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

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

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

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

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

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

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

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

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

AN IMPROVED METHOD OF PERFORMING THE "BOUTONNIÈRE" OPERATION.

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

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

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

If the various surgical works be examined, it will be found that the operation may be divided into four different steps: 1. Opening the urethra at, above, or below the stricture; 2. Finding the entrance to the stricture, and passing a grooved probe or bougie through it. There are, of course, cases where the surgeon fails to find the entrance, and is obliged to dissect on bit by bit along what he supposes to be the course of the canal till he arrives at the pervious urethra beyond. 3. Division of the stricture on the probe-pointed director or whalebone bougie. 4. Introduction of the catheter into the bladder. As a rule, most surgeons commence the operation from above downwards; but Mr. Coulson and others have reversed the step. I extract the following description of the method as pursued by Mr. Coulson from the French, as there is an impression in this country that French surgeons know but little of the opinions or practices of others.

Procédé de M. Coulson. "Ce chirurgien porte un cathéter cannelé jusqu'au rétrécissement, il incise le périnée, et il fait la boutonnière derrière le rétrécissement: par cette ouverture, il introduit un stylet cannelé dans le stricture, jusqu'à ce qu'il soit presque en contact avec le cathéter, appuyé sur la face antérieure de l'obstacle; il incise ensuite sur le stylet cannelé toute l'épaisseur des tissus de la portion de l'urètre rétrécie; il fait ainsi cesser la rétention en même temps qu'il opère le rétrécissement." (*Traité des Maladies des Voies Urinaires*, par le Docteur Ch. Phillips, p. 180.)

The unfrequency of the performance of the operation is, I think, due to two causes: firstly, the difficulty of finding the entrance to the stricture and introducing a probe through it; secondly, even when the surgeon has succeeded in passing a probe, his state of uncertainty as to where it has gone, and his natural dislike to cut in the dark. There must also be added those cases in which it is impossible to pass any guide, and where the surgeon is left to work a way and grope into the bladder as he best can, and may possibly utterly fail to reach the pervious urethra beyond the stricture. Cases are on record in which experienced surgeons have failed to accomplish the operation, and the patient has been taken away from the operating-table with the stricture unrelieved. Lastly, the surgeon may apparently succeed, and find afterwards that he has only cut open a false passage or a fistula, leaving the strictured urethra untouched. When I had performed the operation several times, I found there were two separate defects to be remedied: firstly, the difficulty of finding the entrance into the stricture; secondly, the uncertainty as to the real position of the probe when apparently introduced into the bladder. The first defect arises from two separate causes. *a.* The probe may fail to enter a stricture because it is too large. A ferret will wriggle into a rabbit-hole when a dog cannot, simply because it is much smaller; and a probe will sometimes fail to enter a stricture when a fine whalebone or filiform bougie will slip in with ease, because the metal director is too big. *b.* The probe being constructed of unyield-

it has occurred to very experienced operators to be deceived on this point; and, when they thought that they had introduced the probe into the bladder, they had really passed it into the rectum, beyond the reach of the finger, or into a sac between the rectum and bladder, or into a false passage. It has been my object to remedy this defect, and to convert an uncertain procedure into one in which the surgeon is able to determine absolutely whether his guide be in the bladder or not before he begins to cut, so that he will be saved the mortification of being foiled in the execution of the operation.

I will now briefly enumerate the various steps of the operation. The patient, having had the rectum emptied and the perineum shaved, is to be put into the lithotomy position, and there secured with Pritchard's anklets and wristlets. A straight staff with a groove in it is now to be passed down to the face of the stricture. I know of none better than Mr. Wheelhouse's, in which the groove stops short half an inch from the end of the staff, which is tipped with a button, so that, after it has served as a guide for the surgeon to open the urethra, it can be turned round to hook up the apex of the wound with its button. The urethra is "to be opened in the groove of the staff, not upon its point" (Wheelhouse). There is a great advantage gained in opening just above the stricture, rather than on a level with it; for the *cul de sac* containing the mouth of the stricture is preserved, and not slit up as it would be were the incision made at the point of the stricture. The edges of the wound are to be kept apart with sharp-pointed hooks, or two pairs of artery-forceps with nibbed points, as used by Mr. Wheelhouse; or two loops of thread may be passed through the edges, as introduced by the late Mr. Avery, and highly extolled by Dr. Gouley of New York. I have always used hooks, and found them answer every purpose. Now comes the most important step but one of the operation—searching for and finding the entrance into the stricture, and passing the instrument through it.* English surgeons use metal probes, but I prefer the bougies as employed by Dr. Gouley. My two favourites are depicted in the woodcut at A C. A B is a very fine whalebone bougie, not much bigger than a horsehair, and having each end tipped with an olive. It is an exquisitely made bougie, and was sent to me from Augusta, Georgia, by Dr. Coleman. The bougies for this operation must for the present be procured from France or America, as the English ones which I have seen are not sufficiently fine and smooth for the purpose, and all lack properly made olives at their ends. Having taken the bougie represented, I impart to one extremity a beak such as is depicted at B, and with it I search for the entrance into the stricture. If I fail, I try with the other end of the bougie; and, if I do not succeed, I take one of Leroy d'Étiolles' "bougies tortillées", C. I may say that I have always managed to pass one or other of the bougies through the stricture into the bladder. It sometimes happens that there are several false passages or fistulæ. Each of the openings ought to be filled with a bougie, which is to be left there, and another

