ferences in percussion sounds and the inherent difficulties of taking exact measurements, doubtless afford scope for considerable error, but as these examinations were repeated on many occasions, this error will be minimised by taking the average of many examinations.

The diminution of the cardiac dulness immediately after the bath averaged 1 inch transversely and $\frac{1}{2}$ inch vertically, and on no single occasion did we fail to note a diminution in both directions. On one occasion (December 23rd) the diminution of transverse measurement amounted to $2\frac{1}{2}$ inches, while on December 15th only $\frac{1}{4}$ inch of diminution was noted, the average of 11 measurements amounting, as has been said, to 1 inch. Moreover this transverse measurement tended steadily to diminish from the first. Thus on the 11 occasions between December 10th and January 3rd the measurements were (in inches):

Before bath	...	8 $\frac{1}{2}$	8 $\frac{1}{2}$	7	6 $\frac{1}{2}$	7	7 $\frac{1}{2}$	6 $\frac{1}{2}$	4 $\frac{1}{2}$	6 $\frac{1}{2}$	5 $\frac{1}{2}$	4 $\frac{1}{2}$
After bath	...	8 $\frac{1}{2}$	8 $\frac{1}{2}$	6 $\frac{1}{2}$	6 $\frac{1}{2}$	5 $\frac{1}{2}$	6 $\frac{1}{2}$	4	3	5 $\frac{1}{2}$	4 $\frac{1}{2}$	3 $\frac{1}{2}$

There was no permanent alterations in the upper level of cardiac dulness, only a transient diminution after each bath.

Chest measurements were also taken before and after the baths. Thus the circumference of the chest was taken on twelve occasions; (a) at the upper border of the fourth rib; and (b) at the upper level of the hepatic dulness. Both these measurements were slightly decreased after the bath on eleven occasions. Measurement a (at upper border of fourth rib) gave an average diminution of slightly more than 1 inch, and measurement b (at upper border of liver dulness) gave an average diminution of $\frac{3}{4}$ inch.

The antero-posterior diameter of the chest, from the second rib in front to a corresponding point behind, were taken with calipers on both the right and left side on six occasions, and it was found that there was an average increase on both sides of $\frac{1}{4}$ inch.

On two occasions it was noted that the upper level of the liver dulness had slightly descended after the bath. On one occasion this amounted to $\frac{1}{2}$ inch, and on another occasion to $\frac{3}{4}$ inch, but this fact was not so systematically noted as the others which have been dealt with. There was no permanent change in any of the chest measurements. On no occasion was any difference in the cardiac sounds noted as a result of the bath. The character of the murmurs remained unchanged.

The patient's condition improved, and on January 22nd, after fifty-five days in hospital, he felt well enough to ask for his discharge. The improvement has taken place on two previous occasions, and whether that which took place on the present occasion is to be attributed to the bathing or the rest in bed, or both, is necessarily a matter of doubt.

CASE II.—R. D., a coach painter, aged 26, admitted on December 21st with all the signs of double aortic disease (obstructive and regurgitant). The upper level of cardiac dulness was at the lower border of the third rib, and the transverse dulness extended from the left border of the sternum to $1\frac{1}{2}$ inch outside nipple. The apex beat was in the sixth interspace, just outside the nipple line. This patient had four baths only, and on every occasion the cardiac dulness was diminished both transversely and vertically after the bath, the averages being $\frac{3}{4}$ inch transversely and $\frac{1}{2}$ inch vertically. The apex beat moved about $\frac{1}{4}$ inch inwards after every bath except one, but its position when the patient left the hospital had undergone no permanent change. The bath usually caused a considerable eructation of flatus from the stomach.

Chest measurements were also taken, but the variations in the measurements were so slight that the result of them may be regarded as negative.

I am much indebted to my house-physician Mr. Bates, and to Mr. Ernest Stock, the clinical clerk who had charge of the patient, for the care with which they assisted in making the observations detailed above.

THE Ohio State Legislature has passed a Bill which provides for the execution of criminals by electricity instead of by hanging. The law will come into force on July 1st.

PRESENTATION.—Dr. H. Rogers-Tillstone, of Clevedon, has been presented with a black marble clock by his (female) ambulance and nursing classes, in token of their esteem.

AUSCULTATORY PERCUSSION: A REPLY.

By W. BEZLY THORNE, M.D.

I NEITHER expected nor desired that the paper which I read before the British Medical Association on the subject of Auscultatory Percussion would be allowed to pass without criticism. Allow me, at the outset, to point out that I carefully avoided connecting the name of Dr. Schott with that method of physical examination. He has long practised percussion in a manner, devised by himself and his brother, for which he claims special advantages.

