Correspondence.

"Audi alteram partem."

DR. NEWTON HEALE AND THE ROYAL SOCIETY.

[LETTER FROM PROFESSOR SHARPEY.]

To the Editor of THE LANCET.

SIR,—It is hard to be obliged to repel accusations such as that directed against myself and Mr. Bowman by Dr. Newton Heale, in a letter inserted in your last number; but as there are allegations in that letter which, if allowed to pass unheeded by those at whom they are aimed, might eventually breed mischief, I request of you the favour to give insertion to what follows:—

Several weeks ago, Dr. Heale called upon me to say that he wished to make a copy of a paper of his, on the Bloodvessels of the Lung, read before the Royal Society in 1853, and deposited in the archives of the Society, and also to obtain engravings from some of the drawings which accompanied it. I informed him that, on his applying to that effect, I had no doubt the Council would grant his request, and suggested that if he employed Mr. Basire to execute his figures, the Council might probably entrust the originals into the hands of that artist, (who had been long accustomed to engrave for the "Philosophical Transactions,") whereby the expense of making copies might be saved.

Without waiting for a formal application from Dr. Heale, I applied on his behalf to the Council at their next meeting, and they readily complied with his request, and also with my suggestion as to the drawings, which, it will be seen, was not a condition prescribed by the Council, but a proposal to which they acceded for Dr. Heale's advantage. Of this I informed Dr. Heale by letter, telling him that on any day he found it convenient to call at Burlington House, Mr. Weld, the assistant-secretary, would give him the facilities he required. I need scarcely say that I never doubted that the manuscript and drawings would be found in their proper place.

Accordingly, happening to be at Burlington House when Dr. Heale called with Mr. Basire, to make a selection of his figures, I was no less surprised than concerned to learn that the MS. and drawings were missing. I am unable to account for this unlucky circumstance. I can only suppose that the paper had been accidentally mislaid, and its absence overlooked when the papers for the year were, as usual, bound up into a volume.

A ready explanation, however, has occurred to Dr. Heale. But before speaking of it further, I must notice his complaint of the selection of Mr. Bowman and myself as referees on his paper, and of our proceedings towards him.

Dr. Heale objects to the referees because, he says, they held opinions adverse to those advanced by him in his paper; and he refers, in confirmation, to a work on Anatomy of which I am one of the authors. But he ought to remember that the opinions stated and approved by the writer of a systematic work are not necessarily peculiar to the writer. Moreover, a new paper usually contains, or professes to contain, something new, and therefore, if the above objection were valid, the only fair referee in such a case would be one who had never openly entertained or approved an opinion on the question, and who yet must be known to be conversant with the subject by those who appoint him.

Before drawing up my report (a copy of which will be sent by Mr. Bowman, with his own, for insertion, if you can afford space for it), I made a most careful and, as I believe, thorough examination of the preparations; and, so far from resting on preconceived opinion, I made fresh injections from the bronchial artery of the human lung, both in the child and adult. After my report was handed in, but before it was read to the Council, Mr. Bowman and I met Dr. Heale at Somerset House, to give him the opportunity of personally explaining his preparations. Dr. Heale appears to have forgotten that he was not "required" to attend on that occasion, but invited to meet us, because he wished to do so for his own satisfaction. Nothing came out of his demonstration to alter the conclusions at which I had arrived from my own previous examination. But, according to Dr. Heale's suggested explanation, we were so intolerant of views adverse to our own that we reported unfairly against his paper, and thus prevented its appearance in the "Transactions," and were even so base as to retain or abstract the

manuscript and drawings which had been confided to us, so that they might not be preserved in the archives and bear witness against us some future day.

But Dr. Heale "wishes it to be distinctly understood that he brings no such charge"—not he!—he would not think of doing so against persons of our "character and reputation." The less scrupulous or more suspicious reader may, indeed, put this and that together, and draw his own conclusions. As for Dr. Heale, he refrains from bringing a charge because he has not "a scintilla of evidence to establish it." But still, he significantly adds, "the paper and drawings were entrusted to their custody as referees, and they have not since been heard of."

This last allegation, brought, incidentally of course, into juxtaposition with the "charge understood not to be brought," Dr. Heale, tender as he is of the reputation of others, will be rejoiced to hear has no foundation in truth. The paper and drawings were duly returned by me (as is marked, indeed, in the record kept by the clerk) that they might be sent to the other referee, by whom they were assuredly not retained; and they did not come into the possession of either of us again. Moreover, Mr. Bell, who was then secretary, assures me that he is confident the paper and drawings were, subsequently to their return by the referees, produced by him before the Committee of Papers in October, when the question of printing came on for decision, according to his uniform practice, any deviation from which he would certainly have noted. I succeeded Mr. Bell as secretary, but had nothing further to do with Dr. Heale's paper.