ing metal, it may utterly fail to follow the tortuous passage of the stricture. If a ferret suffered from an ankylosis of the vertebral column, he might possibly succeed in getting into the rabbit-hole; but his progress would soon be arrested by his inability to adapt his body to the windings of the passage. I have, therefore, entirely abandoned the use of metal probes, and taken to whalebone and filiform bougies, as advocated by Dr. Gouley of New York; and I think it will be found that they will overcome the difficulties above alluded to, and prove tractable and efficient instruments in the hands of surgeons.

The second defect is more important. When the probe or whalebone bougie has been passed, the question to be asked is, Whither has it gone? If it could be moved about freely in all directions, were in the median line, and were not in the rectum, most surgeons probably would feel satisfied, and say that it must be in the bladder. But

and another passed till at last one vanishes, apparently, into the bladder. This plan was introduced by Auguste Mercier, and is adopted by Gouley, and is a practical exposition of the method of arriving at a result by "the process of exclusion".

I now come to the very pith and kernel of the operation. I think I have passed the bougie into the bladder, and I ask myself the all-important question, Whither has it gone? Before commencing to divide the stricture, I proceed to prove the position of the bougie, and to demonstrate conclusively whether it be in the bladder or not by

* The comparative lengths of the different instruments represented in the woodcut are not, relatively, quite correct. The bougies A C, and the tube D E, are each ten inches long. The slit F is two inches long, and terminates two inches from the vesical end, G. The vesical half of the catheter, G H, is five inches in length, and the urethral half, I K, is nine inches long.

sliding over and along the bougie the fine silver tube D E,* which is open at both ends, has a slit (F) for the tenotome to run in, and is fitted with the vesical half (G H) of the elastic catheter; so that, when the tube is entering the bladder, we have the combination depicted in Fig. 6, where the silver tube L, carrying the vesical half (M) of the elastic catheter, is seen gliding over and along the bougie (N), which is now to be withdrawn, when urine will flow out of the metal tube if it be really in the bladder. If urine come out, the stricture is to be divided by sliding a probe-pointed tenotome along the slit in the tube, which is to be held in such a manner that the slit, which serves as a groove, shall look upwards. The deep part of the urethra is thus divided subcutaneously without enlarging the original wound. The vesical half (M) of the elastic catheter is now to be gently pushed along the tube (L) till it is fairly in the bladder. If its progress be arrested at any point, the tenotome must be reintroduced, and any opposing tissue divided. The vesical half of the catheter having been passed into the bladder, the metal tube is withdrawn, and the end (I) of the urethral half (K) of the elastic catheter is to be screwed into its fellow-half at H, so as to make one continuous catheter. The next thing to be done is to pass the olive end (K) upwards from the apex of the wound till it emerges at the meatus externus. This is usually easy to do; but, if there be any trouble in doing it, an assistant can pass a large olivary catheter down the penis till the point of the instrument appears in the wound. The surgeon now catches hold of the point, and, having cut off three inches of the protruded catheter, he inserts into its cavity the end of the elastic catheter, K. The assistant, still holding the catheter he passed downwards, now draws it upwards, and thus brings up with it the other catheter, which was inserted into it by the surgeon. The catheters are now unlinked, and the one in the bladder can be retained or not as the operator may fancy. I look upon the retention of a catheter as a relic of a surgical age now past; for Dr. Gouley has, by the narration of a number of facts, conclusively demonstrated that the retention of a catheter after operation is, to say the least, perfectly unnecessary.

The advantages of the operation as above described may be summed up as follows. 1. The greater chance of finding the entrance to the stricture with a fine olive-tipped bougie, and, when found, of passing it through the stricture, than with the stiff metal probe, which is larger. 2. Demonstrating that the bougie is really in the bladder by sliding a fine silver tube over it and drawing off the water. This is really *the point* of the operation, for it enables the surgeon to divide the stricture with certainty, and spares him the mortification of laying open a false passage. 3. The metal tube serves as a guide over which the elastic catheter can be passed into the bladder. 4. Diminishing the risk of the operation by dividing the stricture subcutaneously, instead of enlarging and extending the original wound.

I consider the "boutonnière" the right and proper operation to perform for the relief of impassable stricture, after the surgeon has exhausted all ordinary means to pass a catheter. If retention from urine from impassable stricture set in, then the operation is specially indicated, for by it the cause and effect are treated at one and the same time.

CASES OF OTORRHOEA AFFECTING THE MASTOID BONE.

