could not refuse to Dr. Ayre the means of pointing out to our readers, that his professional brethren who testify to the utility of his treatment of cholera are, at least, as numerous as those who have recorded their objections to it in print.—
ED. L.

The following is extracted from a note addressed to Dr. Ayre by Dr. Sutherland, General Medical Inspector under the Board of Health :—

"I have always heard your treatment placed amongst the very first and most successful, and I have deemed it my duty to say so when asked my opinion. I recommended it to the medical men of Sheffield last week. It is founded on sound principles, and if carefully used, will save lives that might otherwise be lost."

To the Editor of THE LANCET.

SIR,—The thanks of your readers are due to you for the wise resolution you record to close your pages against all personalities which may emanate from the disputants at Hull. The profession at large have no sympathy with the squabbles and jealousies of a provincial town; their concern is with *truth*, not with *party*. Happily, we know by experience that whatever tends to elucidate or establish truth, provided it be written calmly and with fairness, is sure to find ready admission into your journal.

Now, Sir, if I might be allowed, I would make two suggestions with reference to the question as to the efficacy of the small-dose calomel treatment, about which the profession is just now so divided.

The first is, that Dr. Archbald—who is, I believe, Dr. Ayre's colleague, and upon whom devolves the duty of registering all the cases of cholera treated by members of the medical staff at Hull—be requested, as a favour to your readers, to furnish you with a tabular view of the cases of collapse treated after Dr. Ayre's plan, with the results.

And the other is, that those gentlemen in other parts of the country, who state that they have tried this mode of treatment with success, be invited to look over their books, count up their cases of collapse so treated, and furnish you with similar tables.

If, for example, Drs. Crisp, Niddrie, Searle, Norris, Payne, and Badeley, and Messrs. Morley, Spong, Mitchell, O'Shea, Cox, Pickop, Murday, Pritchett, Nankivell, and Evans, who have all told us in your pages that they have more or less confidence in this treatment, would each send in to you a couple of lines or so, recording in exact numbers the results of their experience; then I think, from the facts thus brought to light, much would be done, either, on the one hand, to satisfy the profession as to the value of the treatment, or, on the other, to convince them of its inefficiency.

I remain, Sir, your obedient servant,

October, 1849.

DUBITANS.

THE TREATMENT OF CHOLERA BY CALOMEL.

To the Editor of THE LANCET.

SIR,—Having so frequently trespassed on your columns with regard to the pathology and treatment of cholera, I should not venture once more to encroach on your valuable space, did I not feel myself called on to do so, in consequence of the remarks of your correspondents "A. B. M." and others, appearing in this week's number of your journal.

"Homo" has, I should imagine, never seen a case of real Asiatic cholera—has never beheld the pulseless, collapsed victim, already half a corpse, the clammy dews of death bathing his brow, or he would not certainly advocate the do-nothing system—would not give vent to such effusions as, "Avaunt with your physic!" Assuredly, if there is a malady to which our mortal fabric is liable, which loudly calls for prompt, energetic treatment, and that perseveringly followed up, it is this awful and mysterious epidemic.

"Robertus," likewise, is evidently unacquainted with what little is known of the pathology of the disease, when he lauds the employment of stimulants with opium, and especially fomentations to the abdomen, than which nothing (according to my experience and physiological ideas) can possibly be more pernicious.

"A. B. M.," however, (in a very fair spirit,) has criticized my cases, as reported in your number of the 25th ult., and deduced conclusions therefrom, which I hope to convince him are erroneous. I am afraid that his misunderstanding has arisen in part from my own want of explicitness in the aforesaid reports. The fact being, I was of course anxious to con-

dense as much as possible, for obvious reasons; and hence was not sufficiently accurate as to the quantities of calomel administered, to prevent misconception on that head.

"A. B. M." contends that the aggregate quantity of calomel given, when small doses were exhibited, was much greater than when given in large doses. In this he is in error. When I stated, "reaction in twelve, in sixteen, or in twenty hours," I did not intend to convey that that period of time elapsed before *any* symptoms of improvement manifested themselves, or that, therefore, the remedy was administered at the same ratio during all that interval. It has always been my practice (following, in this respect, the advice of Dr. Ayre) to reduce the frequency of the dose as soon as there were evident indications of the effect of the mercury on the liver, circulation, &c.; in a word, as soon as such a diminution could be adopted with safety. "A. B. M." is consequently also in error, when he asserts that the quantity given, and the time elapsing ere its effects were visible, were great in proportion to the smallness of the dose.

Moreover, on referring to the cases of the three most speedy recoveries in my report, he will find these did not present symptoms of intense collapse such as to warrant the same frequent repetition of the dose, as in most of the other cases; and if he should ask—Why, then, did I give a larger dose? I would reply—First, because I believe large doses, *in powder*, to be more efficacious in relieving excessive cramp, when this is the most prominent symptom, (which it was in these instances;) and secondly, because it is necessary to allow for the waste consequent on this mode of exhibition.

