are liable to some error, inasmuch as lupus produces deformities of the visage, and scars in the neck which are precisely of the kind to suggest the offhand suspicion of scrofula. This may easily happen in cases in which, excepting the lupus itself, there may be an entire absence of all evidence of such taint.³

In spite of my own statistics, however, I cannot avoid the belief that lupus is, in very many instances, a scrofulous disease; but at the same time I am inclined to the belief that, in most instances, the tendency to disease of this type having begun in the skin, restricts itself to it, and shows little or no tendency to attack internal organs. I well remember a family of three sisters, whom I knew intimately in youth, and whose history may illustrate my meaning. The eldest of these had her neck dreadfully scarred by strumous abscesses in childhood, but she subsequently enjoyed excellent health through a long and vigorous life. The second, always delicate, died of phthisis at 50; and the third, who had never ailed anything else, lost her nose from lupus at about 45. There was a history of tuberculosis in the mother's family.

The more definite exemption from tuberculous antecedents or concomitants, the more likely it is, I think, that any individual case of lupus will prove non-inflammatory; but there are exceptions on all hands, and I dare not assert anything in this matter with confidence. If the mucous membranes suffer, there is, I think, almost invariably a tuberculous family history. On the other hand, I must admit that the evidence as to family history of phthisis is as strong, or even stronger in the case of lupus erythematosus, as it is in common lupus.

There is another peculiarity of personal health, which is of much importance, in predisposing to certain forms of lupus. I refer to the proclivity to chilblains. The tendency on the part of the tissues to inflame and become irritable, under the intermittent influence of cold and damp, which constitutes the chilblain proclivity, is so marked in many cases of lupus, that this disease might almost be regarded as being a modified chilblain. This statement concerns the erythematous form more frequently than it does the common lupus, but it is not without its importance as regards the latter.

In what I have been saying I have been speaking of scrofula and tuberculosis as if they were interchangeable terms, or, if not so, as if scrofula still enjoyed an existence, and had not been wholly merged by modern discoveries. It is desirable to explain a little on this point. Those who hold with the greatest firmness the modern doctrine that a bacillus is the one and the only cause of true tuberculosis yet admit that peculiarities in the state of health of the recipient are of very great importance in favouring the development of the parasite. For them tuberculosis is a disease of infection from without, and can never be self-produced; but still only those of certain idiosyncrasy or diathesis are capable of readily receiving the contagion. We have then two things—contagion and proclivity. It follows, I think, that it is quite possible that the state of system which constitutes proclivity may in itself and without tuberculous infection originate some forms of disease. Nothing is more probable than that persons vulnerable in the direction of easily affording a nidus for the tubercle bacillus may also be liable to peculiar forms of common inflammation. That such is the case I would submit we know well by experience. In such patients wounds, bruises, and chilled parts repair badly and are very apt to inflame. If exposed to cold, chronic catarrhs or states of local inflammation may readily follow. Thus ophthalmic surgeons recognise a scrofulous ophthalmia which they do not regard as tuberculous, but which occurs in patients who have tuberculous susceptibilities. I would venture to suggest that the terms struma and scrofula are very suitable for the designation of all such specialised types of common inflammation as we encounter in those who would easily become tuberculous if exposed to contagion, but who have not as yet had the

bacillus implanted. Whatever results from the implantation of the bacillus we may name tuberculosis, whatever results from the constitutional state alone we may denominate scrofula.

It would be of great value if we could obtain any trustworthy evidence as to the frequency of the association of lupus with cancerous tendencies. As yet, however, none such are forthcoming. Many observers have noticed that there is in persons of middle age, who have suffered from lupus, a definite tendency for the scars to become attacked by cancer. I have myself witnessed this repeatedly. I have also, in several cases, seen cancer and lupus existing in different regions and independently in the same patient. The number of such facts is, however, not as yet sufficient to justify us in feeling confident that they are more than coincidences, however strong may, nevertheless, be our suspicions. The observation of histologists (Sangster and others) that, in lupus scars, there is often a definite tendency to down-growth of processes from the Malpighian layer, much after the pattern of epithelioma, is not without its interest in this connection.

A METHOD OF GRAPHICALLY RECORDING THE EXACT TIME-RELATIONS OF CARDIAC SOUNDS AND MURMURS.¹

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INTRODUCTORY STATEMENT BY DR. BYROM BRAMWELL.

IN common with the great majority of physicians, I have been in the habit of thinking that the so-called presystolic mitral murmur is produced by a blood current flowing from the auricle to the ventricle through the stenosed mitral orifice; and I have repeatedly—both by (1) simple palpation with the flat hand; (2) combined auscultation and palpation; (3) combined binaural auscultation and inspection of the apex beat and movements of the cup of the stethoscope; and (4) combined binaural auscultation and inspection of the movement of the lever of a Galabin's cardiograph—investigated the relationship of the murmur to the cardiac impulse, with the result that, in the great majority of cases of presystolic murmur, which I have carefully and critically examined, I have satisfied myself (or, at all events, I have thought that I had satisfied myself) that the murmur does precede the first sound of the heart and the maximum impulse of the apex beat.

