

1889).—1st Herefordshire: William Broome (Giles, Gent., to be Acting Surgeon (dated Oct. 19th, 1889).—2nd (South) Middlesex: Surgeon T. Trent to be Surgeon-Major, ranking as Major (dated Oct. 19th, 1889).—1st Volunteer Battalion, the Highland Light Infantry: Surgeon-Major, ranking as Major, T. Johnston resigns his commission; also is permitted to retain his rank, and to continue to wear the uniform of the Battalion on his retirement (dated Oct. 19th, 1889).

Correspondence.

"Audi alteram partem."

THE PRESYSTOLIC MURMUR.

To the Editors of THE LANCET.

SIRS,—In your issue of Oct. 19th Dr. Dickinson has restated the grounds on which he believes that the murmur characteristic of mitral stenosis is due to regurgitation, and therefore occurs during ventricular systole; but he has not attempted to answer the main objections which have been brought against this view, and has made admissions which much weaken the strength of his position. I have so recently stated at length some of the grounds which seem to me sufficient for rejecting Dr. Dickinson's view that I will not repeat them here. I would, however, venture to suggest one or two points in which, as it appears to me, Dr. Dickinson's arguments are inconclusive, or at variance with facts well known and accepted by physiologists and those skilled in experimental research.

Dr. Dickinson states that the "knock" or snap heard at the end of the murmur of mitral stenosis is *at the end of systole*. (The italics are Dr. Dickinson's.) In his former paper¹ he says the "valve allows of regurgitation during the first part of systole, while it is able to close as the systole proceeds, so as to stop regurgitation towards the end of that movement." It is not easy to understand the various periods of the heart's cycle even if, as here stated, the snap comes *during* systole; but if it occurs at the end of systole, it becomes wholly incomprehensible.

a. In this case it would follow that any murmur subsequent to the snap must occur after the end of systole. In mitral stenosis a soft blowing murmur is often heard following the snap, which is generally admitted to be a systolic or regurgitant following a presystolic or direct murmur. But if, as Dr. Dickinson says, the snap is the *end of systole*, this soft murmur cannot be systolic, and there must be some other interpretation of it than that which is usually given. There is, however, no reason to suppose and no evidence adduced by him to show that this is other than, as generally accepted, a mitral regurgitant murmur heard after the *commencement* and not at the *end of systole*. If the first part of the murmur preceding the snap be really regurgitant, and the second soft blowing murmur which follows it be also regurgitant, then there are two murmurs generated by a flow of blood in the same direction and at the same orifice, differing entirely in character and often separated by an interval, a possibility which cannot be entertained. It is to be regretted, for the sake of clearness, that Dr. Dickinson does not quote passages in support of his statement of the views of his opponents, but only says that "those who believe in the presystolic or direct murmur believe that the blood may change its direction at a valve, stop, and go back, and the murmur not lose its continuity or change its tone." The only passage I can find in which any such statement is made is by Dr. T. C. Turner, who writes, *not in support of the presystolic murmur, but in support of Dr. Barclay's—i.e., of Dr. Dickinson's—views; it is as follows: "If it were true that continuity of the cardiac murmur is inconsistent with a reverse of the direction of the blood stream producing it, this would be fatal to Dr. Barclay's view, which implies a reversal of the blood passing through the mitral orifice between the diastolic part and short 'presystolic' termination of the prolonged bruit, often occupying the whole interval between the second sound and first sound of the heart."*²

b. Further, it would follow, if the snap occurs at the end of systole, that the carotid impulse which most observers admit follows the snap would not be felt until systole is completed; but experimental physiology has demonstrated that the carotid impulse occurs about one-tenth of a

second after the commencement of systole, and immediately after the commencement of the expulsion of the blood from the ventricle.

c. It would also follow that although during the ventricular systole blood has been passing backwards through the mitral orifice into the pulmonary circulation, where the tension is admittedly high, it has not during the same period been forced into the systemic circulation, in which the tension is admittedly low. This is quite contrary to what occurs in ordinary cases of mitral regurgitation, in which the carotid pulse follows the first sound immediately, and is felt not *after* the murmur is over, but *during its continuance*. There is no adequate reason given why in cases where there is regurgitation through a narrowed mitral orifice the carotid pulse should be delayed until after the snap, if, as Dr. Dickinson states, the snap be the end of systole. Again, supposing it to be true that the "knock" occurs at the end of systole, the sound due to closure of the tricuspid valves should precede the "knock" by the whole length of systole, for the tricuspid valves, owing to the increased pressure in the pulmonary vessels, close sharply at the *commencement* of systole. Here also there is no evidence brought forward that such is the case; and it does not seem that any constant want of synchronism between the first sound of the right and left sides of the heart has been noted in mitral stenosis. Again, in cases where there is regurgitation through the tricuspid orifice the murmur is admitted to be systolic in rhythm. If it be so, it should, supposing Dr. Dickinson's hypothesis to be correct, occur at the same time as the presystolic murmur. This, as far as I am aware, is not supported by any recorded case. On the contrary, the tricuspid systolic murmur distinctly comes *after* the snap which terminates the presystolic murmur. And again, if the murmur which Dr. Dickinson calls diastolic is really the direct murmur, and if, as he maintains, the first sound of the heart is only valvular, the sharp closure of the tricuspid valves ought to be heard immediately following it, since he maintains that between it and the subsequent rough rumbling murmur is the commencement of systole. This is not borne out by clinical observation.

In order to account for the sharpness of the sound which ends the murmur under discussion, Dr. Dickinson, without giving any grounds for so doing, discards the explanation of the first sound of the heart which is generally accepted. He assumes that it is due only to the closure of valves, and that muscular contraction has nothing to do with it. If this be so, he should explain why it is that the sound of a hypertrophied heart, in which the cardiographic tracing shows the contraction to be long and sustained, is long and booming; and, on the contrary, why in mitral stenosis, where the sound is short and sharp, the cardiograph shows the contraction to have been more rapid and less sustained; and further, he will have to explain why, when the ventricle has a diminished volume of blood to propel, its systole, contrary to all that can be shown by the cardiograph, is prolonged by the whole length of the presystolic murmur.

Dr. Dickinson is evidently not prepared to accept the view I have lately put forward, and, as I hope, not without adequate reason, that the whole character of the presystolic murmur depends on the flow of blood under high tension through a narrowed orifice into a cavity in which the tension is abnormally low, by which means waves of large amplitude are generated and a thrill produced. He, however, not only gives no alternative, but declines to face the problem, saying, "if the thrill accords, as according to my observation it does, with the heaving of the apex, it is systolic and not made as suggested. How it is made I have not to declare. The circumstances of blood thrills may be various." I can hardly think that such an uncertain statement is calculated, as Dr. Dickinson predicts, to relegate the presystolic murmur to a place "among the errors of the past," and to the rising generation to whom he appeals it may safely be left to judge which view is the more in accordance with facts. I am, Sirs, yours faithfully,

Brook-street, Oct. 22nd, 1889. THEODORE DYKE ACLAND.

THE VALUE OF HYOSCINE AS A SEDATIVE.

To the Editors of THE LANCET.

SIRS,—I note with pleasure an article which appears in the issue of THE LANCET of the 12th inst., on the value of hyoscine as a sedative, and hasten to add my testimony in favour of a drug which I fear is as yet not fully appreciated.

¹ THE LANCET, vol. ii., 1887.

² Ibid., Aug. 18th, 1889, p. 317.