sound; but the true source and chief seat of the sound is in the vibrating strings in the one case, and in the vibrating walls in the other.

By a reference to my writings,* it may be seen that I admit that the auricular valves have their full share with the ventricular walls in producing the systolic sound; and I ascribe the flapping commencement of this sound especially to them. But the prolongation of the sound through the whole duration of the ventricular systole, and its equal intensity over all the ventricles, prove the sound to be general, not partial. The second sound, on the other hand, is more local, being decidedly loudest at the base of the heart and roots of the arteries. The sounds in the great arteries are generally those transmitted from the heart; but, in cases of strong pulsation or dilatation of arteries, there is obviously a distinct arterial sound caused by the impact of the blood against their walls.

I conclude, then, that the sounds of the heart are *not* produced by any *motions or collisions of the blood*, because I find that no such sounds can be produced in fluids alone, nor is there any evidence to show that they can or do occur in the heart; and I conclude that the sounds of the heart are produced by the tightening of the walls and valves, because such tightening does take place in the action of the heart as is always in other cases attended with the production of sound, and I have had positive proof to this effect in my experiments on animals.

AN IMPROVED METHOD OF PERFORMING THE "BOUTONNIÈRE" OPERATION.

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THE nomenclature of the operations performed on the urethra is somewhat confusing. Take, for instance, the name "perineal section": by it the student at Edinburgh understands an operation which is different from that which the student at Leeds sees practised; and, if an English, Irish, or Scotch student were asked whether an internal urethrotomy practised in the bulbous region of the canal were perineal section, he would probably reply in the negative, although an affirmative answer ought to be given to the question. It would be well, therefore, if all the following terms were abolished—*i. e.*, perineal section, perineal urethrotomy, external urethrotomy, external perineal urethrotomy—and the older and more correct names of "boutonnière" and "boutonnière sur conducteur" were employed. The French terms have this great advantage, that surgeons all the world over know exactly what is meant by them. By "boutonnière" is understood an operation for the division of a stricture through which no guide can be introduced when the operation is commenced. "Boutonnière sur conducteur" explains itself, and is the division of a stricture upon a conductor passed through it before the operation was begun.

The "boutonnière" is one of the oldest, most valuable, and difficult operations in surgery. For the relief of an impassable stricture, or retention therefrom, it is unsurpassed, as it remedies the cause and effect at one and the same time. It has been greatly praised and considerably disparaged. In recent times, it owes much for its preservation in France to Sédillot, in America to Gouley, and in this country to Teale and Wheelhouse. In Leeds, the operation is performed systematically, frequently; and I should say that in that town it is done more often in one year than in all the London hospitals put together during the same period. The journals may be searched for years together, and but little will be found regarding this most interesting and useful procedure.

* See especially *The Pathology and Diagnosis of Diseases of the Chest*, etc., 4th edition, 1840. Although this work has been long out of print, it may be consulted at the libraries. In it will be found a full description and discussion of the sounds of the heart in health and in disease, and the rules of diagnosis in valvular disease, now generally adopted, which I ascertained and published in 1835, three years before Dr. Hope published his last set of rules, which were almost identical with those previously described by myself. I also first introduced the terms "regurgitant" and "obstructive," and gave phonetic expressions for the principal healthy and morbid sounds of the heart.