The publication of Dr. Dickinson's paper, in which he stated that, according to his observations, the murmur and impulse of the apex beat are synchronous, and in which he gave theoretical reasons² for believing that the murmur is ventricular systolic, and not auricular-systolic, seemed to show the hopelessness of getting all observers to agree even as to facts—to say nothing of the theoretical conclusions to be based on them—by means of our present methods of investigation, and to call for some new method by which the matter can be graphically demonstrated and finally and conclusively settled.

Some years ago, when the phonograph was first invented, the possibility of recording, on the same revolving drum, a tracing of the cardiac impulse (by means of a cardiograph) and a tracing of the cardiac sounds and murmurs (by means of a modified phonograph) had occurred to me.

The publication in the daily journals (just about the time when Dr. Dickinson's paper appeared) of the extraordinary results which Mr. Edison has obtained by means of his new modification of the phonograph, redirected my attention to the subject; and, with the object of bringing the matter to a practical conclusion, I explained exactly what I thought was wanted to my friend Dr. Milne Murray. Dr. Murray did not think that the phonograph or microphone, so far as their mechanism is at present known, could be made to record a tracing in the manner I desired; but he stated that he did not know the capabilities of Mr. Edison's new instrument, and he advised me to write to Mr. Edison himself.

¹ Communicated to the Royal Society of Edinburgh, on December 19th, 1887.

² It would be foreign to my present purpose to discuss the arguments which can be advanced both for and against the presystolic origin of the murmur of mitral stenosis. On some future occasion I may have to enter into some parts of the subject in detail. Suffice it for the present to say that the balance of evidence has seemed to me to be strongly in favour of the generally accepted or presystolic view.

³ I have, since this lecture was given, collated for statistical purposes some more recent notes which I had by me. The following statements will, I believe, convey a truthful impression of them. In rather more than half my cases the notes expressly state that the patient appeared to be in very good health, and had at no time suffered from any debilitating diseases.

In nearly half special inquiries on this head failed to disclose any particular delicacy or ill-health on the part of the brothers and sisters, or of the parents.

In only 9 per cent. was there reason to suspect phthisis in the individual, and in several of these the evidence was but slight. In nearly 30 per cent. there were enlarged lymphatic glands, but in some of these clearly the gland disease was secondary to the lupus. I am well aware that results of statistical inquiry have been published which differ considerably from mine.

Leloir (*Annales de Dermatologie et Syphilis*, vol. vii) brings considerable clinical evidence to prove that the two diseases are practically identical. Of 17 hospital cases treated by him in 1885, 10 showed unmistakable signs of lung tuberculosis, another suffered from white swelling of the knee. In two of them he proved the presence of tubercle bacilli in the lupus tissues, though inoculation experiments on the peritoneum of dogs failed. In one case lupus of the hand was attended by scrofulous lymphangitis (and formation of "tubercular gummata" along the lymphatics of the arm), followed by tuberculosis of the lung on the affected side. In four cases lymphatic oedema ("white erysipelas") accompanied lupus of the skin, and constitutional disturbance and phthisis resulted.

This I have accordingly done. I have pointed out to Mr. Edison the immense practical advantages which would result if cardiac sounds and murmurs (and, indeed, all other stethoscopic phenomena) could be permanently recorded and phonographically stored up; and have begged him to direct his attention to the subject. I have told him that we physicians and clinical teachers desire two modifications of his instrument, namely:—

1. A phonograph (or phonographic stethoscope, as it might be termed) capable of receiving, recording, and reproducing cardiac sounds and murmurs. I trust that if Mr. Edison will direct his genius to the matter, he may be able to give us such an instrument. If the receiving portion could be adjusted to a limited area of the chest, and if the discharging portion could be made to reproduce its sound into the cup of a binaural stethoscope, the instrument would be exactly what is required. Its introduction would mark a new era in clinical medicine and in clinical teaching. By means of such an instrument, phonograms of all important and interesting cases could be stored up in our case books, and murmurs and other sound phenomena could be kept for any length of time, and reproduced whenever they were required. The teaching of auscultation would be materially simplified. And by means of a modification of the instrument it might perhaps be possible to magnify the sounds produced by such a phonograph, so as to make them audible to a large clinical class. In fact, the possibilities of such an ideal instrument are immense.

2. A phonograph capable of receiving cardiac sounds and murmurs, and of discharging its vibrations, not in the form of sounds, but in the form of movements, capable of being recorded on a revolving cylinder of smoked paper. Such an instrument would enable us absolutely and definitely to settle the systolic or presystolic rhythm of the murmur of mitral stenosis, and many other important questions connected with the physiology and pathology of the heart; for, by recording on the same drum a tracing of the apex impulse (by means of a cardiograph) and, immediately below it, a tracing of the sound phenomena (by means of such a modification of the phonograph), the exact relationship of sound to impulse, and to the different phases of the cardiac cycle, could be settled with certainty; in fact, with absolute mathematical precision.

Although much disappointed with Dr. Murray's opinion as to the possibility of graphically recording cardiac sounds and murmurs by means of the phonograph, I could not get rid of the idea; for some such instrument seemed so exactly what is required for determining the exact rhythm of the presystolic murmur on which my thoughts were at that time intent.

