adverted upon in your last number, I feel called upon to state—

1. That I knew nothing whatever of this case and correspondence, till a printed copy of the latter reached me a few days ago by post.

2. That the introduction of my name in the correspondence has been not only without my authority but without my knowledge.

3. That the "tariff" to which I alluded in my private correspondence with Sir Thomas Gladstone, in 1852, had reference to visits *moderately* "distant" from town, and would not apply to such special services as those of Dr. Vose in the case now in question.

I am, Sir, your obedient servant,

Charlotte-square, Edinburgh, July, 1863.

JAS. MILLER.

BARON LARREY AND COLONEL WAYMOUTH.

To the Editor of THE LANCET.

SIR,—Having seen in your paper of the 4th inst. the "anecdote of Baron Larrey" and Colonel *Weymouth*—(? Waymouth)—I beg, in justice to a brave officer, you will kindly give the same publicity to the true statement as you have done to the imputation that Colonel Waymouth was impelled to charge the French by any other cause than the command of his superior officer and "his own bravery."

The charge of the heavy cavalry at Waterloo is well known. As far as my uncle was concerned this is the case:—The Second Life Guards, in which was Lieutenant Samuel Waymouth, were dreadfully cut up. My uncle coolly and deliberately rode at the head of his troops, going into action with upwards of fifty men, but when he and they had cut through the French, he found himself with only a few troops—I think seven was the number. He tried to make his way back to the British lines, but was taken prisoner. His corporal—there are not sergeants in the Life Guards—was smashed by a canon ball, and the lieutenant was completely covered with the brains and blood of his faithful companion. I have heard my father often relate the tale, and you may depend on the truth of my statement.

I should not have troubled you with this communication, did I not feel sure that you would not willingly allow a slur to be cast on a brave man.

I remain, Sir, your obedient servant,

Lower Park-road, Peckham, July, 1863.

SAMUEL WAYMOUTH.

DR. HUGHES BENNETT'S LECTURES.

To the Editor of THE LANCET.

SIR,—I am sorry to be obliged to admit that Dr. Bennett's reply to my last leaves my mind more confused than ever in what manner his "Molecular Theory" can explain the origin of the vibrones. It is evident, however, that he will not allow them to have sprung from pre-existing "cells." I suppose he means from pre-existing masses of matter, as I particularly qualified the term "cell." If therefore they did not spring from anything pre-existing, they must of necessity have arisen from the elements of the fluid in which they are found, which, as I remarked in my former letter, is nothing more nor less than by "spontaneous generation." I am obliged therefore to end further arguments on this point, for however free I may be to accept this doctrine when proved, I must confess I am scarcely advanced enough yet to receive it. I find I was mistaken in supposing Dr. Bennett had not believed in it.

I cannot agree with the limiting law laid down by Dr. Bennett, as he thinks I shall, that a vegetable cell cannot *grow* at the same time that it is subdividing, either in the linear or in the quaternary direction; on the contrary, I am perfectly certain that it can do so from numerous observations on all sorts of vegetable cells. Nor do I see anything at all inconsistent with our physiological knowledge in the fact; but were it so, I can only say our knowledge was not in accordance with the fact. The process of subdivision by segmentation is merely a variety of the vegetable process; there is nothing to my mind in one process subversive of the other. Enlargement of bulk and the simultaneous division of it constantly takes place; the subdivision growing at the same time that separating process is being completed. Those who have observed the mode of growth of the gonidia of the algæ, lichens, mosses, and of oscillatoria, will I am sure agree with me. This is the mode of growth of

the fronds of the lower plants, and is probably the rule in all vegetative life. I might refer to the accurate and careful description of the growth of the higher cryptogamia by Hofmeister (Ray Soc., 1862), and particularly to Braur's *Rejuvenescence in Nature*, p. 209 (Ray Soc., 1853), and many other works treating of cell growth.

Dr. Bennett will remember that I did not attempt to bring the whole proofs and reasonings which render it certain that the unimpregnated uterus has contractile power. I can lay no claim to the opinion as original.

I am, Sir, your obedient servant,

J. BRAXTON HICKS, M.D., F.R.S.

St. Thomas's-street, London-bridge, July, 1863.

CHOLERA IN INDIA.

To the Editor of THE LANCET.

SIR,—With reference to your review of the Report on Cholera in India in 1861, which appeared in the LANCET of the 27th June, permit me to state that during and since the war in the Crimea I have again and again called the attention of the Army Medical Department, that of the War Office, that of the Horse Guards, and that of the India Office, to the facts, that cholera has never been studied scientifically by the Army Medical Department, that they are unacquainted with the pathology of cholera, and that the medical treatment adopted by them is contra-indicated by the pathology of cholera, and assists cholera to destroy life. The receipt of my letters was acknowledged, but nothing was done.

In the above Report which you have noticed in your number of the 27th ult., it is admitted that cholera has never been scientifically studied in India. At paragraph 427 of that Report it can be seen that the Army Medical Department were not acquainted with the pathology of cholera; and as to methods of cure adopted for this disease, it appears by paragraph 278 of that Report, that the mortality is at the rate of 80, 90, or 100 per cent.—"a mortality," says Dr. Mountjoy in that Report, "which in all probability would not occur if nature were left to herself to battle with this disease."

As cholera has again broken out at Benares, in the interest of humanity I have written to the Minister of War, placing myself at his disposal, to appear before any committee his lordship may please to appoint, and to demonstrate to that committee—

1. That the Army Medical Department have never studied cholera scientifically.

2. That they are not acquainted with the pathology of cholera.

3. That the medical treatment adopted by the Army Medical Department is contra-indicated by the pathology of the disease, and assists the disease to destroy life.

I remain, Sir, your obedient servant,

D. MACLOUGHLIN, M.D.,

Member of the Legion of Honour.

Bruton-street, Berkeley-square, July, 1863.

P.S.—Since writing the above I have been informed by the Military Secretary that the Field-Marshal, his Royal Highness the Duke of Cambridge, has been pleased to refer my offer to appear before a committee, as stated above, to Dr. Gibson, the Director-General of the Medical Department, for his opinion. Although Dr. Gibson has twice prejudged the question at issue, yet as he now sees by the Official Report on the Outbreak of Cholera in India in 1861 that he was in error, it is possible that he will not a third time mislead the authorities and prevent an inquiry which ought to have taken place forty-six years ago, and which would have saved thousands and thousands, if not millions, from untimely graves.

EXCISION OF THE HUMERUS.

To the Editor of THE LANCET.

SIR,—I am requested by Dr. Gilfillan, of Brooklyn, New York, to inquire of you whether you are aware of any recorded instances of excision of the humerus. He has successfully removed the whole shaft in a case of compound comminuted fracture, and with the best results. He has not been able to find a similar case in the records of surgery.

I am, Sir, your obedient servant,

GEORGE SHEARER, M.D.

Windsor-street, Toxteth-park, Liverpool, July, 1863.

* * No such case has, we believe, ever been recorded. A good history would be exceedingly interesting.—ED. L.