caused such an idea not to be dwelt on, was the fear that malignant disease was present. It was remarkable how little disturbance there was to the general health while this great collection was forming, and how it continued to augment the strength of its walls and fill the abdomen, and not come in the least to work towards the surface. The history of this other, somewhat similar tumour, I have not got.

24. A middle-aged thin weak woman came under my charge in the hospital in 1872, with a deep fluctuant tumour over the caput cæcum. She had suffered pain in the part for some months, and had not been laid up till a few days before her admission, when the size of the swelling, the pain, and a feeling as if it would burst, caused her for the first time to keep her bed. She could bear no pressure over the swelling, and she kept her thigh bent on the pelvis to relieve it. There was some pain down the front and inner side of the thigh, but no swelling of the limb. Her bowels were very sluggish, but never wholly closed. Pus was clearly recognised and its evacuation determined on; but I had to leave town for a day, and the operation was delayed. The nurse had given her, at her own request, a dose of castor-oil, and, when she got up to stool during the night, she passed, without effort or pain, about a quart of ill-smelling pus and a little blood. The swelling at once disappeared and the normal action of the bowel was restored. Pus continued to come with each motion during all her residence in the house; but, as the swelling did not reappear, and the pain and discomfort had ceased, she insisted on going home.

25. A female past middle age sought advice at the dispensary of the Royal Infirmary during my service there, on account of a fistulous orifice in her right groin, from which pus escaped freely. She said it had resulted from the bursting of a large abscess which had formed (she believed following a kick) within her belly, deep in the right iliac fossa a year before, and which for sixteen days had caused her much suffering, and, as she alleged, entire constipation for nearly a week of that time. She stated that fæces had at first come away through the fistula, but that for several months she had not observed any mixed with the discharge. Her bowels, which had been regular before the illness, had given her much annoyance since from their tendency to costiveness.

26. This preparation was taken from a poor fellow who was in the Western Infirmary lately, suffering from a vesico-intestinal fistula, resulting, as you see, from *gelatiniform cancer of the sigmoid flexure*. For many days before his death, complete obstruction was present and materially contributed to the rapid termination of his life. His case is worth recording. His age was 44. He was a butler by occupation, and there was no family tendency, so far as is known, to cancer. Twenty-one months before he came under my observation, he began to suffer from frequent micturition followed by pain all along the urethra. Blood early appeared in his urine. He was then treated for "gravel". Pain came on in the hypogastric region and deep in his perinæum, and he suffered greatly. The right testis swelled. A few weeks after the outset of his complaint, he was pressing at stool, when suddenly "a twist" seemed to occur low in his belly and fæces came through his penis. In a fortnight, this ceased to occur; but flatus, accompanied by much pain, continued to pass by the urethra. He slowly recovered, and the rectum and urethra resumed their respective functions. For four months, he remained well, and then, after exertion, retention of urine set in, and, as he was straining to relieve himself, a lump appeared below his penis, near its "root". This became very painful, and, during a sudden effort, he ejected a damson-stone, and the stream of urine was re-established, but accompanied by fæces. He was now confined to bed for six weeks and put on very restricted diet, and that was chiefly administered by the rectum. When he came under my care in February 1876, he was pale, weak, and cachectic. The urine was almost wholly flowing by the rectum (this had only begun about two months before), and most of the fæces (which, however, were scanty) came by the urethra, and their passage occasioned much uneasiness. The whole urethra was much inflamed, and a profuse purulent discharge was present. His abdomen was somewhat full and pressure over the sigmoid flexure caused pain. There was a decided swelling filling the iliac fossa, and giving to the hand a soft doughy sensation. There was dulness markedly elicited by percussion all along the colon from side to side, and at no part was the abdomen resonant. Only a limited quantity of water could be introduced into the bowel, and its presence caused much discomfort. Nothing could be done for him, except to relieve his pain, as he was very weak and emaciated. Complete absence of alvine evacuation and a great decrease of the urine existed for eight days before death. The bowels were very remarkably soldered together by old firm lymph, which hardly allowed them to be separated. There was no trace of recent inflammation. In the left iliac region, there was a mass of soft

jelly-like cancer, which involved the sigmoid flexure and the bladder in one lump. On separating this (as is shown in the preparation before you), the bowel was found to open freely into the upper part of the bladder, and within that viscus, masses of fæces, and colloid cancer were mingled together. The continuity of the gut was wholly severed, though the open ends of both upper and lower portions communicated with the bladder. The rectum was empty. The whole contents of the intestine appeared to be discharged into the bladder, and, when the patient lay on his back (as he usually did), the urine flowed away by the lower portion of the bowel, which opened from the bladder. The walls of the bladder were extensively invaded by the cancer. In the ascending and transverse colons, there were many old hard masses of fæces, by which considerable distension had been produced.

