ever, this motive is always removed, for the tumour is always reduced in size and perfectly quiet as regards increase.

One case is on record in which a surgeon, usually of most excellent judgment, was persuaded by entreaties of friends to do an operation in a girl aged nine. It appears to have been a case exactly like those which I have mentioned, incontinence of urine and fæces being the only symptom. The cyst was protected by a thick covering of fat. This was carefully dissected aside and the cyst opened; and, being found apparently free of nerves, a part of it was excised. The girl died in a few days of meningitis of the cord. It may be added that treatment by puncture and compression had been repeatedly tried before, and without any advantage.

REMARKS

ON

THE PHYSICAL CAUSES OF THE SOUNDS OF THE HEART.

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

Physician Extraordinary to the Queen, etc.

IN the number of the BRITISH MEDICAL JOURNAL for May 12th is a communication from Dr. Little, charging me with some inconsistencies in my explanation of the sounds of the heart, and complaining that I have not given sufficient credit to Dr. Billing as "the discoverer" of the valvular origin of the sounds of the healthy heart.

I am very sorry if I have not done justice to my venerable friend in this matter, and I have waited till my return to London to enable me to refer to the several works on the subject.

The first published statement of Dr. Billing's views I find in the *Lancet* of May 1832, under the title, "On the Auscultation and Treatment of Affections of the Heart, read at the Hunterian Society in February 1832". In this, Dr. Billing says:—"The first sound is caused by the tension produced in the shutting up of the auriculo-ventricular valves, and the second sound is caused by the tension produced in the shutting of the ventricular-arterial valves." This early and clear statement of the valvular theory of the heart-sounds was not known to me when I wrote the summary of views on the subject, which appeared in the third edition of my little work on *Diseases of the Chest* in 1835 (quoted by Dr. Little), and I regret that, through this ignorance, I did not give Dr. Billing's name its proper place among the advocates of that view. But, in my fourth edition, published in 1840, I mention that "this view was proposed by Dr. Billing in February 1832; but, as far as regards the second sound, he was anticipated by Dr. Carswell, whose opinion was publicly announced in the Academy of Medicine at Paris in July 1831". My authority for this statement was the following note in the *Library of Medicine* by Dr. Joy, vol. iii, p. 262:—"The discovery of this theory of the second sound is claimed by Dr. Carswell, and was first publicly announced by M. d'Espine in a dissertation read by him at the Academy of Medicine at Paris in July 1831, though it was made known by Dr. Carswell as early as 1829. There is in the museum of University College a drawing (dated 1829) of the case of an aneurism of the aorta observed by Dr. Carswell in the hospital of La Charité, the physical signs of which case first suggested to him this now almost universally received theory."

In the thesis of Dr. Elliott (*De Corde*, Edinburgh, July 1831), to which my attention was called by my most revered teacher the late Professor Alison, is found the earliest announcement in print of this explanation of the second sound that I am acquainted with. Dr. Billing denies that Dr. Elliott ascribes the second sound to the arterial valves, because he considers the rush of blood from the auricles at the diastole of the ventricles also concerned in its production; but the words which I put in italics in Dr. Billing's quotation from Dr. Elliott's thesis seem to me plainly to point to the arterial valves as a chief cause of the second sound:—"Naturam soni secundi subitam et abruptam fieri credo a diastole ventriculorum tam repente et vehementer inchoatâ ut sanguis vi magnâ auricularum parietes transcurrat: necnon ab impedimento quod in corpore sano ferè instantè valvulâ præstant sigmoideæ sanguini qui in ventriculos, dum horum fit diastole, ex arteriis vult refluerè."

But, although I thus find reason to question Dr. Billing's priority in proposing the valvular theory of the second sound, I am ready to accord to him the full merit of originality in ascribing the first sound to the tension of the auriculo-ventricular valves, and to admire the zeal and ingenuity with which he has supported that view. But I think that

his zeal for his favourite opinion carries him too far when he denies that muscular contraction has any share in producing the sound, as in the following passage. "I maintain that the first sound as well as the second is entirely valvular, and deny that any part depends on muscular noise; for, when there is simple concentric hypertrophy (increase of muscle and muscular action), the cavity of the ventricles being diminished, there is diminution of sound, although there is more of the condition necessary to the supposed *bruit musculaire*." (*First Principles of Medicine*, 1868, p. 43.)

