into a confusion of expression, if not of idea, by making no distinction between the sign and the thing signified, by speaking, for instance, of incoherence of speech, which is a sign, as if it were the vagabond and unassociated flow of ideas, which is the thing signified. It is not strictly and constantly enough remembered by medical men that the object of the legal inquiry is always and only the state of mind, and that all our evidence of sensible fact has reference to that which we cannot touch, or see, or hear, and which we can only infer from the signs which it manifests, the things which are seen being merely the indices of the things which are unseen.

Let me, as an example, refer to a group of signs of a state of mental alienation which an expert might look for in the individual man submitted to his examination. The usual signs of anything are causes and effects, and the surest signs of insanity are states of bodily disease which are known to cause insanity and conduct speech and bodily conditions which are its consequences. General paralysis and epilepsy are the most obvious instances of causal signs, and can it be doubted that the skilled observation of a medical man as to the symptoms of these bodily disorders is not of more worth than all the rude and crude guesses of common men? Let us take the former disease as an illustration. As long ago as 1855, I pointed out, in the *Medico-Chirurgical Review*, vol. xvi, p. 384, that, "on more than one occasion, I had detected the early symptoms of general paralysis in persons accused of theft"; and my observation has since been most fully confirmed by my professional brethren, so that, among well informed alienists, general paralysis is now acknowledged to be a very sure sign of an unsound state of mind in which the commission of theft is highly probable. But the existence of general paralysis in its early stages—that is, of the sign—is by no means an obvious fact, but needs all the skill of an expert for its detection, and, when detected, it needs the knowledge of an expert for the inference which connects it with the offence. A most instructive case will be found in the twentieth number of last year's *Lancet*, in which a man with general paralysis was convicted at Doncaster of stealing a gold watch in a railway train, at a trial (if such ignorant proceedings merit that name) where my friend Dr. Crichton Browne gave evidence. "I shall not easily forget," writes Dr. Browne to Dr. Maudsley, who reports the case, "the smiles of incredulity which pervaded the Doncaster Court House, from the recorder downwards, when I described the inequality of the pupils, the slight tremor of the tongue, and other *little* symptoms, which enabled me to recognise the justice of your diagnosis of general paralysis. I shall not easily forget the derision with which my prediction that the robust healthy looking man standing at the dock would not live above eighteen months was received at all hands." The prediction, however, was fulfilled; for the man, after conviction, died raving mad in less than a year.

Time will not permit me to give other illustrations; but I think the principle will not be gainsaid by any honest, intelligent, and instructed man that, in a multitude of instances, the signs of an unsound mental state which would be admitted to be valid by the consensus of skilled opinion in this and in all other civilised countries are not such as can become known to petty jurymen from the daily experience of common life. If it were otherwise, *use* which gives facility would be a futile waste of labour, and the saying of the man that he did not know whether he could play the violin, because he had never tried, would not be so absurd as it seems.

That the juryman has to decide is true, and I am one of the last men to wish it otherwise; for I am old-fashioned enough still to hold that twelve honest men in a box are the palladium of English liberties; I quite concur also with Lord Westbury that it is always at bottom a moral question which the jury has to decide, and not a physical or scientific one. It is the question whether this man ought to be punished or not for this crime, or whether he was capable of making this will, or whether he is capable to manage himself and his affairs. These are all moral questions, but they all hang directly upon the physical and scientific one of whether the man's brain can or could perform its healthy mental functions or not. And my argument is that, on this, except in rare instances, the common juryman must needs require scientific information and guidance, which he may partly understand, but which he must mainly accept upon good authority.

What is good authority? That is the question which it is the bounden duty of the jury, with the assistance of judge and counsel, thoroughly to sift. If it be not good, it will be misleading and mischievous; for

"Authority, although it err, like others,
Hath yet a kind of medicine in itself
That skins the vice o' the top."

