disregard) their "pronouncement" as being a negligible quantity.

I beg to subscribe myself

A MEMBER OF THAT PUBLIC FOR WHOM THE
April 6th, 1907. PRONOUNCEMENT WAS INTENDED.

** We have received very many letters on this subject and shall possibly publish a further selection of them. We are glad to notice in communications from most of our medical correspondents the absence of the suggestion that political or pecuniary motives induced the publication of the much-discussed document.—ED. L.

ON THE CONDITION OF THE BLOOD-VESSELS DURING SHOCK.

To the Editors of THE LANCET.

SIRS,—Though not acquainted with surgical shock I feel greatly interested in the discussion on the above-mentioned subject. It proves so very clearly how ignorant we are of this difficult and intricate problem of the blood pressure in man. So far, indeed, goes our ignorance that two so widely different opinions are possible about a so marked disturbance of circulation. I cannot confirm Mr. Malcolm's views on shock but have to confess that I am much impressed by his arguments. I think his way of looking at the vessels, as described in his lecture in THE LANCET of August 26th, 1905, p. 573, may be very helpful in detecting the true state of things in many a case. Some time ago I was asked to see a woman aged about 40 years. It was a case of long-standing mitral stenosis. The right heart was still strong enough, the muscular apparatus at the orifices of the veins still working well; there was no general stasis or cedema but this sad condition of overfilling of the lungs which suffocates the patient. She was in the very last period of her disease and died a few weeks afterwards. Still, to save, or at least to prolong, life the doctor prescribed digitalis; not seeing any effect he gave it in a very large dose as an enema. To his great surprise he saw the (still regular) pulse disappearing and asked me to come over and see the patient. I found a filiform, nearly impalpable pulse, but very strong heart sounds and apex beat. Looking at the neck of the patient to see whether there was any venous pulsation I found well-filled, extremely hard, and wildly beating throbbing carotids. It was obvious that the heart was working against a heavy resistance. Why was the pulse so impalpable?

The explanation is, I think, in the line of Mr. Malcolm's suggestions. The large dose of digitalis contracted the arteries higher up than the radials at the wrist, the carotids being not, or not so much, contracted. The heart therefore worked against a heavy resistance, there was a very high intracardial pressure and a high blood pressure in the carotids. Now who will tell what the blood pressure has to be in the contracted radials at the wrists? Is Dr. Williams sure that it will be found high in that part of the arterial system? The problem of blood pressure is *not* settled by saying "if the blood-vessels contract the blood pressure rises; if they dilate the blood pressure falls." This assertion holds good for the intracardial pressure only. At the periphery it depends upon the spot where blood pressure is gauged and upon how far the contraction of the vessels has gone. I think it very probable that under some extraordinary influence (maybe in shock) the contraction of the vessels may go up still higher than the arteries in the arms and involve carotids and part of the aorta. The intracardial pressure may rise enormously and a low pressure be found in the periphery, where a large amount of resistance has been overcome. So I do not say Mr. Malcolm's explanation of surgical shock is the right one, but I think it may be useful in clinical examination and may be a help in explaining conditions which otherwise are not well understood.

I am, Sirs, yours faithfully,

Groningen, March 31st, 1907.

K. Z. WENCKESACS.

To the Editors of THE LANCET.

SIRS,—In my letter on this subject which you were good enough to publish on March 23rd, I invited Mr. J. D. Malcolm to point out the existence of any fallacy in one of the arguments by which the present view is supported. He responds in your issue of March 30th with two statements, one of

which, even if it be a fact, is not a fallacy, and the other of which is certainly neither fallacy nor fact.

The first is that in shock the blood pressure in the splenic vein is raised. Now, the splenic vein is a vessel which, owing to its anatomical position, does not lend itself either to frequent observations or to such as are altogether free from sources of error; but even if this were otherwise, from the interposition of the heart on the one hand, and the capillaries on the other, it must be obvious that the state of the pressure in an individual vein has no necessary or even probable relation with the general state of the pressure in the arterial system. The second statement is that a fall of blood pressure as recorded by the manometer might be due to a vaso-dilatation which is local and not general. Unfortunately for Mr. Malcolm, this is not so. The healthy organism must not, and will not, allow the blood pressure to fall below the normal level. If it does, the medulla is immediately threatened with starvation and the animal with death. A local dilatation is therefore immediately compensated for by a constriction elsewhere, so that the pressure may be maintained at its proper level. When, therefore, a local dilatation causes a fall in blood pressure it means that there has been no compensatory constriction elsewhere. This can only occur when the constrictor power is seriously impaired, as, for example, when the medulla is exhausted. There is thus no escape from the conclusion that, the cardiac power being constant, a low blood pressure indicates a general vaso-dilatation.