On further consideration the following modification of my original plan suggested itself to me: (1) To record on a revolving drum the cardiographic trace by means of a Marey's cardiograph. (2) To listen (the eyes being closed), by means of a flexible binaural stethoscope, to the cardiac sound or murmur the time of which it was desired to mark and ascertain. 3. To strike with the forefinger of the right hand the button of a second Marey's tambour (chest piece) at the moment when the murmur or sound was heard, and so to record it (by means of a second Marey's cardiograph) on the same revolving drum, immediately below the tracing of the impulse of the heart. In this way the impulse of the forefinger—in other words, the exact time occurrence of the murmur, less the "psychical loss" occupied by the passage, so to speak, of the murmur from the ear to the tip of the forefinger of the auscultator—could be exactly marked in relationship to the different parts of the heart's cycle, as traced on the drum by the cardiograph. If the levers of the two cardiographs (one worked by the apex beat, the other by the forefinger of the auscultator) were so adapted as to move in exactly the same vertical plane, the exact time relationship of the sound or murmur to the ventricular systole (or other phase of the cardiographic trace) could be determined, provided that the "psychical loss," previously alluded to, were accurately measured and deducted. (4) In order that the "psychical loss" might be accurately measured and deducted, time markings would have to be made on the revolving drum immediately below the tracings produced by the cardiographic levers; and (5) the exact time which elapses between a sound being made and its being signalled and inscribed on the revolving cylinder by the auscultator, would have to be accurately measured.

Dr. Milne Murray at once saw the feasibility of this modification, and very kindly promised not only to fit up the necessary apparatus, and to place the resources of his laboratory at my disposal, but, what was even more important, to share in the research and to aid me with his knowledge and advice. It is unnecessary to say that I eagerly availed myself of Dr. Murray's offer. My crude idea has in this manner been transformed into an actual working mechanism; and the research being supervised by Dr. Murray, who, in experimentation of this description, has few, if any, equals, the reliability of any

results which are obtained is guaranteed, so far as it is possible to guarantee them.

Dr. Murray suggested that instead of the auscultator signalling the time of the murmur by touching the button of a second Marey's cardiograph, he should do so by means of a Morse's key and electric circuit.

To estimate the "psychical loss," a point about which I had no satisfactory notions, he at once suggested a most ingenious contrivance, which is in every way suited for its purpose. A lever is made to move and record its movement on a revolving drum, and a sound is produced in a telephone at one and the same moment by "making" or "breaking" the same electric circuit; in other words, by touching a Morse's key a sound in the telephone can be produced at will, and the exact time occurrence of that sound is at the same time automatically and, as it were, electrically recorded on a revolving cylinder.

By means of this contrivance the estimation of the exact "psychical loss" is a matter of the greatest simplicity. The individual whose "psychical loss" it is required to measure (the auscultator in the previous experiment), places the telephone to his left ear, and as soon as he hears the telephonic sound produced by a second individual, who is taking the observation, signals to the revolving drum by means of a Morse's key and electric circuit; in this way the auscultator inscribes, as it were, on the revolving cylinder of smoked paper the exact time when he hears the sound; and since the period at which the telephonic sound was actually produced has been already inscribed on the drum by the automatic arrangement previously described, the distance at which the auscultator's signal follows the telephonic signal of course equals the "psychical loss"—that is, the delay which occurs in the sound vibrations passing, as it were, through his psychomotor apparatus to the signal key.

By recording time markings on the drum below the tracings produced by the two signals, the exact duration of the "psychical loss" in fractions of a second can be exactly measured; and the necessary correction in the position of the signal representing the occurrence of the cardiac sound as measured in its relationship to the cardiographic tracing and cardiac cycle (for, in the previous observation, the same time markings were, it will be remembered, taken) can of course be at once made.

The more accurate account of the apparatus is given below by Dr. Milne Murray himself.

DESCRIPTION BY DR. MILNE MURRAY OF APPARATUS USED IN GRAPHICALLY RECORDING THE TIME OF CARDIAC SOUNDS AND MURMURS.

1. *Arrangement for Recording the Cardiac Movements and the Occurrence of the Murmur.*—The binaural stethoscope S, Fig. 1, is retained in position over the region of most audible sound, in the manner described, and attached to the ears of the auscultator, who sits with his back to the observer and the recording apparatus. The cardiograph C, is held in position over the apex beat, either by the patient or by an assistant. The movements of the cardiograph are conveyed to the recording tambour T, through the tube V, and are recorded by the uppermost pen (1). The right hand X, of the auscultator, is placed so as to close at will a properly adjusted Morse key, K, placed conveniently on a table by his side. This key completes the circuit of the battery, I, shown beside it, and actuates the signal magnet, M. The upstrokes of this pen (2) will therefore indicate the movement of the occurrence of the murmur listened to, as indicated by the signals of the auscultator. The magnet marked B. (tracing the third line) is in direct connection with a clock beating seconds, and that marked A. is in connection with a timing-spring beating fractions of seconds (in this case 1-15th).