If it be false, it hides the sore of ignorance, but will not cure it. Mental pathology is certainly no mock science, though it be yet in that

stage of immaturity which lays it most open to the invasions of enemies and of pretended friends, of hostile critics and of charlatan supporters. Of the latter, none are more mischievous than the enterprising speculator who heavily discounts real knowledge to gain credit for remote probabilities. But we mental pathologists are not bound to know more than we can know by painstaking and laborious study. Let us firmly stand upon the generalisations of knowledge upon which we are agreed, and upon the facts which we have ourselves carefully observed, and confining our opinions to the scientific question of the existence of sound or unsound mind, leave its moral corollary entirely to the courts, whose duty it is to infer and enforce it. The judges may decide, as indeed they have done, that one kind of madman shall be punished for crime and another excused; and, with the aid of the legislature, they might even decree that madness shall be no excuse for crime, as drunkenness is not; but, to decide the scientific question of what madness is, is beyond the province of the courts. "A court of justice," says Cornewall Lewis, "which was highly esteemed for its judgments on questions of law, would render itself ridiculous, and shake its authority, even within its own sphere, if it attempted to determine questions of science".

REPLY

TO

DR. LEARED ON THE MECHANISM OF THE SOUNDS OF THE HEART.

By CHARLES J. B. WILLIAMS, M.D., F.R.S.,

Physician Extraordinary to the Queen; Consulting Physician to the Hospital for Consumption, Brompton; etc.

ALTHOUGH Dr. Leared has taken his time in replying to my remarks on the mechanism of the sounds of the heart, which appeared in the *JOURNAL* on September 30th, 1876, he does not appear to have mastered the subject; for his misunderstandings and misquotations are something unusual. For example, he quotes me as saying: "The case is one of striking through a space at a tight barrier", etc.; whereas my expression was: "The case is *not* one of striking through a space at a tight barrier", etc. This reversal of the sense makes nonsense, which he proceeds to deal with accordingly.

Again, seeking for further support for his notion that sounds are caused by the movements of liquids *per se*, he "places the matter before Dr. Stone", and quotes from his reply the following oracular sentence: "Any person, whatever his scientific position, who declares the impossibility of sounds being produced in fluid alone states a fallacy." Either Dr. Leared misquoted, or his oracle misunderstood, my words, which were: "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."

Dr. Leared then cites another authority, Amédée Guillemin, in proof that liquids alone can produce sounds; but this instance—that of the water-syren—I had expressly specified as deriving its notes from the motions of a solid. This instrument, by the revolution of its perforated disc, causes a rapid succession of openings and shuttings of holes through which a current of air or of water passes. The rhythmic puffs or spurts thus produced, when rapid enough, cause a continuous note, the pitch of which is in proportion to their frequency. This is a case of current-sound, the vibrations being produced by the motions of the revolving disc.* But neither in its working nor in its results has it the slightest analogy to the normal sounds of the heart.

So likewise, in the other case mentioned by Dr. Leared, a whistling results from the wind passing through a keyhole, or from a person blowing into a key or through his lips, because the air is thrown into sonorous vibration by the resistance of the edges and hollows of the keyhole, the key, or the lips respectively. These sounds have an affinity, not with the natural sounds of the heart, but with the morbid sounds or murmurs; and, so far as I know, I was the first to point out the analogy between them. Nearly fifty years ago, I expressed my belief that an improved knowledge of the laws of sound would prove that liquids as well as air could be thrown into sonorous vibration by

* It may be questioned whether the note of the syren originates in the perforated disc or in the current which it divides, and which certainly appropriates and conveys the sound. They probably vibrate together; and the movement of the disc must be considered as much the cause of the note as the motions of the wings of insects are of their hum, which we hear through the air.

passing through tubes of particular forms; and that cardiac murmurs, instead of depending, as Laennec conjectured, on a fancied spasm of the arteries, were really produced by the motion of the blood through partial obstructions or modifications in its course.*

