VITAL STATISTICS.

HEALTH OF ENGLISH TOWNS.

In twenty-eight of the largest English towns 5713 births and 3800 deaths were registered during the week ending November 5th. The annual rate of mortality in these towns, which had steadily increased in the preceding six weeks from 16.2 to 21.5, was last week 21.4. During the first five weeks of the current quarter the death-rate in these towns averaged 19.6 per 1000, and was 0.9 below the mean rate in the corresponding periods of the ten years 1877-86. The lowest rates in these towns last week were 12.7 in Hull, 13.2 in Brighton, 14.9 in Leicester, and 16.2 in Huddersfield. The rates in the other towns ranged upwards to 27.6 in Oldham, 29.1 in Preston, 30.7 in Bolton, and 31.0 in Halifax. The deaths referred to the principal zymotic diseases in the twenty-eight towns, which had been 392 and 464 in the preceding two weeks, further rose last week to 466; they included 144 from scarlet fever, 77 from diarrhoea, 75 from measles, 60 from whooping-cough, 57 from "fever" (principally enteric), 32 from diphtheria, and 21 from small-pox. These zymotic diseases caused the lowest death-rates last week in Leicester, Hull, and Huddersfield; and the highest rates in Bolton, Preston, Oldham, and Sheffield. The greatest mortality from scarlet fever occurred in Norwich, Blackburn, Oldham, Birkenhead, Salford, and Preston; from measles in Birmingham, Blackburn, Bradford, Bolton, and Derby; and from "fever" in Derby and Sunderland. The 32 deaths from diphtheria in the twenty-eight towns included 19 in London and 3 in Manchester. Small-pox caused 21 deaths in Sheffield, but not one in London or any of the twenty-six other large provincial towns; the 13 cases of small-pox under treatment on Saturday last in the metropolitan hospitals receiving cases of this disease showed a further slight increase upon recent weekly numbers. The number of cases of scarlet fever in the Metropolitan Asylums Board hospitals and in the London Fever Hospital had increased to 2692 at the end of the week. The deaths referred to diseases of the respiratory organs in London, which had increased in the preceding four weeks from 206 to 485, further rose last week to 506, and exceeded the corrected average by 115. The causes of 74, or 2.0 per cent., of the 3800 deaths in the twenty-eight towns last week were not certified, either by a registered medical practitioner or by a coroner. All the causes of death were duly certified in Leeds, Bristol, Sunderland, Preston, and in three other smaller towns. The largest proportions of uncertified deaths were recorded in Oldham, Leicester, Huddersfield, and Halifax.

HEALTH OF SCOTCH TOWNS.

The annual rate of mortality in the eight Scotch towns, which had been 19.1 and 22.2 per 1000 in the preceding two weeks, declined again to 20.6 in the week ending Nov. 5th; this rate was 0.8 below the mean rate during the same week in the twenty-eight large English towns. The rates in the Scotch towns last week ranged from 15.1 and 16.1 in Leith and Edinburgh, to 24.0 in Dundee, 25.9 in Perth, and 29.1 in Paisley. The 515 deaths in the eight towns last week showed a decline of 39 from the number returned in the preceding week, and included 26 which were referred to whooping-cough, 14 to scarlet fever, 8 to diarrhoea, 6 to "fever" (typhus, enteric, or simple), 6 to diphtheria, 6 to measles, and not one to small-pox; in all, 66 deaths resulted from these principal zymotic diseases, against 68 and 70 in the preceding two weeks. These 66 deaths were equal to an annual rate of 2.6 per 1000, which corresponded with the mean rate last week from the same diseases in the twenty-eight English towns. The fatal cases of whooping-cough, which had been 30, 16, and 26 in the preceding three weeks, were again 26 last week, of which 10 occurred in Glasgow, 4 in Greenock, 3 in Edinburgh, and 3 in Dundee. The deaths from scarlet fever further declined last week to 14, from 23, 22, and 19 in the preceding three weeks; 9 occurred in Glasgow, 4 in Dundee, and 2 in Edinburgh. The 8 deaths attributed to diarrhoea showed a further decline from recent weekly numbers, and included 5 in Glasgow and 2 in Edinburgh. The 6 deaths referred to "fever" corresponded with the number in the previous week; 3 were returned in Paisley and 2 in Edinburgh. Four of the 6 deaths from diphtheria were returned in Glasgow; and of the 6 fatal cases of measles, 2 occurred in Glasgow, 2 in Dundee, and 2 in Perth. The deaths attributed to acute

diseases of the respiratory organs in the eight towns, which had increased in the preceding four weeks from 87 to 124, further rose last week to 131, and exceeded by 13 the number returned in the corresponding week of last year. The causes of 57, or more than 11 per cent., of the deaths registered in the eight towns during the week were not certified.