2. *Arrangement for Determining the Time Intervening between the Reception of an Auditory Impression and the Transmission of a Signal by the Auscultator, that is, the "Psychical Loss" in the Observations.*—The pendulum F, makes contact with the platinum points at each swing, and closes the circuit of the cell E. This circuit includes the primary coil of the induction apparatus G, and the magnet C, acting on the pen 2. At each swing of the pendulum a current will pass through the coil G, and the moment of its passage will be recorded on the drum by the pen 2. This current will, moreover, induce a current in the secondary coil of G, with which the telephone H. is connected. This telephone is held to the ear, O, of the auscultator, who will thus listen to the clicks of the telephone produced by the induction currents in the secondary coil of G. The right hand X, of the auscultator, is conveniently placed on the key K, and with this he signals the moment when he hears the clicks in the telephone. These signals are marked on the drum by the magnet D. and the

pen 1, which are in the same circuit as the key. The loss of time between the reception of the auditory impression from the telephone and the transmission of a signal (in other words, the length of time required for the sensory impression at O., to be transformed, so to speak, into a muscular contraction at X.) will be measured by the distance between the up-stroke of 1 and 2. The pens 3 and 4 mark seconds and fractions of seconds in the same way as in the previous arrangement.

STATEMENT BY DRs. BYROM BRAMWELL AND MILNE MURRAY
OF THE RESULTS OBTAINED IN THEIR FIRST SERIES OF
EXPERIMENTS.

Our first observations were made on November 14th, in a case of typical presystolic murmur. In this and the subsequent experiments in this series Dr. Bramwell listened to the murmur and signalled it, while Dr. Murray superintended and worked the recording apparatus. For the purposes of description, and to avoid the needless repetition of names, we will term the former the auscultator and the latter the observer. The receiving tambour of the Marey's cardiograph was

self. The advantage of having the end of the stethoscope held in position by an assistant or by the patient is that the auscultator is then altogether disconnected with the patient, except through the flexible tube of the binaural stethoscope, and there is, consequently, no possibility of his receiving any information with regard to the rhythm, systolic or otherwise, of the murmur by means of the tactile sense. The auscultator then closes his eyes, and concentrates his whole attention on the cardiac sound or murmur the time of which he desires to record, and proceeds to mark the time of the murmur by depressing with the forefinger the Morse's key, conveniently placed at his right hand.

After the auscultator has for some time signalled the murmur in this way (in other words, as soon as he proceeds to discharge and signal the time of the murmur in a regular and rhythmical manner,) and when the levers connected with the cardiograph and the auscultator are both working to the observer's satisfaction, the drum is started, allowed to make its circuit, and stopped. The observation is then completed. By this means four tracings are recorded on the smoked paper; the first or uppermost is the cardiographic trace; the

Fig. 1.—The apparatus used by Drs. Byrom Bramwell and Milne Murray, for graphically recording the exact time relations of cardiac sounds and murmurs.
(For detailed description of the figure, see text.)

held in position over the apex beat by an assistant. The method of working was as follows:—

The patient was conveniently placed (seated or lying) at a distance of about four feet from, and facing, the recording drum. The cardiograph was then adjusted, so that a sufficiently satisfactory movement of the lever connected with the recording tambour was obtained. The assistant then took charge of the cardiograph, and kept it in accurate contact with the apex beat. The auscultator then seated himself in front of the patient, with his back to the recording drum, and by means of a binaural stethoscope determined the part of the chest where the murmur could be most distinctly heard.

In the case of presystolic murmurs, the area of which is often very limited, it is not usually possible to place the cup of the stethoscope over the exact point of maximum intensity of the murmur, for the receiving tambour of the cardiograph covers this spot; but in the two cases we have so far investigated, a position was easily obtained where the murmur was distinctly heard.

During the observation, the cup of the stethoscope may be held in position either by the patient, an assistant, or the auscultator him-

self. The advantage of having the end of the stethoscope held in position by an assistant or by the patient is that the auscultator is then altogether disconnected with the patient, except through the flexible tube of the binaural stethoscope, and there is, consequently, no possibility of his receiving any information with regard to the rhythm, systolic or otherwise, of the murmur by means of the tactile sense.

It must be understood that as yet we have made no attempt to time the duration of the sounds or murmurs which we have recorded. Whether this can be at all accurately done we do not at present know, but hope to determine in the course of our investigation. The duration, then, of the auscultator's signal may, so far as our present results go, be left entirely out of account. The commencement of the signal (the point at which the letter c is placed in Fig. 5) is the point to which importance is to be attached.

In this, our first experiment, several observations were made. The preliminary tracings were more or less imperfect, for the necessary adjustments, and the rate of revolution of the drum best fitted for all the requirements of the experiment, had to be ascertained by practice; but we finally obtained a satisfactory tracing, in which the auscultator's signal bore a definite and constant position to the ventricular systole, as traced by the cardiograph in successive cardiac cycles.

The "psychical loss" was then estimated in the following manner. The apparatus having been re-adjusted, the auscultator held the telephone to his left ear, and by means of his right forefinger (the eyes being closed) signalled to the recording apparatus the time when the telephonic sound was heard. The telephonic sounds were produced by the observer "making" or "breaking," by means of a Morse's key, an electric circuit at irregular intervals the exact time at which the telephonic sound occurred being, as previously mentioned, automatically inscribed on the revolving cylinder.