The auscultatory method of percussion was first introduced to my notice by Dr. P. Watson Williams, of Clifton, in the autumn of 1894, as one which "would probably enable me to determine the relative diminution in the area of deep cardiac dulness with greater accuracy than ordinary deep percussion or any other method of physical examination known at present." After a continuous experience of a year and a half, during which time I have had recourse to the ordinary method for purposes of comparison only, I gratefully and emphatically bear witness to the correctness of Dr. Watson Williams's view. I would state that my paper embodied nothing more than my own views, as, at the time when it was written, I was quite unacquainted with the literature on the subject.

In 1876, however, Dr. Noel Gueneau de Mussy published in the *Union Médicale* (3e. serie) an interesting paper on the subject under the denomination of "auscultation plessimétrique" but it is necessary to state that in matters of detail and application there are differences between the practice of Dr. de Mussy, Dr. Watson Williams, and of myself. Such as they are they do not touch the fundamental acoustic facts upon which auscultatory percussion is based. Dr. de Mussy stood at the side of his patient and percussed lightly and sharply on the spinal column near to the vertebra prominens, or on the sternum and clavicles, while his ear, applied to the chest, was moved from place to place on the opposite side of the thorax. Dr. Williams and I practise percussion by somewhat different methods on the same side of the body as that to which the chest piece of a binaural stethoscope is applied to convey the waves of sound to the ear.

On the subject of the acoustic basis of the method Dr. de Mussy suggests two alternative theories, namely, that the waves of sound, starting from the periphery, are either transmitted directly through the body, or conveyed by the enclosing structures, and are in either case modified by the varying densities of the subjacent tissues. It is also laid down that increased density of subjacent tissue diminishes the sonorousness and raises the pitch of the note elicited, and tuberculous, inflammatory, and other indurations as well as the heart and the liver are enumerated as causing the change of note. The author further states that where ordinary percussion yields but doubtful and equivocal results, the auscultatory method may yield diagnostic indications of great value.

My own observations would lead me to believe that the waves of sound are, as regards the thorax, mainly conducted by the walls. The effect of percussion on the clavicles, while the chest piece is made to travel to and from the region of the heart in different directions, as compared with those obtained when percussion is practised on the vertebrae, seems to me to be opposed to the idea that the vibrations are in the main conducted directly through the deeper structures.

Although Dr. de Mussy is able to quote the names of William and Trousseau as practical adherents of auscultatory percussion, it is evident from the following passage that he did not find the path of the innovator to be strewn with roses: "I foresee that one objection will be raised against this, as it has been against every other novel exploratory method; it is formulated in good faith by those who object to being disturbed in the enjoyment of opinions and of habits already formed, or who, without realising that it is so, dislike the labour of initiating themselves in a practice to which they have been hitherto unaccustomed. All this they say is very fine drawn and subtle; you are endeavouring to establish delicate distinctions, so delicate that it is not given to everyone to discern them."

These quotations show that the method under consideration is not, as has been asserted, a new one; they advance higher and wider claims than I ventured to suggest when writing nearly a year ago, and they prepare the way for the replies which I am about to submit to specific objections. Let me also add that the objections which have been expressed in your columns seem to me to be somewhat strange. As far as I can understand, they amount to this: the stethoscope is an instrument of precision of great value in conveying to the ear waves of sound produced by the movement of gases, fluids, and more or less solid bodies within the thorax, but from the moment that you apply it to the appreciation of vibrations extraneously produced on the chest wall as a sounding board the imagination is brought into play, and any statements based on observations made in that manner "must be rejected as absolutely untrustworthy." I confess that such reasoning seems to me to be no more in accordance with logic than with facts.

I will now turn to Sir William Broadbent's objections. It is said that in my diagrams the upper limit of cardiac dullness is made to extend nearly horizontally outwards almost as far as the right nipple line, an extension which, in the absence of aneurysm or mediastinal tumour, is inconceivable at that level. Whichever of the diagrams it may be to which allusion is made, the change of note which was observed at the base of the heart was identical with that which was taken to indicate the extreme limit of ventricular dilatation, the correctness of which was attested by the position of the apex beat. The initial line of diagram No. III was drawn by a London physician, who had called me in to see the patient, on the basis of a change of note which he had elicited by the ordinary method, and was confirmed by me by auscultatory percussion. Further, it is reasonable to suppose that if the extreme ventricular limit may extend so far as the sub-axillary region, as shown both by impairment of resonance and the apex beat, the auricles may in certain cases become proportionately dilated. I do not, however, wish to express myself dogmatically on this subject, as I have never enjoyed the advantage of seeing a dilated heart exposed in living action.