The contention that the condition of shock can be characterised by an unimpaired cardiac power, together with a general vaso-constriction, surely reveals some confusion of thought. Such a combination is very suggestive of a ruptured blood-vessel, but, except by a hamorrhagic route, it seems impossible to understand how it can cause the extreme degree of asthenia which is generally understood by shock.

I am, Sirs, yours faithfully,

London, W., March 30th, 1907.

LEONARD WILLIAMS.

"A PRELIMINARY NOTE ON RINNÉ'S TEST."

To the Editors of THE LANCET.

SIRS,—I hope I shall not seem to be displaying undue haste if I venture to make a suggestion to Mr. Richard Lake in connexion with his "Preliminary Note on Rinné's Test" in THE LANCET of Feb. 23rd, p. 505. I might wait to see the detailed statement which he promises and which I am looking forward to, but then my suggestion might be too late.

If I may be allowed to say it, Mr. Lake's method of recording the subdivisions of Rinné's test seems to me to be lacking in point. The method is accurate, of course, but the idea of each subdivision is grasped only after a more or less lengthy mental effort. Why not use the diagrammatic representation which is in vogue in Professor Politzer's clinic? The positive or negative quality is seen at a glance, and not only can Mr. Lake's six subdivisions be shown but any number of variations in them. Two vertical lines stand for the relative lengths of B.C. and A.C., and as, in the normal state of hearing, the length of B.C. is less than that of A.C., the line standing for the B.C. of normal hearing will be shorter than that standing for A.C. The following seven diagrams, therefore, will indicate normal hearing, and the six subdivisions referred to in Mr. Lake's paper.

The inclination of the line connecting the bases of the two vertical lines at once shows whether the positive or negative quality obtains. I need hardly say that the lines B.C. and

A.C. under "normal" do not pretend to represent *accurate* measurements of B.C. and A.C.

I am, Sirs, yours faithfully,

D. MATHESON MACKAY, M.D. Edin.,
Late Senior Clinical Assistant, Hospital for Diseases
of the Throat, Golden-square, London, W.

Hull, March 22nd, 1907.

THE INCREASE OF INSANITY.

To the Editors of THE LANCET.

SIRS,—I greatly regret that I should have caused annoyance to Dr. Marr by my suggestion that the difference between the increase among general paralytics in Glasgow and in Edinburgh might be the result of better clinical observation in the latter. For this suggestion I have no ground, but still it is a very difficult thing to believe that it is a fact that the disease increases more in Edinburgh than in Glasgow, and I ought to have put it that it probably was a question of difference of diagnosis. Any way I am certain that the careful examination of patients at Woodilee leaves nothing to be desired.

Yours truly,

Devonshire-place, W., April 9th, 1907.

GEO. H. SAVAGE.

A NATIONAL SCHOOL FOR CONSUMPTIVES.

To the Editors of THE LANCET.

SIRS,—My attention having been called to a letter by my friend, Dr. M. S. Paterson, of the Frimley Sanatorium, in your issue of March 23rd, *apropos* of an article by Mr. C. H. Garland and myself under the above title, may I be allowed to correct what seems to be a misconception when he infers that our views were based on our visits to Frimley? Our scheme was framed originally in 1903 and 1904 and in the inclosed copy of my interim report as chairman of the Sites and Buildings Subcommittee (p. 7, Fourthly) printed for March 11th, 1904, you will find that our advocacy of the adoption of the system of employing selected patients in sanatoriums—which, by the way, is no new thing—antedated not only our visits to the Brompton Hospital Sanatorium by a few years but also, I believe, the erection and opening and Dr. Paterson's appointment thereto.

The scheme of the National Association for Establishing and Maintaining Sanatoria, for which I have had the honour and pleasure of working continuously since its inception, has been developed almost without modification on the lines laid down in the report quoted. The other medical members referred to therein were Dr. A. P. Hillier and Mrs. Percy Alden, who were both unfortunately obliged to give up this work by other public calls upon them, but not before giving very substantial assistance. Dr. Paterson has achieved a striking piece of organisation and administration, and no one engaged on the development of similar methods on a large scale in England can afford to ignore his demonstration of the possibilities of this work. I wish to take this opportunity, in case our article did not convey our gratitude as sufficiently as it might have done, of thanking Dr. Paterson and his colleagues for enabling Mr. Garland and myself, as well as Dr. L. Crossley, the medical superintendent of our first sanatorium at Benenden, to see what was being done at Frimley.