I have already, in a previous communication (BRITISH MEDICAL JOURNAL, February 3rd, 1877), as well as in former works, refuted this objection to the muscular origin of the first sound, by proving that the sound of muscular contraction is loud in proportion to the thinness and abrupt action of the muscle, and inversely as its thickness. The movement which causes sound is the sudden change from slack to tight, which is greater in thin muscles and in a dilated heart, and less in thick muscles and in an hypertrophied heart. In an appendix to the second edition of my *Rational Exposition of Physical Signs* (published in 1833), the various views of the causes of the sounds of the heart are discussed in order; and, in reference to the first sound being caused by the closing of the auricular valves, I remark: "The principal objection to this as the only cause of the first sound is, that it must be instantaneous and confined to the first part of the ventricular systole, whereas we know that the first sound is prolonged during the whole period of this action" (p. 199). The same consideration had several years before led me to suppose the muscular contraction itself to be the essential cause of the first sound; and further reflection and investigation led me to refer it to a general law, that sound is produced wherever a soft solid is suddenly and forcibly tightened. The forcible transition from slack to tight in muscular action fulfils the conditions of motion and resistance which constitute sound. After the analytic demonstrations which I considered to have been made by my experiments in 1835, I endeavoured to remove the mystery of the *bruit musculaire* by referring both it and the first sound of the heart to the simple principle of sound caused by resisted motion in the tightening or tension of muscular action. If Dr. Little finds any variation of expression in my later writings from that used in 1828 and 1833, it is not from discrepancy, but from the fuller development of the subject, and from my having been able to trace the sounds of the heart to the general fact of movement from slack to tight, including not only the tightening of the muscular walls, which is the primary and most essential part, but also in the tightening of the valves, which is an accessory and auxiliary cause.*

In conclusion, I beg to remind Dr. Little that my first communication, entitled "A Few Personal Facts concerning Experiments on Animals, etc." (BRITISH MEDICAL JOURNAL, July 22nd, 1876), did not profess to claim for my experiments the *discovery of new views*; the experiments were performed to decide between the many views or conjectures previously advanced to explain the sounds of the heart, and the results proved so positive and so conclusive, that they left little doubt in the minds of those present; and, after having been repeated by subsequent experimenters, these results have been generally confirmed. Dr. Billing's view of the exclusively valvular origin of the heart-sounds was, doubtless, brought forward at a very early period and with great ability; but, although rendered probable by various clinical observations, it had not been brought to the test of experimental trial. As far as regards the second sound, my experiments proved the correctness of Dr. Billing's views; but they showed their insufficiency with regard to the first sound, inasmuch as this sound was distinctly heard when the action of the valves was stopped. This *positive* result is more conclusive than the negative results of Dr. Halford's experiments with the *bull-dog* forceps on a dog. It is asserted that by strong pressure with this instrument on the afferent veins the heart was deprived of its blood, and all sounds ceased, although the ventricles continued to act. As I have before stated (BRITISH MEDICAL JOURNAL, September 30th, 1876), I question the accuracy of this observation as to the complete extinction of the first sound; but I should expect it to be much weakened, not only from the feeble action of the heart deprived of its natural stimulus, but also from the absence of contents to spread the walls into a vibratile condition. An empty sac contract-

* Dr. Little criticises my use of the word "flapping", as applied to the tightening of the valves, because he thinks that the word implies something loose, like the sail of a ship flapping with the wind. I confess that I first used the term in an onomatopoeial sense, like the words clap, rap, slap, etc., as signifying a motion making an abrupt sharp noise; but I am ready to vindicate my use of the term even "at sea" with Dr. Little, by proving that a loose sail flaps noisily only as it is blown tight by the wind from or against the mast or rigging. The heart-valves do not "flap about" in the blood, as the loose sail does in the wind, irregularly; but they do flap, or pass from loose to tight with a noise, in a definite manner, in obedience to the motions of the heart and the pressure of the blood.

