

Correspondence.

LETTER FROM DR. WATSON TO DR. C. J. B. WILLIAMS.

DR. WATSON presents his compliments to the Editor of *THE LANCET*, and requests that he will do him the favour to admit the letter to Dr. Williams, herewith enclosed, into the next number of that journal.

Henrietta street, Cavendish-square, Dec. 1847.

DEAR DR. WILLIAMS,—More than one person came to me yesterday, to call my attention to a letter of yours published in Saturday's *LANCET*. On referring to it, I perceive that you have done great injustice to Dr. Forbes and to me. I say to us in particular, because we are the only physicians who had charge of poor Liston, both in the early and in the latter stages of his fatal illness; and this Mr. Cadge's brief, and in some points imperfect, statement, which I did not see till after it was printed in *THE LANCET*, has made known.

You are quite wrong in supposing and stating that we "found no physical signs of disease," and "therefore took a more favourable view of the case than (you) did."

From the very first, my anticipations of its ultimate event were (as many could testify) most gloomy, and so, I know, were Dr. Forbes's.

Soon after the hæmorrhage, I noticed, and recorded in my note-book,—which you are welcome to see,—some imperfection in the breath-sounds about the upper part of the left scapula, something not quite natural also at the summit of the right, and some appreciable difference in the voice in the two supra-spinal fossæ. I then went out of town, and was absent five weeks. After my return, Mr. Liston called on me, (on the 13th of November,) complaining of cough. He had then manifest shortness of breath. From that time "physical signs" were never absent. A very peculiar, rough, and loud respiratory murmur pervaded both lungs. At first, I confess I apprehended, from this sound, the rapid multiplication of small tubercles; for I had been informed that his mother, and also a sister, had died consumptive; and by these facts I interpreted the previous hæmorrhage. But very soon, Dr. Forbes and I convinced ourselves of the presence of some obstacle in the large and primary air-passages, whence the remarkable sound was propagated. At that time Mr. Liston was bled, with relief, and the blood was distinctly buffed and cupped. For three weeks at least before his death, our opinion (and that of every one else who saw him in our presence) was clear, that some mechanical impediment existed about the bifurcation of the trachea, or in the first divisions of the bronchi. The inspirations were comparatively facile—the expirations long, noisy, and growling. Some dysphagia also was detected, although the patient himself was reluctant to acknowledge it. *Possibly* (we thought) an enlarged bronchial gland, or some morbid growth, was exercising pressure there—*most probably*, an aneurismal tumour.

Of all this, if it were worth while, several witnesses might be called.

From various phrases and sentences in your letter, which I need not cite, as well as from its whole tenour, an ordinary reader, unacquainted with the real facts of the case, could not fail to infer, that *we* did not, as you had done, "warn" our patient; that *we* "countenanced his disposition to make light of his malady," and ought therefore to stand "self-reproached;" that *we* sanctioned his taking "strong exercise in riding a restive horse, which promoted expectoration"; and that had the solemn and responsible office of advising and treating this eminent surgeon been yours instead of ours, a different "result" might have been hoped for; for you close your observations with these words—"After this, I did not see him professionally, as he placed himself under the care of physicians who, both before and after this period, found no physical signs of disease, and who therefore took a more favourable view of the case than I did. The result is known, and I make no further comment on it."

Now the real truth is, (as his family can tell you,) that all our poor patient's imprudent acts were done in spite of our most earnest and reiterated remonstrances—remonstrances so urged, as sometimes to make him almost angry with us, and especially with Dr. Forbes, who, living near him, saw and expostulated with him more frequently than I had opportunity of doing. Our entreaties to him to "avoid every description of excitement and exertion" were as pressing as yours could have been, only (from circumstances) much

oftener repeated. We had even prevailed upon him, just before his first sudden attack of extreme dyspnœa, again to leave town for a while, that he might ensure the means of quietude. Much the same kind of treatment which you affirm would have been appropriate was counselled by us and adopted. He was cupped,—twice, I think,—bled twice from the arm, blistered, and kept on low diet.

In short, all the treatment that you would have recommended, we recommended; all that could be positively known concerning the disease during life, we knew as surely as yourself—viz., the certain existence of mechanical pressure about the lower part of the windpipe; and all that could be reasonably conjectured of its material cause, we saw as clearly as you did—viz., the probable existence of an aneurismal tumour.

I hope I have satisfied you that you have been greatly misinformed respecting the facts and practice upon which, directly or indirectly, you have been pleased to comment, in a tone of disparagement.

Now, was it courteous, or even fair, to publish what you have published in reference to the physicians, who could be no other than Dr. Forbes and myself, without previously ascertaining from one or the other of us whether the facts of the case really were as you understood them to be? Would it have been charitable or generous so to exhibit our mistakes, even if you were sure that we had made them? Do you indeed believe that if our lamented patient had been entrusted solely to your care, and could have been induced implicitly to obey your directions, the fatal "result" of his disease would have been prevented?

One more question I venture, in perfect amity, to propose for your calm consideration. Is it consistent with your character—with your high rank in our profession—with your office (which presents you as an example to so many) in one of our great metropolitan schools of medicine—thus publicly and needlessly, under profession of a zeal for science, to proclaim your own superior sagacity, and (by implication) the comparative ignorance or unskilfulness of others, your contemporaries, pursuing in the same place, to the best of their humbler abilities, the same vocation with yourself, and, in this instance, engaged in the peculiarly anxious duty of ministering to the relief of a professional brother? Would Baillie or Heberden have done thus?—I remain, yours truly,

THOMAS WATSON.

P.S.—I have felt compelled, in justice to Dr. Forbes, no less than to myself, to request the insertion of this letter in *THE LANCET*.

CROSS-QUESTIONS AT THE OLD BAILEY.

To the Editor of *THE LANCET*.

SIR,—In the recent trial of Sheridan, at the Central Criminal Court, for the murder of his mother, it appeared in evidence, that the old woman who washed and laid out the body of the deceased, observed the prisoner take from his waistcoat-pocket two small parcels, like "doctor's powder," and throw them into the fire, upon which they burnt up with a bluish flame: it was put to me, therefore whether arsenic would produce such a result.

Now I had been made acquainted with this piece of evidence two or three days before the trial, and had prepared myself to give an opinion upon it. I found by experiment, and the experiments were performed in the presence of many persons, that when two or three grains of white arsenic were wrapped up in a piece of paper, after the manner described, and thrown into a clear fire, the paper, on igniting, would burn up, as the old lady remarked, "with a bluish flame." I answered the question, therefore, in the affirmative, remarking that it was a very notorious fact, and one known to all chemists, that arsenic had the power of communicating a bluish-white tinge to flame, and that such a flame was known by the name of the arsenical flame; it was one of the great characteristics of the poison. Nevertheless, it was put to me by that sapient and very gentlemanlike advocate, Mr. Clarkson, whether there were not ten thousand things which would give a similar appearance, and whether there were not ten thousand things which would not. In the confusion which ever must arise out of such questions, he argued with the jury that my evidence was a piece of scientific speculation, and he got hold of a witness who was ready to assert, and who did assert, that he had tried the experiment, but without getting any blue flame whatever.

I have troubled you, Sir, with this part of the investigation, to show how the broad and important facts affirmed by a