ABSTRACT OF A

Lecture

ON

THE OCCURRENCE OF THE MITRAL DIRECT OR PRESYSTOLIC MURMUR WITHOUT MITRAL LESIONS.

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THE case at the hospital which was recently brought before you, in which were four heart murmurs referable to the left side of the heart, and associated therewith paroxysms of angina pectoris, is doubtless fresh in your recollection. The patient, as you will remember, died suddenly in a paroxysm of angina, and the case furnished an occasion for considering that affection. I have now the heart before me. You see at once that it is considerably enlarged; the weight is 17½ oz. The enlargement is chiefly of the left ventricle. The ventricular cavity is much dilated, the dilatation evidently predominating over the hypertrophy. Directing attention to the aortic orifice, you perceive that the sigmoid walls are thickened and contracted. There had been, evidently, considerable regurgitation, and this stands in a special causative relation to the hypertrophy and dilatation of the left ventricle. The inner surface of the aorta above the aortic orifice is thickened and velvety. The openings of the coronary arteries are much contracted, and this, taken in connexion with the insufficiency of the aortic valves, must have had for a consequence a deficient supply of arterial blood to the muscular structure of the heart. In my remarks on angina pectoris I stated that, as it seems to me, the most rational view as regards the pathological condition with which paroxysms of angina are connected is to attribute them to ischæmia of the heart. That condition must have existed in this case, and the appearances therefore go to sustain the ischæmic theory.

Let us now direct our attention to the mitral orifice. Here there is not the slightest appearance of lesion. The mitral curtains are perfectly normal. Moreover, before the ventricular cavity was laid open, the water test showed competency of the mitral valves. The specimen has for me great interest in connexion with these facts. The patient, in this case, had undoubtedly both a mitral systolic and a mitral presystolic murmur. I found these murmurs present on repeated examinations, the presystolic murmur varying at different times in its loudness, but never absent. The presence of both murmurs was verified by many others. The case served to illustrate their differential characters to clinical classes. It so happened that Mr. Gould of London visited the ward on the day before the sudden death of the patient, and he listened to the heart as presenting a good example of the presystolic murmur. This murmur had, in this case well marked, its usual distinctive features—namely, a rough vibrating quality, resembling, in this respect, the sound produced by throwing the lips into vibration with the expired breath. From this peculiar quality I have been accustomed to distinguish the murmur as a blubbery sound. Now, the question arises, How could this murmur have been produced without any mitral lesion? I find no difficulty in answering this question, and I ask your attention now especially to the explanation of the fact that a mitral presystolic murmur may occur without any mitral lesion.

In 1862, in an article on "Cardiac Murmurs," published in the July number of the *American Journal of the Medical Sciences*, I reported cases illustrative of the fact just stated. I am not aware of any case having been reported prior to that date in illustration of this fact, nor subsequently by any other observer. The fact was stated in the first edition of my work on the "Practice of Medicine," published in 1866, as well as in the subsequent editions. I have met

with several additional instances within the last twenty years.

As preparatory for the explanation, the mode of production of the murmur, when it represents mitral lesions, is to be borne in mind. Referring to the characteristic blubbery murmur, I believe it to be produced by vibration of the mitral curtains. The mechanism is the same as in the imitation of the murmur by the vibration of the lips with the current of air in expiration. In thus imitating the murmur, note with how feeble a current of air a loud blubbery sound is produced by the vibration of the lips. This at once disposes of the statement which has been made that the current of blood produced by the auricular contraction is too feeble to give rise to so loud a murmur as is the mitral presystolic in many cases. Accepting the mode of production of the murmur as just stated, it follows that when the murmur is organic the mitral curtains must be flexible, that is, capable of vibrating. Whenever the murmur is due to lesions, the flexibility of the curtains is to be inferred, and generally it is a fair inference that there is contraction of the mitral orifice in consequence of adhesions at their sides, being what is known as the button-hole aperture. If the curtains be rigid and notably contracted, the characteristic murmur is not present; either there is no appreciable murmur, or it is a blowing murmur. It may be wanting, and hence absence of the mitral presystolic murmur is not positive proof that mitral obstructive lesions do not exist.

