but I know of no evidence for this statement, and it is contrary to the very numerous observations which are already on record.

There is certainly no pathological connection of the suprarenal capsules with the lymph-glandular system. No less certainly is there no physiological connection between them. The function of lymph-glands with the other cytogenic structures (spleen and thymus, tonsils and Peyer's patches) is to make leucocytes; the function of the adrenals is unknown, if they have any in the adult, which in a recent number of the JOURNAL (December 9th, 1876, p. 741) I have given reasons for doubting. But the structure of the adrenals is totally different from that of cytogenic organs—as different as from that of true (secreting) glands. The absurd name, “ductless gland”, and the ambiguous terms, “glandular” and “adenoid”, have led to endless confusion in histology as well as in physiology, by connecting together such absolutely different organs as the suprarenal capsules, the thyroid, and the true lymphatic or cytogenic structures. That the confusion may not spread to pathology, is my apology for these remarks.

REPLIES TO DR. BROADBENT'S QUESTIONS AND OBJECTIONS IN THE MATTER OF THE SOUNDS OF THE HEART.

By ARTHUR LEARED, M.D., F.R.C.P., etc.,
Senior Physician to the Great Northern Hospital, etc.

THE tone of my friend Dr. Broadbent's communication in the JOURNAL of December 23rd reminds me of that of certain puritan pamphlets still extant. These productions bear such titles as “A Ready Method with Unbelievers”, “The Sinner convinced against his Will”, etc. There really seems to be a resemblance between the polemic effects of theology and those of controversy about the cause of the heart's sounds.

“In contradiction,” says Dr. Broadbent, “to Dr. Leared's statement that it is absolutely impossible to distinguish the sound caused by friction of the heart's surface against the end of the stethoscope from any allied sound—i. e., in this case muscular sound—I maintain, from a large experience of auscultation of the naked heart when assisting Dr. Sibson in his experiments, that it is both possible and easy.” The argument, of course, refers only to the contracting empty heart, because, so long as the organ is acting upon its contained blood, several other alleged causes of sound are in operation. From a large experience of the same kind, I can fearlessly meet one flat contradiction by another equally flat, or, in other words by maintaining my statement as above quoted. Now, the matter in question is stated to be possible and easy. A German investigator evidently did not regard it in this light when he took the precaution of causing the heart to act while surrounded by water. But I do not admit that the faint sound alleged to have been transmitted by the fluid medium was necessarily muscular. The experiment was also exposed to a fallacy arising out of the sound-waves formed by commotion in the water caused by the moving heart.

Let us now see whether Dr. Broadbent's contradiction cannot be met by one of at least equal force and free from all taint of partisanship with my views. Dr. Halford, now Professor of Anatomy in the Melbourne University, is well known to have performed many vivisections in reference to the question of the causes of the heart-sounds; and the conclusions arrived at were, in the main, not in accordance with mine; but he positively affirms that, when the supply of blood was cut off from the acting heart, “neither first nor second sound was heard”.

Dr. Broadbent's reply to my question, Why is it that in anæmia *the first sound of the heart only, and the second never*, becomes replaced by a murmur?—is “by disputing the fact. The first sound is not replaced under these circumstances, but accompanied by a murmur”. If Dr. Broadbent will modify his statement—and it will then equally answer its purpose—by inserting the word “always” before “replaced”, and the words “is sometimes” before “accompanied”, I will at once admit its truth. But how does this “take much of the ground from under” me? My contention is, that the body or main portion of the first sound is constantly transformed, under the influence of the change from normally viscid blood to abnormally thin blood, from a shock sound to a murmur. The accompanying feeble shock-sound heard in certain instances over the aorta may have various origins. “It may,” says Dr. Broadbent, “save trouble to say that I am aware that there are two first sounds, and that I speak distinctly of the co-existence of a murmur and sound both of left ventricular origin.” Now, it may also save trouble to remind the reader that so closely placed are all the apertures of the heart, that the bell of a very small

stethoscope would cover a portion of all the valves which close these apertures.