ing on itself cannot fulfil the transition from slack to vibrating tightness which takes place in a heart propelling a body of blood. The muscular sound, if heard at all, would be muffled and obscure, and to any but an expert observer would appear null, as compared to the clear rhythmical sounds which are heard when the blood restores to the walls and valves of the heart their natural freedom of action. "The argument that, because the heart without blood gives no sound, the blood or the valves must be the seat of the sound, is about as logical as that the bridge of a violin is the source of its notes because the strings yield no sound without the bridge. In either case, the bridge, the blood, and the valves respectively are necessary to complete the instrument which yields the sound, but the chief seat of the sound is in the vibrating strings in one case and in the vibrating walls in the other."

ABSTRACT OF CLINICAL LECTURE

ON

SOME CASES OF DISEASED KNEE-JOINT, WITH SPECIAL REFERENCE TO THE QUESTION OF EXCISION.

By S. MESSENGER BRADLEY, F.R.C.S.,

Surgeon to the Manchester Royal Infirmary; Lecturer on Practical Surgery at Owens College: etc.

THE cases before you are examples of some of the most common, but important, diseases of the knee-joint, viz.: simple synovitis, suppuration within the joint, abscess in the immediate neighbourhood of the articulation, ulceration of cartilage, articular osteitis, and pulpy degeneration of the synovial membrane and adjacent structures. That is to say, we have instances of disease affecting each, and in some cases all, the structures which enter into the formation of the joint; and, in forming an opinion upon any case of diseased knee, this question of site is a very important one. Indeed, *situation* and *diathesis* are the two most important matters to be decided; thus, *e.g.*, in this case of simple synovitis, the *situation* of the disease, revealed by the shape of the affected joint, at once establishes the fact that there is effusion *into* the joint; a little further examination shows that the bones, and cartilages, and ligaments are unaffected, and that the synovial membrane is the sole seat of disease. Then, regarding his general appearance of health, his strong even teeth, his healthy skin, we rapidly decide that here is a man free from any special *diathesis*; and, these two points decided, we at once conclude that this case of simple synovitis in a healthy subject will, with proper treatment, shortly be cured, and the joint be perfectly restored to its *status quo ante*. How different would the prognosis be if the same disease, caused in the same way, occurred in this strumous lad. It is highly important, indeed, that you understand that all, or almost all, the diseases here illustrated may occur in an otherwise healthy subject; or, on the other hand, may be associated with a rheumatic, gouty, syphilitic, or strumous habit—when it becomes a much more important matter to treat the general diathesis than the local manifestation thereof. In the rheumatic, or gouty, or syphilitic habit, after such general treatment, you may hopefully proceed to any operation upon the joint itself deemed necessary; but, in the strumous, you must proceed more cautiously, and in regard to one operation, I mean excision, you are, in my opinion, not justified in resorting to it at all.

Let us now examine these cases a little in detail.

This little humpbacked man is evidently of strumous habit, and has suffered at some time or other from caries of the dorsal spine. He was admitted into the infirmary with a large and painful right knee, the leg bent at right angles, but not ankylosed. There is manifest effusion into the joint, and some of the fluid removed with the aspirator we found to be purulent; yet, although we have here suppuration into this great joint, and the man is of so unfortunate a habit, there is wonderfully little constitutional mischief; the temperature is normal, the pulse is quiet, and the tongue is clean. What is to be done? Constitutional treatment, of course, "goes without saying"; but locally what must we do? I believe the best treatment here is to make an incision four lines in length along the inner border of the patella, and then, by means of a Higginson's syringe, to distend the joint with a solution of carbolic acid in water (1 to 30), according to the plan suggested by Callender for the treatment of abscesses. Having done this, seal up the small wound with collodion (no drain-tube being inserted), and put the limb on a back splint. I have found this plan succeed in such cases, and it is certainly always well worth trying; for the alternative of freely laying open the joint, with the hope of securing

ankylosis in the straight position, is not only a much more formidable procedure, but also not in the least more likely to succeed than the simple method of distension. If either or both these plans fail, and fail they often do, then, in my opinion—reasons for which I will give you more fully by-and-by—you have only one alternative, and that is amputation; excision being an unjustifiable operation in such cases.