With respect to the deaths, I beg to observe, it seems scarcely fair to quote them as instances of the failure of the small-dose system; for of the six, one had decidedly rallied, and was going on favourably, when she succumbed to the additional shock of an unfortunate complication; another was in a state of extreme collapse for eight hours before any treatment was put in force; another was equally neglected for ten hours; another perished through the ignorance of his friends; and another died, not of the cholera, but of the secondary fever, aggravated by the use of brandy and turpentine during the collapse stage.

In conclusion, Sir, I may venture to say, that my opinions of the nature and treatment of the malady remain unshaken.

I remain, Sir, yours, greatly obliged,

Bridgend, Sept. 3, 1849.

WILLIAM J. COX, M.R.C.S.

To the Editor of THE LANCET.

SIR,—Having during the past several weeks been fully engaged in attendance upon cholera, visiting upon an average fifty cases daily at home, besides attending to as many incipient cases daily at my own surgery, I trust myself in the possession of that ample share of experience which justifies observation through the medium of your inestimable journal.

I am sorry indeed to differ so materially in the estimation of those remedial measures employed by Dr. Ayre, and so much eulogized by comparatively a few other medical practitioners; but I hold the opinion, and am actuated in my practice by the same, that calomel is not only useless, but positively a murderous agent in cholera.

The calomel treatment I maintain as objectionable—First, because it increases the purging; secondly, because it is impossible to discriminate between the action of the purported remedy and that of the disease; thirdly, because time is an element of importance in the rapid development of the disease, and the tardy results of the supposed remedy; and lastly, the vast amount of calomel administered, to hundreds of grains, whether passive in itself, or actively affecting the animal economy, in certain stages involves considerations hitherto clothed in mystery.

In the repudiation of another's practice it is but just to make known my own, which I readily do for public good, and for no interest whatever to myself. My practice is based upon experience and personal observation at the bedside of the afflicted, effecting all which seemingly can be effected, obtaining advantage of time, at an early period enabling the medical attendant to determine the results of the case, and so far systematic, as to make both labour and anxiety effective and supportable under a vast pressure of engagement.

It consists, first, in the administration of an emetic of half a drachm of sulphate of zinc (which I always carry about with me ready for instant service) at any stage previous to confirmed collapse, induced by excessive evacuation; it arrests the intensity of both vomiting and purging, and the further administration of twenty to forty drops of tincture of opium after the effects of the emetic (say one hour afterwards) lays the foundation of comparative tranquillity; promotes diffusive warm

and perspiration. It appears that the spontaneous vomiting of incipient cholera does not disgorge that which is needful to be thrown off, and that an irritant calls into action such auxiliary powers, exceeding in effect all that can be possibly induced spontaneously, howsoever protracted the efforts.

The steady employment of opiates keeps the disorder for the most part under control, (say ten drops of the tincture every hour,) to which, in collapse, may be added one tablespoonful of brandy with each dose. Thus have I been enabled to maintain a reputation, of which I am in no way ashamed; and to set at defiance every other remedy boasting of superiority. The same constitutes a line of practice to be adopted in my own house, and I believe sincerely, its timely adoption in every other house, with free ventilation, would secure a success hitherto unknown. If one more sentiment I might be allowed to add, let me urge that it be publicly placarded—Open your windows a little both by day and by night; the word ventilation is not generally known in its practical application. The atmosphere is not generally regarded the great diluent of every noxious element, although the germs of the disorder were possibly to float therein. Whither shalt thou flee to breathe an element more pure?

I am, Sir, your obedient servant,
WILLIAM HENDRY, M.R.C.S., &c.

Hull, Sept. 1849.

PROPHYLACTIC AGAINST CHOLERA IN INDIA.

To the Editor of THE LANCET.

SIR,—Should you deem the accompanying extract from my Annual Report for 1848 to the Medical Board at Madras worthy of a place in your journal, you will oblige me by inserting it. I purposely avoid making any addition to the remarks then made, and should not have forwarded them for publication, but from having recently (a few weeks since) read some extracts from Dr. Billing's book on cholera, in which the same views are taken as to the cause of the disease.—I am, Sir, yours faithfully,

Madras, Aug. 1849.

F. GODFREY,
Superintending Surgeon.

"*Epidemic Cholera.*—With regard to this subject, I may briefly remark that for some years past, I have, during the prevalence of the disease, been in the habit of recommending the use of quinine (three or five grains) daily, as a prophylactic; and have never known a person, whilst thus under its influence, attacked by the disease. My experience, I may observe, has been too limited to form conclusive evidences as to its preventive influence, but offer the result of my observations with deference for the consideration of the Medical Board. On the breaking out of the disease in a regiment, the plan might be put to the test, with some degree of fairness, by having one or two companies subjected to the treatment, and noting, not only the *proportion attacked*, but also the *proportion of recoveries* under such circumstances. At all events, a trial would not be followed by any injurious effects to the persons experimented on.