It is certainly possible by certain manœuvres to distinguish the sounds of the heart in relation to the separate sides from which they spring; but I maintain that, when the normal aortic sound is replaced by one of entirely different character, it is not always possible to guard against error arising from conduction of sound. If the chest-wall did not intervene, the difficulty would be much lessened. Now, an instance in which this difficulty did not exist presented itself to a very competent authority—namely, Cruveilhier—in the case of an infant born with the heart exposed. He found that the maximum intensity of both sounds was at the same place—namely, the base. And he adds explicitly: “Hence it is that at the base of the organ we are to look for the cause of these sounds.” Thus it comes to pass that, although “a systolic murmur and a first sound may coexist”, and is admitted by me, I am not conscious of any insecurity of “the ground”.

The undisputed facts are that, in anæmia without valvular lesion, the first sound of the heart, examined over its left side, is sometimes entirely replaced by a murmur, while at others the murmur is accompanied by a sound more or less resembling the normal first sound. The second sound at the same time never undergoes the like change. My explanation of this is, that in the one case the unnaturally thin blood allows the formation of a blowing sound instead of a shock sound, on known physical principles; while in the other case the intervention of the valve prevents the formation of the blowing sound on the same principles. When, on the other hand, a murmur replaces the second sound owing to incompetency of the valve close to which it is formed, more or less of the normal second sound may sometimes be heard coincidentally with the murmur. Dr. Broadbent would perhaps assign the normal portion of the sound to the vibration of the healthy part of the valve. This, for reasons too long to discuss, I would deny, and would assign simply to the same cause as I have already done the sound heard with the anæmic murmur on which Dr. Broadbent lays so much stress—namely, to conduction. There is, indeed, only one argument that can be brought to bear with effect against my view that sounds are always caused at the outlets of a heart acting with ordinary force, and that the aortic murmur of anæmia is nothing but a modified, or, as I would term it, a degenerated shock sound. That argument has been afforded by Dr. G. Balfour, who, in his lately published work on *Diseases of the Heart*, maintains that the anæmic cardiac murmur is not of hæmic origin at all, and is not formed in the great outlet-vessels. Founding his argument on the circumstance that, according to his experience, the murmur is most distinct just over the left auricular appendix, he ingeniously maintains that it is really mitral and regurgitant. Here the difficult question of conduction again meets us; and there are besides so many reasons for upholding the long established blood-theory, that I do not think he will find many supporters.

Dr. Broadbent ends his paper with a series of questions, which, in order to be answered, must be reproduced; but I shall abbreviate them when it can be done without injury to their meaning.

1. A sound due to collision between blood issuing from the ventricles with stationary blood in the great vessels must be produced at the aortic and pulmonic orifices. How is it, then, that the first sound is better heard over the ventricles, and even at the apex, than at the base and over the great vessels? If the first and second sounds be originated at the same point, they ought to be heard over the same area. Here the difficult question of the propagation and conduction of the first sound again presents itself. When the ventricle contracts upon its contents and the sound is formed at its outlet, it is probable that, owing to forcible contact with the stationary blood, sound-vibrations in the blood are propagated backwards and conducted by the walls of the ventricle, so as to make the first sound most audible over the ventricle and its apex. Such vibrations are prevented in case of the second sound by the closure of the valve, and therefore the second sound is not heard so distinctly over the ventricles. I put this explanation forward hypothetically. But I beg to draw Dr. Broadbent's special attention to the fact that there are exceptions to his statement. In some instances, the first sound is as loud or even louder at the base than at the apex. Moreover, how is he to explain the positive statement of Cruveilhier already mentioned in this connection? His objections are, I think, fairly balanced until these points are set aside.

