

nurse attending was not revaccinated, although an assistant was operated on." The nurse in charge was the one just referred to as having passed through an attack of small-pox at Oldham, and was therefore well protected, the assistant nurse was successfully revaccinated by me before going on duty.

8. "That we have therefore ten persons who were in office at the time the report was made, *four of whom had never been vaccinated*, one in whom the protection had vanished. Of the remaining five, the most recent revaccination was performed two years ago." In the absence of any particulars, by the ten persons here referred to, I take it, are intended myself, the chief inspector, three sub-inspectors, the clerk, the matron, the two nurses who attended the case, and the porter; of these *all had been vaccinated, and all who came in contact with the patients were revaccinated.* I am, Sirs, yours faithfully,

HENRY TOMKINS,
Medical Officer of Health.

Leicester, Oct. 18th, 1887.

JAMBUL IN DIABETES; SACCHARINE IN DIABETES.

To the Editors of THE LANCET.

SIR,—Mr. Hurry Fenwick's letter induces me to give you the results of the trial I have made of jambul powder in five cases of diabetes since the date of Dr. Kingsbury's letter to one of your contemporaries, used in doses of five grains, generally four times, in two instances three times, daily. All the patients were in the General Hospital under my care. I may say at once that no case was cured, but that all left the hospital more or less relieved by treatment, that all were carefully dieted, and that, besides the routine of diet, they were allowed potash imperial, sweetened with glycerine, to drink, and were in several instances treated by vapour baths. The effects of jambul were tested by maintaining uniformity of conditions in all other respects during the time the drug was employed or its use omitted. There were eight distinct trials in the five cases, five being followed by an increase of sugar and three by a decrease. Its disuse was followed on four occasions by a decrease and twice by an increase.

H. G.—, aged thirty-five, was the only case in which the decrease was well marked. On admission he passed 250 oz. of urine containing 9200 gr. of sugar. After being placed on a diet of meat, greens, and gluten bread, and the urine observed for a week, he passed an average of 190 oz. daily, containing 4500 gr. of sugar. He then took 5 gr. of jambul three times a day for twenty-four days, during which time he passed on an average 140 oz. of urine containing 3500 gr. of sugar. The next week he took no medicine, and the urine fell to 104 oz., containing 2400 gr. of sugar. He was then given 1 gr. of opium twice daily for twelve days, the urine falling to 78 oz. and the sugar to 2000 gr. This patient is still under observation.

In this case there seemed to be an improvement taking place independently of the drug, and which in no way ceased when this was discontinued. The other two instances where its use was followed by a decrease were neutralised by the fact that subsequent trials in the same patients were followed by an increase. Thus, in W. S.—, aged twenty-one, the first trial was followed by a decrease, the second by an increase; and in F. M.—, aged thirty-one, the first and third trials were followed by an increase, the second by a decrease. The duration of the use of the drug varied from three to thirty-five days. The five trials that resulted in an increase were respectively three, five, thirty-five, twelve, and fifteen days, while the three resulting in a decrease were three, eight, and twenty-four days. As far as possible the conditions were identical, so that if the drug is to be credited in any instance with the reduction of sugar, it must also be debited with its increase in the other instances, and we must conclude that it did more harm than good to my cases. I do not, however, think so harshly of it; I believe it to be a very harmless substance, and incapable of action either for good or evil. It deserves just as little to be regarded as a specific for diabetes as does arsenite of bromine, salicylate of sodium, bromide of potassium, codeine, salicine, boracic acid, or uranium nitrate, each of which has found enthusiasts to believe in it. I discussed the claims of these drugs in a paper published in the *Practitioner* for December, 1886, to which I would refer any

reader who desires to know more of my opinions as to the rational use of drugs in diabetes.

One word as to saccharine. A convenient solution for sweetening purposes may be made by using bicarbonate of soda as a solvent, in the proportion of five grains of bicarbonate of soda and ten grains of saccharine to one ounce of water; a teaspoonful is sufficient to sweeten a cup of coffee. In preparing potash imperial for diabetic patients, four grains of saccharine to the pint are sufficient to make it very distinctly sweet. Saccharine is not expensive; I understand it can be sold retail at about 1s. 6d. a drachm.

I am, Sirs, yours truly,

ROBERT SAUNDBY, M.D. Edin., F.R.C.P. Lond.
Birmingham, Oct. 10th, 1887.

"THE PRESYSTOLIC MURMUR, FALSELY SO CALLED."

To the Editors of THE LANCET.

SIRS,—I have read with considerable pleasure the attack on the auricular systolic murmur, as such, by so eminent a member of the profession as Dr. Dickinson, contained in the first part of his paper, "The Presystolic Murmur falsely so-called." He treats the subject from a physician's aspect; may I be permitted to make a few remarks from an anatomical standpoint?