"I was led to this practice from considering the poison of cholera as being analogous, if not having an affinity to, the miasmata productive of fevers, and am inclined to believe, that even if the disease occurred to individuals whilst under the influence of quinine, it would be in a mitigated form.

"With regard to the treatment of epidemic cholera itself, I have nothing here to remark—the drugs recommended have been so dissimilar, and the fatal results so nearly uniform."

THE DISCOVERY OF CELLS IN CHOLERA EVACUATIONS.

To the Editor of THE LANCET.

SIR,—In a notice in the last number of THE LANCET, respecting the discovery of certain cells in the evacuations of cholera, I observed some strictures upon my colleague, Mr. Brittan, for anticipating his fellow-labourers in the publication of that discovery.

In fairness to him, I ought to state, that he published with the concurrence both of Dr. W. Budd and myself. It would have been, perhaps, better, had the first discovery been brought forward in the shape of a report from us to the society of which we formed a sub-committee; but circumstances occurred which rendered Mr. Brittan anxious for immediate publication, and we therefore conceded to him the priority in this respect, as we considered that he was the most entitled to it, on account of his subsequent discovery of cells in the atmosphere.

I have had much reason to complain of the manner in which

the first discovery was announced in the newspapers, especially the *Morning Chronicle*, but I am happy to say, that Mr. Brittan's paper, in the *Medical Gazette*, from its fairness, has done much to cancel the injustice which was thus inflicted by other parties upon myself and the other members of the sub-committee.

I am, Sir, your obedient servant,
J. G. SWAYNE.

THE MEDICAL ASSISTANT.

To the Editor of THE LANCET.

SIR,—About a fortnight ago, a gentlemanly person called at my house in the evening, and introduced himself to me as a medical assistant, then engaged to a general practitioner in the country, and bound to go to his situation in a few days. He asked for pecuniary accommodation to a small extent, to enable him to pay the expenses of the journey, and get his clothes from his lodgings, where he was in debt. Without troubling you with all the particulars of his account of himself, which induced me to give credit to his statement, I advanced him what he thought he wanted, and requested him to call again, which he did. In the meanwhile I took care to make such inquiries as satisfied me that his difficulties were of a kind which are inseparable from the vicissitudes of the life of a medical assistant, and proceeded from no faults of his own.

The morning he was to join the gentleman who had engaged him, his agent in town received a letter, declining his services altogether, in consequence of information received from some one he had called on in the midst of his distress, before he came to me, erroneously representing his application as a form of begging, common amongst a set of impostors who infest the medical public. It is unnecessary to enter into the hardship of this new misfortune. His character was good; his distress real. The method he adopted to get assistance might be impolitic, but yet, surely, excusable; he had already passed two nights in the open streets.

There is an institution established for the express purpose of securing for governesses of good character a cheap home when out of a situation. It provides in every way for their protection against the temptations which too often press heavily upon want. The system works admirably; and my object in writing this is, to inquire whether a similar place of refuge could not be provided for the medical assistant.

For the present I leave the question to your well-known advocacy in favour of any plan likely to benefit the condition of a hard-working, useful,—nay, as the profession stands at present,—indispensable body of our fellow-creatures. The members of the profession at large are not likely to find anything better worth their attention for some time to come, and the publication of this communication in your widely-circulating periodical, will, I trust, lead to the adoption of some plan similar to that of the Governesses' Institution. It is far from creditable to us of the nineteenth century that even the meanest member of our profession should find himself, blameless as he may be, a desolate wanderer in the open streets, his means of livelihood utterly exhausted for the time, and gone for ever the moment he confides his misfortunes to those whom he has spent his last days in serving; and in evidence that this is no more than the truth, I adduce the subject of this letter.

I remain, Sir, your obedient servant,
Dorset-place, Dorset-square.
HENRY SAVAGE.

APOTHECARIES AND SURGEONS.

To the Editor of THE LANCET.

SIR,—At the present time there are more than 1500 members of the College of Surgeons, who may be fairly classed under the head of general practitioners, though not possessing any licence from the Apothecaries' Company; more than sixty of whom are surgeons to some of the various Unions. Now it is quite evident that all these 1500 individuals, many of them of long standing in the profession, are liable to be convicted at the County Courts for illegally practising; such is the law. A M.R.C.S. may prescribe in any case, (in fact, the great majority of the pure London surgeons exist principally by attending families,) yet if he dispenses his prescriptions (an act which any man without the slightest qualification whatever can always do by putting Chemist and Druggist over a shop-door) he is liable to a prosecution! It would be useless to enlarge upon the absurdity of this law. If the surgeon has no right to dispense medicine, he certainly has no right to prescribe.

But there are a great many surgeons who would be willing to become licentiates of the Apothecaries' Company, if they could be admitted to an examination, which, as the law now