HEALTH OF DUBLIN.

The rate of mortality in Dublin, which had been 24.1 and 32.1 per 1000 in the preceding two weeks, was 31.0 in the week ending November 5th. During the first five weeks of the current quarter the death-rate in the city averaged 27.7 per 1000, the mean rate during the same period being but 19.0 in London and 18.5 in Edinburgh. The 210 deaths in Dublin last week showed a decline of 7 from the number in the previous week; they included 19 which were referred to scarlet fever, 8 to "fever" (typhus, enteric, or simple), 6 to measles, 5 to diarrhoea, 2 to whooping-cough, 1 to diphtheria, and not one to small-pox. Thus, the deaths resulting from these principal zymotic diseases, which had been 29 and 42 in the preceding two weeks, were 41 last week; they were equal to an annual rate of 6.1 per 1000, the rate from the same diseases being but 2.6 in London and 1.6 in Edinburgh. The fatal cases of scarlet fever, which had been 8 and 11 in the previous two weeks, further rose last week to 19; and the deaths referred to "fever" also showed an increase upon recent weekly numbers. The deaths from diarrhoea, measles, and whooping-cough showed, however, a decline. The deaths both of infants and of elderly persons exceeded the numbers returned in the previous week. Four inquest cases and 3 deaths from violence were registered; and 59, or considerably more than one-fourth, of the deaths occurred in public institutions. The causes of 32, or more than 15 per cent., of the deaths registered during the week were not certified.

Correspondence.

"Audi alteram partem."

"THE PRESYSTOLIC MURMUR, FALSELY SO CALLED."

To the Editors of THE LANCET.

SIRS,—I wish it were possible for me to notice in detail all the valuable communications which my humble paper has called forth, but I must confine myself to some, hoping so to select as to bring into consideration all the more important points.

Dr. Gairdner refers to a feature of the "auricular-systolic" murmur as having been overlooked by myself and others. The murmur, in his view, "may appear to be prolonged out of, or even quite through, the period of rest, being in this last case necessarily associated with a degree of the ventricular-diastolic murmur presently to be described. Its essential character, however, is preserved in every case, as above defined, and as represented in the diagram." In this diagram (in "Clinical Medicine") the sounds are indicated by vertical lines, the murmur by shading which reaches from the second to the first without a break, though not evenly, for it is lower in the middle than at either end, mounting steeply towards the first sound. Thus we have a murmur which begins while the ventricle is in the undoubted act of opening, which can be made only by a current which sets inward, because there is none in the other direction, and yet this is continuous with the murmur the auricular source of which is disputed. Dr. Gairdner does not see how this observation can be reconciled with the view I have presented. Neither do I. If the so-called presystolic murmur is continuous without break, pause, or change of tone, is all of a piece, with that which begins immediately after the second sound, then the two are one, both are auricular-systolic, and my contention falls to the ground. But I have never recognised such a murmur. I have heard not infrequently a soft apex murmur in the diastole close upon the second sound, the old mitral diastolic, and I have known this to be followed by the characteristic note of the so-called presystolic, but never without a break. Dr. Gairdner suggests to me, as the only course open, to explain the

supposed single murmur as two; this suggestion I adopt, not with any intention of warping facts to suit a controversial purpose, but as representing the evidence of my own ears. I may be wrong, and am the more likely to be so since I am opposed to an auscultator of Dr. Gairdner's proved skill and experience; but my conviction is as I have stated it. I have heard, not often, for such cases are rare, but as I have thought clearly, the old mitral diastolic followed after an interval by the so-called presystolic; but the murmurs to my ear have always been separate in time and different in character. Dr. Gairdner, indeed, "is not indisposed to admit with Dr. Balfour that the diastolic element of this murmur is softer, as a rule, than the presystolic; and that it may also occur in some cases where the latter is indistinct or perhaps even absent."¹ Dr. Gairdner admits the existence of these cases "with some difficulty," for to admit them is to admit the old mitral diastolic murmur and mark a distinction between it and the so-called presystolic. The difference between Dr. Gairdner and myself on this point is but of small, and I hope lessening, dimensions; it is nothing more than the halt of a stammerer, or a hyphen between two syllables; but it implies much.