The disappearance (I still hope temporary) of Dr. Heale's manuscript and drawings, although through no fault of Mr. Bowman or myself, has caused me no small concern, both on account of the disappointment thereby caused to the author, and the reflection it naturally brings on the arrangements of the Society. I regret still more that this case has afforded a fresh illustration of the unhappy length which the wounded susceptibility of one who thinks his discoveries or opinions unduly slighted or disparaged may sometimes carry him in misconceiving the motives and misinterpreting the acts of those who differ from him.

I remain, Sir, your obedient servant,

Woburn-place, Feb. 1860.

W. SHARPEY, M.D.

(LETTER FROM MR. BOWMAN.)

To the Editor of THE LANCET.

SIR,—I was asked by the Council of the Royal Society, in the summer of 1853, to report on Dr. Heale's paper. I was far from seeking such employment. The paper impugned no published statements of mine, inasmuch as I had made none on the subject. He was not *required* or *called upon* to show his preparations, but transmitted them spontaneously, and wished the referees to inspect them. This Dr. Sharpey and I did on the 15th of August, in company with Dr. Heale, as stated in my report written on that day, a copy of which, together with a copy of Dr. Sharpey's, I append to this letter for publication. Dr. Sharpey had previously carefully examined the specimens sent by Dr. Heale, and on the 15th of August we examined them together, in Dr. Heale's presence, and with his explanations. We were judges appointed by the Council to report on the fitness of the paper for publication, and had no other interest in the inquiry than a regard for scientific truth, and for the reputation of the "Philosophical Transactions."

Dr. Sharpey's report is conclusive as to the patient and candid attention he had given to the subject, and is an honourable example of how much thankless labour is spent in the Royal and other Societies on this item of "reports." But for Dr. Heale's letter of last week, it would never have seen the light. Such reporting is necessarily, in a great degree, of a private nature; but, from all I know of it, I am satisfied that it is, with hardly a single exception, honestly and ably done. When I was habitually referred to at a period of greater leisure than at present, I never myself had the slightest objection to the authors being made acquainted with the facts of the reference; indeed, I used to prefer that they should be so, and I was always ready to inspect any preparations which they might consider to support their views. It now appears that greater reserve on my part might have been sometimes more prudent.

As to the absence of the paper from the archives of the Society, I am sorry for it, but have no personal concern in it whatever, the paper having been certainly returned to the Council by the referees, as shown by the tick-mark in the Society's own book. Had it not been so returned, the Society's officers would, no doubt, have required it at their hands. It may have been mislaid amongst other papers, or during the remove from Somerset to Burlington House. But however that may be, as Dr. Heale still thinks his results worthy of publication, I am glad to find he has a rough copy at home, and that the preparations admit of duplicate drawings being made. It might possibly save anatomists trouble, if he would, in the interval preceding publication, descend from his height of "confidence in the irrefragable correctness of his statements," and reinvestigate, for a little time, the simple question of the distribution of the blood supplied by the bronchial arteries; taking care, however, that the material injected through those arteries reaches some capillary network or other. Under the circumstances, it would really be a pleasure to me to find that the bronchial coats are not supplied with blood by their own vessels.

I am, Sir, your obedient servant,
Clifford-street, Feb. 1860. W. BOWMAN, F.R.C.S.

[COPY.]

Report of Dr. Sharpey on Dr. Heale's Paper.

In this communication the author professes to have discovered that none of the branches of the bronchial arteries terminate in the capillaries of the lining membrane of the bronchial tubes, as is commonly believed, but that the capillary bloodvessels of that membrane are continued from those of the air-cells, and are therefore derived from the pulmonary artery. He conceives that the blood is returned from the bronchial membrane by the pulmonary veins solely, and not jointly by these vessels and the bronchial veins, as generally understood.

In support of his views, the author appeals to the results of injections of the vessels made by himself, which are represented in the drawings accompanying the paper, and may be seen in corresponding preparations which he has transmitted for the sake of verification.

On examination, I find there is nothing to be objected to the fidelity of the drawings; but I am satisfied that neither they nor the actual specimens which they represent afford any valid proof of the peculiar opinion maintained.

Of the preparations which bear on the question, the greater number are portions of sheep's lung, in which the pulmonary arteries are injected with yellow and the pulmonary veins with red material,—the former imperfectly, the latter (so far as I can judge with a lens of low power) very completely,—whilst no injection has been introduced into the bronchial arteries or veins. In these specimens the capillaries of the air-cells have got filled with red injection from the pulmonary veins, and the vessels of the bronchial mucous membrane are also minutely injected with the same colour. This, however, proves nothing more than that the pulmonary veins return the blood not only from the capillary network of the air-cells, but largely also from that of the bronchial membrane—a fact already well known. It is known also that the *fine branches* of the air-tubes, especially when their walls become cellular, receive a share of the general capillary network formed by the terminations of the pulmonary arteries and commencing pulmonary veins on the adjacent air-cells; but the specimens exhibited appear to me to afford no evidence that the capillaries of the bronchial mucous membrane *generally* are derived from those of the air-cells as maintained by the author.