In this way, four tracings were again obtained on the smoked paper. The first or uppermost represented the auscultator's signal; the second the exact time of production of the telephone sound; the third, the time in seconds; and the fourth, the time in fractions of seconds. The psychical loss, estimated in this manner, was found on this occasion to be, roughly, about a fifth of a second. The auscultator was conscious of the very long psychical delay which occurred in signalling the intermittent telephonic sounds, produced at irregular intervals in this manner. The conditions were obviously quite different from the rhythmically and quickly recurring sounds produced in the human heart. It was, therefore, decided that, in future observations, the telephonic sounds should be produced in a rhythmic and regular manner.

The next observations were made on November 25th. On this occasion, two cases were examined. The first was another absolutely typical case of mitral stenosis (presystolic murmur). Several tracings were obtained, in which the murmur (as signalled by the auscultator) occupied a definite position in successive cardiac cycles (as traced by the cardiograph); and the position of the murmur corresponded exactly to the position of the murmur in the previous case of mitral stenosis.

In the present communication, we do not propose to make any further statement with regard to the position of the murmur in these two cases of mitral stenosis. The exact time relations of the murmur being in dispute, we intend to hold our judgment in suspense, until we have satisfied ourselves by repeated observations of numerous cases, that we have sufficient grounds for coming to a reliable conclusion on the point.

In the second case which was examined on this occasion (November 25th), there could be no possible doubt as to the nature of the clinical phenomena; and the tracings obtained, both of the aortic second sound, and of the telephonic sound which was artificially produced, in estimating the psychical loss, were so definite, so unexpected, and yet so mutually corroborative the one of the other; and the final result, as to the position of the second sound in the cardiac cycle was so absolutely in accord with all that is known of the physiology and pathology of the heart, that the reliability of the method, as a practical means of demonstrating the time relations of cardiac sounds and murmurs to the different parts of the cardiographic tracings is, we think, to a large extent established by this single series of observations alone.

The second case which we observed on November 25th, and to which

we now wish to refer in detail, was that of an intelligent man (W. K.), aged 41, a gentleman's servant, formerly a soldier, who consulted Dr. Byrom Bramwell at the out-patient department of the Edinburgh Royal Infirmary on November 5th, 1887.

The clinical details of the case are as follows. Four days previously, when playing at dominoes with his brother, he was suddenly seized with a localised epileptiform convulsion (attack of Jacksonian epilepsy), in which the head and eyes were twisted to the right side, and then affected with clonic spasms. Consciousness was retained during the attack, which lasted about a minute and a half. He was unable to see during the attack, in consequence, he thinks, of the eyes being turned so far upwards and to the right. The arm and leg were not affected.

On inquiry, it was ascertained that the patient had, previous to this attack, enjoyed excellent health; in fact, he stated that, with the exception of being occasionally laid up with the effects of drink, he had never known what it was to be ill. Some years ago, he used to drink and smoke very heavily; but, for the past eight or nine years, he has been perfectly steady in both respects. In particular, the patient stated that he never had syphilis, and that he had not suffered from

headache or vomiting. During the three weeks preceding the convulsive seizure, he had occasionally felt giddy. Four months ago, he "saw double" on three or four different occasions, but this double vision only lasted for a few minutes at a time. He had also, two or three times, seen bright flashes of light before both eyes. He stated that he had never had rheumatism; that he was not short of breath; and that he had neither pain nor uneasiness in the region of the heart; in short, he had no suspicion whatever that there was anything wrong with that organ.

On examination the optic discs were found to be normal, and there were ab-

solutely no physical signs of central nerve disease. On examining the heart, the apex beat was found to be ill-defined, in the erect position, beating behind the sixth rib, in the line of the nipple, or perhaps a little to the left of it. When the patient was lying on his back, the cardiac pulsation was abnormally forcible and heaving. The area of precordial dulness was distinctly increased, and there seemed to be a decided increase of the deep dulness over the ascending thoracic aorta. A well-marked double aortic murmur was audible up and down the sternum. At the second right interspace, the commencement of this murmur was accurately marked by a sharp, short, but loud and ringing, second sound, from which it, (the murmur) tailed off. The heart's action was perfectly regular, and its frequency 72 per minute. The diastolic murmur was prolonged far into the diastole, which did not seem to be materially shortened, as it is in free aortic regurgitation. The radial pulse was moderately jerking, visible, and tortuous, but was not suggestive of free aortic regurgitation—an observation which was confirmed by the character of the sphygmographic tracing taken a few days subsequently. (See Fig. 3.) The cardiographic tracing shown in Fig. 4 was taken by Galabin's

Fig. 2.—The apparatus used by Drs. Byrom Bramwell and Milne Murray, for estimating the "psychical loss." (For detailed description of the figure, see text.)

instrument on the same occasion as the sphygmogram, the heart's action at the time of the observation being somewhat accelerated and excited.

The diagnosis was dilatation (perhaps aneurysmal) of the ascending thoracic aorta, with some, though not free, aortic regurgitation. The cause of the attack of Jacksonian epilepsy need not be discussed here; but it was probably due to a small embolism, though the possibility of a commencing localised "coarse" cortical lesion cannot be absolutely excluded.)

