between the commencement and the end of the murmur.
4. The roaring murmur is caused by the ventricular systole.
5. The termination thereof (or snap) represents the concussion of the mitral flaps and the arrest of regurgitation.
6. The thrill is produced by a rigid flap vibrating in a stream powerfully propelled.

Propositions 1, 2, and 3 may perhaps be granted. Propositions 4, 5, and 6 do not probably as yet meet the views of the profession; but, if I read Dr. Dickinson's paper aright, they are held by him as well as by myself.

I am, Sirs, yours obediently,
Seymour-street, W., Oct. 1887. SEYMOUR TAYLOR.

To the Editors of THE LANCET.

SIRS,—May I be allowed space in THE LANCET for a few sentences in defence of the auricular-contraction theory of the production of the presystolic murmur, which was assailed in last week's number by so great an authority as Dr. Dickinson, who contends that the murmur in question is really a regurgitant murmur of systolic rhythm?

With regard to actual points of observation, I am disposed to disagree with Dr. Dickinson only on one, albeit an important one. It is his statement that the presystolic murmur accompanies, and does not precede, the apex impulse. I have over and over again convinced myself, and others who were with me, that the presystolic murmur and its accompanying thrill do precede the impulse of the apex, and, moreover, close abruptly with it. Coming to matters of theory, a large class of cases of mitral stenosis which never present a presystolic murmur at all demonstrate the fact that the closure of the mitral valve takes place at the beginning of the systole, for in these cases both a systolic sound and a systolic murmur are present, but the sound invariably precedes the murmur. I quite agree with Dr. Dickinson that a mitral systolic murmur must be regurgitant. The preservation of the first sound in cases of mitral regurgitation, accompanied by a systolic murmur, is matter of common observation, not only in cases of regurgitation through a constricted orifice, but in cases where there is no disease of the valves, their incompetence depending on the muscle of the heart, as well as in cases of structural change in the valve curtains, while yet it is in an early stage, as witnessed in the course of acute rheumatism. Dr. Dickinson, in the following sentence seems to ignore altogether the large class of mitral regurgitant murmurs which are preceded by a first sound: "The ordinary or old-fashioned mitral regurgitant murmur simply takes the place of the whole first sound, and lasts to its end." In nearly all cases of mitral stenosis accompanied by presystolic murmur, a systolic murmur indicative of regurgitation through the deformed orifice may from time to time be heard; but this murmur invariably follows the first sound. Moreover, the *crescendo* character of the presystolic murmur and *diminuendo* character of the systolic murmur (usually soft and blowing in character) are well marked, and the two murmurs are separated by the first sound. The systolic is, no doubt, regurgitant, but it follows the first sound. The peculiar accentuated and shortened character of the first sound, I believe, depends on the small quantity of blood in the ventricle at the time of its contraction. The left ventricular walls are usually healthy, and contract with vigour, so to speak, on a light burden.

Dr. Dickinson refers to the difficulty met with in attempting to account for the absence of a pause between the presystolic murmur and an accompanying systolic murmur, but a first sound is always represented in these cases, and it is to it that the presystolic murmur "runs up." The sound implies only the closure of the valves, and the abrupt cessation of the current producing presystolic murmur is just the condition that explains the abrupt cessation of the murmur coincidently with the first sound.

Dr. Dickinson states that he has found that the presystolic murmur begins with systolic (i.e., ventricular?) movement, but such movement does not necessarily imply contraction. It seems very likely that there will be some movement communicated to the ventricles by the contraction of the auricles.

The difficulty of explaining the loudness and roughness of the presystolic murmur by a current of blood resulting from contraction of the thin-walled auricle is considered an objection to the auricular contraction theory, but a diastolic murmur due to mitral constriction sometimes possesses

almost as much loudness and roughness as the presystolic murmur, and is quite commonly accompanied by marked thrill, yet the forces producing the soniferous current can only be the blood-pressure in the lungs and the sucking action of the expanding ventricle. The theory that explains the loudness and roughness of the presystolic murmur must also explain its remarkable limitation to the apex and its extraordinary capriciousness, as shown in its coming and going, while the structural conditions remain unaltered.

I am, Sirs, your obedient servant,
Manchester, Oct. 4th, 1887. GRAHAM STEELL.

To the Editors of THE LANCET.

SIRS,—Dr. Dickinson, in his excellent remarks on this subject, speaks of the hardihood of daring to take into question one of the accepted canons of cardiac medicine—viz., that promulgated by Dr. Gairdner. It will, I hope, be some satisfaction to Dr. Dickinson to know that I, for one, have never accepted this unphysical and unphysiological doctrine, but, on the contrary, have always taken the opportunity of exposing its fallacy to the members of my class.

The arguments which I have used to disprove an auricular systolic sound are precisely those employed with so much force by Dr. Dickinson—viz., first, that the auricular contraction is insufficient to produce the necessary vibration of the diseased valve; and secondly, and very obviously, there is no break in the so-called presystolic and systolic sounds.