2. In cases of high arterial tension, with its accompanying increased propulsive power in the left ventricle and consequent increase in the violence of the collision between the ventricular and aortic blood, “the first sound near the seat of this collision, its supposed cause, is enfeebled. Will Dr. Leared explain this?” He will, and, he believes, satisfactorily. From my various experiments on the production of

sound by the propulsion of a moving column of fluid through a stationary body of the same fluid, I arrived at this conclusion: that, when pressure upon the fluid was raised to a certain point, sound could be with difficulty obtained. The application of this observation to Dr. Broadbent's question is obvious; and I have to thank him for bringing under my notice a pathological point which has a direct bearing on my theory of the first sound.

3. There is a well recognised difference in character between the synchronous first sounds, formed respectively in the right and left sides of the heart. "How does Dr. Leared explain these (differences), if the sounds be the product of collision of the blood, and not of tension of the ventricular walls and valves?" I explain this very simply by the fact that the conditions under which both these sounds are formed are as different from each other on my theory as they are on his, assuming Dr. Broadbent's theory to be the true one. I have just shown that pressure has much to do with sound; and I have a word more to say bearing on this point presently.

4. In dilatation of the ventricles, and in stenosis of the mitral orifice, the first sound is often louder than usual, but it is short and sharp like the second sound. "This is easily explained in the case of dilatation, if the ventricular walls and valves are supposed to be the origin of the sound. An explanation on the hypothesis of collision of moving ventricular and standing aortic blood is requested." It would require too much space to go into this matter in detail. But, so far as an indication of the direction of how it is to be explained, the request shall be complied with. First, I would say that I consider Dr. Broadbent's assertion that the matter is easily explained on the hypothesis mentioned, although plausible, is a long way from being proved. I have spoken of the difficulty experienced in forming sounds in fluids when they are under a certain pressure. If water be employed in the experiment, the sound produced under considerable pressure is always a blowing sound; although, if the pressure be increased, it approximates more or less to a shock sound. If a sufficiently viscid fluid be employed, and a low pressure, the sound is also a murmur. But, if the pressure be increased to a certain point, the sound becomes more or less a shock sound. If the pressure be still further increased, the shock sound becomes lowered in tone; and, if increased beyond a certain point, can hardly be heard at all. The explanation of Dr. Broadbent's difficulty lies in this: the first sound is loudest when the blood is under moderate pressure. If the pressure be much diminished, the sound is transformed into a murmur, as has been observed repeatedly in cases of excessive hæmorrhage. Probably, the first sound is loudest just at the point when the conditions, as regards pressure, are sufficient to form a shock-sound instead of a murmur. Now, in diminished ventricular power (dilatation) and in diminished pressure from want of supply of blood (stenosis), we have two conditions which lessen pressure and increase the loudness of the first sound.

5. In aortic dilatation and in aneurism, the second sound is louder and greatly changed in tone. This is easily explained, if it be admitted that the change results from the tension and vibration of the enlarged area of resonant membrane, but could scarcely be the effect of increase in the size of the column of blood.

But, why not? The resounding nature of membranes seems to haunt some men when arguing on this subject. They never can get the drum and tambourine out of their minds. Let me assure Dr. Broadbent that, when he whistles, it is the air issuing from his lips, and not the lips themselves, or the vibration of any of his membranes, which forms sound. In case of an aneurism, the enlarged body of fluid exercises its peculiar effect on the production and conduction of sound, so much so, that good authorities have maintained that sounds resembling both heart-sounds are formed independently in aneurismal sacs. But this subject is too large for further discussion here.

Dr. Broadbent's questions leave the main argument unassailed. They grasp at the shadow instead of the substance, and do not, except indirectly, attack the salient points, namely, the close analogy between the known conditions on which certain hydraulic sounds are formed and the existence of similar conditions in the human body. One point only to which I still attach "capital importance" has been handled, with what result let the "candid reader" decide.

LARGE AND LONG NEGLECTED POLYPUS OF THE EAR.

By H. BURFORD NORMAN, F.R.C.S. Eng., Southsea.

THE following case may serve as a useful companion to those cases of neglected aural polypi which were recorded by Mr. Field in the JOURNAL of December 16th.