Now this case seemed singularly well fitted for testing the capabilities of the method of timing cardiac sounds and murmurs which we are now describing, the apex beat being sufficiently well defined to give a decided systolic up-stroke corresponding to the ventricular systole, and the exact commencement of the diastolic murmur being most accurately and sharply defined by the short accentuated aortic sound, which occurs, as everyone admits, either immediately, or very shortly, after the termination of the ventricular systole—that is, when the lever of the cardiograph, which has been raised during the ventricular systole, is beginning to fall.

Fig. 3.—Sphygmogram (taken with Mahomed's modification of Marey's instrument) in the case of W. K. The tidal and diastolic waves are well-marked, and the pulse frequency is not increased.

The results of our observations in the case of W. K. (accentuated aortic second sound and aortic diastolic murmur) were as follow. The cardiographic tracing which was obtained by Marey's tambour apparatus, though amply sufficient for our present purpose (since it clearly shows the systolic and diastolic portions of the cardiac cycle and the point where systole commences and ends), is by no means equal to that which is obtained by Galabin's cardiograph. (The difference in the result is probably due to several circumstances, namely, (1) the inherent qualities of the two instruments; (2) the fact that the apex impulse was less distinct, the heart's action being much less forcible, when the laboratory observation was made; and (3) to the circumstance that the auscultator is accustomed to work with Galabin's cardiograph, and is well acquainted with its capabilities, but is not in the habit of working with Marey's instrument.)

Fig. 4.—Cardiogram (taken with Galabin's cardiograph) in the case of W. K. When the tracing was taken the heart's action was somewhat excited and accelerated. The tracing shows powerful and well sustained ventricular contraction, some impediment to the passage of the blood from the ventricle into the aorta, and no marked rise of blood-pressure on the ventricle during its diastole.

A sufficiently satisfactory movement of the lever connected with the cardiograph having been obtained, the murmur was timed, signalled, and recorded with all the precautions previously described, the auscultator's attention being concentrated on the sharp accentuated second sound which marked the commencement of the murmur. The signal of the auscultator therefore represents the commencement of the aortic diastolic murmur, or the occurrence of the second (aortic) sound of the heart.

The timing in this case was very easy and very satisfactory, owing

to the sharpness and loudness of the sound to be observed, and the slowness (72 per minute) and regularity of the cardiac action.

The duration of the observation seemed to the auscultator inordinately long, and he was conscious towards its termination of a feeling of strain, and of cerebral surprise at the duration of the experiment, and of anxiety as to its result. The long duration of the experiment was due to the fact that the observer, being so pleased with the result, had not contented himself, as in former observations, with taking one tracing, but had three times raised and readjusted the drum, and had taken four successive tracings before giving the auscultator the signal to cease the auscultation by nudging him in the back. Exact reproductions of portions of these four tracings are shown in Fig. 5. The full length of each tracing was 19 inches; more than half (11½ inches) has consequently been cut off, so as to adapt the the woodcut to the size of the JOURNAL (8½ inches).

The total number of pulsations (cardiac cycles) recorded in the four tracings was twenty-eight, representing a period of nearly twenty-three seconds. The length of the observation was, however, very much longer than this, for the repeated readjustment of the drum occupied a much longer period than the taking of the tracings; and the auscultator continued to signal the murmur for some time both before the commencement of the first, and after the termination of the final, cardiac pulsation marked on the drum.

This experiment was obviously a severe test of the capabilities of the method and of the mechanism, for the long duration of the experiment, and the feeling of fatigue, cerebral surprise, and anxiety for the result, and expectancy for the termination of the observation, which the auscultator experienced towards the end of the experiment, might reasonably have been expected to interfere with the accuracy of his timing in the later stages of the observation; and to a very slight extent this does seem to have been the case. In the last three cycles traced, which are not shown in Fig. 5, the timing is not quite so accurate as in the other parts of the tracings.

It will be seen by reference to the tracing (Fig. 5) that the result in this case is very definite. The up strokes indicating the occurrence of the second sound, as signalled by the auscultator, and to which the letters C. point in the figure, practically occupy exactly the same place in each cardiac cycle throughout the four tracings. (The cause of the slight differences in position seen in successive cardiac beats or cycles will be explained presently.)

We observed with much surprise that the second sound, as signalled, exactly corresponds to the point of the cardiographic tracing which represents the termination of the systole; in other words, exactly corresponds to that part of the cycle in which we know that the second sound does actually take place. This result was difficult to understand, for no allowance or correction had been made for "psychical loss." In our previous observations we had roughly estimated the "psychical loss," which occurs in signalling intermittent and irregularly recurring sounds, at a fifth of a second; and if any such deduction had to be made in the present instance, the result of the observation would be an obvious absurdity, for it would cause us to place the occurrence of the second sound back, quite in the early part of the ventricular systole.