"In fact, all these murmurs are produced by the passage of liquids through solid tubes or apertures, in the same manner as analogous sounds are produced by the passage of air through pipes or holes of different kinds. They are the music of water-instruments, as the latter are of wind-instruments. There are only these differences between them: that liquids, being more sluggish than air, are less susceptible of the sudden motions which constitute sonorous vibrations; and, not differing so much in density from the solids in which they move, liquids will have little of those reflected or echoed vibrations which increase the sounds produced in air-filled tubes. Holding in mind these qualifications, we may explain the murmurs heard in the heart and arteries by referring them to parallel instances of the tones of wind-instruments; nay, we may find parallel phenomena in the rhonchi, respiratory and vocal sounds of that most complete and diversified wind-instrument, the windpipe and its branches. Like in these, there are varieties of sound, in generating which the solids and the current have different shares; thus, in sawing, grating, and the stronger droning murmurs, the vibrating resistance of the solid is chiefly concerned, and its vibrations are transmitted to the adjoining parts as well as to the current, so as to produce in them a thrill, which may sometimes be felt by the hand. Being in the rhythm of the heart's motion, the thrill resembles that felt on the back of a purring cat, whence Laennec called it the *fremissement catinaire*. These have their parallels, in the case of sonorous rhonchus, in reed-instruments, and in those imitations of these murmurs which we can produce by forcibly breathing through the nearly closed teeth, tongue, and lips, which in like manner communicate a sensible vibration to the solids, as in ringing the letter K in a whisper. Again, in the blowing, hissing, whistling, and cooing murmurs, the vibrations are more those of the current reflected by the solid, in the manner of cavernous breathing, the sibilant rhonchus, blowing or whistling with the mouth, or of the flute class of musical instruments. Here, there are no perceptible vibrations in the solids; they are less actively concerned in the production of these sounds, which are rather transmitted in the direction of the current."†

Such is my explanation of current-sounds or murmurs; but, I repeat, the normal sounds of the heart are totally distinct from murmurs; they are produced by the intrinsic action of the heart and its valves as it propels the blood; and all the varieties which they present in health and in disease admit of an easy explanation. When murmurs are present from disordered mechanism, the normal sounds of the heart may be modified by the murmurs in a way which I will presently explain; but I must first endeavour to correct the curious and ingenious misapprehensions into which Dr. Leared has fallen in commenting on passages in my former communication.

His omission of the word "not" in the passage before quoted naturally throws Dr. Leared into a state of confusion, and it may be well to restate the argument in other terms. Dr. Leared holds that the first sound of the heart is caused by the impact of the blood issuing from the ventricles on the blood in the arteries, "which, supported by the semilunar valves, had attained a state of momentary repose". To prove that sound could thus be produced, he made the experiment of forcibly squeezing water from an India-rubber bottle through the side of a reservoir containing water; the result was a bellows-murmur at the orifice. On substituting glycerine for water, the bellows-sound was not heard, and there was difficulty in producing any 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, was heard.

My comment on this is, that the sound produced in the water being a bellows-sound, proves nothing but that a current issuing from

an orifice into water causes a current-sound, which we knew before, and have referred it to the vibration of the edges of the solid orifice. The experiment with glycerine proves my statement of the difficulty of producing sounds in liquids, especially in those that are viscid; but, when the moving column struck against a solid (the stethoscope), the sound was loud enough.* Doubtless, the viscosity of the blood renders it less susceptible of the motions which constitute sound. It was to this property I alluded when I used the word sluggish, meaning not slow in motion, but somewhat cohesive, and less liable than thinner liquids to be thrown into vibration. When the heart propels this cohesive liquid into the arteries, the motion is one of silent displacement, and, however forcible, is kept silent by the fulness and equal distension of the arterial column, allowing no space for shock or impact. The interior of the heart and arteries is likewise peculiarly smooth, to render the onward passage of the blood as easy as possible in this, the most forcible part of its course. In the auricles and auricular portions of the ventricles, there are the projections and irregularities of the tendinous cords and muscular network; the blood pours slowly over these during the diastole and rest; but the arterial portions are quite smooth and funnel-shaped, to permit the onward passage of the blood during the mighty systole to be as free and unresisting as possible. All this points to the silent movement of the blood as most consistent with normal action, and contrasts strikingly with the friction and noise developed in the same parts by obstructive disease of the aortic orifice.