Mrs. C., aged 62, but much older in appearance, consulted me on November 29th, on account of a growth in her left ear. It had existed ten or twelve years at least, and, for a large part of the time, without causing any great inconvenience. She had often shown it to her medical man, who never advised her to do anything for it; and, as she says, her mind being much more concerned for the care of her family than of herself, she let the matter take its course. Of late, however, the growth having increased much in size, and caused incessant pain and fetid discharge, with occasional bleeding, she thought she might get something done for her relief. On examining the ear, I found a large pinkish and somewhat spongy-looking mass filling up the cavity of the outer ear, pushing the tragus forward: it sprang from the depth of the auditory canal, which it filled. On examining the growth by the probe, and gently lifting it in a pair of bowed pile-forceps, a considerable gush of blood took place. In front of the ear, the skin was of a dusky-red colour, and swollen nearly to the cheek, and the temporal artery was felt throbbing and hard just in front of the tragus. The outer ear was also in a similar state—red, thickened, and rigid; whilst, below the ear, and on the side of the neck, besides a reddened state of skin, there was some induration of the lymphatic glands, and an ulcer discharging thick gummy matter. I put a compress and circular bandage on to stop the bleeding caused by the examination, and ordered the use of tepid-water and alum-injections whilst deciding what to do with the growth. The hearing on the diseased side was impaired, but not so much as might have been expected; and the growth seemed to have a perfectly "benign" character, for in its long life it had remained quite isolated.

From these considerations, I thought it probable that the growth was connected with some part of the circumference of the auditory canal, and not with the membrana tympani. I made several attempts to ascertain the point by the use of the probe, but in vain; it gave me no help, and could not be passed on all sides and around the growth, in consequence of its not only filling the meatus, but swelling out much, mushroom-like, over the edges of that passage. The same circumstance made it at least very doubtful whether I could either put a wire around the growth, or cut it off by knife or scissors at sufficient depth to be effectual. I therefore decided to apply torsion, either so far only as to cause strangulation, which I have seen answer well in slight cases, or to twist the growth off.

On Dec. 3rd, I put this plan in operation, grasping the outer part of the growth in the bowed forceps, and slowly twisting it once quite round. Finding this not very painful, I carried the proceeding further, and was gratified at finding the growth snap off when the second turn or twist was about half made, and in my possession a polypus more than two inches long, the lower and more slender part of which, having a smooth mucous covering, had filled the meatus, and measured two inches and a half in circumference; while the larger and protruding part was granular on its surface, and measured between three and four inches round.

A great gush of blood followed the removal of the growth, but it was quickly stopped by plugging the ear with long strips of lint soaked in strong solution of perchloride of iron, and this was secured by a compress of lint and bandage round the head. In plugging the ear, my forefinger passed readily to the bottom of the meatus.

Next day, I removed the compress and part of the plug, as they caused uneasiness; and, on the second day, the remainder of the plug, which was loose. Since then the case has done very well, and I saw my patient yesterday in good health, the swelling and redness of the auricle and neighbouring integument all gone, the ulcer on the side of the neck healed, and the auditory canal much reduced from its lately exaggerated size, and this notwithstanding a slight attack of erysipelas which followed the operation.

This is by far the largest polypus of the ear, and altogether the worst that I have ever met with—worthy, I think, of being recorded for an example of what may be done in such a case.

SOME EXPERIMENTS WITH NITRITE OF AMYL.

By W. LEMON LANE, M.B., etc., Crossgate, Dunfermline.

THERE is so little known about the action of this medicine, that I think a few experiments which I made with it will be interesting.

Experiments 1 and 2.—Two kittens of the same age and size got chloroform simultaneously. When both were thoroughly insensible, I gave one of them two minims of nitrite of amyl to inhale. The result was this. In the kitten which had no nitrite of amyl to inhale, reflex motion appeared in five minutes, and sensibility returned in eleven minutes thirty seconds; while, in the other kitten (which had amyl-