

their necessity, and with a view not only to the present utility of the Society but of its permanent safety. The Society wants nothing more than the practical recognition of its value on the part of the profession, to render it not only a highly successful undertaking, but an example worthy of imitation.

I am, etc.,

ALEXANDER HENRY,
Secretary to the Medical Provident Society.

15, George Street, Portman Square, W., October 21st, 1865.

QUANTUM AND QUALE.

SIR,—It is always pleasant for a critic to find that his remarks have had influence; and it may even be a satisfaction to him, if attacked in turn, to find that the weapon used is drawn from his own armoury.

I see that Dr. Anstie, in the *Lancet*, has taken what he considers an opportunity of returning to you, with all the air of a man who has discovered a novelty, the lesson which he learned from your pages before "Mill on Hamilton" was published. It certainly was not familiar to him before, or we should not have had his work on *Stimulants and Narcotics*, which, from first to last, is full of confusion on the point he urges against you.

Anstie and Mill.

"I commend to the attention of the writer in the journal alluded to, the considerations touched upon in the following passage of Mr. Mill's examination of Sir Wm. Hamilton's philosophy.

"One of the commonest modes of loose thought and sources of error, both in opinion and practice, is to overlook the importance of quantities. Mathematicians and chemists are taught by the whole course of their studies, that the most fundamental differences of quality depend on some very slight difference of proportionate quantity," etc."

From first to last, agent and action are confused by Dr. Anstie, through neglect of this important principle, that the action is the *quale*, dependent on the *quantum* of the agent. One sentence out of many may be taken to prove this. At page 181, he proposes "that agents which produce excessive and morbid action be refused the name of stimulants, even though small doses of them may act in a truly stimulant manner."

If Dr. Anstie would try and re-write his book with Mill's and your critic's remark constantly in view, the result would be, not a second edition, but a new work.

I am, etc., SARTOR RESARTUS.

Ireland, September 27th, 1865.

TREATMENT OF PHTHISIS.

LETTER FROM G. BODINGTON, L.R.C.P.Ed.

SIR,—By this day's post I venture to send you a copy of my Essay on the Treatment of Consumption, published in the year 1840. I do so in consequence of seeing in the *BRITISH MEDICAL JOURNAL* of September 23rd, the announcement of a work on the

same subject by Dr. MacCormac. Allow me to intimate to you, that in my work the principle is declared (and for anything I know to the contrary for the first time) that in phthisis and other diseases of debility the pulse is reduced in velocity by the administration of certain stimulants, as wine, etc. This doctrine was condemned by the critics in the medical journals of the day. Subsequently, however, the late Dr. Todd carried the hypothesis to an absurd length, and exaggerated in practice a theoretical truth, until it became suspected of being a mischievous error.

Adhering to my original principle, I claim to be exonerated from the extravagant views held by Dr. Todd on this subject. I announced also in the *Essay* that the terms and their meaning "phlogistic and antiphlogistic" would come to be expunged from medical literature, and I believe and hope they are so. At the time I wrote, they constituted the common language of the profession. Will you allow a humble member of that profession the opportunity of thus attempting to vindicate in the *BRITISH MEDICAL JOURNAL* his earlier views and published opinions?

I am, etc., GEO. BODINGTON.

Sutton Coldfield, October 7th, 1865.

[Dr. Bodington's Treatise gives him much more credit than he claims for it. It is a most sensible and practical Essay. The rational principles of the treatment of the disease which are accepted as orthodox at the present moment will be found there laid down in it twenty-five years ago. Dr. Bodington repudiated the treatment of consumption with "a meagre diet of vegetables, rice and water, aided by tartarised antimony: I should recommend to one thus consuming away, under the influence of this *wasting disease*, a nutritious diet of mild, fresh animal, and farinaceous food, aided by the stimulus of a proper quantity of wine."]

THE TREATMENT OF CHOLERA.

SIR,—Will you allow a former worker amidst epidemic cholera to corroborate Dr. Bullar's recent testimony, concerning the value of a hot bath with mustard in the treatment of cases of that disease?

Observation of the malady in 1849 led to a belief, which increasing experience strengthened, that, during the stage of "collapse", the external application of heat and friction was extremely beneficial; that it was, indeed, a necessary and most effectual method of treatment when it was performed properly—i. e., zealously, assiduously, perseveringly. At the same time, the chief aim in the administration of internal medicines seemed to be the *maintenance of life* until the patients could get rid of the poison which they had imbibed, and which was the cause of all the symptoms of disease. Ammonia, chloric ether, etc., with small quantities of ice, therefore, were given according to circumstances and the different exigencies of particular cases.

The mode in which heat and friction may be applied must be regulated by varying conditions. This note has been written merely in order that the principle on which the treatment mentioned by Dr. Bullar was founded may be affirmed and kept in view on one side; and that, on the other, an indiscriminate use of the hot bath may not occasion disappointment and a consequent depreciation of a very valuable remedy. The use of a bath cannot be recommended always. Whenever it is tried, however, its efficacy will depend greatly on the care with which the water is made very hot, and on the mixture of a large quantity of good mustard with the water.

In hospitals, or in well furnished houses, such a treatment as that which Dr. Bullar has described

will be very beneficial; but the method will not have a fair trial amongst those who are the chief sufferers during an epidemic of cholera. Amongst the poor, it is impossible to procure the proper means of success—hot water, flannel, blankets, nurses, good beds, etc. In practice amidst such people, recourse must be had to friction and dry heat.