27. I shall refer only to one further cause of obstruction, and that because it is a very unusual one. I had this session in my clinical ward a man, aged 62, who was suffering from violent constipation, retention of urine, and severe œdema of the right leg. These symptoms were due to an enormously enlarged prostate gland—the largest I ever encountered. It was stony hard, and seemed to fill the hollow of the sacrum. This had formed slowly during several months, according to his account. He required a very long catheter, and it was only by the use of a strong enema-pipe and a long tube that his bowels could be relieved. By attention to his bladder and bowels, he improved in every way during his residence in the hospital, and returned home to Ayrshire, with instructions to continue the same treatment. I have since learned that, a few weeks after reaching home, his bowels became entirely obstructed, and that he died in consequence.

I shall not further multiply examples of the affection considered in this paper, but pre-ent the foregoing as illustrating several of the sources from which obstruction of the bowel may arise.

REPLY TO DR. C. J. B. WILLIAMS'S REMARKS ON THE MECHANISM OF THE SOUNDS OF THE HEART.

By ARTHUR LEARED, M.D., F.R.C.P., M.R.I.A.,
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IN a communication to the JOURNAL of September 30th, Dr. C. J. B. Williams has undertaken to refute my explanation of the causes of the sounds of the heart; and, with regard to his own researches, he says: "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." Dr. Williams's researches have undoubtedly been of great value to pathology. His theory of the sounds of the heart stands in the same relation to pathology that certain astronomical theories stand in to the science of astronomy. Although a given theory may not itself be true, certain deductions founded upon it may be true. But, even in the abstract, accurate knowledge is always desirable. Moreover, I have shown that the theory of Dr. Williams does not explain some leading pathological facts, which my theory explains.

Dr. Williams's paper consists of two parts: an attack upon my position and a defence of his own. As the attack comes first, it shall be met in the same order.

A single paragraph from my thesis, *On the Sounds caused by the Circulation of the Blood* (London, 1861), will place before the reader what is essential to be borne in mind in order to understand the controversy. It is this:

"All sounds formed in connection 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 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."

"I would object," writes Dr. Williams, "that of all bodies, liquids are the least adapted to produce sound; in contact with air, they become 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 siren and other water-instruments. But, without

air or without a solid, it is difficult to make a sonorous vibration in a liquid."

In order to test this matter, and also the effect of viscosity upon any sound produced by the friction of fluid against fluid, I devised a simple experiment, by which it was shown that sounds *are* produced in this way, and that they are modified by the consistency of the fluid. An intermittent column of fluid was forced, by an India-rubber bottle having a tube fitted to it, into a body of the same fluid. By this means, a current sound (murmur) was produced in water, and in a viscid fluid a shock-sound resembling the first sound of the heart. But these sounds are attributed by Dr. Williams to the "vibrating resistance" of the discharging orifice; and he says "the whole idea of noisy collision of fluids, without air or solids, is a mistake". He winds up as follows: "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 by fluids alone."

Although fully convinced by my own experiments that sounds are caused by the movements of fluids *per se*, it was desirable that this point should be further supported. I, therefore, placed the matter before Dr. Stone, who deservedly stands in the foremost rank of physicists in this country. Here is an extract from his reply: "Any person, whatever his scientific position, who declares the impossibility of sounds being produced in fluid alone, states a fallacy. By fluid, I understand incompressible fluid to be intended, though for accuracy it should have been so stated."*

But it would be easy to find further support. Let one extract from a recently published work suffice, as it has a direct bearing upon Dr. Williams's objection—that the sound in my experiment was really due to the vibration of the discharging tube. Speaking of the action of the siren in water, the author says: "The sound which follows proves that liquids enter into direct vibration, like gases, without sound being communicated to them by the vibration of a solid".†

In the case of sounds caused by the circulating blood, I do not deny that they may be variously modified by contact with the tissues containing the blood. What I contend is, that they are essentially blood-sounds, and are not caused by contracting muscle, nor by vibration of valves, nor of tubes. As regards vibration, will any one maintain that, when the wind whistles through a keyhole, or when one whistles by blowing into a key, or with his mouth alone, the sound is due in the respective cases to the vibration of the door, or of the key, or of the lips, and not to the motion of the air itself?