To take another particular which relates to the continuity of murmurs. In presenting the general conception of the "presystolic" tune, and while disputing its meaning describing the sounds involved according to common acceptance, and as I thought correctly, I spoke of the so-called presystolic murmur as "running up to the systole, as being united to the systolic sound without separation, and as being *inseparably blended with the systolic murmur, should there be one.*" I was here referring, not especially to Dr. Gairdner, but to the presystolic creed as generally held. How common a clinical experience is it for a murmur which has been under discussion as to whether presystolic or systolic to be satisfactorily settled as a presystolic running into a systolic. How often it happens (and here I speak for myself) that a so-called presystolic and an undoubted systolic are associated, variously interchanging, sometimes more of the one, sometimes more of the other, but never separated by any definite demarcation. I need scarcely recur to my purpose in insisting on this fusion in repute and in fact. The argument is simple. I say it is impossible for an auricular systolic to mix with a ventricular systolic, for the blood is going different ways. The so-called presystolic thus mixes, therefore it is not auricular. Dr. Gairdner here protests against admitting this mixture of murmurs. But in the oft-quoted "Clinical Medicine" (p. 557), in speaking of the various combinations of murmurs, he says: "It is not unusual to have an auricular systolic and a ventricular systolic murmur combined as in the figure; *they may even appear to be so combined as to constitute but one murmur*; you will commonly, however, be able to detect the first sound in the middle of this murmur, splitting it, as it were, into two." (The italics are mine.) The diagram represents the supposed two murmurs by continuous shading, through the densest part of which a line passes to indicate the position of the first sound, but no interval is represented. Again, Dr. Gairdner says, in speaking of combinations of murmurs, "The greatest degree of difficulty" in distinguishing them "is when the normal sound is merged in the murmur, as it often is when an auricular-systolic and a ventricular-systolic are combined," and then Dr. Gairdner goes on to explain that the distinction rests on a difference of tone between the former and latter part of the combination, the former represented by repeated *r*'s, the latter by repeated *f*'s. Dr. Balfour also gives a figure² similarly constructed to Dr. Gairdner's, which is designed to show "a diastolic murmur separated from the succeeding presystolic murmur by a pause, while the *auricular-systolic murmur passes directly into a ventricular-systolic one.*" (The italics are mine.) The diagram shows a space between the shadows which represent the diastolic and the presystolic murmurs, while the presystolic shadow is continued into the following systolic, though a line through its densest part marks the position of the first sound. I quote these descriptions of Dr. Gairdner and Dr. Balfour in the belief that they are absolutely correct as representing what is heard, apart from any differences of opinion as to what the murmurs should be called, and how explained. What I doubt is not whether the murmurs so mix, but whether the two which so mix are made in opposite directions. But now Dr. Gairdner insists with more emphasis on the separa-

tion of these murmurs than on their union. This involves, as will be seen, an important admission. "I have been at the utmost pains to show," says Dr. Gairdner, "that these two murmurs are never thus inseparably blended, that they always preserve their exact separate rhythm, and are sharply demarcated from one another, either by the first sound remaining clear and definite (as it usually does) between the two, or by an abrupt change in the character of the murmur itself, marking, as it were, an interval between them (though the interval may be too brief to appear *soundless* to the listening ear)." Even now we have not got to an interval, but only to an interval "as it were." But at least there must be a sharp demarcation of some sort, in the absence of which I gather that Dr. Gairdner would not allow the murmurs to be of opposite kinds. I can only rejoice that so great an authority should concur with me on so important a point as the necessary separation of an auricular-systolic from a ventricular-systolic murmur. I shall henceforth hold with increased tenacity to the belief that a murmur which runs into, or coalesces with, an undoubted systolic is not presystolic, but only falsely so called. We have here a test which will, I think, be decisive against many murmurs which have hitherto passed as auricular.