In the course of no malady, probably, has there been a larger proportion—in few has there been an equal proportion—of deaths which (if we may use the phrase) ought not to have taken place, which might have been averted by careful nursing and assiduous treatment—the very advantages which it is, unfortunately, almost impossible to ensure in attempts to withstand the influence of a malady so rapid in its progress.

I am, etc., W.

THE YELLOW FEVER IN SWANSEA.

LETTER FROM GEORGE PADLEY, M.D.

SIR.—Two letters have appeared in your JOURNAL from Dr. John Rose Cormack, calling in question the propriety of designating as “yellow fever” a disease which lately made its appearance in this place (but which happily now no longer exists), and attempting to trace its origin to causes of a local nature, the result of an assumed neglect of the hygienic conditions of the town. Whatever view may be taken as to the essential nature of the malady, his latter position is altogether untenable.

A ship leaves a port in the West Indies where yellow fever is endemic. Shortly after her departure, a disease breaks out on board, having the well-known characters of that fever, and several of the crew die from it on the passage. On her arrival in port, one man suffering from the same disease is carried on shore, and dies the same evening. A few days afterwards, a man working in the neighbourhood of the ship, and who had been on board, was seized with symptoms similar to those which had affected the crew, but differing from any of the forms of fever which had heretofore fallen under the notice of the medical practitioners of the town. This case, which proved fatal after a few days' illness, I saw. It is unnecessary to occupy your space by detailing the symptoms. They fully accorded with the description of the disease given in standard authorities on the subject.

If Dr. Cormack would inform us what, according to his view, really constitutes yellow fever,—what pathognomonic conditions there are which would enable us to distinguish cases having the history and symptoms of those we have lately had to deal with, from yellow fever, or, as the term is objected to, from the disease under that name which appears in tropical countries,—if he would mention any symptom or symptoms essential to that fever which, we might then find, these cases had not, we should have something tangible to assist us, assuming we are wrong, in coming to a right conclusion. The question of the identity of this disease with true tropical yellow fever will probably be dealt with by abler hands than mine. Dr. Buchanan, commissioned by the Privy Council, has been here; has investigated with great zeal and ability all the circumstances connected with the appearance, extension, and characters of the disease; has pronounced it to be specific yellow fever; and has repeated this opinion at the last meeting of the Pathological Society.

There is one point, however, on which I wish to say a word or two. It is with respect to the “important items of truth” which are stated to “have oozed out” since Dr. Cormack's first letter; and the “explanation of the whole matter” which he expects

to find in the “recent disclosures regarding the state of Swansea when they (‘the yellow cases’) occurred.” The answer to the above is a brief one—viz., that the “items” alluded to are not those of truth; that the “disclosures” are either without foundation or are greatly exaggerated; and that the arguments based upon them are therefore worthless. Immense sums of money have of late years been expended upon the drainage and water supply of the town, which is, I believe, upon the whole, at least as well furnished with these sanitary essentials as any town of its size in the United Kingdom. The water, which is of the best and purest kind, is now brought in abundance from large and well-constructed reservoirs eight miles distant; and the system of drainage is ample and formed upon the most approved principles, such as exist in but few other towns in the kingdom. It is to be regretted that Dr. Cormack should, in supposed corroboration of his views, so hastily have adopted the statements of a newspaper paragraph, the production, it is said, of a person whose interest centres in a rival port; and who has either never visited our town, or whose ideas of the points of the compass must be somewhat confused. The “Sandfields” spoken of (certainly not situated in the “north-west”) is a new portion of the town which has rapidly sprung up, the drainage and other sanitary arrangements, of which have not yet been completed—it may, indeed, be admitted that they have been too long delayed; but it so happens that in this very district, thus singled out, there has not been one instance of the disease referred to; so that here also Dr. Cormack's argument fails. That in a town rapidly increasing in extent and population, as this has done, there should be room for improvement in its sanitary arrangements is not surprising; and some parts of it would doubtless be much benefited by a more active supervision; but we are certainly not worse off in this respect than other towns similarly circumstanced, and much better off than many. The impression intended to be produced that the town is the especial abode of “slow fever and malignant diseases of all shapes and kinds,” would be as unfounded as are most of the assertions contained in the paragraph which Dr. Cormack has quoted and commented upon. The disease, unquestionably imported by the *Hecla*, cannot be said to have “spread” here, limited as it was to a few cases, and it would probably never have extended beyond the single case landed, had it not been for the exceptional circumstance of the almost tropical heat which prevailed at and before that time.

The relation which existed between the fever, whatever view may be taken of its nature, and the arrival of the *Hecla*, is to my mind too evident to be disputed; and the “good people of Swansea” had certainly no thought or desire to make the ship a “scapegoat” for their shortcomings, as imputed to them by Dr. Cormack. They have, indeed, had no interest or anxiety in proving the outbreak (if such a term can be justly applied to so small a number of cases) to have been that of genuine “yellow fever.” If it can be shown on sufficient evidence to be anything else, we shall be both ready and willing to accept the truth and to submit to the correction.

There is one fact of importance which ought not to be omitted—viz., that there has been no instance of the disease having been communicated to any person who has not been more or less engaged within a limited area near the spot where the ship was first moored. It is, therefore, greatly to be regretted that there should have been so great a panic in consequence of a few cases of disease thus restricted, and, as the result both now and formerly has shown, incapable of extension in this climate.

Oftentimes, especially in such matters, “out of evil