I am, Sirs, yours faithfully,

Brook-street, W., April 2nd, 1907.

THOS. D. LISTER.

SANITARY ADMINISTRATION IN PAIGNTON.

To the Editors of THE LANCET.

SIRS,—I have had a copy of your editorial note under the above heading sent to me, and as a resident in Paignton for the past 18 years and recently a member of the urban district council I wish to reply to some points upon which I venture to think you have been misled by the report of the medical officer of health, who has evidently overlooked the fact that the report may come into the hands of those who have no local knowledge.

I will take the case of the supposed disregard of, the by-laws, where it is said that the council is permitting a road 600 feet long to be only 24 feet wide. The report does not state that this is a parish road leading from Paignton to Marldon and having an average width of 13 feet. All the council could compel the adjoining owners to do in the event of houses being erected would be to keep the frontages back 18 feet from the centre of the road. An owner has offered to give a strip of land which would enable

the road to be made 24 feet wide to form a footpath with curb and channel if the council will lay the sewer and ballast and form the new piece of road. The question whether this was a new road within the meaning of the Private Street Works Act was raised at the Local Government Board inquiry and disregarded by the inspector, and the sanction for the loan has been received. The new water scheme referred to by you is an auxiliary scheme for distributing the water to the higher levels of the town. The main water scheme is finished, and the water which has been brought 18 miles from the centre of the moor is now running into the home reservoirs, and the formal opening is fixed for June. The total cost of this portion of the scheme is £120,000, and both Teignmouth and Brixham will be supplied from Paignton. The dispute as to the relative values of the three schemes submitted to the council is purely an engineering question and cannot affect the health of the town in any shape or form, but the fact that the council has decided to leave it in the hands of the engineer, who is responsible for and has carried out the main scheme, would seem to be the right course to have taken.

I consider the statement of your local critic as to illness in the town over-coloured and made upon hearsay evidence. My own son is one of those who are at present suffering from scarlet fever, and the most careful research fails to show how it was contracted or any connexion with the few other cases in Paignton. The question of an additional ward at the hospital should be considered to overcome any future difficulty caused by the wards being occupied by enteric cases, but I believe the fact that the hospital is empty for months at a time could easily be proved. Your correspondent has omitted to point out that last summer a large sum was spent on the outfall of the sewer, that a break was located and mended, and the sewer was carried 250 feet farther out to sea. With regard to the water, I can only say that I have never had to boil it and my Pasteur filter shows no trace or indication of any organic matter being contained in the water.

With regard to the infantile death-rate, it should be considered that the wealthy class in Paignton are largely people who come here after retiring from the army, navy, or business, and are therefore for the most part past middle life. It follows that by far the larger number of births are among the artisans and labouring classes amongst whom the rate is always high.

I am, Sirs, yours faithfully,

Paignton, April 4th, 1907.

W. PIERPOINT ROBERTS.

EIGHTH INTERNATIONAL CONFERENCE OF RED CROSS SOCIETIES.

To the Editors of THE LANCET.

SIRS,—I have seen in THE LANCET of March 23rd an article on the subject of the forthcoming International Red Cross Conference. The statements it contains require correction. In regard to no official intimation of the conference having been given, an official notice was sent to you and was published in THE LANCET on Feb. 16th. It was also extensively circulated throughout the press of the country. I inclose a fresh copy of this notice which fully states who are responsible for the arrangements and where the meetings will be held. Further than this, you had on Feb. 2nd already published an article giving the same information, and in regard to the exhibition we advertised it in THE LANCET of, I believe, Feb. 9th.

I may add that all the Central Red Cross Societies who are entitled to send delegates to the Conference have been made fully acquainted with the manner in which it has to be conducted, that having been prescribed at the last conference at St. Petersburg in 1902.

In reference to your general remarks about this society perhaps you will allow me to send the inclosed pamphlet which explains its scope and origin. I shall have much pleasure at any time in providing you with the fullest information should you give me the opportunity, and I think you will find that the Conference will be conducted in every way in a manner worthy of the occasion.

Believe me, yours faithfully,

(Signed) J. DANVERS POWER,

Chairman, Conference Committee.

British Red Cross Society, 9, Victoria-street, London, S.W.,
March 27th, 1907.

* * We regret to have been misled with regard to the steps taken by the Red Cross Society for giving publicity to the Conference of 1907. We refer our readers to an editorial article in THE LANCET of Feb. 2nd, p. 311.—ED. L.