I will take Dr. Balfour's letter next, for part of it is answered by what I have already said in reply to Dr. Gairdner. To take Dr. Balfour's points in their order: He thinks it "remarkable that the ventricle should be supposed capable of producing this loud rough murmur at the first beginning of its systole, when it is acknowledged that as this systole nears its acme and gains in force the murmur grows less and actually becomes soft and blowing." I thought the murmur, as a rule, began feebly and gathered strength as it went on—a mode of proceeding which might be expected, presuming it to be of ventricular origin. Again, in referring to the time of the mitral diastolic murmur, I had spoken of a period after the conclusion of the systole during which the ventricle "waits empty and still." This is not important for the purpose of the argument, but Dr. Balfour spies an error and quotes Hope to the effect that "the expansion of the ventricle immediately succeeds its active contraction," and, adds Dr. Balfour, "I know of no modern observations tending in the least degree to invalidate this statement." Let me refer Dr. Balfour to the well-known text-book of physiology by Dr. Michael Foster, fourth edition, pages 149 and 154, where he will find evidence that "the ventricle continues contracted after its contents have entirely left the cavity," and that the time for which it does so has been the subject of measurement. And now Dr. Balfour, by way of giving me the *coup de grace*, being already sore wounded, takes up anew a point which has already been dealt with by Dr. Gairdner. Says Dr. Balfour: "Dr. Dickinson objects to the term *continuous* being applied to the double murmur audible at the apex in many cases of mitral stenosis, and composed of the rough auricular-systolic murmur, immediately followed by the blowing ventricular-systolic murmur." Here I am a little misinterpreted. I do not object to—in fact, I commend—the use of the term *continuous* as applied to the so-called presystolic and the undoubted mitral regurgitant murmurs. I think the term represents the fact. What I do object to is the inference that of the two murmurs thus continuous, one goes one way and one another. This argument appears to have told, if I may judge by the efforts made to escape from it. Dr. Balfour explains: "The auricular-systolic murmur runs up to, and is continuous with, the ventricular-systolic (regurgitant) murmur, in the same sense of the word that the aortic-systolic murmur may be said to run up to and be continuous with the aortic diastolic (regurgitant) murmur, and when the systolic portion of this latter murmur is rough the analogy is very complete." Does Dr. Balfour really mean this? Does he ask us to believe that the so-called presystolic murmur is no more united with the undoubted systolic than are the opposed aortic murmurs with each other? Has Dr. Balfour never heard the double aortic murmur likened to the noise of a saw, or known it to be mistaken for the double rub of pericarditis? Has he ever heard the apex combinations in question so likened, or has it ever seemed to him to resemble the noise of pericarditis? Is it not going rather too far to say, as in the passage I have quoted, that the analogy between the murmurs in question is, in certain circumstances, "very complete." This appears to present a change of front; I welcome it as indicating a weakness of position. At any

¹ See foot-note on page 782.² Diseases of the Heart, p. 119.

rate, on this point we are agreed—if two apex murmurs are virtually inseparable, we are no longer to call one auricular and the other ventricular. Dr. Balfour tells us, in conclusion, "that it will be seen from the foregoing that" my "quarrel is with physiology and with facts, and not merely with opinions." I really do not see it "from the foregoing," which consists chiefly of what I have referred to; but when Dr. Balfour says I quarrel with physiology and facts, does he not really mean "with physiology and facts as interpreted by Dr. Balfour"? If he means this, and he cannot well mean otherwise, I am with him.

Dr. George Johnson points to the difference in conduction between the murmur of mitral constriction and that of acknowledged regurgitation as evidence that the currents which make them are in different directions; one murmur, as is well known, is nearly limited to the apex, the other is heard at the back. The fact has often presented itself to my mind, with this as possibly its explanation. The regurgitation with the so-called presystolic is through a small hole, and necessarily of small volume; it is not likely to carry its murmur so far as the copious gush which goes through the uncontracted opening. Dr. Johnson has satisfied himself that the murmur of mitral constriction precedes the impulse of the contracting ventricle, and is therefore presystolic in rhythm. I can say nothing to this, excepting that my observations have led me to a different conclusion. It has been objected, says Dr. Johnson, that "the auricular contraction would be insufficient to cause a murmur in a constricted mitral orifice." I never said so; on the contrary, I gave examples of murmurs thus produced. But I endorsed Dr. Barclay's words, that this force was insufficient to produce "one of the loudest and roughest murmurs ever heard in disease." This opinion I adhere to. It is true, as Dr. Shearer and Dr. Johnson say, that a considerable murmur like the *bruit de diable* in the neck may be made with little force or rapidity of current, but neither is the murmur connected with anything which suggests force to the touch; it is *vox et præterea nihil*; but the "presystolic" is associated with powerful movement, as the impulse and thrill testify. Where this murmur has been intense, I have sometimes been struck after death with the small amount of hypertrophy of the auricle; not what one would have expected had the murmur been auricular. I am aware, as Dr. Johnson has pointed out, that the cardiograph has been supposed to indicate that the first movement of the ventricle is coincident with auricular contraction; but the auricular part of the movement is small, probably inappreciable except by the machine, and not likely in any circumstances to be magnified into the long and rising impulse which keeps time with the "presystolic" murmur.

I had hoped to have replied separately, as their value demanded, to all the communications which have presented views contrary to my own; I regret that this has become impracticable, but I think I have omitted no essential objection. Neither need I refer in detail to those who have concurred with me, but will be content with acknowledging in general terms the important support I have received.