In considering the causation of the murmur, a comparison is necessary between the normal and the stenosed mitral orifice, more especially with reference to the position of the valves during diastole and beginning contraction. During the diastole of the ventricle blood from the pulmonary veins is gradually filling the auricle, flowing on into the ventricle, lifting up the valves from the ventricular wall, and producing an orifice similar to, though larger than, the stenosed one. The auricular contraction commencing in the great veins forces the blood through this funnel-shaped orifice into the ventricle, the effect of which is the distension of the ventricle and the complete approximation of the valves before the commencement of the ventricular contraction, thus preventing any regurgitation. I am aware that this latter assertion is denied, and that many prefer to regard it as being brought about by the beginning ventricular contraction. In support of what I have stated I may mention the following experiment, well known to your readers:—

The heart being removed from the body, with the ascending part of the arch of the aorta and pulmonary artery, which are tied so as to occlude their orifices, the left auricle is opened, and a stream of water is made to flow through the mitral orifice; the valve segments are then seen to leave the ventricular wall, and gradually to approach each other; the stronger the stream, the more rapid and firm the approximation. Instead of water, I have often, with a like result, used air, by placing the nozzle of a syringe at the mitral orifice and forcing air into the ventricle—this under conditions similar to those existing in the body. If a force, therefore, equal to the auricular contraction can approximate the valves, why is it necessary to assume that these are only approximated by the commencing ventricular contraction?

In applying the above experiment to a heart with a stenosed mitral orifice, I have noticed that such is not closed until a greater force of water is used than is necessary to close a normal mitral orifice. And in stenosis, with a definite ventricular systolic murmur during life, the orifice cannot be accurately closed by such procedures. This being allowed, then, it follows that the auricular contraction will not approximate the thickened fibroid valve segments, or will not completely close the funnel-shaped aperture bounded by the ring of adherent valve segments present in mitral stenosis. The ventricular contraction following, regurgitation through the imperfectly closed mitral orifice results; it is this regurgitation which has been described as of direct mitral origin. Again, as Dr. Dickinson points out, may not the character of the murmur be an additional argument in favour of the ventricular systolic theory? For has it not been described "as running up to the systolic sound," and "increasing in intensity until lost in the systolic sound"? If the mitral valves at the beginning ventricular contraction are not approximated, but become so during its contraction, the orifice will therefore become narrower and, the force remaining the same or increasing, the resulting murmur will increase in intensity, and on the complete approximation of the valves will be followed by the sharp sound

which, though described as the first sound, yet differs materially from the booming character of the normal first sound. I am, Sirs, yours very sincerely,

Crewe, October 4th, 1887.

W. OWEN TRAVIS.

To the Editors of THE LANCET.

SIRS,—By a somewhat remarkable coincidence two men of unquestionable ability and originality have almost simultaneously attacked the commonly accepted belief on the manner of production of the so-called presystolic or auriculo-systolic murmur—a murmur which, curiously enough, is accepted by observers as of all cardiac murmurs the only one the mere recognition of which is absolutely diagnostic of organic valvular disease. Dr. Dickinson in your own pages, and Dr. McVail before the Medico-Chirurgical Society of Glasgow (*Brit. Med. Jour.*, Oct. 8th, 1887), have both asserted their belief that the auricular contraction is incapable of producing the presystolic murmur, which, they say, is really ventricular systolic in its rhythm and regurgitant in its nature; and Dr. McVail has gone into elaborate detail to show its method of production. Now, it seems to me that, without discussing the position further—and there is much room for discussing it,—there is one series of well-ascertained facts which is absolutely fatal to its acceptance. The presystolic murmur as we general practitioners see it—and it is exceedingly common—occurs in the majority of cases in a heart which is perfectly regular in its action and moderate in its rate of contraction, and is thus in a condition favourable to its careful study. It begins *almost immediately* after a faint and perhaps reduplicated second sound, and runs right on to the short, sharp, and loud first sound (or what represents the first sound), and thus it abruptly ends. Occasionally this is followed by an ordinary ventricular systolic murmur; but its existence is of no importance to my present purpose. Then comes the second sound again, in its usual course, and the cycle is complete. Now, if the theory which these observers advance be correct, the ventricular systole commences *immediately* after the closure of the sigmoid valves, and ends only with their closure a second time; for it is clear that, whether there be a characteristic mitral systolic murmur present or not, the completion of the ventricular contraction must be at once followed by the closure of the sigmoid valves, and the production therefore of the second sound. In the extremely brief period, then, which intervenes between the completion of the ventricular contraction and the production of the second sound, *plus* the short period during which that sound lasts, *plus* the very brief interval which elapses before the murmur begins again, we are asked to suppose that there occur, in succession, the cardiac pause, the ventricular diastole, the auricular contraction, and the complete ventricular distension; whilst, on the other hand, the ventricular contraction, which we have hitherto regarded as short and sharp in its character, is drawled out, as it were, during the long presystolic murmur, the occurrence of this first sound, and up to the fraction of a moment before the occurrence of the second sound. In other words, we are asked to believe that all the essential work of cardiac rest, collection of blood in the auricles, and the filling of the ventricles, takes up greatly less than the half of a cardiac revolution, whilst the work of emptying the ventricles into the great arteries takes up far more than the half, and that, too, in the case of a left ventricle, which in uncomplicated cases is undilated and unhypertrophied, if, indeed, it be not actually atrophied. It seems to me, therefore, infinitely more difficult to accept the position which is being now proposed to us, than to believe that the contraction of the auricle is itself the cause of the murmur. How strangely views change on these matters, for not seventy years ago Laennec believed that the auricular contraction was sufficient to explain the second sound, and was, indeed, its cause. There can be little doubt that he not only recognised the presystolic murmur, but correctly connected it with contraction of the mitral orifice, rightly thinking it auriculo-systolic in rhythm, but unfortunately basing his opinion upon the false belief that the contraction of the auricle was the cause of the second sound. But why should it be thought so impossible for the auricular contraction to produce this long rough murmur? If there is a true diastolic mitral murmur, there can be no other possible explanation for it than that the current of blood, accelerated perhaps by the suction power of the dilating ventricles, is thrown into sonorous vibration