Having shown the fallacy of Dr. Williams's objection, I shall again, but more fully, explain my own views. Many of the facts and arguments which support them must necessarily be omitted.

The normal first sound of the heart is caused by the propulsion of blood out of the ventricles against the opposing blood in the great outlet vessels, provided that three conditions are fulfilled; namely, that the blood is of requisite consistency, that it is of requisite volume, and that it is propelled with sufficient force.

If water instead of blood circulated in the body, the normal shock-like first sound would never be heard. The mobile particles of the fluid would admit of so much friction that a murmur would be the invariable result. But in the case of sufficiently viscid blood, interference with the rapid and intimate intermixture of its previously separated portions modifies the sound. In the so-called anæmic condition of the blood, the first sound of the heart is, for this reason, very frequently found to have degenerated from the normal shock-sound into an abnormal current-sound. But, if the first sound essentially consisted in noises formed by muscular contraction or valvular tension, then in no case would the normal sound be completely absorbed or drowned by a murmur, as it so often is; unless, possibly, in certain rare instances of extremely loud murmur. I maintain that this position is incontrovertible.

The proofs that the two remaining conditions are requisite, must be briefly touched upon. As regards volume, it is well established that, in case of excessive hæmorrhage, by which the pressure in the vessels is greatly diminished, the first sound of the heart becomes changed to a murmur. In this case, diminished pressure seems to produce effects equivalent to those produced by insufficient viscosity with normal pressure; that is to say, the blood-particles intermingle so rapidly that a current or friction-sound is produced, instead of a shock-sound. The third condition—sufficient force—virtually applies to every theory of the sounds, and need not detain us.‡

* For mathematical calculations relating to the flow of liquids through tubes, etc., see Dr. Stone's instructive lectures on "The Physical Basis of Auscultation" (*Medical Examiner*, February 24th and May 4th, 1876).

† *The Forces of Nature*, by Amédée Guillemin. Translated from the French by Mrs. Norman Lockyer. London: 1872. Page 155.

‡ I omit from consideration alterations in the normal sounds caused by leakage or obstruction.

But, says Dr. Williams: "I have already shown that no sound results from the impaction of one portion of fluid against another; when no air intervenes, the whole movement is one of quiet displacement."

"The case is one of striking through a space at a tight barrier, but of a soft, rather sluggish fluid in close contact." It is implied, in the first of these statements, that the presence of air at the outlets of the heart would contribute to sound. But, as will be shown further on, the very contrary is the fact; for air, in these positions, would annul any sounds formed in the blood. With regard to the second rather curious statement about a soft sluggish fluid in close contact, striking through a space against a tight barrier, has Dr. Williams forgotten the force with which blood issued from the punctured aorta in his vivisection experiments? The force of the adult left ventricle was estimated by Valentin as sufficient to overcome a pressure of four pounds.

The second sound of the heart is caused by the recoil of the blood in the great vessels during diastole. It is not caused, as held by Dr. Williams and others, by mere vibration of valves; but by a sudden shutting off of the descending blood by valves, surrounded and supported by blood in the ventricles, and by the tissues of the heart itself.

In the production of the normal second sound, the consistency of the blood has no place. Hence, even in the most marked cases of anæmia, in which the first sound is replaced by a murmur, the second sound never undergoes the same change from this cause. Force and pressure in the circulation are requisites, only as affecting the loudness of the sound. No variation of these conditions causes degeneration of the second sound into a murmur. But, as obstruction in any part of the arterial system causes an increase of pressure behind the obstruction, increased loudness of the second sound—accentuation, as it is termed—is taken as an indication of a contracted condition of the minute arteries. In case of mitral valve incompetency, the second sound is changed from a shock to a current sound, on the same principle as when the first sound is changed into a murmur, because of an insufficient volume of blood, as occurs in excessive hæmorrhage.

I have introduced, in my thesis, the case of the noise made in a force-pump by the action of the moving water against the water at rest in the discharge-pipe, as an analogy for the first sound of the heart. And I was also the first to point out the close analogy between its second sound and the sound caused by turning a cock at the end of a pipe of some length, through which water is flowing from a cistern. Nothing, indeed, can be clearer than this analogy. For the pressure on the blood, in vessels sufficiently rigid for the purpose, supplies the place of the perpendicular pressure in case of the tube and cock. If a loud sound be produced in one case, owing to the contact of water with metal, a low and comparatively obscure sound might be expected to be formed under fairly similar conditions in the other. But, if it can be conclusively shown that the second sound cannot be caused by mere valvular tension, then there is no reasonable alternative but to accept the tube and cock analogy, as explaining the second sound.

