are the conclusions which I trust the majority of our profession have arrived at, "that they have tortured the latter hours of departing life." How can such latter-hour attempts border on what is laudable and praiseworthy? Further, medical men may differ much respecting the benefit attained during the late epidemic cholera, but many will be ready to aver that great benefits have been obtained: they have often felt the still pulse again to pulsate, and the cold feel of death on the skin turn to warmth and perspiration. The attempts of medical men have indeed been laudable and praiseworthy, and I as verily believe that they never torture the latter hours of departing life. Respecting Dr. Howison's allusion to venous injection—if cholera in Edinburgh has been anything like the cholera which so often temporarily, as well as lastingly, stops the pulses of the native Indians in the East, amongst whom I have been the witness of several hundred cases, I do not think that venous injection would be of any use. If there be no pulse, venous injection cannot produce one: if it beat so soft and small that a medical man would almost doubt its pulsation, venous injection would neither increase nor strengthen it: venous injection would not affect the heart when the circulation is either stopped or almost stopped. In such a state, venous injection, I think, cannot be a general stimulus; the gone powers of the system must be first restored by internal and external stimuli, and I esteem the carbonate of ammonia in solution in frequent doses one of the most certain. These remarks are purposely made brief; and not in a spirit of opposition to Dr. Howison's allusion to venous injection, for Dr. Howison's judgment on it does not appear. I rather infer that he is averse to its use. The other parts of the question have been answered, and I do think that on second thoughts, Dr. Howison would wish the latter lines of his question blotted out from your pages.—I remain, Sir, your constant reader,

THOMAS GRIGG,
(Surgeon) Madras Army.

Plymouth, March, 1833.

P. S. Has sulphate of quinine been used? I never tried it in cholera; but had I a few cases to treat, I would do so, under our present indefinite knowledge of the successful treatment of the disease.

* * Many other letters in reply to Dr. Howison have reached us, but we can find room only for two, and therefore select from among those that are authenticated by names. Our intelligent correspondent, "T. M. C." (Birmingham), will find Dr. H.'s paper at page 203, last vol.—ED. L.

NOTE FROM DR. WRIGHT.—CAUSE
OF CHOLERA.

To the Editor of THE LANCET.

SIR,—The purport of my letter to you from Leeds, on the 27th of February, which I am sorry to find has not reached your office, was as follows:—After I read Dr. O'Shaughnessy's observations in THE LANCET of the previous Saturday (23rd), I attentively reperused those parts of his valuable report to which he refers. Except in an ambiguous use of the word "cause," in the first line of page 628, I think I have not misrepresented Dr. O'Shaughnessy's opinions, even although they do not *exactly* accord with those of other professional men in the general classification my sentence assumes. Taking for granted that the primary "cause" of cholera is "unknown," it results that all we do know must be *effects*. These effects, however, in their turn, become causes of certain symptoms, and hence arises frequent ambiguity of terms in treating of the origin of disease. The real difference between Dr. O'Shaughnessy's opinions, and the view advocated in my essay, seems to be, that setting out from an "unknown cause," Dr. O'Shaughnessy lays hold of a change in the constitution of the blood as the first *tangible* characteristic of the disease. To it he ascribes its principal phenomena, and on it are based his chief indications of treatment (*vide* Report); whereas it was my object to trace the main symptoms of cholera to a source anterior to a change in the constitution of the blood, and endeavour to grapple with the pest, a step nearer what—with such professional investigation as Dr. O'Shaughnessy has carried into a search for it—will, I trust, ere long, be made manifest as its fountain head.

I am, Sir,

Your most obedient,

T. G. WRIGHT, M.D., &c.

Wakefield, April 15, 1833.

P.S. Allow me to remark, that there is an omission of a short interrogative sentence in my paper as printed in THE LANCET (Feb. 9):—In page 629, col. 1, line 24, insert—"What does experience say to this?" The date also ought to be 1832 (not 1833), for the paper was written eight or ten months since, and at the time it was printed I had left Stockton.