To conclude, let me hold up to view a common so-called presystolic murmur which begins gradually and ends abruptly, like something which expends its force against an obstacle, a steam-engine which rolls up against buffers. Its rumbling tune is all of one sort, and I presume that no one will now maintain that the beginning and the end of it belong to contrary murmurs. It is therefore all ventricular or all auricular. If auricular, what has become of the systole of the ventricle? The action of the heart, like the Government of the Queen, must be carried on. On the auricular theory there is no time for the ventricular contraction unless this laborious piece of work is comprised within the brief knock or snap which immediately follows the movement the nature of which is in question. This is short allowance. This snap is immediately or almost immediately followed by the second sound, with which the ventricular systole obviously terminates. Assuming, on the other hand, that the tangible impulse and rolling murmur are ventricular, how well the theory holds together. The idea of ventricular contraction suits the snap which comes at its end, presuming this to be made by the shutting of the mitral valve, together with tension of the chordæ tendinæ. That the short sharp sound is of this nature is borne out, as I think I have shown, by the post-mortem appearances; it must necessarily come at the end of the ventricular systole, because the flaps are rigid and must come together only slowly and when the ventricular walls are approximated. I therefore end as I

began, with the belief that the so-called presystolic murmur is one of mitral regurgitation. Dr. Gairdner allows that there are mysteries about the "presystolic" murmur as he views it. From my point of view I see nothing mysterious about it.

If I have spoken emphatically, I trust I have said nothing to imply want of respect for the great authorities whose views I have presumed to criticise. If there is any truth in what I have urged, it must add value and permanence, because it supplies explanation, to the great clinical guide which we owe chiefly to Dr. Gairdner. The points of difference must be settled by further observation rather than by argument; this belief, at least, we may all share, and may agree to address a final appeal to the independence, experience, and wisdom of the future.

I am, Sirs, yours truly,

Chesterfield-street, W., Nov. 1887. W. HOWSHIP DICKINSON.

"IS CANCER CONTAGIOUS?"

To the Editors of THE LANCET.

SIRS,—This interesting question, which THE LANCET has put before the profession, will find many ready to accept the belief, upon mere analogy, that cancer is contagious. There are those who have been for years diligently searching for the germ which may have excited the cells of a part to such excessive abnormal action; and these observers have been at times ready to state that they had procured a distinctive organism. There is something so temptingly inviting in the idea that a cancer is a human oak-gall—that what the penetrating animal diplolepis excites in the oak may possibly have its prototype in the ravages of some minute vegetable organism in man. Moreover, the growth excited by the bacillus of tubercle gives further support to such credence. I do not profess to approach the subject from any but a clinical aspect, and I can see that a mass of evidence must be collected before any true decision can be arrived at upon such data. Experiment affords a much more promising field. It has been tried, but with little success, owing probably to difficulty of proving a suitable nidus, and perhaps, too, from the inoculations having been made with the cell results rather than with the cell causes. This, however, I would suggest to such operators: that, as cancer is a disease of age, the oldest animals procurable would probably be its most ready recipients. Since THE LANCET put the question on Oct. 8th there have been three cases published in which cancer of the womb in the wife was associated with epithelioma of the penis in the husband. It is an interesting coincidence, but we have no evidence that the cancers were of the same type. If it were shown, for instance, that in the woman the growth was a colloid cancer of the body of the uterus, would that entirely invalidate the idea of contagion when the husband's growth is known to be epitheliomatous? We now, in explaining the well-recognised heredity of tubercle, are fain to believe that in a particular type of the human species Koch's bacillus finds a peculiarly appropriate soil. Will the individual and the part attacked explain the form of cancer? Besides the three cases referred to, one of your correspondents has put forward a case of cancer of the womb in the wife associated with epithelioma of the tongue in the husband. If, however, it may come about that some one germ may produce different forms of cancer in different individuals, modified also by the part attacked, then cases occurring in persons living together will become of great interest and importance, apart from direct inoculation.

The following curious instance of three different forms of cancer, occurring in three individuals living together in the same house, may some day receive an explanation other than that of mere coincidence. In 1881, and again in 1883, I operated on a gentleman suffering from rodent ulcer of the eyelid and forehead. He is still living, but with a return of the growth. In 1884 I operated on his wife for scirrhus of the breast; and during the past week a partner, who has always lived in the same house with them, has come under my care with epithelioma of the tongue. I would not suggest that there is of necessity anything more than accidental association between these cases, but the coincidence is the more interesting in that there is no blood relationship between them. Before one has learnt to attribute the ravages of