police surgeons receive no fee whatever in such cases and we have good reasons for believing that the list of places where this demand for gratuitous medical evidence prevails might be considerably augmented. Will you kindly allow us through your columns to ask any of your readers (whether police surgeon or not) who is not paid by the magistrate in non-committal cases to send his name to one of us?

We are, Sirs, yours faithfully,

FRED. W. LOWNDES (40, Knight-street, Liverpool),
H. CULLIFORD HOPKINS (6, Bladud-buildings, Bath),
Honorary Secretaries, United Kingdom Police Surgeons'
May 27th. Association.

"THE PATHOLOGY OF THE PERICARDIUM."

To the Editors of THE LANCET.

SIRS,—I am much indebted to Mr. Harold L. Barnard for his courteous reply in THE LANCET of May 20th to my letter published in THE LANCET of April 29th. I trust he will not think me ungracious when I say that his letter leaves me in much the same position as before. I do not, of course, for a moment deny that the respiratory movements may under certain circumstances aid the circulation. On the contrary, I am sure they do; and it is imperative that we should understand what those circumstances are and what kinds of breathing are most favourable for the transit of blood from the cavæ to the aorta. What I contend is that the mere expansion and contraction of the lungs have very little, if any, accelerating effect on the blood-flow in ordinary quiet breathing and that thoracic aspiration is to a large extent independent of thoracic movement. Mr. Barnard writes as though it were much greater during inspiration than expiration. Thus, he says: "During deep respiration in man the negative pressure of the thorax varies from -1 to 2 mm. Hg in expiration to -30 mm. Hg during inspiration." Now, in a respiration of this kind there is a continuous increase of suction during inspiration from -1 to -30 mm. Hg followed by a continuous decrease during expiration from -30 to -1 mm. Hg. That is to say, very shortly after expiration commences suction may still be high and if we strike the mean suction for expiration it may not be much less than that for expiration. It is, however, less for the reason given in my last letter.

I have assumed Mr. Barnard's figures to be correct, although as a matter of fact the intra-thoracic pressure at the end of a complete expiration is decidedly positive, especially when the thorax is highly elastic—a fact of some importance in reference to Mr. Barnard's statement regarding the breathing of runners. He writes: "I can positively state that in a well-trained cross-country runner whose heart and respiratory pump are efficient for the exercise he undertakes inspirations are profound and expirations are complete. The air exchange at each respiration is maximal." Now, Sirs, while not denying the implication that expirations may be more complete in well-trained runners than in those who are not well trained I confess that I shall be greatly surprised if observation confirms Mr. Barnard's statement that the well-trained cross-country runner expires completely when running. In long-distance running expirations are possibly more complete than in sprinting, but it would be very difficult to expire completely during any kind of running, for, as just observed, a complete expiration involves a positive intra-thoracic pressure, and apart altogether from the vascular obstruction which it induces in the lungs themselves such an expiration leads to pressure upon the auricles and large intra-thoracic veins—a condition of things which could not be tolerated at a time when it is urgently necessary to facilitate the circulation to and from the right heart.

Mr. Barnard's observation that in the well-trained runner the abdominal wall contracts and hardens with each inspiration is interesting, though I should not have expected it to occur except under rare circumstances. Mr. Barnard does not specify the particular abdominal muscles which contract. He cannot, I presume, be referring to those abdominal muscles which depress the thorax—e.g., the recti and obliqui externi—since a complete inspiration would be prevented by their contraction. (Nor does the diaphragm—one of the muscles constituting his "abdominal sphere"—descend to any great extent in a complete inspiration in the normal young adult man. A complete inspiration tends, indeed, to induce a

negative intra-abdominal pressure.) Therefore such inspiratory abdominal contraction as does occur in running must, presumably, involve the transversely-disposed muscles, especially the transversales which are *par excellence* the retractors of the abdomen, bringing as they do the anterior and posterior walls of the abdomen into firm contact.¹ The presence of respiratory waves on the pulse-tracing cannot, as Mr. Barnard contends, be accepted as proof that the respiratory movements aid the circulation. All they show is that the movements in question affect the circulation in some way or other; it is quite easy to produce waves in an elastic tube containing a flowing fluid without accelerating the flow.

I am still somewhat doubtful whether in ordinary quiet breathing the descent of the diaphragm appreciably quickens the flow of portal blood through the liver. It is tempting to suppose that some such aid is needed, seeing that there is only a moderate pressure of blood in the portal vein to drive the blood through a complex system of vessels containing about one-fourth of the entire blood-mass. Two facts, however, should be remembered in this connexion: (1) a complex vascular system need not oppose great resistance, the non-recognition of which fact has led physiologists into many errors regarding the circulation; and (2) in birds there is no diaphragm and therefore in them the circulation through the liver needs no help from the diaphragm. I trust that it may not be thought from these somewhat sceptical observations that I under-estimate the valuable work which Mr. Barnard has done on the circulation; it has been most helpful to me.—I am, Sirs, yours faithfully,

Wimpole-street, May 30th, 1899.

HARRY CAMPBELL.

A BLIND LEADER.

To the Editors of THE LANCET.

SIRS,—I enclose one of the notices posted in various parts of this town by a local (Greek) practitioner. It is interesting as expressing the opinions given vent to by other local physicians as well as his own. I may say that we are having a well-marked epidemic of typhoid fever and the cases are well known to be more numerous by far than for some years past, probably owing to contamination of the water-supply which is usually very good. The following is a literal translation of the notice:—

"As some persons practising medicine in the town have spread a report that the disease found at present at Acca and supposed to be bilious or typhoid fever is caused by the water, and so the minds of people have been disturbed without legal cause and they have begun to drink boiled water, I come now with this notice, for the sake of humanity, declaring to the people that the cause of this disease is in the air, not in the water as is said, and it is not the disease supposed by these persons.

"Precautions to be Taken.

"Avoid cold, dampness, drinking iced water, fruit and food hard to digest; also avoid the boiling of water, which deprives it of its nutritive qualities and renders it harmful. It is best to filter it.

"May 3rd, 1899."

(Signed)

"GEORGI IOLAKIAS.

With regard to the native population I may state that a good many of them can see the folly of the above, but almost all without exception refuse to boil their drinking water.

I am, Sirs, yours faithfully,

J. CROPPER, M.A., M.B., B.C. Cantab.

Acca, Palestine, May 19th, 1899.

"A CRITICISM OF THE MORE COMMONLY EMPLOYED TESTS FOR ALBUMIN IN URINE."

To the Editors of THE LANCET.

SIRS,—The carbolic acid test for albumin as first described by Dr. Colquhoun in his paper differed from all others with which I am acquainted in that he recommended a saturated solution of phenol in absolute alcohol. In his letter in THE LANCET of May 20th, however, he corrected this and substituted a solution which is only about half saturated. The test is now so like one described by Dr. C. Meymott Tidy in THE LANCET for May 14th, 1870, that I may be pardoned for drawing Dr. Colquhoun's attention to it. Tidy added equal parts of alcohol and carbolic acid separately to the urine, shaking after each addition, and although he mentioned that a ready prepared mixture might be used he maintained that the separate addition of the reagents gave better and more delicate results, one part of albumin in

¹ Respiratory Exercises, p. 58.