attack. This result seems to be incompatible with the view that the inspiratory spasm is secondary to bronchial narrowing and wholly compensatory in nature and suggests either that it is a primary event or, again, that there is some obstruction to the emission of air which cannot be overcome by the expiratory muscles, in which case we should expect the latter to be violently contracted during the attack. Are they? This is a point on which information is very desirable. Another possible explanation of the beneficial effect of Dr. Morison's manoeuvre is that it helps to break down the nervous paroxysm on which we may suppose the asthmatic seizure to depend. But whatever the explanation, certain it is that in the thoracic expansion characteristic of the acute dyspnoea of heart disease, pneumonia, and pleural effusion, or in the physiological dyspnoea caused by muscular exercises the adoption of this method of treatment would be fraught with disaster. Apologising for the length of this letter,

I am, Sirs, yours faithfully,
Wimpole-street, W., Nov. 26th, 1905. HARRY CAMPBELL.

To the Editors of THE LANCET.

SIRS,—I was much interested in Dr. A. Morison's lecture on the above subject published in THE LANCET of Nov. 25th and I can fully confirm the benefit from the mechanical treatment of spasmodic asthma which he claims. But, as pointed out, the treatment is so obvious that Dr. Morison is incorrect in supposing that it has not been previously practised. Seven years ago when an undergraduate at Cambridge it occurred to me while watching the struggles of a fellow student and we practised it with much relief to my comrade. Dr. W. Ewart at St. George's Hospital has long taught artificial expiration but his method has been to seize the lower ribs firmly in both hands and squeeze toward the mid-point of the thorax—i.e., inwards, upwards, and backwards. I have practised both this method and that of Dr. Morison on many patients. In stout people Dr. Ewart's method will be found almost useless unless the operator has abnormally large hands. The relief afforded is undoubted but I cannot say that I have found the treatment greatly to lessen the number of attacks.

I am, Sirs, yours faithfully,
J. SIDNEY PEARSON, M.B. Cantab.
Sydenham, Nov. 28th, 1905.

THE ESSENTIAL CONDITIONS OF STEAM DISINFECTION.

To the Editors of THE LANCET.

SIRS,—It is not a desire to have the last word that prompts me to answer Dr. J. C. Thresh's last communication but rather a wish to arrive at a solution of the problem under discussion. Dr. Thresh shelters himself behind the apparatus in which he is interested. I do not blame him. He is perfectly right when he declares that a misunderstanding exists between us. He believes that a current of non pressure steam which takes 20 minutes to penetrate 16 folds of blanket and at the expiry of another 20 minutes fails to kill anthrax spores enveloped in the blanket is as certain in its action as pressure steam which penetrates in six minutes and kills anthrax spores in 12 minutes. It is too late in the day to declare that the apparatus designed by him was only intended to deal with the pathogenic organisms of diphtheria, typhoid fever, and scarlet fever. I was always under the impression that steam disinfectors were capable of destroying any pathogenic organisms and he must know that among the diseases communicable to man anthrax is one. I fancy the omission of small-pox from the list given by Dr. Thresh is a *lapsus calami*. Moreover, anthrax has always been adopted as the standard organism for carrying out steam disinfection tests.

It will not do for Dr. Thresh to taunt Mr. Defries and myself regarding our strong advocacy of thorough air expulsion. Koch's dogmatic assertion, quite unfounded, as has been proved over and over again, is largely made use of in connexion with an apparatus which is familiar to Dr. Thresh in order to discount pressure steam. Surely then those of us who know why pressure steam excels its low pressure rival have a perfect right to indicate wherein the secret of this superiority lies. If the person supervising a certain high pressure steam disinfectant omits the expulsion of air he may have non-disinfection and may leave fleas uninjured; I could specify the apparatus to which Dr. Thresh alludes but,

to apply the *tu quoque* argument, if the low pressure attendant is in a hurry and expects to do his work in a short time the disinfection will be an absolutely negative one.

I am, Sirs, yours faithfully,

W. ROBERTSON, M.D. Glasg., D.P.H.,
Medical Officer of Health.

Public Health Office, Leith, Nov. 27th, 1905.

To the Editors of THE LANCET.

SIRS,—In the interesting correspondence which for some weeks has appeared in THE LANCET between Mr. Wolf Defries and Dr. W. Robertson on this subject I notice in the letter of Dr. J. C. Thresh in your issue of Nov. 18th the following statement: "In my long experience I have never known a machine used for disinfection after anthrax."

It may interest Dr. Thresh to know that it is my practice in Bradford to disinfect by means of steam all textile articles which have been soiled by persons suffering from anthrax either of the internal or external form. Voluntary notification of anthrax was established in Bradford some four years ago and since then infected houses have been visited and materials known to be infected have been disinfected.

I am, Sirs, yours faithfully,
Bradford, Nov. 28th, 1905. W. ARNOLD EVANS.

THE INVERSE RELATIONS BETWEEN PYREXIA AND GOUT.

To the Editors of THE LANCET.

SIRS,—Dr. Francis Hare, in an interesting letter on the above subject in THE LANCET of Nov. 25th, p. 1575, kindly refers to details in a short article of mine in a previous issue headed "A Case of Diphtheria in a Patient 79 years of age with Subsequent Freedom from Old Gouty Symptoms." Does Dr. Hare attribute the inverse relationship between acute disease and gout to the pyrexia alone, no matter from what disease the pyrexia may have arisen? In the case of diphtheria mentioned there was a rise of temperature on the second day only of my attendance, it being no higher than 101° F., and had dropped to normal by the next morning. I referred in the same article to a case of acute recurrent gout in which freedom from symptoms had been established for three years after an attack of pneumonia. In both of these cases transient albuminuria existed at the height of the attack. It does not seem quite clear to me why the previous pyrexia of the repeated acute attacks of gout had not a salutary effect such as had followed the pneumonia unless it be that, in addition to, or apart from, the pyrexia, there may be a specific antidote to gout in some of the acute febrile diseases, or is it that pneumonia being a disease which runs a definite course, in it combustion is not retarded in spite of treatment? The treatment, however, in the case of diphtheria was rapidly effective and the slight rise of temperature was checked by the antitoxin neutralising the poison.

I am, Sirs, yours faithfully,
Belgrave-road, S.W., Nov. 26th, 1905. JOSEPH BIRT.

POSTURE IN EXAMINATION OF THE HEART.

To the Editors of THE LANCET.

SIRS,—I am grateful to Dr. A. Morison for emphasising the importance of the changes which occur with change of posture in the physical signs of the normal heart and for drawing my attention to his own very interesting book which I am sorry I have not read till now. I see, however, no reason to change the passage which he alludes to—viz., "The change, however, in the width of dulness is strikingly constant, so much so that it seems curious that it has not been observed before." This change was a widening of the normal cardiac dulness when the recumbent was changed for the upright position. The passages which Dr. Morison quotes from the writings of Dr. P. M. Latham and Dr. Octavius Sturges have nothing whatever to do with this, and I cannot see how Dr. Morison's own figures can be described as "essentially the same as mine." Neither Fig. 17 (p. 19) nor Fig. 22 (p. 33) (the two which illustrate the influence of posture) is taken from a normal heart; Fig. 17 does not show the width at all in either posture and Fig. 22 omits the leftward limit of dulness in the recumbent posture;