Now for the explanation of the mitral presystolic murmur without mitral lesions. In all the instances which have come under my observation there have been aortic lesions permitting regurgitation, as in the heart which is now before me. With regard to this specimen, consider the physical conditions in life at the instant when the auricular contractions took place. The left ventricle was filled with blood from the current passing from the auricle to the ventricle through an unobstructed orifice by gravitation, and, in addition, the regurgitant current from the aorta. As a consequence, the mitral curtains were floated away from the ventricular walls, and were not only approximated but in absolute contact. Recollect the physiological experiment by which it is shown that the mitral valves may be completely closed by injecting a liquid into the left ventricle through the mitral orifice. These conditions existing, the auricle contracts and forces an additional quantity of blood into the ventricle. This mitral direct current passes between the valvular curtains which are in apposition, and throws them into vibration precisely as the lips are made to vibrate with the breath. If I have made myself clearly understood, you will admit, as it seems to me, that the explanation is rational and adequate. Looking no further than the mitral orifice, the murmur is inorganic or functional, but, occurring without mitral lesions, it is an effect of aortic lesions allowing free regurgitation. I have as yet met no instance in which aortic regurgitant lesions did not coexist. Under these circumstances the presystolic murmur is apt to be variable in its intensity at different times, and it may not be always present—that is, it is liable to come and go according to variations in the physical conditions involved in the mode of its production.

To this statement concerning the variableness and intermittency of the mitral presystolic murmur without mitral lesions, I will add a fact, which I do not recollect to have seen stated by any author except myself, relating to the murmur when it represents mitral obstructive lesions. The fact is this, the murmur, in the progress of the cardiac affection, may become variable and intermittent. The fullness and cessation of the murmur, under these circumstances, are not of good omen, but quite the reverse. They denote feebleness of the auricular contractions from dilatation. The coexisting signs and symptoms will be found to sustain this view. Do not, therefore, fall into the error of giving to a patient, or to friends, false encouragement when, connected with mitral lesions, a murmur which has been loud and constant becomes feeble or ceases to be heard.

In the case which furnished the specimen before us there was also a mitral systolic murmur, and, as has been stated, the water test did not give evidence of insufficiency of the mitral valves. How is this fact to be explained? I do not propose to discuss the various explanations of the production of a mitral systolic murmur without mitral lesions. Suffice it to say that in this case, taking into view the considerable dilatation of the left ventricle, it seems to me probable that, although the ventricle held water when filled from an open-

ing made through the apex, the ventricular contraction in life occasioned some regurgitation. A regurgitant stream, be it never so small, may give rise to a murmur. I submit this explanation without undertaking at this time to account for the apparent incongruity that the mitral valves may admit of some regurgitation during life, and yet he found competent after death.

Clinical Lecture

ON

THE MECHANICAL TREATMENT OF CARIES OF THE LUMBAR VERTEBRÆ.

Delivered on December 7th, 1882,

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GENTLEMEN,—Let us examine the patient before us, with the hope of eliciting facts that will be of service in determining upon a plan of treatment. To save time I will tell you that this little four-year-old girl is suffering from chronic spondylitis of the first, second, and third lumbar vertebræ. The disease has existed for more than a year. It is slowly but surely affecting a solution of anterior vertebral continuity, while the posterior part of the column has remained intact. The arches and spinous processes of the involved vertebræ retaining their integrity, there is a sinking of the bodies of the healthy segments of the spine above, and a forward curvature of that part of the column. As a result we have a corresponding posterior projection, which in this case, though well marked, has not proceeded so far as to render its ultimate obliteration hopeless. If we closely watch our little patient as she moves about from place to place, picking up pennies from the floor, you can observe two things. First, all her movements are conducted with unusual care for a child, and with great regard to the avoidance of any sudden motion or jarring of her body; secondly, owing to a spasm of the muscles of the back, there is no lateral or antero-posterior flexion and extension in the lower dorsal or lumbar regions. With her spine held rigid she squats upon the floor instead of bending over in the usual manner. Upon placing her in the ventral decubitus, and attempting backward extension of the dorso-lumbar vertebræ, we are at once met with opposition on the part of the tetanised muscles. Furthermore, the patient shows evident signs of discomfort. There is a reflex muscular spasm which prevents movement of the vertebræ. This condition of reflex spasm, securing articular immobility, can also be observed, as I have told you, in cases of otitis of the hip, knee, or ankle. It is said to be Nature's method of placing the joint at rest and securing freedom from irritation to the diseased structures, which would otherwise result from articular motion. Upon this observation alone has been built up the prevailing system of mechanical therapeutics as applied to the treatment of chronic inflammations of the joints. To carry out this idea, "fixation" splints and "absolute immobility" splints and dressings have been devised for all the principal joints of the body, and are to-day in use throughout the civilised world.