In the specimens just referred to, the bronchial arteries were not injected; but in another preparation of sheep's lung (shown in drawing No. 8), these arteries alone have been injected, while the pulmonary arteries and veins have been left empty. In this specimen, branches of the bronchial arteries filled with yellow injection are seen running on the outside of the bronchial tubes, and also beneath the pleura on the surface of the lung, but no injection has reached the bronchial mucous membrane. I am satisfied, however, that this negative result is owing merely to failure of the injecting process. The author, it is true, considers the injection as complete, and, in a figure of the opposite lung (injected simultaneously), represents vessels on the surface, which he regards as bronchial veins filled with yellow injection from the bronchial arteries; but in the specimen sent I can find no *capillaries* filled with yellow injection in any part—none, for example, in the interlobular cellular tissue or underneath the pleura, where, not only by

common consent, but according to the view of the author himself they ought to have been conspicuous had the injection of the bronchial artery been successful. The yellow vessels under the pleura, indeed, are many removes larger than capillaries, and in various places the injection may be plainly seen to have stopped in considerable-sized branches, the empty continuations of which are quite obvious.

It thus appears to me that the evidence adduced by the author does not bear out his opinion as to the termination of the bronchial arteries; and I may add that positive evidence against it is furnished by the general experience of anatomists as to the result of injecting the vessels in question. For, in point of fact, there is usually no difficulty in filling more or less completely the capillaries of the bronchial mucous membrane with a fine injection from the bronchial arteries. I have myself repeatedly made the experiment, and have never met with a failure.

The subject I have here reported upon constitutes almost the only matter of interest in the communication, and, seeing the opinion which I have been led to form respecting it, I cannot recommend the paper for publication in the "Philosophical Transactions."

(Signed)

July 7th, 1853.

W. SHARPEY.

[COPY.]

Report by Mr. Bowman on Dr. Heale's Paper.

I have had an opportunity to-day, in company with Dr. Sharpey and Dr. Heale, of examining the preparations illustrating Dr. Heale's paper and of hearing Dr. Heale's remarks and explanations, and I have no hesitation in agreeing with Dr. Sharpey in the report which he has drawn up, that the conclusions of the author are not so borne out by the preparations as to warrant the publication of the paper in the "Philosophical Transactions."

(Signed)

5, Clifford-street, Aug. 15th, 1853.

W. BOWMAN.

(LETTER FROM DR. THOMAS WILLIAMS.)

To the Editor of THE LANCET.

SIR,—The letter of Dr. Newton Heale, of Winchester, in THE LANCET of the 4th instant, has awakened in my mind slumbering recollections of some few incidents in the history of my intercourse with the Royal Society. I have presented two papers to the Royal Society, both of which have been published in the "Philosophical Transactions." These papers cost me an amount of labour, time, money, and anxiety which now, at this distance of time, I could neither venture to weigh nor to measure in the balance. When they were under reference, I suffered intensely from the agonies of several feelings:—1st, from the months of heartless delay; 2ndly, from the ice-like indifference of the Society; 3rdly, from what, in one case, I imagined to be jealousy and antagonism on the part of the referees, &c. When I presented my second paper, to which I attached the highest value, and about which I was extremely anxious, I made an appeal to Dr. Sharpey, the physiological secretary, on one point. I said that, in my paper, I had had occasion to controvert the views of certain gentlemen whom I knew to be intimately connected with the Royal Society, and from whom, therefore, in the natural course of "human nature," I anticipated that kind of opposition which proceeds from *bad personal feeling*. I requested it as a favour that my paper should *not be referred* to either of those gentlemen. Nothing could have exceeded the prompt kindness of Dr. Sharpey on that occasion, although I was to him personally unknown. Two referees were appointed who were believed to bear towards me no other than a friendly feeling. By them my paper was judged with the utmost rigour and exactness; and I travelled from here to London with various specimens and preparations in order to convince them with respect to certain points under dispute. I found it exceedingly difficult to convince them, by aid even of drawings and preparations, as regards the questions under doubt; and if I had written an account of my interview with them, I should have warmly embraced the strongest language of complaint. But shortly after, those very referees, in honour of their desire to do justice, further referred my paper to Professor Milne-Edwards, and, I believe, M. Quatrefages, in Paris, gentlemen whom they conceived to be better acquainted with the subject of my paper than they were themselves. Professor Milne-Edwards returned a favourable answer, and my paper was ordered to be published in the "Philosophical Transactions." When all this examination was gone through, of course I attached to my paper infinitely greater value than I did originally.

Now, let us review the case of Dr. Heale. He contributes a