Another subject on which we differ relates to the noise caused in a long water-pipe descending from a cistern when the tap at the bottom is suddenly closed. I endeavoured to show that this phenomenon has no analogy with the heart-sounds, being a simple case of succession in a rigid tube from the momentum of the current being suddenly stopped. Dr. Leared, on the other hand, considers this water-tap noise to represent the manner in which the second sound of the heart is produced; it is not surprising, then, that he should object to my view of the matter. But, as he has quite misunderstood the too concise explanation which I gave, I think it necessary to compare the two cases more fully.

The noise in the water-pipe occurs only when there is a descending pipe of several feet in length, which gives the current a powerful momentum; and, where the length is very great, the descending force is so strong as to bulge out or burst the pipe above the tap.† But there is no such backward momentum in the arterial trunks, nor can any analogy be found in "the pressure on the blood in vessels sufficiently rigid for the purpose". At the end of the ventricular systole, the only backward motion of the blood is limited to a few inches above the valves, which are immediately expanded and tightened by it, thereby stopping further reflux, and, by their sudden tightening, producing the second sound. Instead of a long back current in a rigid tube suddenly stopped, the movement in the large arteries consists of a strong forward pulse during the systole, alternated with a very short backward motion from the arterial distension at the beginning of the diastole, immediately stopped by the closing of the semilunar valves, both movements being regulated by the equalising pressure of the arterial tension. The whole mechanism of the circulation exhibits an admirable combination of efficient force with gentle steadiness of action, which contrasts strongly with the jarring violence of the downward rush of water in a rigid tube, to which Dr. Leared would assimilate it.

In my former paper, I mentioned that, in some instances, the concussion on stopping a running water-pipe is repeated two or three times; and that this seemed to me due to rebounds of the column of water, which implies a momentary separation of the surfaces at the closed end of the tube, the interstice being a quasi-vacuum, but really containing such rarefied air or vapour as ordinary water gives out *in vacuo*, and which, on the return of the column, is as quickly recondensed.‡

* Dr. Leared seems not to be very successful in his experiments. He says (page 11, note): "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."

† As Dr. Leared mentions, this can be prevented by introducing into the pipe a small tube communicating with an air-chamber, which serves as an elastic buffer, softening the shock and preventing the noise. Dr. Leared proposes to insert a similar diverticulum in the aorta of a living animal (rendered insensible), expecting to extinguish both sounds of the heart, and thus to demonstrate his explanation of their cause. I venture to prophesy that this will prove another of his unsuccessful experiments.

‡ Disregarding my expression "momentary separation", Dr. Leared erroneously supposes me to mean that palpable or permanent air is evolved, and proceeds to attack this man of straw. It is unnecessary to follow him through his mistaken argument; but it may be remarked that his statement, that the presence of air in the arteries destroys the second sound, is readily explained by its interfering with the action of the semilunar valve, which is adapted for liquids only. It is thus that injection of air into blood-vessels destroys life.

* *Rational Exposition of Physical Signs*, etc., 1828. It was not till after this date (I write from memory, not having access to books here) that Cagniard de la Tour invented the syren, which exactly verified my prediction, by producing sound in water as well as in air. Dr. Hope and subsequent writers adopted the same view of the cause of cardiac and arterial murmurs; but its demonstration was not complete till the appearance of the second report of the London Committee of the British Association for the Advancement of Science in 1837, in which it was proved that every description of murmur could be exactly imitated by the flow of water through India-rubber tubes.

† *Pathology and Diagnosis of Diseases of the Chest*, fourth edition, 1840, p. 216. When I wrote the first edition of this work in 1827, acoustics were very little understood; and not finding the requisite information in scientific works, I had myself to study it by experiment and reflection, to enable me to compose a chapter on sound to explain the phenomena of auscultation. I was thus led to the knowledge of the properties of sound, which were subsequently more fully explained in the writings of J. Herschell, Wheatstone, and others.

I conclude, then, that Dr. Leared has failed to establish any instance of the production of sounds like the normal sounds of the heart by liquids alone, the cases which he has cited depending on the agency of a solid as well as a liquid; and, in most examples, the result being a current-note or murmur instead of a shock-sound, like those of the heart. The case of the water-pipe I have shown to have no analogy with that of the heart in either factors or results.