There still remains another principle which is largely used in mechanical treatment of articular inflammations, which we have not yet elicited from the examination of our patient. If, with the diseased segments intervening, we make traction upon the spine in opposite directions (extension and counter-extension), and continue it for a sufficient time, we will find that the tetanised muscles will gradually soften and the patient will experience a greater degree of comfort and less apprehension. This observation has in recent times added another idea to the mechanical treatment of articular diseases—viz., that of extension for the relief of reflex mus-

cular spasm. There are, therefore, two principal ideas involved in the prevailing methods of mechanical treatment: "traction" (extension) and "fixation," and these two ideas have been based upon the observations we have just made.

The point which I wish to make now is that these observations, though correct so far as they go, do not place us in possession of the whole truth. In carrying out this idea of extension, surgeons have generally failed to recognise the fact that the "extension" enforced by means of their mechanical appliances was not the same kind of extension which they had previously made with their own hands, and ascertained to be so acceptable to the patient. Manual extension is elastic; the prevailing mechanical extension is rigid. Let us examine our little patient still further, and see if we can elicit other facts which will point to wider truths and serve as guides to more rational treatment. As she stands in front of me I place my hand upon her thorax, just under the armpits, and lift gently upwards. Holding her with a firm and steady hand in this position, let us observe the result. At once she says "that feels good." In support of the truth of her statement let me incidentally tell you what happened when I first suspended her some weeks ago for the purpose of taking a plaster mould of her body. So great was the relief afforded that within two or three minutes after she was placed in the apparatus and the superimposed weight removed from the diseased vertebræ she fell asleep, and though we continued our work, she remained in this condition until she was taken out of the swing.

Continuing to hold my little patient in this position, the reflex muscular spasm which has existed all along is gradually and surely overcome, and now my assistant, laying hold of her pelvis, makes forward, backward, and lateral movements of the spine in the affected regions, and, as you see, apparently without the slightest inconvenience or discomfort to the patient. Thus we have demonstrated that "fixation" of the diseased vertebræ is not essential to the comfort of the patient, and that manual "extension" (which is necessarily elastic extension), combined with thorough support, are really the important factors in overcoming reflex muscular spasms, abolishing the feeling of apprehension and affording entire comfort to the patient.

Now, let us summarise the results of our observation. By efficient manual extension and support, relieving the involved segments of the spine from superimposed weight, we have, without "fixing" or "immobilising" the carious vertebræ, secured entire comfort to the patient. The traction used to overcome reflex muscular spasm has been made with our hands, and hence has necessarily been elastic; there has been no rigid extension or fixation, only efficient support and elastic extension; neither have we interfered in any way with the proper performance of function by the healthy part of the spine. Could this condition of perfect comfort, so easily brought about by skilful manipulation of the inflamed vertebræ, be prolonged indefinitely, all must agree that it would be of the greatest advantage in promoting speedy recovery. We would thus be enabled at once to so circumstanced the diseased spine that its motions would not be materially interfered with, and hence an important aid to the maintenance of a normal circulation would be retained throughout the period of treatment. The point which I here wish to emphasise is that articular motion throughout the body, both in the lower animals and in man, like the analogous movements in plants, is of much greater importance than has hitherto been supposed. It aids in the maintenance of a normal circulation, and hence in supplying the requisite nutrition and sustaining a favourable temperature for the growth and development of the individual, the renewal of tissues subject to successional decay, and the carrying on of reparative processes in case of disease. Furthermore, I shall demonstrate to you, that articular motion in disease, in conjunction with muscular action, can, by the aid of properly constructed mechanical appliances, be made to subserve the same general and local purposes which it does in health. One of the main features of this disease, as I have already pointed out, is the lowering of general and local nutrition. Hence, if we can by any possible means secure all the benefits which the united testimony of the profession have ascribed to "rigid extension" and "fixation," without "fixation" and without "rigidity," we will have at once more nearly approximated the condition of health, instead of having imitated the conditions we find in disease. We will have thus gained a great practical point, for "fixation" of a joint necessarily has a depreciating influence upon the