It does not surprise me to find that such fancies as the one Dr. Dickinson combats should occasionally take the place of plain facts, when we find that there are a number of intelligent men who are unable to appreciate the simplest and efficient cause of the first sound of the heart. There are other *impenetrabilia* in our constitution, I fear, besides "colour blindness."

I am, Sirs, yours truly,
Stratford-place, W., Sept. 30th, 1887. JOHN HARLEY.

GOUTY DIABETES.

To the Editors of THE LANCET.

SIRS,—Will you allow me to call attention to a class of cases which is not only a large one, but the cause of much anxiety and frequently of considerable disappointment to practitioners who have not had the opportunity of studying it on a sufficiently extensive scale and in detail. I allude to that variety of suppressed—or, more accurately, *undeveloped*—gout, in which the symptoms are chiefly and often characteristically "diabetic." Persons affected with this malady are generally of large frame, with little stability or power, full-sized but flabby muscles, and a tendency to the accumulation of fat. They are great—or intermittingly great—eaters, and energetic or impulsive, but weak and easily exhausted. The skin is pale, and they suffer from coldness of the extremities. They sleep badly at night, and worry in their wakefulness; while by day they are drowsy, and when falling asleep breathe heavily, or, if not roused, sink into a state verging on coma with puffing, sighing, or laboured respiration. They are commonly subject to attacks of vertigo, in which surrounding objects seem to race from left to right; and not seldom they have weak hearts with rhythmically irregular pulses, and perhaps a short mitral systolic murmur, which is not constant, but more likely to be heard when the heart is acting languidly than when it has been recently stimulated by exercise. The characteristic phenomena are: (1) very great variability in the quantity, colour, and density of the urine passed in twenty-four hours, and the want of relation between the bulk of the fluid consumed and that excreted; (2) sugar is present when the urine is small in quantity, rather than when it is abundant; (3) the acidity is abnormally low in proportion to the percentage of urea; (4) there is generally much uric acid (Haycraft's test) when the sugar is absent, and less than the average proportion of health when sugar is present; (5) albumen is often found soon after a meal, either with the sugar or during a free elimination of uric acid, and when there is no albumen the phosphates are generally in excess; (6) the liver is enlarged *vertically*, and the bowels are constipated, with occasional attacks of diarrhoea, the stools being pale when scanty, and bilious when profuse. Hæmorrhoids are usually present and troublesome in these cases, and when they do not bleed they

often attain great size. Not infrequently there is also a prostatic gleet.

I will not trouble you with any attempt to account for this curious and, as it may appear, contradictory combination of symptoms and indications, although I have my own theory, and may hereafter have something to say on the subject; nor will I venture to set out the *rationale* of the line of treatment which has proved serviceable. Suffice it to state the clinical facts as I have myself observed them, and to report the means by which the symptoms have been generally ameliorated—often, as it would seem, wholly removed. In the management of cases of this class many difficulties have to be encountered, and they are far from easy to overcome. Meanwhile, one thing is certain—namely, that dieted as it is the fashion to diet patients suffering either from gout or diabetes, and treated on conventional principles either at home or at the continental spas, the subjects of gouty diabetes do not do well. They may be for the moment relieved, but they quickly relapse and, on the whole, go from bad to worse steadily. It is with a view to plead for a more generous regimen and tonic treatment I am writing. Wisely, but liberally fed, with meat in such quantities as the stomach will take, ordinary bread and vegetables,—in short, a full mixed diet—dry champagne, which contains a minimum of alcohol and a large proportion of ether, and well-matured port wine, in appropriate and measured quantities (the effect being carefully watched), they do well. The albumen and sugar disappear; the quantity of urine excreted becomes normal and relatively regular. The pulse gains strength and rhythm, and the condition throughout improves, under some modification of the following treatment: Ferrilactatis, 10 gr.; acidi lactici, 15 m.; quiniæ puræ, 1 gr.; extracti convallariæ fluidi (*e floribus*), 10 m.; aquæ ad $\frac{1}{2}$ oz.; every sixth hour, or in bad cases every fourth, or even third, hour. Should there be constipation, I add five or ten minims of the fluid extract of cascara sagrada (U.S.); or diarrhoea, or excessive diaphoresis, ten minims of tincturæ cottonis. The results of this management, in a considerable number of cases which have recently fallen under my observation, have been so good that I think it right to place the facts on record without further remark.

I am, Sirs, yours faithfully,

Hanover-square, Oct. 1887.

J. MORTIMER GRANVILLE.

COMPULSORY NOTIFICATION OF INFECTIOUS DISEASE.

To the Editors of THE LANCET.

SIRS,—Dr. J. H. Walters undertakes, in your last issue, to prove that, as regards the town of Reading, compulsory notification has been shown by six years' experience to be valuable as against disease and also acceptable to the profession. I should like him to support this opinion by categorical answers to the following questions:—

1. In what respect and to what exact extent, as shown by the death-rate of, say, the last two years, has Reading benefited in health by the system? I do not want statistics of notifications, but of deaths.