Having negatived the liquid solution of the question, I revert to my positive proofs that the solids are the essential cause and seat of the normal sounds of the heart. I before alluded to the facility with which sounds can be made with solids, as contrasted with the difficulty of producing them in liquids. Let anyone take a large basin or tub of water, and, plunging the cone of an ear-tube, listen to motions made under the water; for example, by forcibly compressing an India-rubber bottle (without air). No sound will be heard, except from the muscular action of the hand, or when the current grates on the edges of the orifice or strikes against the ear-tube, and then nothing like the heart-sounds. Then let the experimenter try any sudden motion of a solid, whether by knocking, scratching, or tightening it near the orifice of the tube, and he will hear sounds distinct enough, however gentle the motion be. As I before said, the sudden tightening of a piece of tape, cloth, or membrane, makes a noise most like the heart-sounds, the second sound being exactly imitated by tape or ribbon, and the first less perfectly by a thicker fabric, such as woollen cloth.

If we extend our observations, from inanimate solids suddenly stretched by external force, to muscles which tighten by intrinsic power, we find that their movements also yield sounds, when sufficiently brisk and strong. Slow and weak contractions cause little or no sound, but those that are abrupt and vigorous are abundantly sonorous; the quality of the sound varying with the thickness of the muscle and the duration of its action. Thick muscles, as those of the thigh, give a dull and prolonged sound, like that of a thick-walled heart. A thin expansion of muscle, as the platysma myoides in the neck, or the panniculus carnosus in quadrupeds, yields sounds loud enough and sharp enough to imitate the sounds even of a dilated heart. But, in any case, if the muscular contraction be continued, the sound becomes more dull and rumbling, such, in fact, as that described by Dr. Wollaston in the *Philosophical Transactions* of 1810. The best imitation of the normal first sound of the heart that I have obtained was in the abdominal muscles. By using a flexible stethoscope, anyone may make this experiment on himself in a sitting or lying posture; the conical end being firmly applied to the bare abdomen, the muscles are to be started into sudden momentary contractions, and this can be so varied as to imitate several phases of the systolic sound of the heart.

It being thus shown that muscular action is adequate to produce the first sound of the heart, I need only refer to my experiments to demonstrate that this is in reality its essential cause. In the donkey (quite insensible), the chest being laid open, the hard tightening of the ventricles of the exposed heart was felt to correspond with the first sound so exactly in sensation as to carry conviction that it was the same movement. We noticed a like correlation of motion felt, and sound heard, over the arterial valves at the instant of the second sound. In both instances, we seemed to *feel* the sound.* And this double sensation enabled us to localise the source of the sounds more exactly than we could do by hearing alone. Thus the systolic shock as felt, was more distinctly limited to the ventricles than the sound, which was heard also in the arteries, but not so loud; the second sound was loudest about the valves of the arteries, where alone its shock was felt; on the ventricles, this sound was weaker and without shock. So far as regards the first sound, these observations are opposed to the views of Dr. Leared, who places the seat of this sound at the arterial orifices. As I argued more than forty years ago, "if this were true, the large arteries rather than the heart should be the principal seat of the sound, and the sound would be increased by an hypertrophied heart and diminished by a dilated heart; yet the reverse of these is presented in Nature". (*Rational Exposition*, etc., second edition, 1833, p. 166.)†

* The capacity of *feeling* sounds by the fingers applied to the sounding part, is familiar in the purring thrill which occurs in cases of diseases of the heart and arteries, in venous murmurs in the neck, in some cases of friction-sound and bronchial rhonchus, and in natural and morbid vocal vibration. I have found its indications in many cases instructive; and in teaching, I used to advise students to educate their fingers as well as their other organs of sense. Dr. Isidor Hein of Vienna draws attention to this subject in a paper "On the Relations between Perceptions of Touch and Hearing" (see *BRITISH MEDICAL JOURNAL*, December 16th, 1876). In 1833, I wrote this description of my first observation on the ass: "When the pericardium was laid open, and the large heart exposed, vigorously pulsating, the eye watching it, the hand grasping it, and the stethoscope applied to it, gave perfectly corresponding impressions, inasmuch that on substituting touch for hearing, it was difficult to banish the impression that one still heard the double sound."