Dr. Williams writes: "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." The promise implied at the beginning of this paragraph has not been fulfilled. Nothing has been more fully explained. But, with regard to the objection raised, it is impossible to say what exact share the water has and what the cock in forming the sound. The noise

* Dr. Williams has here fallen into an error, which is traceable to Dr. Shapter's work, *Notes and Observations on Diseases of the Heart*. Misquotations in the same book, from his own work on the *Pathology and Diagnosis of Diseases of the Chest*, are spoken of by Dr. Williams. Dr. Shapter has, at page 12 of his book, reprinted a letter of mine from the *BRITISH MEDICAL JOURNAL* (1866), and also an extract from Dr. Arnott's book, in parallel columns. The matter discussed in both instances is the formation of sound by the sudden arrest of water flowing through a tube; and in addition, in the case of my letter, the analogy between this sound and the heart's second sound. But Dr. Shapter has added to the extract from Dr. Arnott a paragraph marked like the previous portion, with inverted commas, which is not to be found in Dr. Arnott's work. The effect is to make it appear that I had been anticipated in this explanation of the second sound. No mention of the analogy in question is anywhere made by Dr. Arnott.

is undoubtedly made louder by metallic contact; but if it be admitted that the noise is partly due to the water and partly to the cock, and the analogy be applied to the second sound of the heart, it does not shake my position as to the mechanism of the sound.

By causing a flow of liquid through the heart and aorta removed from the body, I have succeeded in closely imitating the second sound of the organ. But if great precaution be not used in excluding air, so as to prevent it from reaching the aortic valve, even in very small quantity, no sound will be produced. This is because the elasticity of the air breaks the force of contact between the fluid and the valve.* But supposing, with Dr. Williams and others, the sound to be caused by the vibration of membranes in fluid, the presence of air would tend to increase the sound instead of destroying it altogether.

Now, Dr. Williams says that he had in his garden, at Cannes, "a water-pipe exhibiting remarkably this phenomenon" of noise on turning its tap. "The noise", he says, "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."

I have shown that the separation of the surfaces by gas or vapour, instead of multiplying the sound, would destroy it altogether. If the aorta were a metallic tube, the heart-sounds would undoubtedly be very loud sounds; but I deny that the elasticity of vessels or atmospheric pressure are sufficient to prevent the formation of low sounds by the heart in the bodies of animals.

A simple device, which has a direct bearing on what I have just stated, is sometimes employed by plumbers to prevent the noise made by water turned off by a tap. It is to insert one end of a small tube into the pipe, just above the cock, while the other end of the tube is connected with an air-chamber. The concussion between the water and the cock is thus prevented as effectually as it would be by the intervention of a layer of "gas or vapour" between them.

I have devised an experiment, founded on this practice, which circumstances have not allowed me to put into execution. It is this: Having rendered a large animal insensible, to fix in the aorta, close to the valve, a tube fitted at one end with a properly adapted screw-flange, and at the other with an air-chamber. If the result were (what I expect) that both sounds of the heart were extinguished, my explanation of their cause might be regarded as demonstrated.

Dr. Williams recapitulates the observations made in his vivisection experiments; but most of the conclusions drawn from them are not antagonistic to my theory. One only calls for special remark; namely, that whether the heart was empty or full, whether it was moving feebly or forcibly, some (so-called) muscular sound was always present. My explanation of this circumstance, deduced practically from my own experiments, is as follows. When a stethoscope is applied directly to a contracting heart, it is *absolutely impossible* to distinguish the sound caused by the friction of the heart's surface against the stethoscope from any allied sound. The first sound of the heart may, indeed, be closely imitated by movements of the palm of the hand applied to the bell end of a stethoscope, when the other end is applied to the ear.

Instead of attributing the various sounds of the circulation to widely different causes—such as muscular contraction, valvular vibration, and flowing blood (murmurs)—all the sounds, according to my views, are closely related. Under certain conditions, one sound changes into the other; and, therefore, all are formed essentially by the same mechanism.

It results from this, that pathology gives to my theory an amount of support which no other theory can claim. Many phenomena could be adduced in support of this statement, if my limits allowed. I will, therefore, conclude with a question, because, when put on a former occasion, it remained unanswered.

Why is it that, in anemia, the first sound of the heart only, and the second sound never, becomes replaced by 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.

* See my thesis, page 15, in which I have pointed this out as the reason why Sir D. Corrigan was unsuccessful in imitating the second sound of the heart. I beg here to say that I shall be glad to forward a copy of the thesis, on application, to any one specially interested in the subject of the heart sounds.