2. Is the form of notification such as to permit the practitioner, as he may in Edinburgh or elsewhere, to exclude the health officers from the disinfection and sanitation of the infected house if it is his interest to do so?

3. What proportion of the deaths are recorded by the burial authorities as having been attended by chemists and other unqualified practitioners?

I put these questions because I have in vain asked for evidence, presented by death-rates, that notification towns have improved in health in any greater degree than non-notification towns similarly situated; because I have reason to know that in towns in which the medical attendant is allowed (by stating in the notification form that the interference of the health officers is not necessary) to keep those officers from interfering with his private cases, the notification is a complete sham, many of the cases notified and paid for not being infective at all, and many infective cases being allowed to fructify just as if their existence had never been notified.

Finally, I have reason to believe that, if inquiry were made as to the number of persons attended by quacks, chemists, and other unqualified persons in notification towns, it would be found that the dread of the interference of the health officers drives very many of the sick into the

nets of these persons. Can anyone produce actual figures to disprove these statements? If not, no one should trust in the unsubstantiated imaginings of Lord Basing or any other pro-compulsion partisan.—I am, Sirs, yours &c.,

Dublin, Oct. 8th, 1887. ARCHIBALD H. JACOB, M.D. Dub.

EMPHYEMA FOLLOWED BY MELANCHOLIA.

To the Editors of THE LANCET.

SIRS,—I have been greatly interested by the account of Dr. Squire's case at the North London Hospital for Consumption, recorded in THE LANCET of Sept. 17th, and I am tempted to give you the notes of a somewhat similar case of my own, the curious point being that the mental symptoms in my case occurred within a few days of Dr. Squire's patient becoming melancholic.

W. B—, aged sixty-five, was taken ill about Christmas, 1886. Pleurisy was diagnosed early, but was almost devoid of pain. The left side was involved. Hiccough became incessant about the beginning of February, when my partner (Mr. G. H. Jackson) was called in. On Feb. 14th the patient was aspirated and nearly six pints of pus were removed. On the 17th I saw him with Mr. Jackson. The temperature had only once reached 99.8°, and that was quite early in the case; it was then 98.4°; respiration 24; pulse varied, about 90. The heart was displaced a little to the right. The dulness extended downwards to a distance difficult to ascertain, but the thoracic cavity gave very little evidence of fluid or consolidation. He lay on the left side. On the 19th Mr. J. Hutchinson saw him and agreed in employing free drainage, but advised us not to excise any bone, as he considered the space between the ribs sufficient. On the 20th I incised the seventh space in the anterior axillary line and inserted a very large drainage tube. A lead probe passed nearly up to the clavicle and in all directions freely. About a quart of pus was removed. Temperature 98.8°; respiration 22; pulse 96. The operation was performed under chloroform, but only lasted a few minutes. Next day his pulse intermitted every third beat with perfect regularity. The apex beat was now one inch to the inner side of the nipple. On the 27th delusions were noticed, and his mental condition grew worse daily up to March 20th, when improvement began to be seen. During this time the wound had gone on perfectly well, and as there was very little discharge the tube was removed on March 8th, in the hope that if by chance the chest closed the general health would improve. On the 14th the temperature rose to 99°, and the pulse was 103 and rather weak; the wound had closed. On April 4th the old wound burst open and a large quantity of pus escaped, and for a week the amount of discharge was great. The refilling had been watched with anxiety, but the choice lay between leaving nature to find a vent, or risking a recurrence of the insanity, from which he had now so nearly recovered. About April 14th the discharge had abated materially, but I kept the tube in till May 7th, when, on its removal, the wound closed at once. His mind had now perfectly recovered, and on June 24th his only complaint was a dragging sensation on the left side. Practically nine weeks elapsed between the operation and complete closure.

I consider that the age should be noticed as an encouragement in similar cases. The melancholia assumed at one time a very dangerous type, but the watchfulness of his attendants avoided any disaster such as occurred in Dr. Squire's case, though a similar attempt was made. Did the melancholia result from weakness? Sir James Paget speaks of insanity after operations, and Dr. Savage has, I believe, recently made some remarks as to insanity in women after chloroform or other anæsthetics. I have attempted to glean information on this subject, but in vain, and I should very much like to hear from anyone who has met with a case of insanity after operation, as I propose to collect all the cases I can with a view to comparison. I wish to mention that agraphia was very much marked, and his penmanship improved *pari passu* with his general health. As a rule, the temperature was subnormal. He was not kept in bed a day longer than was absolutely essential, being out of bed thirteen days after the operation.

I happen to have seen Mr. B— to-day (Sept. 30th). He tells me he is remarkably well, and weighs only 21 lb. less than the greatest weight he ever remembered scaling.

I am, Sirs, yours faithfully,

Tottenham, Sept. 30th, 1887.

A. OGIER WARD, M.D.