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THE diseases attacking the mastoid bone, primary or secondary, are amongst the most serious with which aural surgeons have to contend, whether we regard the danger to the patients or the prompt treatment required. I do not intend, in this paper, to separate the different kinds of mastoid inflammation, but to record several cases illustrating the various forms of this affection which have come under my care in public and private practice during the last few years.

In a paper read before the Clinical Society in 1872, Dr. S. Gee pointed out very concisely that the connection between cerebral symptoms and purulent affections of the ears might be arranged under the three following heads: 1. Symptoms of otorrhoea preceding those of meningitis; 2. Symptoms of meningitis preceding those of otitis; 3. Symptoms of otitis latent throughout meningitis. In most of these cases, we find that some form of mastoid disease has existed in the course of the complaint. Only occasionally do we get fatal meningitis due to ear-disease without any implication of the mastoid.

The following cases will illustrate the symptoms of mastoid disease as a complication.

* Since writing the above, I have removed the handle of the silver tube, so that the elastic catheter can be slipped over it after it has been passed into the bladder.

CASE 1.—T. L., aged 34, gardener, came under my care at the Manchester Institution for Ear-Diseases in August 1874, having suffered for the previous two years from repeated earache, with purulent discharge from the right ear. He could not account for this condition, except great exposure to cold and frequent use of a pin in that ear. He had gradually become worse; he suffered intense earache, deeply seated, and most marked at night. On examination, the hearing distance was $\frac{9}{8}$ for the right ear; the left ear normal. The tuning-fork placed on the vertex was heard almost entirely by the left ear; it was very slightly perceptible at the right external meatus, the walls of which were much swollen, red, and bathed in offensive-smelling pus. After syringing, a small granular polypus could be seen coming through the membrana tympani, of which only a narrow rim remained. The right Eustachian tube was impervious, and loud rushing tinnitus aurium was constantly present. He was ordered a lotion of sulpho-carbolate of zinc, to be instilled warm and retained for ten minutes three times a day; the meatus to be packed with powdered oxide of zinc every evening. The compound liniment of iodine was directed to be painted over the mastoid bone two or three times a week. Under this treatment, the pain was much relieved, and the discharge became less copious and offensive; but the hearing distance did not exceed $\frac{2}{3}$.

October 27th. The otorrhoea was very slight. He complained of great itching in the ear, and sometimes had a sudden severe attack of deeply seated earache, chiefly at night. The patient's general health had much improved under the use of cod-liver oil and iron; but he still looked very anxious, thin, and pallid.

November 17th. The swollen condition of the walls of the meatus having disappeared, and a more healthy state having been attained, I endeavoured to find if there were any fistulous openings into the mastoid bone, but failed, as on previous occasions, to detect caries with the probe. The polypus was reduced in size, and I intended to apply astringents at the next visit; but he ceased to attend, and did not return until

April 2nd, 1875. He stated that he had been very ill with inflammation of the lungs. He now had a more profuse otorrhoea than before. The hearing distance was $\frac{9}{8}$; the tuning-fork was not heard at the right external meatus, nor in the right ear, when placed on the vertex. He complained of intense pain in the ear and round the face and right side, worse at night, and accompanied by severe headache. He was very feverish and emaciated.

April 9th. He was much improved after taking quinine with morphia, and resuming the local treatment originally ordered, which had been omitted during his illness. The meatus was wiped out with solution of nitrate of silver (gr. 40 to 3j) every other day. The lotion now passed from the ear into the mouth very freely.

April 30th. Lotions of iodine with glycerine, and weak solutions of muriate of iron with opium, were used alternately.

May 7th. Frequent applications of tincture of perchloride of iron were made to the polypus through the speculum. He experienced some difficulty in closing the right eyelid, and in inflating the right cheek. The pupils were equal, but the right was less sensitive to light.

May 14th. A soft red swelling had arisen on the right mastoid process, containing pus, into which a deep incision was made, affording relief from pain, and removing the otorrhoea from the meatus. Poultices were ordered to the incision. A probe passed into the incision came upon soft carious bone; and, on exploring the walls of the meatus for the first time, some dead bone was felt at the superior and posterior walls.

June 18th. Frequent warm-water douches, leeches to the tragus, morphia pills, and the application of small pledgets of cotton soaked in acetate of lead and opium, afforded temporary relief to the pain and discharge.

June 29th. The meatus was found blocked up by a large piece of necrosed bone, which appeared to have descended from the posterior and superior wall of the meatus, and was firmly implanted. The polypus and membrana tympani could not be seen.

July 9th. There was no improvement. The patient was in extreme pain, and complained of numbness of the right half of the face.

The patient died of meningitis, with convulsions, on July 24th, as I was informed by letter from Mr. Renshaw of Altrincham, who attended him. Unfortunately, I did not hear of his death until some time had elapsed, and too late to have a *post mortem* examination.

In this case, there was unusual difficulty in applying efficient local applications, owing to the smallness of the polypus, and the perforation of the membrana tympani, and to the swollen state of the meatus. The situation of the caries and sinuses was made out with difficulty; and the size and fixed position of the necrosed bone did not warrant much attempt at removal in the patient's state of health at the