† I have just seen Dr. Broadbent's "Observations on Dr. Leared's Reply", etc.,

But the most conclusive proof that the walls of the ventricle, and not the blood, are the seat of the systolic sound, is found in those observations in which the sound was heard when the heart contained no blood. The sound was feeble, it is true, but quite distinct. Dr. Leared replies to this: "When a stethoscope is applied directly to a contracting heart, it is absolutely impossible to distinguish the sound caused by friction of the heart's surface from any allied sound." Dr. Broadbent's testimony in contradiction to this assertion is most valuable; he says: "I maintain, from large experience of auscultation of the naked heart when assisting Dr. Sibson in his experiments, that it is both possible and easy; the sound heard during the contraction of an empty heart is not frictional, but muscular." This was the clear result of my experience also. When the heart is kept moist and the stethoscope is carefully and steadily pressed on it, to prevent the entrance of air, there is no friction or any other extraneous sound, but simply a weak repetition of the usual systolic sound. That it should be weaker than in the heart entire, with valves and blood, is only what might be expected. The contraction (change from slack to tight) was strong enough to produce sound, but not so strong as when reinforced by a body of blood, with the auricular valves tightening behind it. The muscular contraction is the only essential cause of the sound; but both the blood and the valves contribute to increase it; the blood, not by any collision or impact, but by forming a body around which the walls and valves tighten more tensely; and the valves, both by their completing the enclosure of this body and by the additional vibrations of their own tightening cords and membranes. In my work before quoted, I have explained how variations in these several elements contribute to produce the variety of the first sound which we meet with in practice. It would take up too much space to detail these here; but I will advert to the case mentioned by Dr. Broadbent, that of the impairment of the first sound by a high state of tension of the aortic system, which is quite contradictory of Dr. Leared's views. I would explain this in the same way as I explain the diminished loudness of the sound in sthenic plethora. The arterial tension being increased, whether by fulness, by increased tonicity, or by both, the heart's systole is resisted in the arterial column by a more constant and prolonged pressure than usual; the sound is also prolonged, but less abrupt, and therefore less noisy. In a more empty or flaccid state of the heart and arteries, on the other hand, the ventricular systole is abrupt, the transition from slack to tight more sudden, and the sound is shorter and louder. So likewise with dilated hearts, the systolic sound, like the motion that produces it, is loud and short, as contrasted with the duller and longer sound and motion in hypertrophy.

I now have to reply to Dr. Leared's concluding question and challenge: "Why is it that, in anæmia, the first sound of the heart only and the second never becomes replaced with a murmur, unless it be that, according to the views stated, the blood loses in this disease one of the conditions necessary for the formation of the normal sound; namely, sufficient viscosity? If Dr. Williams or any one else will explain this one point in accordance with any muscular or valvular theory of the first sound, I shall begin to feel doubtful about the conclusions herein set forth."

Like Dr. Broadbent, I begin by disputing the fact. With him, I assert that, in anæmia, the normal sounds are not *replaced* by a murmur, but that, in all cases, the natural sounds continue; but, in some, they are *accompanied* by a murmur at the base and in the course of the arteries. I can further add from my own observations that, in some cases of anæmia, I have found a short murmur preceding the second sound.* In all these instances of anæmic murmurs, thinness of the blood is their principal cause. When the blood is thin, from deficiency of albumen and corpuscles, it is more readily thrown into sonorous vibrations in its course. This was illustrated in our experiments on the production of sound in tubes.† Every description of murmur could be readily imitated by a current of water passing through an India-rubber tube; but, when the water was made glutinous with size, the sounds were less easily produced and more force was required in the current. So it is quite intelligible how thin blood squirted through the arterial orifices is readily thrown into vibration, especially when the heart's action is excited, and thus a murmur or current-note is added to the normal ventricular sound. But, with the second sound, there is not the same facility to produce an accompanying murmur. The semilunar valves close so simply and quickly, that, if they permit any reflux

in the *BRITISH MEDICAL JOURNAL* for December 23rd. Among other able objections to Dr. Leared's views, he restates the above argument. It would not be difficult to point out other pathological facts which are opposed to them.