as it passes over the thickened and roughened cusps of the mitral valve. And if this be so, why should the still more accelerated current of blood forced into the ventricle through a narrowed orifice by the prolonged contraction of a dilated and hypertrophied auricle be unable to produce an audible sound? Indeed, it is not to be forgotten that a murmur, prolonged and even musical in quality, can be produced under circumstances of surprisingly little force or rapidity of current, as is evidenced by the occurrence of the *bruit de diable* in the veins at the root of the neck.

I am, Sirs, yours truly,

Paisley, October, 1887.

FRANK SHEARAR, M.B. Glas.

“COMPULSORY NOTIFICATION OF INFECTIOUS DISEASE.”

To the Editors of THE LANCET.

SIRS,—Dr. Jacob has misapprehended the purport of my letter in your issue of the 8th inst., in which I did not undertake to prove anything, but merely stated that compulsory notification of infectious disease was acceptable both to the profession and laity of Reading, and worked smoothly and well. The question of its value as against disease is a different and larger one, to which the demands upon my time will not permit me to do justice, nor can I, owing to the death of our medical officer of health, under whose supervision our present system has grown up, obtain access to, or deal with, the statistics of the borough, as he, had he been alive, would have done.

1. I much regret that I am unable to give a satisfactory reply to Dr. Jacob's first question as to the influence of compulsory notification on the death-rate from zymotic disease. The reason is that until November, 1875, Reading had no complete system of drainage, and during the past ten years the number of houses connected with that system has been gradually increasing from 2003 in 1876 to 8765 in 1886. Therefore the two factors of improving house sanitation and compulsory notification have been concurrent, rendering it impossible to ascertain to what extent each is responsible for the reduction of the zymotic death-rate from 2.88 per 1000 of the population in the five years 1876 to 1880 inclusive to 1.96 in the next quinquennial period 1881 to 1885, being a reduction in five years of nearly 1 per 1000 living.

2. The notification certificate is a plain statement that, in the opinion of the practitioner, A. B. is suffering from an infectious disease, and specifying the situation of the building, the name of the occupier, and the nature of the disease. The practitioners here feel that they and the medical officer of health have no conflicting interests, and the latter uses his own discretion as to whether he visits the house, sends his inspector, or does neither. Our late medical officer was on the best terms with all the medical men, and would, if he thought it necessary to visit a house, consider the wishes of the medical attendant, and go with or without him, as the latter might prefer. He never interfered unpleasantly, his assistance was constantly requested, and his visits objected to by none. The disinfection and sanitation of the house was, in the case of intelligent and well-to-do householders, left to the direction of their own doctor, who could obtain, if he liked, the services of the officers of the sanitary authority, or have the same carried out by other competent persons. In the case of the poor, disinfection was carried out by the authorities, and both doctors and patients were glad to have it done. In *all* cases, on the receipt of the certificate, a printed notice was sent enjoining isolation for a specified time from the date of the last case in the house, giving particulars as to the disease and its sanitary treatment; direction for proper disinfection of the patient, premises, and clothing; advising medical attendance, and reciting the penalty for the various infringements of the Act of Parliament by infectious persons. It cannot be denied that all this is useful and to the point in tending to make people careful, and so limiting the extension of disease; and without compulsory notification these precautions could not be advocated, and probably would not be adopted. Errors in diagnosis of course occur, and in cases felt to be doubtful it is quite usual for the practitioner to ask the medical officer of health to see the case and give his opinion as to its nature. I have asked dispensary and club doctors, and they assure me that the Reading working classes have no objection to notification, and no dread of