We proceeded, therefore, with much interest and anxiety to re-estimate the "psychical loss." With this object, Dr. Murray placed into "circuit," with the telephone and recording apparatus, a clock, so constructed that a "make" and "break" of the electric current were effected every two seconds, the rhythm, however, being interrupted every tenth beat. In this way a "make" and "break" sound, separated by a short interval, were produced in the telephone every two seconds, but interrupting every tenth beat, so to speak, and the exact period of occurrence of these "make" and "break" sounds was automatically recorded on the drum, as in previous experiments. The auscultator found that the signalling of the sounds produced in this way did not materially differ from the signalling of the intermittent and irregular sounds produced on the previous occasion; the sounds did not occur sufficiently rapidly to enable him to time them rhythmically, more especially since the interruption every tenth beat entirely disorganised the rhythm. In timing the sounds produced in this way, he had, he found, to watch for every separate sound, and to time it separately and individually, as if it had been produced in an altogether irregular and arrhythmical manner. As a matter of fact the "psychical loss," estimated in this way, was found to be exactly 3.15ths of a second.

We then determined to make the conditions, so far as possible, identical with those in the case of diastolic aortic murmur previously examined; in other words, to time a well defined sound, that is produced regularly, at the interval of a second. To effect this the clock, which in previous experiments had been used to mark the time in

Fig. 5.—Facsimile of four consecutive tracings in the case of W. K. The letters *a*, *a* point to the commencement, and *b*, *b* to the termination of the ventricular systole, as seen in the cardiographic tracing; *c*, *c* to the occurrence of the second aortic sound, as signalled by the auscultator; the spaces between the letters *d*, *d*, and *e*, *e*, in the third and fourth lines of each tracing indicate periods of a second and a fifteenth of a second, respectively.

Fig. 6.—Facsimile of a portion of the tracing obtained in the estimation of the "psychical loss," as described in the text. The letters *a*, *a* point to the time at which the telephonic sound was signalled by the auscultator; and the letters *b*, *b* to the time at which it actually occurred. The two are, it will be observed, practically synchronous. The distance between *c* and *c*, in the third line, represents a period of two seconds.

seconds on the tracings, was put into circuit with the telephone and its recording lever; and the two seconds clock (that is, the clock which makes and breaks contact every two seconds, intermitting every tenth beat) was used as a time marker, though, with the telephone signal already marking seconds, this was not, of course, necessary.

The result of this arrangement was the production in the telephone every second of a "make" sound and a "break" sound, separated by so short an interval that the sound vibrations appeared almost to run one into the other; the result produced very closely resembled a short, sharp cardiac murmur, occurring in a heart beating with absolute regularity sixty times a minute.

The auscultator, as soon as he listened to the sounds produced in this way, and before any tracing had been taken, expressed absolute satisfaction with the conditions. He stated that any "psychical loss," which was observed in timing these sounds would, he felt confident, apply to the observations made in the case of W. K. Further, it was obvious that the conditions exactly resembled the timing of a short, sharp systolic cardiac murmur, for the up stroke of the telephonic signal (which marked the occurrence of the telephonic sound) might be regarded as the systolic up stroke of the cardiographic lever, which marks the commencement of the ventricular systole, and therefore the commencement of a systolic murmur in a cardiographic tracing.

After careful adjustment of the telephonic lever and the signal lever of the auscultator, a tracing was taken with all the precautions previously described (the eyes of the auscultator being closed). The result (see fig. 5) was again a surprise, and an interesting physiological observation—which, so far as we know, has not been previously made—for it showed that, under the conditions of the experiment, there is absolutely no “psychical loss.”² The absence of any “psychical loss” under such conditions is a most important practical fact for the purposes of our present research; for it shows that in recording, by means of the apparatus which we have described, the sounds of a rhythmically contracting and slowly acting heart, the personal equation (as regards “psychical loss,” but not, of course, as regards the auscultator’s capability of perceiving cardiac sounds or murmurs) may be left out of account. Within what limits, as regards frequency of action (in other words, frequency of sound), this statement applies we do not at present know; but we hope in the course of a short time to be able to determine.

There are also several other interesting questions connected with this result (not, however, bearing directly upon our present research) which suggest themselves for solution, and which, if time permits, we hope to investigate.

The fact that there is no “psychical loss,” under the conditions we have mentioned, confirms in the most striking and absolute manner the reliability of the previous experiment, which had located the occurrence of the second sound just at the end of the ventricular systole. In fact, the two experiments are mutually confirmatory the one of the other; for the fact that the second sound does occur immediately, or almost immediately, after the ventricular contraction ceases, is, of course, one of the absolutely certain and fixed events in cardiac physiology.

These experiments seem to show that the method is a practical and satisfactory method, and well fitted to serve the purposes for which it is intended.

The explanation of the absence of any “psychical loss” in this experiment is not far to seek. It is obvious that the discharge of the motor centre concerned in the movement of the forefinger, which depresses the signal key, becomes rhythmical; and this rhythmical discharge of the “signal centre,” as it may be termed, takes place synchronously with, or even rather tends, as some of our observations show (unless the attention of the auscultator be very closely concentrated on the sound) to anticipate the recurring sensory sensation (telephonic sound), with which it corresponds in time. It is quite obvious, therefore, that any given sensory impression or telephonic sound cannot regulate the motor discharge, with which it corresponds in time (and on the drum); but that the preceding sensory impression or telephonic sound guides the succeeding motor discharge; or, at all events, that the rhythmical recurrence of the motor discharges is regulated and guided by the rhythmically recurring sensory impressions which precede them.