* Anæmic murmurs accompanying the first and second sounds may be described phonetically as *bloo-trrht*, or *bloo-tst*, instead of the normal *lubb-diip*.

† Second Report of the London Committee of the British Association for the Advancement of Science on the Motions and Sounds of the Heart. 1837.

current at all, it can only be in the act of closing, thereby changing the normal second or flap-sound in the manner just described. In many cases of anæmia, the second sound is not altered, and, in not very few, neither is the first sound, except when the heart is excited; these differences point to slight normal varieties of structure which become evident only under the test of disease. But, in all cases of simple anæmia, the normal ventricular sound may be heard distinctly in the cardiac region, and in many accompanied by a murmur, loudest at the base.

In certain cases of extensive disease of the valves, both obstructive and regurgitant, I admit that murmurs are produced which, to a great degree, supersede the natural sounds of the heart. This is when the lesions so much derange the mechanism of the heart as to convert the normal shock-sounds into vibrating current-sounds. For example, in obstructive disease of the aortic orifice, the systolic action meets with such a vibrating resistance to the blood which it expels as to react on the tightened ventricle and make it vibrate in unison with the obstacle and current in front; thus the normal shock-sound originating in the walls and valves merges in a noisy current-sound or murmur, whose vibrations pervade the whole cardiac region, but still are loudest in the direction of the current. In extensive regurgitant mitral disease, a like vibrating current goes back through the mitral orifice, masking or superseding the normal first sound by a loud murmur transmitted through the cords to the apex below the left breast. In more moderate degrees of this lesion, the normal double sound may still be heard at the base and over the right ventricle.*

In regurgitant disease of the aortic orifice, the normal second sound is absent or impaired, being replaced by a diastolic murmur. The reason is obvious, the valves can no longer tighten; but even then the simple flap of the pulmonary valves is often still heard to the left of the mid sternum. In that remarkable and not uncommon lesion, button-hole contraction of the mitral orifice, which is both obstructive and regurgitant, the long systolic and presystolic murmurs often supersede all normal sounds at the apex, but at the base the normal sounds are usually still more or less distinct.

In conclusion, I have only to repeat the inferences with which I terminated my former communication; and, to make them more intelligible, I will adopt some expressions borrowed by Dr. Leared from French writers.

The normal sounds of the heart are of the nature of *shock-sounds*, which can be neither produced nor imitated in liquids alone. These sounds can be readily imitated by the tightening of woven fabrics or animal membranes under water. Similar sounds constantly accompany the forcible contraction of various muscles in man and other animals. It has been demonstrated by experiment that a similar sound continues to accompany the contractions of the ventricles of an insensible animal without the presence of blood and without the action of the valves. Although it is thus proved that the essential cause of the first sound of the heart is the muscular contraction of the ventricles, it also appears from experiments, and is conformable to reason, that the presence of the blood and the action of the auriculo-ventricular valves contribute to its loudness and clearness by increasing the strength and extent of the sonorous vibrations.

The second sound of the heart has been clearly proved by experiment to be caused by the tightening of the arterial valves by the recoil of the blood in the great arteries at the moment of diastole.

Murmurs are essentially *current-sounds* produced by some modification of the direct and usually silent course of the blood in the heart or blood-vessels. As they can be exactly imitated by the flow of water through an India-rubber tube, there is no reason to doubt that they are produced in a similar manner by the passage of the blood through or across a form of solid, which throws it into sonorous vibrations. This may occur at any of the orifices of the heart or in the blood-vessels. When in the heart, a murmur may either accompany or replace the normal sound. In cases of moderate disease and of anæmia, it merely accompanies it; but, if the lesion causing it be such as seriously to damage the structures, it may impair the normal sounds, both valvular and muscular. Further, a strong grating murmur at an orifice may not merely mask the normal sound, but may assimilate its vibrations to its own and convert the whole sound into a loud murmur.