The next objection is that the bipartite outline is even more irreconcilable with anatomy and with clinical experience, and that there can be no possible correspondence of the notch with either of the auriculo-ventricular rings, because it is made to occupy the place where the pulmonary artery and aorta would be found.

In order that that argument might be valid it would be necessary to establish the contention that it is not possible to distinguish between the impairment of resonance caused by the subjacent heart and the vessels respectively; but the precise opposite is the case. Not only have I myself often done so, but I have frequently seen the position of those vessels marked out by percussion, and a line drawn at the point where the vascular dullness ends and the cardiac dullness commences. Moreover, when last year I was attending a medical man affected with aortic disease and aneurysm of the ascending arch of the aorta, the patient himself was invariably able, by his unaided sense of hearing, to confirm the accuracy of my observations on that very point. Indeed, it was after frequent conferences with him, when we had observed that the notch always preserved its anatomical relation to the great trunk vessels, that we came to the conclusion that it must correspond to the auriculo-ventricular rings. It may be pertinent to add, as a single instance of confirmation of the occasional existence of the notch, that quite recently I was present when a medical man of repute as a scientific clinician marked out the area of dullness in a patient of his own in my presence by bimanual percussion, and indicated the existence of a bipartite outline. The notch was of the depth of about an inch, and was found to be at about the same distance to the left of the midsternal line.

There was, therefore, no question of the percussor having been misled by the sternal change of note. The observation was then confirmed by auscultatory percussion, the chest-pieces of our respective stethoscopes being so placed as to stand in different relations to the line of percussion. I could adduce a score of such instances of independent confirmation of the presence of the notch, and I am not prepared to believe that all the practised auscultators who have been

concerned in these observations have been the victims of a misguided imagination.

The next point is that, inasmuch as the fibrous tissue which enters into the composition of the auriculo-ventricular ring is thinned out until it ceases to exist at a point which is opposite to that from which it springs from the central fibro-cartilage, and contracts and expands with the ventricle, it can have no more resistant power than the substance of the ventricle itself. The ring is, however, a mechanical and anatomical fact, and I find it difficult to believe that it would not prove to be, in the first instance, less yielding than the fibres of a degenerating myocardium.

The next paragraph proceeds to imply that the diminution in the volume of the heart which is observed to take place as the result of recourse to properly administered baths and exercises is no greater than that which may ensue under other methods of treatment. I venture to affirm that, according to the universal testimony of those who have extended opportunities of watching the effects of systematic physical treatment, the diminution of volume which is effected by that means is generally greater, and more rapidly effected, than that which follows any other known remedial or natural process.

One word as to the criticisms of Dr. Herschell in the BRITISH MEDICAL JOURNAL of April 11th. He noted in the course of his experiments that, when percussion is practised radially from the chest piece applied to the thoracic wall, a certain change of note takes place at a distance from the chest piece which varies in different cases, and, though he admits that he can offer no explanation of the phenomenon, affirms that such change of note offers a *reductio ad absurdum* of all the results and advantages which have been claimed for the auscultatory method. I think that I can supply the missing interpretation. The pressure of the chest piece, however light, interferes with the vibration of the component parts of the chest wall within a radius which varies according to circumstances. Had a physician of so great ability and acumen as Dr. Herschell pursued his investigations a little further, he would have learned to distinguish between that change of note and another and a different one which is characteristic of deep cardiac dullness. Little practice is needed to train the ear; but, if a modification of de Mussey's method be adopted, and the chest piece or the ear be made to travel over the thorax while percussion is made on the clavicles, the block over which Dr. Herschell has stumbled will be avoided, and the ear will soon learn to appreciate the cardiac note.

A casual reader of the last paragraph of Sir William Broadbent's "note" might gather the impression that he wishes to convey that I—the only person specifically alluded to—am in the habit of indiscriminately applying the Schott methods to cases where no real heart affection exists, and have made statements which seem to imply that it is equally efficacious under all circumstances. On reflection, however, it will become obvious that, if Sir William had intended to lay such a reflection on me, he would have done so in a manner so definite and circumstantial as to enable me to meet it no less definitely. It only remains for me to add that there is no one in the ranks of the medical profession to whom I should be prepared to haul down my flag more frankly and loyally than to Sir William Broadbent, while at the same time I do not at present think that he has shown cause for my taking that step.

PLACENTAL TISSUE: FRESH AND OLD.

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STATEMENTS are frequently made to the effect that in "placental polypus" and "sarcomatous" or "fleshy" mole, placental tissue can remain fresh and comparatively unaltered for many months after the death of the foetus. The conclusions I draw from the specimens figured in the accompanying illustrations, and from others I have examined, are quite opposed to these statements. The drawings are from sections of placental tissue which has been retained *in utero* for varying periods after the death or expulsion of the foetus.