† I have elsewhere sought in this phenomenon for an explanation of reduplication of the second sound of the heart (*Lancet*, October 17th, 1868).

A CASE OF CARCINOMATOUS TUMOUR OCCUPYING THE RIGHT POSTERO-PARIETAL LOBULE OF THE CEREBRUM: BLINDNESS FROM DOUBLE OPTIC NEURITIS.

By JAMES RUSSELL, M.D.,

Physician to the General Hospital, Birmingham.

REFERRING to the recent observations by Dr. Ferrier on the Functions of the Cortex of the Cerebrum, it appears (*Proceedings of the Royal Society*, 1875, No. 161) that, after stimulating the postero-parietal lobule or the superior extremity of the ascending parietal convolution in monkeys, he obtained results which led him to regard this particular region as a centre for the movements of the hind leg, and apparently for those concerned in walking. Further, in two experiments, iv and v (*Philosophical Transactions*, 1875), after destruction of the postero-parietal lobule (foot-centre), the ascending parietal convolution (hand and wrist) with a portion of the ascending frontal (arm and leg centre)—the latter convolution to a different extent in the two experiments—in the one case, hemiplegia of the opposite side was produced; in the other, paralysis of the muscles governing the ankle and wrist. In the case I have to narrate, the only distinct symptoms referring to the condition of the motor function, which could be eliminated from the general depression of all the nervous functions which prevailed, were occasional dropping of the arm from brief temporary paralysis, and unquestionably a state of permanent weakness of that member, appreciable by comparison with the opposite limb. Muscular energy was generally in so low a condition that it was difficult to say to what extent the very feeble power of locomotion as regards the lower extremities was to be assigned to each of the several elements—general hebetude, awkwardness from recent loss of vision, and local muscular weakness. Brief attacks of loss of speech must also be noted, together with abnormal sensation in the lips and tongue; these attacks, together with the passing arm-paralysis, were doubtless to be referred to irritation (vascular contraction?) and not to destruction of tissue. Had the disease been of an irritative rather than of a destructive character, a syphiloma rather than a carcinoma, we should probably have to note more characteristic phenomena.

POST MORTEM EXAMINATION.—The sinuses of the upper part of the dura mater were empty, the surface of the brain was pale, the convolutions were much compressed, and the sulci were narrowed to mere lines. There was no ventricular effusion. A tumour, having the microscope characters of carcinoma (Dr. Saundby) of oval shape, two inches in its antero-posterior diameter, was buried in the superficial part of the right cerebral hemisphere, occupying nearly exactly the site of the postero-parietal lobule. It was clear of the ascending parietal convolution in front; and behind, it did not quite touch the external parieto-occipital fissure; below, it extended just short of the intraparietal fissure; and, on the inner face of the hemisphere, reached downwards in the quadrilateral lobe for half an inch. It extended into the cerebral substance for the depth of one inch. On its surface, it presented some traces of the convolutions it had supplanted; below, and especially towards the ventricle, the whole substance of the hemisphere was extensively softened, shreddy, and of a yellow colour. The opposite hemisphere was quite healthy throughout, and so were the pons Varolii and medulla oblongata. The cord was typically healthy till we reached the lower three inches of the dorsal region, where it was very soft, though white and without any vascularity or intermixture with blood-material; nor could Dr. Saundby or myself detect any foreign matter in its composition. As this part of the nervous centres was not examined till four days after death, we should unhesitatingly have referred the softening to *post mortem* changes, but for the contrast afforded by the other portions of the cord. The lumbar enlargement was quite healthy. All the other organs of the body were quite healthy. The cerebral tumour was primary.

The patient was forty-nine years of age, and was kindly sent to me, as an in-patient of the Birmingham General Hospital, by Mr. Eales, Surgeon to the Eye Hospital, on September 28th. He was then almost completely blind, and exhibited the typical characters of double optic neuritis, as indeed had been previously determined by Mr. Eales. His symptoms dated entirely from June of the present year. First, his left hand dropped for a short time, and the dropping recurred several times, but loss of power was always regained in an hour's time or less. He had also had brief attacks of loss of speech, and had felt numbness in his lips and tongue; but asserted very clearly, in reply to questions, that the peculiar sensation affected *both sides* of the mouth and tongue. He had had numbness in the tips of his fingers and the left hand. It was found quite impracticable to ascertain how often these attacks had occurred, or what was the relation of the different attacks to each other;