The next case to which I draw your attention is one of abscess in the cellular tissue about the joint, and was sent into hospital as one of disease of the joint itself; but that such is not the case, I will proceed to show you. First, you perceive there is no *effusion* within the joint, by the absence of the characteristic swelling beneath the subcrureus or by the sides of the patella; next, the movements of the joint being quite normal and painless, you infer that the ligaments are unaffected; the bones do not in any way differ from those of the opposite side, nor is there any pain on squeezing the femoral condyles or tibial tuberosities, or on forcing the patella backwards, nor does the patient suffer nocturnal exacerbations, whence we conclude that the articular ends of the bones are sound; on pressing the articular surfaces together, no grating sensation is produced, by which we know the cartilages are intact. In a word, all the main joint-structures are healthy; and this fluctuating swelling is external to the articulation. Being quite out of the way of important vessels or nerves, without more ado I open it with this bistoury, and, as you see, give exit to a quantity of pus. I now distend the sac in the usual way, introduce a small vulcanite drain-tube, which I prefer to the ordinary India-rubber ones, because, unlike the latter, they do not collapse on pressure by the bandage; and now, I think, we shall be justified in predicting a rapid restoration to perfect health.

Here we have a typical example of ulceration of the encrusting cartilage. This man has no pain in his knee, no effusion into the joint; but, when you press the patella back and move it from side to side, you feel a distinct bony grating, owing to destruction of the layer of cartilage. Too much, I think, is made of diseased articular cartilage. Certainly, in regard to treatment, this is true. It may ossify, it may atrophy, it may disappear, and unless the contiguous bone become affected, very few, if any, symptoms appear, and very little damage is done. As matter of fact, however, this implication of the bones is the rule and not the exception; such, *e.g.*, was the case with Helen T., whose knee-joint I excised about eighteen months ago. For a long time, she had no symptoms beyond bony grating, then pain manifested itself, especially at night; and this proving quite intractable, in spite of active and long-continued treatment, the joint was resected, when we found much diseased bone beneath the eroded cartilage. I may for a moment dwell upon this case to say that, by some, it would be regarded as a successful example of excision of the knee, inasmuch as the bones united, the pains ceased, and the woman got about again; but, to my mind, it is not satisfactory; the limb is shortened, she is soon tired, and, after walking a short distance, has pain; she would be better off with a good stump and a wooden leg.

This little boy and girl are illustrations of another very common disease of the knee: I refer to articular osteitis. You can see at a glance that there is no effusion into the joint, and may also note that in both the leg is flexed at a right angle with the femur, and that subluxation backwards of the tibia exists, caused by the continued pull of the hamstring muscles. No sinuses have yet formed, and the enlargement of the bones, though manifest enough, is not extreme. Subjectively, we have pain on pressure and nocturnal exacerbation, with muscular spasm. Both little patients, too, have a pained and wearied look, very sad to see in young children, and both are losing flesh. The osteitis, in such cases as these, is generally set up by some unlucky blow or fall; and if this blow chance to alight upon a strumous soil, the osteitis will probably proceed to general arthritis; yet, if we see cases like this at an early date, we may hope for a happy issue out of the trouble by long-continued rest. Gentleness and quiet are, indeed, our most potent aids in treatment; and after a time, when all inflammatory action is at an end, we may endeavour to restore the limb to a straight line, and overcome the subluxation by very gradual extension. If, by long-continued flexion, the hamstring tendons hamper us in their contraction, there is no objection to dividing any such constricting band with a tenotome.

And now, gentlemen, in the last place, I call your attention to this strumous lad, who has been in the infirmary for the last month with white swelling, *tumor albus* of the knee. He tells me that, before he felt pain, he noticed a difference in the shape of his two knees; on the affected side, the furrows on each side of the patella filled up, and soon after he began to feel pain at times and limped in his gait. Seeing this, his mother, wiser than most, brought him to the hospital, where he was at once admitted. The joint is generally enlarged, and,