* Dr. Broadbent says: "I have long been in the habit of noting the occasional presence of a first sound, together with a systolic mitral murmur, as a favourable element in prognosis." My experience is the same, and extends, as a general rule, to most structural diseases of the heart and lungs, to this effect: lesions are serious, not in proportion to the new signs or symptoms which they cause, but more in proportion as they supersede the signs and symptoms of normal structure or function. I referred to this inference in a paper on the "Prognosis and Treatment of Organic Diseases of the Heart" in the *London Journal of Medicine* for April 1850.

AN ADDRESS

ON THE

EXPERIENCE AND EXPECTATION OF CLINICAL RESEARCH.

Delivered before the Clinical Society of London, on Friday, January 26th, 1877.

By GEORGE W. CALLENDER, F.R.S.,

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GENTLEMEN,—If an earnest desire to promote the exact observation of clinical facts should largely influence me as a member of this Society, that earnestness should be intensified by the responsibility I have incurred in venturing to accept office as your President—intensified by the knowledge that your friendly judgment has placed me in a position which demands that, by precept and example, I should plead for precision in clinical research.

If I had only to speak to you in words of thanks, my duty would soon be ended; for I can but say that I value most highly the honour you have conferred upon me. In recalling your confidence, I shall be encouraged in the endeavour, so far as I can, to do it justice.

It has, however, become a custom of the Society that your Presidents should venture from time to time to offer suggestions as to how the objects we have in view may be best promoted; a custom which it grows more difficult to observe as its novelty wears off, and yet—having in mind changes in the grooves of thought, in the method of pursuing investigations, the gradual accumulation of facts tempering and consolidating the knowledge with which we begin each new session of work—one which it seems to me most desirable to conserve. In addresses which have been given from this chair, your Presidents, in a practical way, and with the force of experience (all the more forcible from the clear and thoughtful words in which it has been spoken), have described the duties we owe to a Society which is founded expressly for "the cultivation and promotion of practical medicine and surgery": words which express as could scarce any other the character and scope of our task, for they foreshadow an increase of knowledge; but, however far in advance of the past, a present knowledge which in its future must always be capable of cultivation and promotion. Our work is, indeed, bounded by a horizon which recedes before us; but, in our pursuit of it, as we ascertain and establish facts, and, so far as we can, as we eliminate sources of fallacy, we shall find that we are making progress and are strengthening our position for a further advance.

If I diverge in any way from the steps of my predecessors, who have spoken chiefly on the manner in which the objects of the Society may best be attained, it will be to take advantage of the experience we have thus far acquired, not only for the evidence which it gives of the extent to which we have acted up to the expectation of our founders, but as showing the direction in which we are being carried, and in which our future work promises, I think, to give the best clinical results.

We began by setting up for ourselves a high standard, and, if we have not quite attained to it, we can trace through our *Transactions* an honest endeavour to do so; and we can be sure that we have encouraged—I would venture to say we have practically established—a precision and order in the observation of clinical facts, which by itself is a substantial gain. If we look away from our more immediate proceedings, and if we remember that those who were interested in the formation of the Society are largely responsible for clinical research in our hospital wards and elsewhere, we shall recognise in every direction that an effort is being made to observe disease and its treatment in a more critical and accurate manner than heretofore. In the treatment of wounds, for example, whatever plan is followed (and many are and have been advocated), all have in common the care which conducts them, and which insures that every detail shall be scrupulously attended to. If I may single out one fact in illustration of this, I would remind you that, after being present at, and taking part in, our debate on pyæmia, Mr. Cadge effected a revolution in the results of the treatment of wounds in the Norwich Hospital; above all things, by seeing that the nursing, the cleansing, and the dressing were strictly, and hence successfully, directed. I say work is more precise in hospital practice and elsewhere; for, in using the latter word, I would recall the fact that there are points, such as those of history and of affinity, and the close watching of disease through the course of many