Such a view explains the fact that the signal marking the occurrence of the second aortic sound, in the case of W. K. (the points at which the letter C. is placed) is not absolutely the same in each succeeding cardiac cycle. It will be seen that in some cycles it slightly precedes, while in others it slightly follows, the termination of the ventricular systole, as indicated by the cardiographic lever. The reason is, that (in consequence, no doubt, of the influence of respiration), the heart does not beat with the regularity of clockwork; that in a normal and apparently perfectly regularly beating heart, some cardiac cycles are longer, while some are shorter than others. We may, therefore, expect that if a short cardiac cycle, or a short ventricular systole, follows a long cycle, that the signal representing the second sound, since its time is regulated not by the second sound, with which it corresponds in time and on the drum, but by the preceding, or several preceding, second sounds (regulating the rhythmical contraction of what we may term the “signal centre”), will slightly succeed; while, in a long cardiac cycle, it will slightly precede the point in the cardiographic tracing at which it ought actually to be placed. Whether this is so or not we have not yet determined by actual exact measurement.

It will nevertheless be noted, that although successive cardiac cycles vary considerably in their duration, the relative position of the second sound, as signalled by the auscultator, to the termination of the ventricular systole, as recorded by the cardiograph, is practically maintained throughout the whole four tracings; in other words, the

slight error in one cycle is not magnified in the next cycle, and that the relative position of these two points (second sound as signalled, and termination of systole as traced) is practically the same, both in the first and in the last cycles of the series; an argument, we think, in favour of the view that the preceding sensory impression (sound) regulates the occurrence of the succeeding motor discharge (signal).

The practical outcome of this, we fear, somewhat long explanation is, that the exact position—point of commencement—of a cardiac sound or murmur, even granting that it is signalled with absolute precision, cannot by this method be absolutely and accurately determined from the observation of any single cardiac cycle alone; but that, by taking the average, so to speak, of a sufficiently long series of cycles, such location can be determined with very great exactitude, always, of course, granting that the auscultator signals correctly the exact commencement of the sound or murmur. Further, the error which, under such circumstances occurs in any individual cycle, in consequence of the diminished or increased length of that cycle as compared with the previous cycle, is so slight, that for practical purposes, for determining, whether a murmur is systolic, diastolic, or presystolic, it may be left out of account.

It must be remembered that these statements only apply, so far as our present observations enable us to judge, to a regular and rhythmically acting heart. To speak with absolute accuracy, we have only proved that they hold good in timing a sound produced with exact regularity every second; but no one, we presume, will for a single moment be inclined to doubt that this limit, sixty times a minute, may be considerably extended in each direction. By future observation we hope to determine this point.

Further, we hope that by this method we may be able to demonstrate the exact rhythm of the murmur of mitral stenosis, presystolic murmur; and perhaps, after we have perfected our cardiographic apparatus and succeeded in recording on the drum a more ample and satisfactory tracing of the impulse of the heart, that we may be able to indicate the exact points, in the up and down strokes of the tracings, which mark the occurrence of the first and second sounds of the heart.

The apparatus seems also fitted to record other clinical phenomena, as, for example, the exact duration of the delayed sensation which occurs in some cases of nerve disease.

In conclusion, it is perhaps hardly necessary to state that this method of graphically recording the exact time relations of cardiac sounds and murmurs is not intended, nor indeed fitted, for ordinary every day clinical use. The apparatus is far too complicated and delicate for that. But, indeed, the ordinary method of auscultation, aided, if necessary, by the administration of digitalis or strophanthus, with the object of slowing, steadying, and strengthening the cardiac contractions, and so, of making the intensity and rhythm of the murmur more distinct, is, for practical purposes, all that the experienced observer requires. We hope, however, that the method which we have described may enable us to decide some of the questions which are at present in dispute.

Note.—Since this paper was written, Mr. Edison has kindly promised to direct his attention to this subject.

AN ADDRESS

ON

THE THERAPEUTICS OF THE URIC ACID DIATHESIS.

Delivered at the Opening of a Discussion on the Subject in the Section of Pharmacology and Therapeutics at the Annual Meeting of the British Medical Association, held in Dublin, August, 1887.

By I. BURNEY YEO, M.D., F.R.C.P.,

Professor of Clinical Therapeutics in King's College, London; and Physician to King's College Hospital.

I HAVE, in the first place, Mr. President, to thank you for the compliment you have paid me in inviting me to open this discussion.

The therapeutics of the uric acid diathesis is a very wide subject, and one surrounded by conflicting theories and divergent opinions; but it is within our power to limit this discussion to the consideration of certain definite questions of practical interest, in the examination of which we may hope to elicit, from the ripe experience and trained observation of the many distinguished physicians who are present here to-day, facts and suggestions of much value.

² This fact was demonstrated by the authors of the paper to the members of the Edinburgh Pathological Club, on November 30th. Although the demonstration was made at 12 P.M., when the auscultator was exhausted by a hard day's work, and at the end of a meeting of four hours' duration, in which a good deal of tobacco was going the whole time, the result was perfectly satisfactory, the “psychical loss,” under the same conditions, being again nil.